Antoinette M.G.A. WinklerPrins
Kent Mathewson *Editors*

Forest, Field, and Fallow

Selections by William M. Denevan

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Foreword

If I'd had any idea who William M. Denevan was or what he had accomplished, I doubt I would have had the nerve to contact him. But I didn't, so I did.

At the time I was working on a news article about new archaeological findings in the Amazon, and I had seen Denevan's name somewhere. He responded courteously to my initial note, which will surprise nobody who has ever met him. This courtesy extended to his responses, when I called, to what I presume were embarrassingly uninformed questions.

Denevan's name kept cropping up, in multiple different contexts, as I continued working on my article. When I turned in the first draft, the editor objected to my description of him, which was something like "a University of Wisconsin researcher who is an expert in the archaeology, ecology, and historical demography of Latin America." That's a mouthful, the editor complained. I said that Denevan was, professionally, a professor of geography. My editor said that readers wouldn't understand everything Denevan had studied from the single word "geographer," and returned to my original phrasing. At the last minute, to save space, a copy-editor substituted "archaeologist." I didn't notice the change, and the incorrect descriptor was published. Exasperated letters, arriving in quantity from exasperated geographers, alerted me to the error.

In a way, the editor's confusion was merited. Denevan's fingerprints can be found in so many disciplines—from archaeology to agriculture, from ecology to demography, from area studies to plant science—that it both seems foolish to label him simply as a geographer and impossible to describe him as anything else. His most prominent research concerns Amazonia, but he has also made important contributions in Central America, the Andean highlands, Mexico, and the US Southwest. Unusually, his work extends from the empirical descriptions of new phenomena to continent-wide syntheses of data to theories of culture and landscape. It is hard to imagine that the same person wrote what is probably the first scholarly warning about Amazonian deforestation, a ground-breaking reevaluation of tropical agriculture, and the definitive challenge to the once-ubiquitous "pristine myth" (a myth that he named). But that is what Denevan did.

Denevan describes himself modestly as continuing in the tradition of geography begun at Berkeley in the 1920s by Carl O. Sauer—"the dean of American historical geography," as he is often called. (Denevan co-edited an anthology of Sauer's work in 2009; 6 years later, he helped complete a biography of Sauer by Michael Williams, who passed away before finishing it.) In Sauer's vision, geography was, as Geoffrey Martin put it, "the biography of human impress on the face of the earth." Like human biographies, the geographical biography was a web of particular, contingent, historical influences rather than the inevitable outcome of universal laws. To write that biography, Sauer brought into being at Berkeley a carefully woven interdisciplinary school that traced the interactions of humankind and the physical environment through time and space. Although Denevan's graduate advisor was Sauer's disciple, James Parsons, Denevan took numerous classes and seminars from Sauer. He followed Sauer's concern: the human use of the environment.

Sauer cast his net widely but is best known for his work on Mexico. Similarly, Denevan touched on many areas but his most renowned inquiries were in the greater Amazon basin. Perhaps as much as any other researcher, Denevan recast our understanding of Amazonia, transforming it from a trackless, unstoried wilderness into a humanized landscape with a history as rich and complex as the French countryside that inspired Marc Bloch.

Denevan's Amazon work is highlighted in this book, as it should be. But no book can capture his influence as a teacher and mentor. I was not one of his many students—the third generation of the Berkeley school, as they are sometimes called. But I caught a glimpse of what it meant to be his student years ago, when he kindly offered to review the draft manuscript of what would become my book on Native Americans before 1492. I sent off the manuscript with less trepidation than I should have felt, because past experience had suggested to me that people like him did not have time to read books like mine. In this case, I was quite mistaken. A surprisingly short time later, I received a copy of my manuscript that was peppered with marginal notes in his tiny, super-legible handwriting. His suggestions were clear, thoughtful, practical, and kindly put. They ranged from catching typos to querying arguments to suggesting a complete, wall-to-wall rewrite of this or that section. He even had some (very useful) suggestions for formatting endnotes. It was one of the best, most thorough edits I have ever received. I thought, "How does he find time to do this sort of thing?" It was not the last time that this question occurred to me.

It is also hard for a book to evoke Denevan's generosity, modesty, and humor, or the respect of his friends, colleagues, and students. I first grasped some of this more than a decade ago, when I happened to be at his table at the keynote session of a geography meeting. Sitting with us were several of Denevan's former students and colleagues, all of them prominent researchers with distinguished careers who had spent long periods in remote, uncomfortable places. The keynote address was by a well-known researcher. It amounted to a spirited defense of totalizing laws of human development and environmental determinism—exactly the type of thinking that Sauer had devoted his career to fighting. In my recollection, Denevan listened politely. At the end of the talk, he turned to the table and said, "Well, I haven't heard that for a while!" Everyone laughed—despite the mild tone, the dismissal was withering. But I noticed that all of these well-known people were looking at each other as they laughed. And I realized that despite their august status in academia all of them were thinking, “Boy, I hope he never says that about me!”

As I write this foreword, I am making tentative plans to visit Denevan in his home in northern California. He is retired now, but I know he will ask me in detail about what I am working on. And I will still be anxious about his reaction. “Boy, I hope he is not going to say that about me!”

Charles C. Mann

Introduction

Abstract This volume is a tribute to both William M. Denevan and to the many scholars—geographers as well as kindred scholars in a dozen different discipline—that he has mentored, influenced, and collaborated with over the course of his long career. Through presentation of examples of his published work, this collection chronicles and catalogues the 70-year career of one of Latin Americanist geography's most productive and committed practitioners. Three of the entries provide biographical background and personal reflection on this career. These three contributions are buttressed by a Preface by noted Journalist/Scholar Charles Mann and a "Complete Chronological Bibliography" of his works. The meat of the matter is presented in seven topical sections featuring two selection of representative reprinted publications. Each topical section has an equally representative "Introducer" providing context and insight on the import and impact of the selections. An eighth section, also with a distinguished Introducer, showcases three of his "Synthetic Contributions," demonstrating that Denevan's trademark empirical bent could be leavened from time to time with more theoretical explorations. This volume promises to provide current and future students and scholars of Latin American life and landscapes with a collection of studies and commentaries that will stand time's tests, and a vede mecum that points the way in the direction of geography's most exemplary traditions.

Keywords Adaptation · Environment · Landscape · New World · Indigenous knowledge · Scholarship

This collection honors William "Bill" Maxfield Denevan for a lifetime of service to his chosen disciplines, his close colleagues and students, and to his many contributions to fields of study that can only partially be subsumed in the labels "cultural and historical ecology." The book offers a wider audience an ample sample of Denevan's work and writing, both the solid data collected, but also the paradigm pushing insights and findings that have yielded new ways of seeing the New World's tropical peoples and landscapes, past and present. And why not provide a forum for some of his closest colleagues and former students to comment on what Denevan has done, what he has published, and what it means? For more than 60 years, Bill Denevan has been revisiting old footpaths, blazing new trails, and pointing out best places to stop and view the larger panoramas and implications of his subjects of study. We invite the reader to either follow along sequentially or just jump in as the contributors take measure and comment on Denevan's publications[1] in the eight thematic areas in which he has been the most active and prolific.

What makes this book valuable in the twenty-first century is that it is not simply a tribute to a retired scholar, it is a volume wherein active scholars reflect on and demonstrate the continued impact of Denevan's thinking; how ground-breaking and ahead of its time it was, and how it subsequently redirected lines of inquiry. It also demonstrates Denevan's range across topics, locations, and time-periods. Additionally, the themes highlighted in the book are ever more relevant in this age of accelerating global environmental change, necessitating a reconsideration of the relationship between humans and the physical environment in the Anthropocene (ArchaeGlobe Project 2019). Fundamentally Denevan's work examines the role of humans in crafting landscapes—his interests were always focused on the reciprocal relationship between humans and the physical environmental and the range of human capacity to overcome environmental adversity. Denevan was a pioneer in examining human adaptation to environmental challenges, as for example, examining agricultural techniques that yielded harvests in places with too much or too little water. In his writings Denevan provided deep insight into, and respect for, indigenous strategies that worked with natural processes to live off the land sustainably. We desperately need reminders of the many ways humans have done this in the past, and could do again, if only we were to think more holistically about the human–environmental relationship as Denevan does. This volume is meant as a guide and resource for students and other scholars, young and old, interested in the way conceptualizations advance over time.

Bill Denevan sitting on his favorite bench near his home at Sea Ranch, CA, September 2018. (Photographs by Antoinette WinklerPrins)

[1] Corrections, updates, some deletions, and clarifications have been made to the texts of all the original articles and some figures were removed or rearranged for the purposes of this volume. A few pertinent references have been added. Only Chapters 3.1 and 3.2 were significantly reduced from the original; 3.1 is presented as an excerpt, and 3.2 has the section on the Bora removed as that is covered in 6.2.

Every publishing project has its own history. Like many edited collections, it was long in gestation and even longer in delivery. The idea of putting together a tribute volume was first discussed more than 20 years ago, by Greg Knapp and Kent Mathewson. It was to be a rather conventional *festschrift*, but with a large number of short original essays. The idea was in keeping them short, and inviting dozens of contributors, it could not go wrong. But it did. It did not go very far. A little while later, Dan Gade proposed an ambitious plan for a *festschrift* around the idea of a series of long "state of the art" review essays on the subfields that Denevan is identified with. The focus was to be less on Denevan's work per se, and more on progress-to-date in various subfields reflective of Denevan's example and influence. Gade recruited former Denevan student Oliver Coomes to co-edit the proposed book. That did not get off the ground either. The third iteration has not exactly been charmed, but it has survived the test of time. The current project began appropriately enough during a coffee break at the Conference of Latin Americanist Geographers (CLAG) meeting in Bogotá in late May 2010. Antoinette WinklerPrins proposed the idea of doing a tribute volume for Bill Denevan. Knowing the history of at least two previous attempts, we realized that a different tack was needed for this attempt to reach conclusion. We decided to model our project after the tribute volume for fellow Berkeley geographer Homer Ashmann (Pasqualetti 1997). The Ashmann volume features eight themed parts, with three to five of Ashmann's publications accompanied with introductory essays by Ashmann's close colleagues and former students. Not only is it a convenient collection of some of Ashmann's most representative work, the introductory essays offer short expert statements on the themes as well as Ashmann's contributions to them. Accordingly, this is what we have attempted to do with this collection—provide a set of authoritative statements on Denevan's main topical pursuits and illustrate these interests with a selection of publications for each. The editors have no good excuses for the time it has taken to bring the volume into print, other than that this project was one of many we are engaged with. We are tempted to invoke Carl O. Sauer's famous admonition—"locomotion must be slow, the slower the better." But that was meant as wise counsel concerning moving about in the field, not bringing books to press. Nor should the other contributors be implicated. While the essays did not pop into our email inboxes overnight, neither did they require much badgering before they were submitted. One casualty that our pace has occasioned, and we keenly regret, is that two of the original invited contributors, Dan Gade and Bill Woods, did not live to see the published book. Dan, perhaps primed from his earlier run at this project, or simply being "*ya mismo*" Dan, was the first—by a number of months—to submit his contribution. As it turned out, it was not quite in the format prescribed, i.e., it was heavily biographical along with addressing Denevan's cultural biogeographical interests in general. A separate Denevan biographical sketch had always been planned, so to avoid redundancy, Dan's essay has been edited and included as a separate tribute piece. To fill in for Dan's introductory section piece, we invited Karl Zimmerer, Bill's former colleague in the geography department at the University of Wisconsin, Madison, an Andeanist and cultural biogeographer like Gade. Similarly, Bill Woods was slated to write the introductory essay on indigenous ecology, especially soils. Various mishaps in the field and maladies at home prevented him from completing his introduction. His Amazonianist colleague Charles Clement kindly stepped in to do the indigenous ecology introductory piece. The rest of the contributors have not jumped ship. Their forbearance is strongly acknowledged and greatly appreciated.

While the table of contents tells much about what follows, and the Notes on Contributors provide snapshots of the participants, some mention of how each is connected to, or located within, the larger collective Denevan *tableau vivant* seems appropriate. And also a few words about their contribution and the publications that accommodate it. In order of appearance then, along with co-authoring this introduction and editing the introductory essays, Kent authored the biographical essay on Bill Denevan. Denevan must be commended for not just providing abundant material to work with—hardly surprising for those who know Bill—he went well beyond this in producing a 20-page autobiographical statement for our use and to pass on to his children. Needless to say, it was invaluable in piecing together the essay. Would that every scholar that expects memorialization, either with a tribute document and/or an obituary, think to do this while most of materials and memories are retrievable! Kent's own stepping into the Denevan tableau dates to early 1973, when he returned from travels in Mexico to enter graduate school and work with him at University of Wisconsin, Madison. He soon found that Denevan hospitality was legend. Bill and wife Susie insisted he stay with them until he found his own place. Not only were his graduate students treated "as family," but for Kent, joining the Denevan camp meant a place (albeit a twig) on the Carl Sauer genealogical tree. Kent, having had something of a conversion experience to Sauerian cultural geography as an undergraduate, nothing could have been better. Yet, this affiliation was not simply conferred, it had to be earned—by degrees. Kent's master's project was on irrigated, raised bed horticulture in highland Guatemala, under the initial suggestion/direction of B.L. Turner, II, fellow Denevan student, and provider of a reconnaissance trip to Mesoamerica to scout out potential field projects. Kent's dissertation was on ancient raised fields in coastal Ecuador. The initial Guayas raised fields had been discovered by James J. Parsons, Denevan's Ph.D. advisor, thus Kent's "academic grandfather." With both projects Kent was solidly on Denevanian terrain.

Antoinette WinklerPrins is what they call in Latin America "*la autora intelectual*" of this project—one who provides the idea and inspiration. In Antoinette's case, she has also shared in aspects of the planning and execution. Besides co-authoring this introduction, Antoinette painstakingly worked to streamline all items into a whole and compiled the complete bibliography of Denevan's publications, although, as with the biographical essay, Denevan's assistance and editing was instrumental. This task is quite apropos as Antoinette was Bill's hourly administrative assistant when she started her Ph.D. at Madison in 1990 and worked for 2 years helping Bill with "word-processing" many manuscripts and learning Bill's inscrutable attention to detail in all matters of citation and bibliographic content. She has become a stickler ever since. Antoinette is one of many "sort-of" Bill students, circulating in his aura and inspired by his ideas. She started as an Amazonianist with Bill but ultimately worked on her dissertation with Karl Zimmerer. Bill inspired Antoinette to pursue questions of soils and agriculture on the Amazon River floodplain in the Brazilian Amazon, and later to investigate the formation of Amazonian dark earths in homegardens.

The rest of the cast of contributors includes specialists in Denevan-style geography, archaeology, and tropical agroecology. Each has connections with Denevan, in most cases spanning decades. Whether as his students, colleagues in conference settings, with him "out in the field," or through collaboration and correspondence, all have come to know him as a totally committed and conscientious scholar of his multiple, though largely interconnected, areas of research. In choosing eight themes to display and comment on some of the published results of these interests, we feel we have covered the core. There may be a few peripheral topics that escaped attention here, but these should be evident in the complete bibliography of his publications we have included. The Foreword is written by Charles C. Mann, award winning journalist and science writer. Mann is perhaps best known for his best-selling twin books *1491: New Revelations of the Americas Before Columbus* (2005) and *1493: Uncovering the New World Columbus Created* (2011). Most of the contributors to this volume either make cameo appearances in the books, or their research areas are integral to what Mann relates in these superb accounts of human-environment interactions on both sides of the Columbian Divide. A more fitting and astute foreword contributor could not be asked for.

Historical geographer W. George Lovell leads off the topical sections and commentaries with his introduction to Denevan's contributions to Historical Demography. Denevan's synthetic compilations of New World aboriginal population numbers count as among his most widely known work. In meticulously reviewing previous estimates and proposing new figures, his estimates are generally seen as the standard against which all others must be measured. Lovell's work has been primarily focused on Guatemala, but their paths crisscross at conferences, in publications, and Lovell is a loyal attendee at the periodic "Friends-of-Denevan" gatherings at the Denevan retirement compound at Sea Ranch, Sonoma County, California. Like Lovell, archaeologist Clark L. Erickson was not formally a Denevan student, but while still doing graduate fieldwork in highland Peru became enlisted as one of Denevan's many informal mentees. Both Erickson's dissertation research in Peru and his subsequent work in lowland Bolivia have concentrated on ancient raised fields. As many might contend, Denevan's single most significant research contribution has been his "discovery" of raised fields in the Llanos de Mojos of Bolivia, and then serving essentially as the "*padrino*" of all subsequent raised field studies in the Americas since the mid-1960s. For the Mojos theatre of this multidisciplinary/ multinational research arena, Clark Erickson has very ably continued what Denevan initiated. Erickson introduces the part on Agricultural Landforms.

Karl Zimmerer, geographer, agrobiodiversity specialist, and political ecologist, introduces the Cultural Plant Geography part. Like Denevan, Zimmerer's dissertation research at Berkeley was under the direction of James J. Parsons but separated by several decades. They were also faculty colleagues at the University of Wisconsin, Madison before Denevan's retirement in 1994. Zimmerer stepped in on short notice to comment on the cultural plant geography pieces as Dan Gade's replacement. Like Denevan, much of Zimmerer's research has been in Peru and Bolivia, and they share with Parsons, a cultural geographic approach to plant geography studies. Susanna B. Hecht is yet another Berkeley geography Ph.D. graduate, whose graduate research on ranching and landscape degradation in Amazonia followed up Denevan's

1973 controversial if prescient denunciation of Amazonian development schemes. While Hecht's main haunts have been in the Brazilian Amazon, her work there has been complementary in various ways to Denevan's pursuits in South and Central America. Here she introduces the part on Human Environmental Impacts. Charles R. Clement, botanist and agroecologist, is also primarily a Brazilian Amazonianist, based at the Instituto Nacional de Pesquisas da Amazônia in Manaus. His research topics include plant domestication, agroforestry, and dark earths, or anthropic soil formation in Amazonia. The last two topics have been among Denevan's later career concerns. As mentioned previously, Clement introduces the part on Indigenous Ecology, originally slated for Bill Woods. Christine Padoch, ecological anthropologist and economic botanist, introduces the part on Tropical Agriculture. Padoch's research on agroforestry and tropical farming has focused on Amazonia and South East Asia. She has collaborated with Denevan in the field, as well as at the University of Wisconsin, Madison where she was faculty member in the Institute for Environmental Studies and the Department of Anthropology before a long career at the New York Botanical Garden.

The final two introducers/commentators are directly in the Denevan line, both "genealogically" and topically. Bill Doolittle introduces the Livestock and Landscape part and wrote his dissertation under the supervision of Denevan Ph.D. student, B.L. Turner, II. Doolittle invited Rick Hunter, student of his student Andrew Sluyter, to co-author the final version. Beyond questions of livestock and colonial landscapes, Doolittle's studies of pre-European agricultural systems in Mexico and North America extend aspects of Denevan's research interests into these regions. As is appropriate, B.L. Turner, II serves as both the specialist and the synthesizer in introducing and commenting on the Synthetic Contributions part. Turner studied with Denevan at the University of Wisconsin, Madison, producing a paradigm-shaking (if not fully shifting) dissertation on ancient Maya intensive agriculture. Since his early career, Turner's research interests and involvements have multiplied into a number of arenas, most recently Land Use/Land Cover analysis, and sustainability science. The two other contributors are also Denevan Ph.D. students, Greg Knapp and Dan Gade. Knapp speaks for the larger cohort of Denevan students who are not directly represented in this volume with his "Being a Student of Bill Denevan." Both Knapp's master's research—on "sunken fields" in coastal Peru, and his dissertation on raised fields in highland Ecuador are clearly in the Denevan mold. His subsequent research has continued this trajectory, though with some nods to contemporary developments. In a sense, Dan Gade is given the last word. He was actually Denevan's first Ph.D. mentee, though Denevan shared as in "shared" the dissertation directorship with senior Wisconsin Latin Americanist Henry Sterling. Gade, of all the contributors enlisted here, had perhaps the widest array of research interests, and certainly the most far-flung research sites, including the Americas from top to bottom, Mediterranean Europe, and East Africa. But among the many topics he researched and published on, especially those grounded in his favored Andean landscapes, the Denevan convergences are well evident.

As always with a book project, acknowledging all those who have helped along the way presents a challenge. The circles of assistance, encouragement, and inspiration might be best viewed as concentric. At the core is the man himself—William

Maxfield Denevan. Without his decades' long labors, there would be nothing to republish, nothing to comment on, and nothing to celebrate. He also was instrumental in editing the scanned articles to be sure that the conversion was accurate. Also, in this inner circle are the other contributors to the volume. In these pages each have paid tribute to, but also in their own careers pursued, the kind of research that is indelibly stamped "Denevan." The second circle includes a number of "behind the scenes" contributors, whose manual and mental assistance was literally "instrumental." First, all the diffuse and hard to specify material support and personnel at Louisiana State University, especially the graduate students who helped transcribe the publications from photocopies to Word files. In chronological order they include: Joseph Powell, Jessica Sims, Michel Pujazon, Jamie Digilormo, Ashley Allen, and Brett Spencer. Second, the time afforded to Antoinette during the 2019 lapse in government funding helped the project consolidate. Third, all the various editorial efforts and assistance provided by the staff at Springer Publishers, along with the labor that actually went into manufacturing the book and distributing it. In a third and even wider circle, an effectively limitless assemblage of individuals and influences could be acknowledged—all that have aided, directly and indirectly, the careers of Denevan and each contributor—from mentors and field informants to family members and friends that were always and simply there. And finally, there is a fourth circle: those "scholarly figures in the firmament" that have served to inspire and guide. Foremost, we dedicate this book to the memory of Dan Gade and Bill Woods. But it is also dedicated to the memory of those—and each contributor will have several figures in mind (no doubt many in common)—that pointed the way.

K. Mathewson
A. M. G. A. WinklerPrins

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The editing of this book was produced while Antoinette WinklerPrins worked at the the National Science Foundation. Any opinion, finding, conclusions, or recommendations expressed in this material are those of the authors and editors and do not necessarily reflect the views of the National Science Foundation.

Contents

Notes on Contributors

Charles R. Clement is an ethnobotanist and historical ecologist and researcher at the Instituto Nacional de Pesquisas da Amazônia (INPA, the National Institute of Amazonian Research) in Manaus, Brazil. He is an authority on Amazonian plants and human uses. He is the co-editor of *Selected species and strategies to enhance income generation from Amazonian forests* (U.N. 1993) and other publications on Amazonian plant domestication and crop diversity, especially as related to prehistoric settlements and dark earths.

William E. Doolittle is the Erich W. Zimmermann Regents Professor of Geography and the Environment at the University of Texas at Austin. His research has focused on ancient, historical, and present-day irrigation and agricultural landscapes in the Iberian world. He is the author of *Pre-Hispanic occupance in the Valley of Sonora, Mexico: Archaeological confirmation of early Spanish reports* (Arizona 1988), *Canal irrigation in prehistoric Mexico: The sequence of technological change* (Texas 1990), and *Cultivated landscapes of native North America* (Oxford 2004).

Clark L. Erickson is Professor of Anthropology at the University of Pennsylvania. He is interested in how archaeology can provide a long-term perspective on environmental change, biodiversity, and sustainable management. He is co-editor of *Time and complexity in historical ecology: Studies in the Neotropical lowlands* (2006) and *Landscapes of movement: Trails, paths, and roads in anthropological perspective* (2009).

Daniel W. Gade (deceased) was Professor Emeritus of Geography at the University of Vermont. His main research interests were in the fields of cultural and historical geography, as well as ethnobotany, cultural ecology, and mountain research. He is the author of *Plants, man and the land in the Vilcanota Valley of Peru* (W. Junk 1975), *Nature and culture in the Andes* (Wisconsin 1999), *Curiosity, inquiry, and the geographical imagination* (Peter Lang 2011), and *Spell of the Urubamba: Anthropogeographical essays on an Andean Valley in space and time* (Springer 2015).

Susanna B. Hecht is Professor in the Luskin School of Public Affairs and the Institute of the Environment at the University of California at Los Angeles and Professor of International History at Graduate Institute for International Development Studies, Geneva. She has focused on the intersections of economies, cultures, and land use and the socio-environmental effects of these processes. She is the co-author of *The fate of the forest: Developers, destroyers, and defenders of the Amazon* (Verso 1989), author of *The scramble for the Amazon and the lost paradise of Euclides da Cunha* (Chicago 2013), and co-editor of *The social lives of forests: Past, present, and future of woodland resurgence*.

Richard Hunter is an independent scholar, a former doctoral student of William Doolittle. His research focuses on the spatial patterns and environmental consequences of colonial land use change in Mexico as well as the linkages between historical land use changes and the Holocene. He has published in a variety of journals including *Local Environment*, *The Holocene*, *Rangelands*, and the *Journal of Historical Geography*.

Gregory W. Knapp is Associate Professor of Geography and the Environment at the University of Texas at Austin. His research has focused on adaptive dynamics of prehistoric and traditional agriculture in Peru and Ecuador, and more recently on agricultural modernization in the Andes, especially floriculture. Among other books he is the author of *Andean ecology: Adaptive dynamics in Ecuador* (Westview 1991), co-editor of *Human impact on mountains* (Rowman & Littlefield 1988), and editor of *Latin America in the twenty-first century: Challenges and solutions* (Texas 2002).

W. George Lovell is Professor of Geography at Queen's University in Canada. His research relates to an abiding interest in the nature of the colonial experience in Latin America, especially Guatemala, and also how it reflects on the present. Among many other books he is author of *Conquest and survival in Colonial Guatemala: A historical geography of the Cuchumatán Highlands, 1500-1821* (Queens 1985) and *A beauty that hurts: Life and death in Guatemala* (Texas 1995), co-author of *Demography and empire: A guide to the population history of Spanish Central America, 1500-1821* (Westview 1995), and co-editor of *Secret judgments of God: Old world disease in colonial Spanish America* (Oklahoma 1992).

Charles C. Mann is a journalist and science writer and a contributing editor for *Science*, *The Atlantic Monthly*, and *Wired*. Among his many subjects are medicine, history of science, environmental degradation, endangered species, the Internet, and the environmental history of the New World discoveries. Among other books, he is the author of *1491: The Americans before Columbus* (Knopf 2005), *1493: Uncovering the New World Columbus created* (Knopf 2011) and *The wizard and the prophet: Two remarkable scientists and their dueling visions to shape tomorrow's world* (Knopf 2018) and co-author of *Noah's choice: The future of endangered species* (Knopf 1995).

Kent Mathewson is retired Fred B. Kniffen Professor of Geography and Anthropology at Louisiana State University. His research has focused on agricultural landscapes, plants and people, and the history of geography. Among other books, he is the author of *Irrigation horticulture in highland Guatemala* (Westview 1984) and co-editor of *Re-reading cultural geography* (Texas 1993), *Dangerous harvest: Drug plants and the transformation of indigenous landscapes* (Oxford 2004), and *Carl Sauer on culture and landscape: Readings and commentaries* (2009).

Christine Padoch is a Senior Curator Emerita in the Institute of Economic Botany of the New York Botanical Garden. From 2011 to 2017 she was the Director of Research on Forests and Human Well-Being at the Center for International Forestry Research (CIFOR). An anthropologist by training, she spent more than 40 years carrying out research on smallholder patterns of forest management, agriculture, and agroforestry in the humid tropics, principally in Amazonia and Southeast Asia. Previous to her position at CIFOR, Padoch was the Matthew Calbraith Perry Curator of Economic Botany at the NYBG. She is the author or editor of a dozen books and of approximately 100 scientific articles and book chapters. Christine Padoch has served as a scientific advisor to many international projects and has been a member of the boards of several international research institutions, including the Center for International Forestry Research (CIFOR), the Amazon Institute for Environmental Research (IPAM), and the Earth Innovation Institute (EII). She holds a Ph.D. from Columbia University.

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1.0 Historical Demography

Introducer: W. George Lovell

Abstract In the context of the lifework of William M. Denevan, an interest in historical demography, especially the size of indigenous populations in the New World prior to Old World intrusion, began early and lasted long. Two selections from his teeming oeuvre see him grappling with the issue at mid-career and toward its close, a remarkable intellectual trajectory this collection rightly celebrates. The first selection, "Native American Populations in 1492: Recent Research and a Revised Hemispheric Estimate" (1992), sees him at his synthesizing best, revisiting New World contact scenarios he first addressed a quarter-century earlier. His 1976 hemispheric estimate of 57.3 million he adjusts downward to 53.9 million, a reduction of some 3.4 million. The second selection, "The Native Population of Amazonia in 1492 Reconsidered" (2003), again sees him ruminate on how many inhabitants the region may have supported at first contact. His astute, assiduous reckoning indicates some five to six million for Greater Amazonia and at least three to four million for the Amazon basin itself.

Keywords Historical demography · Native American populations · New World Pre-European America

Introduction

Ideas that change how we think and truly alter our views of the world are few and far between. One such idea burrowed its way into the mind of Carl O. Sauer (1889–1975) when the great geographer was in his intellectual prime. Having dedicated himself, with customary aplomb, to reconstructing patterns of land and life in Northwestern Mexico during pre-European times, Sauer (1935, 32) sums up his research findings as follows:

> The record, as interpreted, gives an aboriginal population between Gila and Río Grande de Santiago in excess of half a million, almost three-fourths of the number now living in this part of Mexico. Bit by bit, the theme has obtruded itself that aboriginal rural populations and present ones are much the same. This, I believe, is not a sensational conclusion, but a quite natural one.

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What Sauer concluded, and subsequently advocated beyond the confines of Northwestern Mexico, *was* in fact "sensational" in terms of conventional thinking eight decades ago, namely, that culture and civilization had evolved in the New World prior to European intrusion so as, in certain favored regions, to sustain sizable human populations, ones of a magnitude and social complexity hitherto seldom invoked. Sauer's notion, and the assiduous manner in which it was reached, fired the imagination of William M. Denevan when he was a graduate student in geography at the University of California, Berkeley. His curiosity fueled even farther by the miscellaneous mindset of his doctoral supervisor, James J. Parsons (1915–1997), himself a doctoral charge of Sauer's, Denevan embarked on aboriginal population studies of his own, one focus of a career trajectory of considerable note. Two contributions to that element of his distinguished career are here featured and commented upon: "Native American Populations in 1492: Recent Research and a Revised Hemispheric Estimate," published in 1992, and "The Native Population of Amazonia in 1492 Reconsidered," published in 2003.

In the second of the two readings, Denevan charts the beginning of the trail, in which another of Sauer's associates, Woodrow W. Borah (1912–1999), played a key role. Borah's invitation "to present a paper at a symposium he was organizing on historical demography" resulted in Denevan's transition "from being an interested observer to center stage of the great debate over the size of native American population numbers at the time of Columbus." The overture was timely. Denevan informs us that, though he "took no courses from Borah" while a graduate student at Berkeley, "in 1963 I discussed my doctoral research on Mojos in the Bolivian Amazon with him." In accepting the invitation, made in 1965 in relation to a congress that would convene the following year in Argentina, Denevan told Borah that he "would prepare a paper on Mojos for his Mar del Plata symposium." The paper would draw on "a chapter of my dissertation" that "examined the native population" according to accounts furnished of it by Jesuit missionaries. This original intention, however, was set aside and instead, "being young and bold," Denevan "developed a speculative statement on western Amazonia," which he later published in its own right, not as part of the conference proceedings (Denevan 1970). The stage was felicitously set.

In the paper he prepared for presentation in Mar del Plata, the population estimate that Denevan advanced entailed (1) generating "a few local population densities for each of the major habitats in Amazonia," (2) averaging them out, and (3) multiplying "each resulting habitat density times the total land area…to get total estimated populations for each habitat." Six habitats were identified: floodplain (*várzea*), lowland savanna, upland savanna, low *selva*, high *selva*, and coast. After deriving an estimate of 369,000 for the Peruvian Amazon and 664,000 for the Bolivian Amazon, a combined total of 1,033,000, the intrepid researcher (brimming with youthful confidence) extended his "questionable method" to include "all of Greater Amazonia (tropical interior of South America)," thereby coming up with an "overall density of 0.59 per square kilometer and a total population of 5,750,000." How was the exercise received? "Although Borah was quite supportive," Denevan reveals, "there was little response to my method or results at the symposium," nor in its aftermath "to the published version." Not quite the calm before the storm, but a lull nonetheless.

The foray led Denevan to contemplate a volume in which he would assemble already published pieces "on the Indian demography debate," an initiative he pursued with the backing of both Borah and Sauer, the latter rightfully acknowledged as "the founder of the Berkeley School of historical demography" (Denevan 1996). His approaches to publishers proved fruitless, but he doggedly soldiered on. In the end his persistence elicited interest "in a collection of mostly original articles, which I edited." Thus materialized *The Native Population of the Americas in 1492*, first published in 1976 and reprised (as well as revised) to coincide 16 years later with the Columbus quincentenary, an event that sealed the book's benchmark status and has kept it in print ever since. From it is excerpted the first of our two readings.

Estimating the Unknown – Or the Unknowable?

"Change," Evelyn Waugh muses in *Brideshead Revisited* (1945), "is the only evidence of life." Denevan, one suspects, figured that out early on in his career and has remained loyal to the realization ever since. It comes as no surprise, therefore, that, when the second edition of *The Native Population of the Americas in 1492* appeared, Denevan chose to begin it with an essay in which, comprehensively and systematically, he reviewed earlier assessments to lower his 1976 hemispheric estimate from 57.3 million to approximately 53.9 million, a reduction of some 3.4 million (Table 1.1) in Denevan's "Native American Populations in 1492: A Revised Hemispheric Estimate. The basis upon which his revised hemispheric estimate is offered leans on a supplementary bibliography of some 140 titles, from which he culls, region by region, new data available for North America, Mexico, Central America, Hispaniola, the Andes, and Amazonia. Keeping up with this teeming, at times giddying field of research is no easy task, but Denevan has done so in typical stalwart fashion.

North America drops by 610,000 in Denevan's revised schema, despite "a provocative argument" by Henry F. Dobyns (1983), who champions a contact population of a lofty 18 million; a "heated debate" is noted between Dobyns and several other researchers, among them Dean R. Snow and William A. Starna (1989) and David Henige (1998), in significant part because Dobyns asserts that epidemic disease "spread widely and decimated Indian tribes prior to initial counts and estimates of numbers." The latter camp claims that "there is little or no evidence for such declines." George R. Milner (1980) "agrees that there were epidemics in the Southeast prior to 1700" but contends that "those diseases did not necessarily spread throughout North America." Denevan, however, takes pains to include the views of Ann Ramenofsky (1987), who casts her lot with Dobyns, maintaining that depopulation "tended to precede written documentation and was catastrophic in nature."

For Mexico, studies begun at Berkeley by Sherburne F. Cook and Lesley B. Simpson (1948) and thereafter developed by Cook and Borah, "as well as by William Sanders and others," resulted in contact population estimates shifting "from low to high figures." In the case of Central Mexico alone, the range was from a substantial 11 to a staggering 25 million. For this region, influenced by William T. Sanders (1976), B. H. Slicher van Bath (1978), Thomas M. Whitmore (1991), and Rudolph A. Zambardino (1980, 1981), Denevan adjusts his 1976 estimate downward by more than 4.5 million, settling on 13,839,000, "which may still be too high." The findings of Peter Gerhard (1979) cause him to decrease his estimate for Chiapas by 525,000, yet to increase his estimate for North Mexico by 680,000, based not only on the industry of Gerhard (1982) but also the endeavors of Daniel Reff (1991).

Central America is adjusted downward too, but only by 25,000. In the decade and a half between Denevan's first and second editions, "there has been more original research on the historical demography of Central America than on Mexico" – or any other region of the Americas, it could be argued. The sterling work of Linda A. Newson (1986, 1987) is drawn upon to raise Honduras by 100,000 (including 50,000 "added on for Belize") but to lower Nicaragua by 175,000. For El Salvador, the investigations of Fowler Jr. (1988) are deemed to warrant an increase of 250,000. Perhaps the most intriguing of Denevan's calculations for Central America pertain to Guatemala, where little work prior to 1976 was undertaken on indigenous population history. Denevan (1976, 291) somehow came up with a figure of two million, attained by what he calls "comparative" reckoning, defined as "an estimate based on incomplete documentary figures, on other forms of evidence, and on comparisons with comparable regions with better information." For Guatemala south of the Petén rainforest, my own research with William R. Swezey and Christopher H. Lutz, based on myriad subregional studies conducted in the interim with much better data than Denevan initially had, also generated a figure of two million (Lovell and Swezey 1982; Lovell et al. 2013). Convergence as opposed to divergence of opinion thus allows Denevan to record "no change" for Southern Guatemala, a designation similarly afforded Costa Rica, though it has yet to be afforded the same attention as Guatemala in that scholarly regard.

Hispaniola continues to spark the most controversy, on account of Columbus himself playing a prominent role in its ruination and because the variation in contact estimates for it is so vast. Sauer's *The Early Spanish Main* (1966), in which the Admiral of the Ocean Sea is given scrupulous treatment, remains the gold standard against which all studies in the historical geography of Latin America are measured. The sources consulted by Sauer were more or less the same as those at hand for numerous other researchers; yet Charles Verlinden (1968) managed to compute 60,000 from them, while Cook and Borah (1971, 407) arrived at eight million. "Who, among those born in the centuries to come," asked Bartolomé de las Casas ([1516–1561] 1957–1961, 2, 106) most presciently at the time, "will believe this?" The Dominican friar had his own reasons for overlooking the disease factor as a means of explaining the erosion of native lives, stressing instead Spanish cruelty and excess. Until recently, there was no convincing argument or available documentation to link precipitous indigenous demise in Hispaniola to Old World epidemic intrusion early on. Lately, however, evidence has come to light that indicates either swine fever (Guerra 1985) or smallpox (N.D. Cook 2003) as having been present in Hispaniola as early as 1493, thus changing the contact scenario, and the numbers involved in immediate disease-propelled death, considerably. For his revised calculations, Denevan had access to the clinical sleuthing of medical historian Francisco Guerra, but not that of the ever-alert Cook, whose revelations of smallpox from hitherto unknown correspondence of Columbus have been authenticated by Juan Gil and Consuelo Varela (1997): the word *viruelas*, identification as unequivocal as it was lethal, is used in relation to "one of the four Indians taken [to Spain] the year before, who was put back ashore [near Samaná], unlike the [three] others who had died of smallpox after we sailed from Cádiz." Had the evidence for smallpox been available when Denevan was undertaking his 1992 revision, quite possibly he would not have lowered his tally for Hispaniola by as much as he did (950,000). Similarly, his reckoning might also be different had he been able to peruse a recent study by Massimo Livi Bacci (2013), the final chapter (if surely not the final say) in a volume edited by Frank Moya Pons and Rosario Flores Paz (2013) that deals entirely with Hispaniola's inhabitants at the time of first contact and in its fateful wake. As it transpires, Denevan opts for the estimate of one million proposed by the British mathematician Zambardino (1978), a figure close to that of Guerra's and "the oft-repeated figure" (Sauer 1966, 65–69) of 1,100,000, the number that Las Casas was told by Archbishop Deza of Seville had been mentioned to him when he was discussing matters with Columbus.

As with Hispaniola, so for the Andes do studies that post-date Denevan's 1992 revisions justify further scrutiny of his tallies. Newson (1995, 2003) would certainly offer him food for further thought on Ecuador, and Juan and Villamarin and Villamarin (2003) for Colombia. Venezuela – Argentina and Chile for that matter too – still has to catch a historical demographer's critical eye; Denevan, for instance, judges the Villamarins' contact estimate for Venezuela of 200,000 to 350,000, advanced in 1975, "much too low." Any grappling with Peru, he well recognizes, is indebted to the labors of N.D. Cook (1981). Cook's estimate of 14 million for the Central Andes – the former Inca Empire, encompassing present-day Bolivia and Ecuador, and highland and coastal Peru – Denevan trims, however, to 11,696,000, though that figure is still over four million more than his 1976 appraisal. For Peru, Cook offers four projections of between 5.5 and 9.4 million before settling on an estimate of nine million.

Return to the Amazon

Amazonia as an integral part of the first reading, in effect, is reappraised by Denevan himself in our second reading, affording him the benefit not only of research findings published after 1976 but also those that appeared in the decade between 1992 and 2002. The solicitation to contribute a paper to a special issue of the *Revista de*

Indias, one that paid homage to Woodrow Borah 4 years after his death, was indeed propitious. Besides allowing Denevan the opportunity to cast Borah in a warm personal light – "young historians told me that they had found Borah to be intimidating, but that was not my experience" – and influential professional guise, responding to the overture of guest editor Nicolás Sánchez-Albornóz, himself one of Spain's most eminent men of letters, moved Denevan to reflect on a lifetime of research on Amazonia. The result is a rare piece of stock taking – expansive but grounded, sweeping yet focused, simultaneously empirical and theoretical, retrospective and forward looking, above all testimony to voracious reading, incisive critique, committed engagement, and (in the end) continued self-questioning and probing as opposed to providing definitive, final deliberations.

Denevan recaps as follows: (1) his 1966 Mar del Plata estimate, 5.75 million for Greater Amazonia, he initially adjusted upward in 1976 to 6.8 million before lowering it to 5.1 million, to allow for a "buffer effect" that applied to "zones of no man's lands between Indian groups," which he reckoned at 25 percent; (2) his 5.1 million he then raised to 5.7 million in the second edition (Denevan 1992) of his landmark edited volume, taking into account a buffer zone not of 25 but of 15 percent; and (3) reconsideration of better data for *várzea*, *terra firme*, and savanna locales leads him to "reject the habitat-density method I used in the past to estimate a Greater Amazonian population in 1492 of from 5.1 to 6.8 million." He concludes: "There was too much variability in each habitat to be able to formulate meaningful average densities on the basis of a few sample densities."

At this juncture a wry sense of humor creeps in. "So what," Denevan wonders, "would my mentor on historical demography, Woodrow Borah, say? Thirty-five years of trying to estimate the native Amazonian population in 1492, and now Denevan gives up!" Fortunately for geography and the social and natural sciences writ large, he hasn't. While an epiphany of sorts may elude him, perspective "from all these years of attempting to make estimates" has not. "This perspective," he writes, "which I believe permits more than just guesswork, gives me confidence that the Indian population in 1492 was indeed *at least* 5 to 6 million for Greater Amazonia and at least 3-4 million for the Amazon basin." [He later, in 2014, raised the total to eight to ten million.] Does that same confidence, looking back, extend to his 1992 estimates of the native population of all the Americas?

Another Mar del Plata forum is called for.

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1.1 Native American Populations in 1492: A Revised Hemispheric Estimate

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Abstract In 1976 I reviewed the various estimates of Native American populations at the time of European arrival. Based on my evaluations of those estimates, I derived a total hemispheric population of 57 million, with a range of from 43 to 72 million. Subsequently in 1992 I examined the considerable new relevant demographic research, and as a result revised the total downward to 54 million, with a range of from 43 to 65 million.

Keywords Historical demography · Native American populations · Pre-European America

Introduction

This year, 1992, is witnessing an international reexamination of the significance of the "discovery" of the New World in 1492. The size of the native population at the time of Columbus has a bearing on many of the themes of exploration, conquest, settlement, labor, food production, environmental modification, and demographic decline that are central to colonial history. The question of Native American numbers remains highly controversial, one of the great debates in history. Were there few or many? Did Europeans discover, occupy, and fill in relatively empty lands, or did they invade and destroy a world as Bartolomé de Las Casas said, "full of people, like a hive of bees" (in MacNutt 1909).

In 1976, in the first edition of *The Native Population of the Americas in 1492*, I presented some original essays on the topic by authorities, provided regional reviews of methodologies and estimates, and gave my own regional estimates and a hemispheric total of 57.3 million (Denevan 1976a, 291).

Since publication, most of the individual chapters have been cited frequently, some favorably, some less so. There were at least 20 reviews, including several by historical demographers.[1] These reviews and subsequent research are indicative of the continuing importance of the topic and the stimulus of the 1976 volume. The original regional introductions and the eight individual essays are reproduced intact in the 1992 edition.

What has happened to the authors of these essays since 1976? Woodrow Borah, the dean of Indian demographic studies, has retired from Berkeley but remains actively involved (Borah 1976b, 1991a, b; Cook and Borah 1971–1979). Ángel Rosenblat, after a lifetime of researching the topic of Indian numbers and defending his low estimates, died recently in Caracas. In a letter in 1977, he expressed appreciation for the opportunity to present his case in English. David Radell and Jane Pyle have left academia, somewhat unaware of the impact of their contributions to the great debate. Douglas Ubelaker remains at the Smithsonian Institution, still very much involved with estimates of North American populations (Ubelaker 1981, 1988, 1992). William Sanders, Pennsylvania State University, is firmly established as one of the principal archaeologists of Mesoamerica; his demographic interests continue (Sanders and Murdy 1982). Daniel Shea at Beloit College has been working on the archaeology of terracing in the Colca Valley of Peru (Shea 1997). And I have persisted with cultural-ecological research in the Peruvian Andes and Amazonia (Denevan 1987; Denevan and Padoch 1988). We thank the University of Wisconsin Press for this opportunity to have our research and thinking from the 1970s reheard and reexamined.

Following is a brief review of the considerable amount of research and commentary published since 1976 on Native American populations at contact and on their subsequent decline. The matter of the rate and degree of decline, especially from disease, is significant for estimating original numbers, and this has received substantial attention. General discussions of epidemics include Cook and Lovell (1991), Crosby Jr. (1976, 1991), Guerra (1986), Henige (1986b, c), Joralemon (1982), Lovell (1992b), and Verano and Ubelaker (1992). Crosby Jr (1996) examines the well-documented Hawaiian depopulation from epidemics (from possibly 800,000 to only 48,000 in 100 years) as a model for a rapid Native American decline. He also notes the importance of considering a reduced birth rate as well as a high death rate.

[1] Reviews include Blakely (1979), Cook (1977), Crosby Jr (1978), Dobyns (1978), Fowler (1978), Hudson (1978), Jacobs (1978), Lombardi (1977), Lurie (1977), Mörner (1979), Paull (1978), Sánchez-Albornoz (1977), Quirk (1977), Smith (1979), Swann (1978), Stewart (1984), West (1978), and Zelinsky (1978).

North America

Subsequent to the pioneer studies in the 1920s and 1930s by Sauer (Denevan 1996b), Mooney (1910), and Kroeber (1934), there were few immediate attempts to provide regional and tribal estimates of Indian populations in North America. The one major exception was the research on California Indians by S.F. Cook, much of which was republished in a single volume after his death (S.F. Cook 1976a).

Since 1976 there has been a resurgence of interest in Indian numbers, with controversy particularly over the role of disease in the proto-historic period. There have been more recent publications on Indian demography for North America than for any other region of the New World. Besides numerous articles, reviews, and commentaries, several books have been published: Dobyns (1983), Ramenofsky (1987), Reff (1991), and Thornton (1987). Most of the articles in Verano and Ubelaker (1992) are on North America. The stimulus for this activity likely includes Ubelaker's review of Mooney's figures (1976a) and his separate estimate (1976b); the tribal estimates prepared for the *Handbook of North American Indians* (Sturtevant 1978–1996; see Ubelaker 1988); and a provocative argument by Dobyns (1983), 42, 300) that there were 722,000 Timucuan Indians in Florida in 1517 and 18 million in North America, based on evidence for early pandemic diseases.[2] A heated debate has thus ensued, mainly between Dobyns and several archaeologists and historians, as to whether epidemics spreading widely and rapidly decimated Indian tribes prior to initial counts and estimates of numbers. Maintaining that there is little or no evidence for such declines are Henige (1986a, b, 1989), Snow and Lanphear (1988, 1989), and Snow and Starna (1989). Dobyns (1988, 1989a, b) subsequently defended his position. Milner (1980) agrees that there were epidemics in the Southeast prior to 1700, with significant demographic and social impacts, but maintains that those diseases did not necessarily spread throughout North America. Smith (1987) presents archaeological evidence of massive depopulation in the Southeast in the sixteenth century. Ramenofsky (1987), 173–76), who calls for a greater contribution by archaeologists to the debate, agrees with Dobyns that population decline "tended to precede written documentation and was catastrophic in nature." Disease was definitely a sixteenth-century factor in the Southeast, but she believes that a broader impact elsewhere is "probable" and that there was a differential rate of survival regionally. Upham (1986, 1987) and Reff (1987, 1989) discuss the dating of the introduction of smallpox in the Southwest. In a superb treatment of the documents, Reff finds evidence of numerous early epidemics in the Southwest "prior to sustained contact with Europeans"; he believes that "native populations throughout the Greater Southwest were reduced by upwards of 90 percent prior to 1678" (Reff 1991, 276–277). The controversy was discussed at the 1989 Smithsonian Institution Conference on "Disease and Demography in the Americas" (Verano and Ubelaker 1992; see also Roberts 1989).

[2] A listing of reviews of Dobyns' controversial 1983 book is provided by Henige (1986b, 307).

One of the main means of estimating Indian populations at contact for North America, as well as elsewhere, is working backward on the basis of mortality rates from epidemic disease. Thornton et al. (1991) point out that this assumes no population recovery between epidemics or additional decline from other factors between epidemics. "Most populations have a surprising ability to recover from severe high mortality" episodes (Thornton et al. 1991, 29). Thus, earlier population size may be greatly overestimated. On the other hand, additional mortality factors and indirect effects of epidemics such as decreased fertility and food shortages could minimize recovery. Kay (1984) presents evidence for the Sauk, Fox, Menominee, and Winnebago in Wisconsin that population growth between 1700 and 1840 increased rather than decreased. Other tribes for which population apparently increased in the nineteenth century, at least temporarily, were the Navajo and Gila River Pima (Meister 1976) and the northern plains tribes in Canada (Decker 1991, 387).

Recent regional and tribal estimates of contact populations include, for New England, 72,000 (S.F. Cook 1976b, 84), 105,200 (Snow 1980, 35), and 126,000 to 144,000 (Salisbury 1982, 26–27); for the Mohawk, 13,700 to 17,000 (Snow 1980, 41); for the Virginia Algonquin, 14,300 to 22,300 (Feest 1973, 74); for the Arikara, 30,000, and for the Pawnee, 100,000 (Holder 1970, 30); and for the Iroquois, 20,000 (Trigger 1976, 98; see also Engelbrecht 1987). Clermont (1980), however, estimated a total Iroquoian population of 110,000. Trigger (1985, 234) recalculated the pre-epidemic Huron population to have been 23,500, but Dickinson (1980) gives 25,000 to 30,000 in 1600. For the Canadian Maritime provinces, Miller (1976, 1982) estimates the late prehistoric Micmac population at 50,000, but Snow (1980, 36) has only 12,000. Trigger (1985, 229–251) provides an overview of estimates for southeastern Canada and New England and the evidence for early epidemics. Riley (1987) provides a good review of the various estimates for tribes in the Southwest. Reff (1991, 229), on the basis of reported baptisms, estimates over 100,000 Pueblo Indians in 1598. Other new tribal estimates appear in the 17 volumes of the *Handbook of North American Indians* (Sturtevant 1978–1996). Ubelaker (1988, 291) provides regional estimates mainly based on these.

In 1976, Ubelaker (1976b), using available *Handbook* data, estimated a total Indian population for North America at contact of 2,171,125. In 1988, with more tribal information available, he revised this downward to 1,894,350 for the year 1500. The tribal estimates, however, are still primarily for the times of initial contact and often later and assume little or no prior decline from larger populations. This is a very conservative position, even granting that epidemics were localized in the sixteenth century. A doubling of Ubelaker's total to 3.79 million is a reasonable minimum estimate for 1500 AD; this is what I did to obtain my estimate of 4.40 million (1976a, 291). This doubling is modest and is supported by Reff's (1991, 276) 90 percent decline prior to 1678. One other recent estimate for all of North America is seven million plus by Thornton (1987, 32). Earlier he gave 1,845,183 for just the United States in 1492, assuming simple linear decline (Thornton and Marsh-Thornton 1981, 51).

Finally, Ubelaker (1981) provides a useful discussion of methodologies for estimating prehistoric demography, and Dobyns (1976) reviews some of the studies of North American Indian populations. Johansson (1982) provides a bibliographic essay and raises important issues such as the distinction between physical and cultural extinction.

Mexico

Central Mexico was the focus of Indian demographic research from 1948 through the mid-1970s by Berkeley scholars Lesley Simpson, Sherburne Cook, and Woodrow Borah, as well as by William Sanders and others. Their estimates, ranging from 11 to 25 million, were instrumental in shifting New World population estimates from low to high figures. Subsequently, there has been little new research, but there have been several critiques and revisions of the Berkeley calculations.

Rudolph Zambardino (1980, 1981), a British mathematician, has done a systematic examination of the methodology and estimates of Borah and Cook (1963). He finds their 2.65 million for 1568 reasonable but reduces their figures for 1548 from 6.3 million to 3.6 million and adjusts their figure for 1532 from 16.87 million to a meaningless 2.7 to 35 million and for 1518 (based on Aztec tribute data) from 25.2 million to an also meaningless range of 2.2 to 28 million. Based on extrapolation from his own figures, Zambardino arrived at an estimate for 1518 of six million, or a range of from 5 to ten million, which is supported by “the evidence gathered and presented by Borah and Cook far more accurately than their estimate of 25 million” (Zambardino 1980, 22). He later suggests that eight to ten million is credible for Central Mexico (Zambardino 1981, 240).

Whitmore (1991, 477), using computer simulation models, obtains a 1519 population for the Basin of Mexico of 1,590,000. This compares to 2,960,000 derived from Borah and Cook (Whitmore 1991, 466) and 1,155,000 (averaged) by Sanders (1976, 130). If Whitmore’s total for the Basin is projected to Central Mexico, his total would be 13,536,000 (Whitmore 1991, 466). For all of Mexico, “using a scaling procedure,” Whitmore gives a figure of 16 million. For the Basin, he uses a nadir of 180,000 for 1607, a decline of about 90 percent from 1519 (Whitmore 1991, 477, 483).

Another recalculation of the Indian population of Central Mexico based on the Borah and Cook data is by Slicher van Bath (1978). By adjusting the factor for converting tributary counts to total populations, he reduces the Borah and Cook total of 25.2 million for 1518 by 15 percent to obtain 21.42 million, a number still too high for some interested scholars.

Henige also criticizes Borah and Cook for their methodology for deriving a 1519 population from Aztec tribute data: “I find the methods adopted by Borah and Cook for Central Mexico even less acceptable than their results” (Henige 1978b, 711). Another critique is that of Jacques Houdaille (1986), who refers to the “résultats sensationnels de l’école de Berkeley.” On the other hand, Dobyns continues to find Borah and Cook conservative. He recently increased his original (1966) estimate for Central Mexico from 30.0–37.5 million to 58,178,666 for 1492 and 51,600,000 for 1516, no method given (Dobyns 1988, 9).

Elsewhere, Gerhard (1979, 160, 169) using archival documents and other sources obtains contact populations for several regions of southern Mexico: for Soconusco, 80,000, and for Chiapas, 275,000. Wasserstrom (1983, 11) gives 200,000 for Chiapas, and Gasco (1987) has 67,500 to 90,000 for Soconusco. Watson (1990, 243, 377) estimates 350,000 for Chiapas, but his evidence is not presented. Pollard and Gorenstein (1980, 276–277) calculate 60,000 to 100,000 in the Lake Pátzcuaro Basin in Michoacán, compared to a projected 210,000 for Borah and Cook. Doolittle (1988, 55) has 15,000 for the Valley of Sonora. For Baja California Robert Jackson (1981) estimates 60,000 Indians in 1697 and then examines the evidence for rapid decline of mission Indians in Baja California. Finally, Reff's (1991) tribal totals for Northwest Mexico in 1500 come to over one million (Sinaloa, Durango, Chihuahua, Sonora, and Baja California), and Gerhard (1982, 24) estimates 1,218,000 for Northwest Mexico and 340,000 for Northeast Mexico.

Central America

Since 1976 there has been more original research on the historical demography of Central America than on Mexico. Two historical geographers have been particularly active – George Lovell on Guatemala and Linda Newson on Honduras and Nicaragua. A collection of relevant papers for Guatemala is provided by Carmack et al. (1982).

Lovell examined the Cuchumatán Highlands of northwest Guatemala, and based on size of Indian armies and partial tribute counts, he suggests a total of 260,000 in 1520 (Lovell 1981a, b, 1982, 1992a). For other regions of Guatemala, Veblen (1977, 497), in a study derived from his 1974 dissertation, obtains 60,000 to 150,000 Indians for Totonicapán in 1520. Zamora Acosta (1983, 318) gives 210,000 for western Guatemala for 1524 and 315,000 for 1520. Fowler (1989, 151) gives 100,000 for the Pipil area. Madigan (1976, 176–206) and Orellana (1984, 142) give 48,000 for Atitlán.

For Guatemala south of the Petén lowlands, Lovell and colleagues examined data and estimates for the various regions and estimated a total of two million, comparable to my (1976, 291) two million for all of Guatemala, including the nearly empty north (Lovell and Swezey 1982, 75; Lovell et al. 1984; Lovell and Lutz 1992; Lovell and Lutz 1995). Lovell (1991) also examines disease and depopulation in Guatemala.

Other estimates for Guatemala include 500,000 to 800,000 by Sanders and Murdy (1982, 32) for the highlands only. This is for 1524 and does not take into account the earlier devastating epidemics (possibly smallpox) of 1519–1521 (Lovell 1992b). Solano (1974, 70) gives 300,000, but the area included is unclear and his analysis is unreliable (Lovell and Lutz 1995).

For Honduras, Newson (1981, 1986, 90–91) obtains 800,000 Indians at contact; for Nicaragua she obtains 825,000 (Newson 1982, 1987, 88); and Fowler Jr. (1988, 114) obtains 700,000 to 800,000 for EI Salvador, all totals which are fairly close to my (1976, 291) 750,000, 1000,000, and 500,000, respectively. Radell (1976, 74) estimates that 448,000 Nicaraguan Indians were shipped as slaves to

Panama and Peru between 1527 and 1548. Newson (1987, 105) gives a range from 200,000 to 500,000, the high figure being for slaves shipped from all Central America. Sherman (1979, 82) estimated that only 50,000 Indian slaves were shipped from all of Central America, but MacLeod (1973, 52) earlier estimated at least 200,000. Fowler (1989, 151) has 100,000 to 140,000 for the Nicarao in western Nicaragua in 1519.

There have been several reexaminations of the population of all of Central America (minus Panama), for which Rosenblat obtained a total of only 800,000 (1976, 3). There are general discussions by MacLeod (1985), Newson (1985), and Lutz and Lovell (1995). The latter obtain 5,105,000 including Chiapas and Soconusco (now in Mexico), which compares with my (1976, 291) total which would be 5,450,000 for the same area. Sherman (1979, 5) suggests only 2,250,000. Lutz and Lovell (1995) provide a useful annotated bibliography of publications on the historical demography of Central America.

Hispaniola

The Caribbean Island of Hispaniola continues to be a focus of heated demographic controversy, in part because it involves Columbus himself and in part because of the great extremes in the estimates by modem scholars: 100,000–120,000 for 1492 by Rosenblat (1976, 59) in contrast to eight million by Cook and Borah (1971–1979 1, 407). All are examining the same evidence: an apparent census or partial census of the island in 1496 by Bartholomew Columbus and reports by Las Casas and others of populations of one to three (or four) million in the period 1492–1496 (Sauer 1966, 65–68).

Henige examines the early reports and concludes "that it is futile to offer any numerical estimates at all on the basis of the evidence now before us" (Henige 1978a, 237). Zambardino (1978, 707), on the other hand, concluded that a 1492 "population figure of around one million can well be justified." Henige (1978b) responded by attacking Borah and Cook's methodology for estimating contact populations for Central Mexico.

The issue in part revolves around whether or not there were any epidemics on Hispaniola sufficient to have reduced a population of a million or more in 1492 to about 15,000 by 1518. Henige presents convincing evidence that smallpox was not present on Hispaniola, or anywhere else in the New World, until 1518. He is apparently wrong, however, in asserting "that there had been no serious or epidemic incidence of infectious disease in Hispaniola before late 1518" (Henige 1986c, 19). The one new contribution to the discussion is by the Spanish medical historian Francisco Guerra (Guerra 1985, 1988). He finds good evidence for a major epidemic of swine flu[3] on Hispaniola in 1493, which devastated the Spaniards and Indians both there

[3] The influenza "epidemics with excessive mortality...are due to animal viruses, particularly that of the swine, which incidentally infect human beings and against which persons have less resistance than they have against human influenza viruses . . . the influenza pandemic . . . that in 1918 killed over 10,000,000 was due to the virus of swine influenza" (Guerra 1988, 319–320).

and on other islands. The Indians "perished almost completely," from a 1492 total accepted by Guerra (1988, 319–323) of 1.1 million.

There are other recent estimates. The Dominican Republic historian Frank Moya Pons (1977, 15), based on projections from censuses in 1514 and 1508, obtains 377,559 (4.8 per square kilometer) for 1494. Presumably the total was higher in 1493 or 1492 before the swine flu epidemic. Watts (1987, 71–75) in his historical geography of the West Indies reviews Rosenblat, Cook and Borah, Sauer, Zambardino, and Henige and "broadly" accepts the estimate of Las Casas of three to four million and double that for the total on the other islands ("the generally accepted hypothesis").

Andes

The Indian population of Peru from 1520 to 1620 is examined by N.D. Cook (1981, 1982b) who presents several methods or models for estimating the 1520 numbers. The four best projections range from 5.5 to 9.4 million. However, he believes that the upper part of the range is more likely, and he proposes a total estimate of nine million (Cook 1981, 113–114). Without eastern (Amazonian) Peru, the total would be 8,520,000. Nine million, or a range of 4–14 million, mainly based on Cook, is accepted in the demographic history of Peru by Varillas Montenegro and Mostajo de Muente (1990), 6, 35–36). More recently, Cook suggested a total of 14 million for the Central Andes (Inca Empire), primarily highland and coastal Peru, Ecuador, and Bolivia (N.D. Cook, pers. comm.; also, Roberts 1989). To be consistent, however, if a ratio is used based on the proportions in Shea (1976, 174), Cook's Central Andes total, using 8,520,000 for Peru, would come to 12,852,000.

The best review of Cook's estimates is by Zambardino (1984). He finds most of the models of limited use but apparently believes the census projection method, using the decline rate from 1570 to 1600, is the best (as does Denevan 1983). This utilizes 1570 and 1600 data to project a total of 3.3 million for 1520. Cook (1981), 95) considers this to be an absolute minimum, which does not take into account a higher rate of population decline from 1520 to 1570 than from 1570 to 1600. If extrapolated by ratio to all of the Central Andes, the total is 5.13 million. Zambardino is critical of Cook for using "impressionistic" estimates not based on census data in his final averaging. On the other hand, Cook is the most knowledgeable scholar regarding the Peruvian data and context, and weight must be given to his judgment of the appropriate evaluation.

Another recent estimate, using depopulation ratios, is about ten million for the Inca Empire in 1530 by Wachtel (1977, 90). This would be increased by 1,200,000 if projected back to 1520, using ratios based on Shea (1976, 174).

One of the lowest estimates of the population of Peru and the Central Andes is that by Shea, whose study examines disease behavior and decline rates to derive a total of 1.34 to 1.94 million for Peru and 2.03 to 2.93 million for the Central Andes (Shea 1976, 174). Cook (1977, 1981, 95) finds Shea's uses of documentary sources incorrect and hence argues his estimates “must be rejected.” Cook (1981, 41–54) also criticizes the methods and estimates of Rowe, Dobyns, Smith, and Wachtel.

Thus, other than Dobyns (1966, 415), who had an unreasonable estimate of 30 to 37.5 million, recent estimates for the Central Andes in 1520 range from Shea’s 2.03 million to Cook’s 14 million. By 1620 only about 670,000 Indians remained in Peru (Cook 1981, 246).

Cook (1982a) has done a study of a regional population in Peru, that of the Colca Valley, for which there are *visitas* (censuses) for 1572, 1591, 1604, and 1616. Projecting backward he obtains a 1530 total of 62,500 to 71,000, compared to only 32,826 in 1972 (Cook 1982a, 83–85). The Colca is a densely terraced valley, with about 61 percent of the terraces now abandoned (Denevan 1987, 31). A study of the carrying capacity of the terraces of the Colca Valley village of Coporaque by Treacy (1989, 230, 242–243) gives a potential population of either 5,889 or 6,452, compared to his estimated 1520 population of 5,957 and a 1985 population of only about 1,200.

Elsewhere in the Andes, the Villamarins have estimated a 1537 contact population of 120,000 to 160,000 for the Chibcha in the Sabana de Bogotá in Colombia. For Venezuela they give a contact population of 200,000 to 350,000, which seems much too low (Villamarin and Villamarin 1975, 83, 113). Newson (1991) and Brown (1984) documented the frequency of early colonial epidemics in Ecuador, as has N.D. Cook 1992, 2000) for the Andes in general.

Amazonia

In 1976 (Denevan 1976b, 230, 234) I estimated the 1492 population of Greater Amazonia to have been 6.8 million, but then reduced it by 25 percent to 5.1 million to allow for buffer zones between tribes or villages. This was probably too large an adjustment (see Dobyns 1978); 10 to 20 percent would have been more reasonable. In any event, I was criticized for being both conservative (Myers 1988, 63, 69) and excessive (Mörner 1979, 28), while others found my densities the most acceptable to date (Frank 1987, 113). Certainly, these figures are problematic, but there is not much evidence to draw on.

My 1976 estimates are based on “reasonable” habitat densities. For the *várzea* (floodplains), I obtained 28.0 per square kilometer for the large floodplains and an average of 14.6 for all floodplains. The large floodplain density was based on counts of Omagua Indians in 1647–1649 by Laureano de la Cruz (1942, 43–46). Based on Sweet’s (1969, 41–43) interpretation, the density was 8.0 per square kilometer, which I multiplied by a depopulation ratio of 3.5 to get 28.0 per square kilometer for 1540. However, a recent and more thorough examination by Porro (1981) of the counts by Laureano de la Cruz gives a density of 5.2, or 91,000 total, for 1647, and with my multiplier of 3.5, the density for 1540 is 18.2. Applying this to the Amazon River *várzea* in Brazil and using half this density (9.1, given an estimated 50 percent unoccupied) for the main tributaries in Brazil and in Peru and Bolivia results in a reduction of 84,500 in my floodplain totals. [Also see Denevan 1996b.]

On the other hand, Myers (1988, 1989), based on an examination of Laureano de la Cruz by Grohs (1974), obtains only 5,500 Omagua for 1646. Myers, however, assumes disease impact prior to Orellana's 1540 voyage down the Amazon and then assumes a 75 percent mortality rate during each epidemic and a 1 percent recovery rate per year between epidemics. This gives a total of 1,974,950 Omagua in 1524. He further suggests ten million for the entire Upper Amazon, including 5 to ten million for Mainas and nearly 1.28 million for the Cocama on the Río Ucayali (Myers 1988, 68–70, 76–77). These figures seem unreasonable as they imply several tens of millions overall in Amazonia.

Meggers et al. (1988, 291), on the basis of archaeological analysis of numerous prehistoric Amazon settlements, believe that most sites with large surface areas represent "multiple reoccupations rather than large single villages." Given an apparent similar settlement size and mobility for prehistoric and present-day *tierra firme* (interflueve, upland) tribes, which is questionable, Meggers (1992) concludes that the population density of the former is similar to the latter, ca. 0.3 per square kilometer. She assumes that the *várzea* density was the same, but provides no evidence other than to argue that while local densities were higher, these were offset by large uninhabited areas. Historical accounts of high floodplain densities are discounted as exaggerations. For Amazonia she projects the 0.3 density to a pre-European population of 1.5 to 2.0 million (Meggers 1992). The same density would give a total of 2.9 million for Greater Amazonia (9.8 million km^2). For the *várzea* I believe that 0.3 per square kilometer is unreasonably low. Large empty lands have not been proven. Densities were locally much higher in the seventeenth century. Ann Roosevelt (1991 and pers. comm.) reports archaeological evidence for very large numbers on Marajó Island and for the Tapajos Indians in the fifteenth and sixteenth centuries. On the other hand, for the *terra firme* forests, Meggers' 0.3 density exceeds my own 0.2. I think the latter, or even less, is justified given that prehistoric Indians were dependent on the very inefficient stone axe for clearing forest and hence probably practiced limited shifting cultivation (Denevan 1992).

Hemming (1978, 492–493) did a tribe-by-tribe count for all of Brazil, but mainly Amazonia, based on the earliest data, often not until the nineteenth century or early twentieth century, arriving at a total of 2,430,000 million [revised in 1995 to 3,235,000].

Dean (1985) examined evidence for the Tupinamba on the central Brazilian coast. He obtained a density of 9.0 per square kilometer, compared to my 9.5 (1976, 219). For just the coast of the State of Rio de Janeiro, his total for 1501 is 103,000 (Dean 1985, 42). For the full central coast (105,000 km^2), his total would be 52,500 less than my 997,500 (1976, 230).

My density of 1.3 to 2.0 for the lowland savannas of Amazonia is based on research on Mojos in northeastern Bolivia which indicates about 100,000 in the 1690s based on Jesuit reports, with a modest 3.5 multiplier giving a contact population of 350,000 in 1580 (Denevan 1976b, 211–213) prior to reported epidemics. Leandro Tormo Sanz (1972) also looked at the Jesuit estimates and suggested only

7,200 Mojos Indians in 1679. However, the area covered is only the main trunk of the Río Mamoré and does not include the large additional terrain in Mojos, which contained numerous other tribes.

For the Orinoco Llanos of Colombia and Venezuela, Morey (1979) believes that my estimate of 513,000 for 1492 is too low (1976, 230). He esxamines population decline but does not give a regional estimate.

Elsewhere in lowland South America, a critical region is that of the Tupi-Guarani Indians of central and southern Brazil and Paraguay. A study by Clastres (1973, 32) for the region between the Río Paraguay and the Atlantic coast and 22° to 28° S. latitude (ca. 500,000 km^2) gives a density of 4.0 per square kilometer for the Guarani, for a total of 1,404,000 (or 1.5 million) in 1530 in an area of 350,000 km^2. In the other 150,000 km^2, there were additional tribes, both farmers and hunter-gatherers, with, I believe, a probable density of well under 1.0 per square kilometer. The Villamarins (1975, 106) estimated only 200,000 Guarani in Paraguay at contact.

Revised Hemispheric Estimate for 1492

Table 1.1 shows my revised regional populations for 1492, based on the various new estimates since 1976. The total of 53,904,000 is a reduction of 3,396,000 from my total of 57,300,000 in 1976 (291). Fifty-four million is a significant increase over the early estimates by Rosenblat, Kroeber, and Steward (8.4 to 15.5 million). It is well below the estimates by Dobyns (1966) of 90 to 113 million, and by Borah (1976a, 17) of 100 million, but it is close to Sapper's 37.0 to 48.5 million and Spinden's 40 to 50 million (Denevan 1976b, 3, 15).

For most regions, the adjustments have been relatively minor, up or down. The major reductions are for the Caribbean (2,850,000) and for Central Mexico (4,461,000). The estimates for Central Mexico and the Central Andes (Inca Empire) are both unsatisfactory because they are averages of several conflicting figures. For Central Mexico, 13.8 million may still be too high, especially if more weight is given to Sanders (11.4 million) and to Zambardino (8 to ten million). The Central Andes have been increased by 4,196,000 to a total of 11,696,000, which still may be too low given Cook's confidence in his figure of nine million for Peru alone.

I believe my 1976 margin of error of 25 percent can now be reduced to 20 percent. Given the total of 53,904,000, the new range is from 43,123,000 to 64,685,000, or, more roughly, 43 to 65 million. Future regional revisions are likely to maintain the hemispheric total within this range.[4]

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[4] Colonial historian McAlister (1984, 85) accepts my 1976 range of 43–72 million. Recent hemispheric estimates include 80 million by Schwerin (1984, 34), 40 million by Lord and Burke (1991), 40 to 50 million by Cowley (1991), 43 million by Whitmore (1991, 483) and 46.5 to 53.5 million by Alchon (2003, 147–172). See Henige (1998) for a detailed critique of "high counter" numbers. However, he considers as estimate of 40 million to be "not unreasonable" (Henige 1998, 120).

Table 1.1 Revised estimates of aboriginal American populations, ca. 1492

Region	1976	1992	Change
North America	4,400,000	3,790,000[a]	−610,000
Mexico			
Central	18,300,000	13,839,000[b]	−4,461,000
Chiapas	800,000	275,000[c]	−525,000
Yucatán-Tabasco	1,600,000	1,600,000	No change
Soconusco	None	80,000[d]	+80,000
Central America			
Southern Guatemala	2,000,000	2,000,000[f]	No change
Honduras-Belize	750,000	850,000[g]	+100,000
El Salvador	500,000	750,000[h]	+250,000
Nicaragua	1,000,000	825,000[i]	−175,000
Costa Rica	400,000	400,000	No change
Panama	1,000,000	800,000[j]	−200,000
Caribbean			
Hispaniola	1,950,000	1,000,000[k]	−950,000
Other islands	3,900,000	2,000,000[l]	−1,900,000
Andes			
Central	7,500,000	11,696,000[m]	+4,196,000
Colombia	3,000,000	3,000,000	No change
Venezuela	1,000,000	1,000,000	No change
Lowland South America			
Amazonia	5,100,000	5,664,000[n]	+565,000
Argentina	9,00,000	900,000	No change
Chile	1,000,000	1,000,000	No change
Remainder	1,500,000	1,055,000[o]	−445,000
Totals	57,300,000	53,904,000	−3,396,000

[a]Double Ubelaker's (1988, 291) 1.894 million at contact
[b]Average of Zambardino's (1981, 240) 8 to 10 million, Sander's (1976, 81) 11.4 million, Slicher van Bath's (1978, 92) 21.42 million, and 13.536 million projected from Whitmore's (1991, 477) Basin of Mexico total of 1.59 million
[c]Gerhard (1979, 160).
[d]Gerhard (1979, 169)
[e]Northwest Mexico, 1,040,000, based on Reff's tribal figures; Northeast Mexico, 340,000, from Gerhard (1982, 24)
[f]No change; Lovell and Swezey (1982, 81)
[g]Honduras, 800,000 from Newson (1986, 91); 50,000 added for Belize
[h]Fowler Jr. (1988, 114).
[i]Newson (1987, 88).
[j]Reduction based on comparison with the rest of Central America.
[k]Zambardino (1978), 707).
[l]Double the estimate for Hispaniola; Watts (1987, 74)
[m]Average of adjusted totals for the Central Andes based on ratios derived from Shea (1976, 174); 12,852,464 from Cook (1981, 114); 8,520,000 for Peru only (without eastern Peru); 10,592,723 from Cook's (1981, 113) four most "reasonable" model totals for Peru only (without eastern Peru), which average 7,022,448; 11,200,000 from Wachtel's (1977, 90) 10,000,000 for 1535 adjusted to 1520; and 12,140,000 from Smith (1970, 459), unadjusted. Estimates by Rowe (1946), Dobyns (1966), Shea (1976), and others have less basis for inclusion in this average
[n]Reduction of *várzea* by 84,500 based on Porro (1981); see Amazon discussion above. Reduction of central Brazilian coast by 52,500 based on Dean's (1985, 42) 9.0 per square kilometer density instead of my 9.5; and an increase based on a buffer zone of 15 (10–20) percent instead of 25 percent (Denevan 1976b, 234)
[o]Southern Brazil and Paraguay-Uruguay. Based on Clastres' (1973, 31–32) 4.0 per square kilometer density for the Tupi-Guarani region (350,000 km^2 minus 105,000) for the central coast (included in Amazonia), and allowing a rough 0.5 per km^2 for the other tribes in the region (150,000 km^2)

If the total Indian population for the hemisphere had dropped from about 53.9 million in 1492 to only about 5.6 million by 1650 (Dobyns 1966, 415; Ubelaker 1988, 292), then the decline amounted to 48.3 million, or 89.6 percent. This is a human toll of a magnitude comparable to that suffered during World War II (ca. 45–50 million), although the Native American demise was spread over a longer period of time and involved not only brutality, including genocide, but the inadvertent introduction of disease.

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1.2 The Native Population of Amazonia in 1492 Reconsidered

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Abstract Since 1966 Denevan made several attempts to estimate the native populations of Amazonia in 1492. His method was to determine rough habitat densities, which project to totals for Greater Amazonia of from 5.1 to 6.8 million. He now rejects this method, given that the denser populations were mostly clustered rather than evenly dispersed. Nevertheless, he still believes that a total of at least 5 to 6 million is reasonable.

Keywords Amazonia · Native population · Habitat density method · Clustering versus dispersal · Reconsideration · Woodrow Borah

Woodrow Borah and the Mar del Plata Symposium

On June 10, 1965, while living in Peru and undertaking fieldwork in the Upper Amazon, I received a letter from Woodrow Borah asking me to present a paper at a symposium he was organizing on historical demography at the 37th International Congress of Americanists to be held in Mar del Plata, Argentina, in September 1966.[1] This led me from being an interested observer to center stage of the great debate over the size of native American population numbers at the time of Columbus.

[1] *Simpósio Demografía Retrospectiva: Nuevos Aportes y Nuevos Métodos*.

When I was a graduate student in geography at the University of California at Berkeley, I was certainly aware of Woodrow Borah through his close relationship with historical geographers Carl Sauer and James Parsons and some of their students. I took no courses from Borah, but in 1963 I discussed my doctoral research on Mojos in the Bolivian Amazon with him. Later, while I was in Lima in 1966, he arrived for a month to consult with various scholars, and we spent considerable time together socially and discussing historical demography. A particularly notable afternoon was spent with Woodrow and John Murra at our apartment. The two had a vigorous dialogue about Indian populations in Peru and Mexico, while I sat quietly in a corner overwhelmed by the power of their exchange. Later, young historians told me that they had found Borah to be intimidating, but that was not my experience.

I told Woodrow that I would prepare a paper on Mojos for his Mar del Plata symposium. In a chapter of my dissertation, I had examined the native population of Mojos based on Jesuit accounts (Denevan 1966, 112–120). However, instead of presenting this, I developed a speculative statement on western Amazonia, which was later published (Denevan 1970a). The symposium participants, besides Borah and myself, included Ángel Rosenblat, Pierre Chaunu, Juan Friede, and others. The highlight was a one-on-one debate, actually a gentlemanly discussion, between Borah and Rosenblat.

Initial Consideration

I wrote the Mar del Plata paper in Lima with limited source materials. Even if I had had access to a good library and archives, it was unlikely that I would have found much numerical data on Amazonia for the sixteenth century. Other methods were of limited utility (Denevan 1970a). Consequently, my methodology was largely theoretical, what I have since called the "habitat-density" method, not unlike the culture-area density method essentially used in Julian Steward's 1949 population density map for South America in AD 1500 (Steward 1949, 659). I reasoned that if I could obtain a few local population densities for each of the major habitats in Amazonia, I could average them and then multiply each resulting habitat density times the total land area in each habitat to get total estimated populations for each habitat. The habitats used were floodplains, lowland savanna, upland savanna, coastal, low *selva*, and high *selva*. I then applied this method to Peruvian and Bolivian Amazonia, obtaining overall densities of 0.47 and 0.96 per km^2 and populations of 369,000 and 664,000 for a total of 1,033,000. Feeling confident, being young and bold, I then applied this questionable method to all of Greater Amazonia (tropical interior of South America) and obtained an overall density of 0.59 per km^2 and a total population of 5,750,000 (Denevan 1970a, 79–82).[2] There was little response to my method or results at the symposium, although Borah was quite supportive, nor to the published version.

This venture into historical demography led me and others to consider publishing an anthology of existing articles on the Indian demography debate, with encourage-

[2] This total compares with prior estimates for just Brazil of 1.0 million by Rosenblat and 1.1 million by Steward, but 6.0 million for tropical South America by Dobyns (Rosenblat 1954, 102; Steward 1949, 666; Dobyns 1966, 415, note f).

ment from Borah and also from Carl Sauer, the founder of the Berkeley School of historical demography (Denevan 1996a). This plan was aborted by a lack of interest from university presses, and instead we opted for a collection of mostly original articles, which I edited (Denevan 1976a). It consists of eight essays plus my regional surveys.[3]

Borah initially believed that all our papers from the Mar del Plata symposium would be published in the Americanist Congress Proceedings; however, after several years of waiting none were, for whatever reason.[4] However, three of those papers were included in my 1976 volume: Borah, Rosenblat, and Denevan. Borah's essay is a review of the debate over the size of Indian populations – "an attempt at perspective." In it he repeated his earlier estimate of "upwards of one hundred million in the New World" ca. 1500 (Borah 1964, 381), with the qualification that this was "an admittedly hasty and general estimate" (Borah 1976, 17). Rosenblat's chapter (Rosenblat 1976) was taken from an expanded version of his Mar del Plata paper which had been published in Mexico (Rosenblat 1967, 7–23; 82–84), an aggressive defense of his previous conservative population estimates dating to 1935. I translated the Hispaniola section into English, and Rosenblat made revisions and added an "Addenda" of eight pages, which is particularly a critique of Sauer's population material on Hispaniola (Sauer 1966, 65–69). Rosenblat indicated before his death in 1984 that he was quite pleased to have had his position on low Indian population numbers published in English (Rosenblat, pers. comm., 1976). For Hispaniola he held to an estimate of only 100,000 to 120,000 (Rosenblat 1976, 59), far below the estimates of Cook and Borah (1971), 407–408), Sauer (1966), 55–69), N.D. Cook (1993, 219–220), and others. In any event, the Mar del Plata symposium has had long-term repercussions.

In my own article in the 1976 volume, I considerably revised my Mar del Plata paper on Amazonia, with changes in the habitat densities, overall density (to 0.70), and total population for Greater Amazonia (to 6.8 million) (Denevan 1976a, 230–234). However, I also allowed for a "buffer effect," or zones of no man's lands between Indian groups, of 25 percent, thereby reducing the total to 5.1 million. This article received considerable attention, and the totals have been frequently cited, mostly uncritically. On the other hand, I was challenged by a few authorities for being either too conservative (Smith 1980, 63–69) or excessive (Mörner 1979, 27–28), while others found my densities to be acceptable (Frank 1987, 113).

In the introduction to the second edition of *The Native Population of the Americas in 1492*, I again revised my regional estimates for Amazonia, using the same habitat-density methodology, but taking into consideration demographic information and estimates from new research by various scholars. For Greater Amazonia my new total was 5.7 million, using a buffer zone of only 15 percent (Denevan 1992, xxix).

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[3] Upon reviewing population estimates for the major regions of the hemisphere by various scholars, I suggested a total of 57.3 million, or, when allowing for a degree of error of 25 percent, a range of from 43 to 72 million (Denevan 1976a, 291). I later revised the total downward to 53.9 million, or, with a 20 percent margin of error, a range of from 43 to 65 million (Denevan 1992, xxix).

[4] Borah, in a letter to me dated November 17, 1969, commented: "It would be odd to omit our symposium from the *Proceedings*, but I suppose it might be done. The reasons could be so diverse that I see no point in speculating until we know more. Since all reports on the [Congress] stressed [our] symposium, it will be an interesting mystery for future readers."

Reconsideration: The *Várzea*

The method used in the 1966 Mar del Plata paper and in the several published versions (Denevan 1970a, 1976a, 1980, 3–41) assumed that in each of the habitat types used, the population density was more or less uniform. Thus, known local densities could be extrapolated to the full areas of each habitat to obtain total populations for each. However, this is a fallacy. We now know that for the *várzea* (floodplain), *terra firme* (upland forest), and also the *llanos* (savannas), populations were concentrated into large, probably semipermanent villages, separated by extensive areas of small villages and sparse, dispersed populations.

I became aware of this patch pattern for the *várzea* when I reexamined settlement locations. Sixteenth-century accounts indicate long stretches of large linear villages, with gaps that I initially assumed were buffer zones. Most of these villages were not located in the floodplains but rather on the adjacent bluffs, well above periodic flooding, and were apparently supported by a complementary subsistence system that depended on both seasonal floodplain *playa* cultivation (plus fishing) and semipermanent year-round cultivation and orchards (plus hunting)[5] on the bluff top (Denevan 1996b). However, I argued, this system could only function where continuously navigable river channels impinged against a bluff, not where the main channels were in mid-floodplain or against the opposite bluff. This pattern is supported by ethnohistory, archaeology, and the presence of *terra preta* (black anthropogenic soil)[6] along the bluff tops.

The mid-sixteenth-century and early seventeenth-century chroniclers of the Amazon River, Carvajal, the men of Úrsúa/Aguirre, and others, reported linear villages on the bluffs, as long as 1 to 5 leagues (ca. 6 to 30 km), in places as large as 800 to 3,000 people and some possibly as large as 10,000, with sectors of empty lands between villages (Denevan 1996b, 657–664). In 1557 the Salinas de Loyola expedition on the Río Ucayali in Peru reported village house numbers that may have contained as many as 4,000 and 8,000 people (Myers 1974, 140–141). While exaggeration is possible, support is provided by lengths of *terra preta* of up to 6 km and measuring several hundred ha in extent (Denevan 1996b, 664–666). Sites at Araracuara on the Río Caquetá in the Colombian Amazon were occupied for 800 years or more (Herrera et al. 1992, 107, 110). At Açutuba on the lower Rio Negro, *terra preta* extends for 3 km with 30 ha of settlement activity, with nearly

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[5] A case can be made for semipermanent prehistoric cultivation because of the inefficiency of stone axes for clearing forest, in contrast to shifting cultivation in colonial and modern times made possible by much more efficient iron and steel axes. With stone axes, forest once cleared would likely be kept in production rather than fields being frequently shifted. See Denevan 2001, 115–127.

[6] The origin of *terra preta* has long been debated (Smith 1980, 553–556). Scholars now mostly agree that the black form, generally filled with ceramics and bones, is a pre-European midden (garbage)-derived soil associated with long-term settlement. A brownish form (*terra mulata*) is much more extensive and appears to be the result of semipermanent cultivation involving mulching, composting, and frequent in-field burning. These are fertile soils that seem to be self-sustaining (Woods and McCann 1999; McCann et al. 2001).

continuous occupation from at least AD 1 to 1440 (Heckenberger et al. 1999, 357–364).

Possibly only about 20 percent of a bluff along one side of a floodplain has easy river access and thus to *playas*, aquatic resources, and river travel (Denevan 1996b, 673). This is where the large semipermanent villages were located, in narrow strips separated by longer strips of sparse populations. For the well-settled sectors, with an estimated sustaining area of 15 km depth (10 km in from the bluff edge and 5 km into the floodplain), the associated riverine population density would be 10.4 per km^2. The total riverine population for the main Amazonian rivers would be about 1.5 million (Denevan 1996b, 673). This compares with my earlier estimate of a floodplain population of about 1.8 million (Denevan 1976a, 230). However, the 20 percent densely occupied sectors and the 15-kilometer-deep sustaining zone, based on this "bluff model," are rough estimates.

Reconsideration: *Terra Firme*

For the upland forests, I originally used a conservative density of 0.2 people per km^2 (Denevan 1970a, 72; 1976a, 225), as did Steward for marginal (nonagricultural) tribes (Steward 1949, 661). Even this low-density produces a total of 1.26 million people given the enormous area of upland forest involved (nearly six million km^2 in Greater Amazonia). The 0.2 density was assumed to represent a relatively uniform distribution of well-spaced small villages, mostly of 100 people or less (Steward and Faron 1959, 299). Meggers believed that tropical forest village size did not exceed 1000 people each, based on her perceived "environmental limitation on the development of culture" (Meggers 1954, 814).[7] In making my own estimates, I ignored reports that there were also some much larger villages numbering several thousand people each.

The evidence for large villages in the uplands is of three types: archaeological, *terra preta* soil, and historical (Denevan 2001, 115–132). The best archaeological examples are in the Upper Xingu basin. Recent excavations there indicate village extents of from 30 to 50 ha, with village populations of "at least 1,000 to 1,500," compared to the largest, current nearby Kuikuru village of only about 6 ha and 330 people (Heckenberger et al. 1999, 370). These villages were occupied continuously for hundreds of years, with ringed moats and roads and *terra preta* soil, and they apparently were supported by semipermanent cultivation. A site partially surrounded by long trenches near the Rio Kuluene, also in the Upper Xingu region, covered 110 ha (Dole 1961, 403–404), but it was not likely a single village. An Aratu village site in Goiás, 10.3 ha in size, "had an estimated maximum population between 1,043 and 1,738 persons" (Wüst and Barreto 1999, 14).

[7] Meggers later argued that large archaeological sites in Amazonia reflect multiple occupations of these sites rather than large single, permanent villages (Meggers 1992a, 2001).

Large *terra preta* sites in upland forests include over 120 ha at Oitavo Bec south of Santarém (Woods and McCann 1999, 12) and 200 ha at Comunidade Terra Preta between the lower Tapajós and the Rio Arapiuns (Smith 1999, 26). Other very large expanses of *terra preta* have been reported but not measured (McCann, pers. comm.). Such large sectors of anthropogenic soils, however, are in large part created by cultivation and do not represent single village sizes, but they are indicative of large, permanent villages. There are also large numbers of small *terra preta* patches, 1–2 ha or less (Smith 1980, 563), likely occupied by just a few people over a long period of time or frequently reoccupied.

There are numerous historical reports of large forest villages of 1,000 or more, dating from the sixteenth century to the early twentieth century, despite depopulation and social disruption after initial contact. For example, the largest, principal Tupinambá villages ranged up to "perhaps 6,000 to 8,000" (Sturtevant 1998, 141–142). Clastres estimated that Tupinambá village size ranged from about 400 to 3,000 or more, and he cites André Thevet in 1556 as reporting single villages of 6,000 and 10,000 people, probably an exaggeration (Clastres 1974, 87). A Bororo village in the early twentieth century had an estimated 1,500 people (Wüst 1994, 323), and another in the 1930s had "an estimated population of more than 1,000 people" (Wüst and Barreto 1999, 14). A Kayapó village in 1896 had about 1,500 Indians (Posey 1994, 323). The outline of the Kayapó village of Pyka-tô-ti, which existed to possibly 1919, covers 87 ha and was estimated by Posey to have had "perhaps 3,500 to 5,000 people" (Posey 1987, 147). Paresi villages in the early eighteenth century were as large as possibly 1,200 people, and in the sixteenth century, one Xarae village may have had 7,500 people (Wüst 1994, 337). An Apinayé village in 1824 was said to have numbered 1,400 (Nimuendajú 1967, 12).

None of these reports is conclusive as to actual village size, but they do suggest that villages over 1,000 were not uncommon during both pre-European and historical times in the upland forests. It is also known that many smaller villages were present. Overall there was clearly a wide diversity of village sizes, possibly a range from one single-family house to as high as 5,000 people. Many if not most villages were probably permanent or semipermanent, given the evidence of *terra preta* and my argument that semipermanent cultivation was practiced. Villages may have been shifted over short distances within sectors of *terra preta* soil and anthropogenic vegetation, but the high degree of village mobility known from historical times may not have been as common in prehistoric times.[8]

Arguments that environmental limitations held average population densities to those of surviving Indian groups of 0.3 per km^2 or less (Meggers 1992a, 203), or even that the size of permanent villages could have been up to 2,000 people based

[8] Village fissioning as a result of internal conflict (social tension) undoubtedly did occur (Neves 1995). However, this did not necessarily result in village abandonment. Abandonment of both houses and villages because of conflict, death, disease, vermin, house deterioration, witchcraft, etc. could result in shifts of houses to an immediately adjacent spot or a few hundred yards away, not necessarily to a distant location, especially if there had been large local investments in the existing village, intensive cultivation, roads, and defensive works (moats, palisades).

on shifting cultivation (Carneiro 1960, 232), must be reconsidered if semipermanent cultivation was indeed practiced. Higher densities and larger villages were possible. One argument against large villages away from rivers is that there were inadequate sources of animal/fish protein for a crop diet based on protein-poor manioc and other tubers (Denevan 2001, 81–83). However, on fertile *terra preta* soils, near-continuous cultivation of protein-rich maize is possible.

Thus, for *terra firme* forests, there seems to have been great variation in population density, with sparse populations surrounding patches of dense populations in the sustaining areas of scattered large villages. We have no idea how many of these large villages existed, so it is impossible to estimate an average population density and a total population. If large villages were fairly numerous, then pre-European overall densities of 0.2 to 0.3 per km^2 (Steward 1949, 661; Denevan 1976a, 225; Meggers 1992, 203), based on such densities for small villages today,[9] could be much too low.

Reconsideration: The Savannas

I originally estimated densities of 0.5 per km^2 for the well-drained upland savannas (mainly in central Brazil) and 1.3 to 2.0 for the seasonally flooded lowland savannas (mainly Mojos and the Orinoco Llanos) (Denevan 1976a, 228, 230). The former density was based on a Nambicuara density of 0.2 in 1907, adjusted to 0.5 on contact. The lowland density was based on my estimate for the Llanos de Mojos in Bolivian Amazonia and a guess for the Orinoco Llanos. All these densities are problematical.

It is now obvious that there was just as much demographic variability in the savannas as in the upland forest. People in both types of savannas were concentrated at forest edges, in forest islands, and in gallery forests, utilizing resources from both forest and savanna. Villages within savanna were usually near forest and lakes or rivers. Settlement in open savanna was usually sparse and seminomadic. Exceptions were in the raised-field clusters that occur in the seasonally flooded savannas of Mojos, the Orinoco, and the coast of the Guianas (Denevan 2001, 230, 288). Densities were probably over 50 people per km^2 of combined ditch and field surfaces. Associated villages were located on artificial mounds in open savanna.

For savannas, there are several estimates of large village size based on archaeology. Most notable, Marajó Island in Brazil, ca. AD 400–1300, had enormous artificial mounds covering as much as 50 and 90 ha, with villages on the largest mounds having had about 1,000 people and large multi-mound sites having had several thousands, one with 40 mounds "likely to have had a population of more than 10,000

[9] For 13 *terra firme* groups, Beckerman (1987, 86) indicates that the [current] average population density of 12 of them was between 0.1 and 1.0 per km^2.

people" (Roosevelt 1991, 31–38).[10] The Gavan site in the Orinoco Llanos, AD 550–1000, had an estimated 670 to 1,000 people (Spencer et al. 1994, 134).

Examples of reported historical village sizes include Mojos (1617), single-family house counts of 280, 350, and 400, which at a rough 5 people per house equal 1,400, 1,750, and 2,000 each (Denevan 1966, 112); Caquetío, Orinoco Llanos (l539), one village with 4,000 people (Spencer 1998, 108); and Orinoco delta (mixed forest, swamp, savanna) (1583), villages of 2,000 and 4,000 (Whitehead 1988, 13). On the other hand, many savanna villages had less than 100 people and seminomadic bands such as the Nambicuara. Sirionó and Yaruro had only a few small temporary huts, at least in recent times.

Thus, the savannas also had a wide range of population densities and village sizes, with some villages numbering several thousands and with very high densities in sectors with raised fields. Large areas elsewhere had either small frequently shifted villages and very low densities (probably under 0.1 per km^2), as in the soil-impoverished central Brazilian plateaus where agriculture is difficult today without heavy inputs of chemical fertilizers, or were only utilized for hunting by people who lived and farmed in adjacent forests. Estimating average population densities for the savannas with any confidence is impossible. If it were just a matter of very sparse seminomadic populations, an approximation could be made, but there were also large villages and we don't know how many.

Conclusion

Consequently, I now reject the habitat-density method I used in the past to estimate a Greater Amazonian population in 1492 of from 5.1 to 6.8 million (Denevan 1976a, 230, 234). There was too much variability in densities in each habitat to be able to formulate meaningful average densities on the basis of a few sample densities.

The other main methodology used is totaling tribe-by-tribe counts plus estimates based on various criteria, as done by Steward to obtain 1.1 million and by Hemming to obtain 3.2 million (Steward 1949, 666; Hemming 1995, 521), both for all of Brazil.[11] This is just as unreliable as habitat-density calculations because the counts used are often well after the sixteenth century, because recent populations are

[10] In a harsh critique of Roosevelt's 1991 book, Meggers (1992b, 27–28, 36) only briefly questions Roosevelt's population estimates, primarily the 1,000 for the small (3.0 ha) Teso dos Bichos mound, and she acknowledges the large size of some of the other mounds, but not of any as extensive as 60 and 90 ha.

[11] The totals by Steward and Hemming for Brazil can be compared with recent estimates by Denevan (1996b, 656), 3.3 million for the Amazon basin (including western Amazonia but not southern Brazil); by Meggers (1992a, 203), 1.5 to 2.0 million for the Amazon basin (not defined), based on a 0.3 overall density derived from present densities; and by Newson (1996), 4.86 to 5.46 million for the Amazon basin, projected from her estimates for eastern Ecuador. On the extreme side, highly speculative, are estimates of ten million by Myers (1988, 69) for just Upper Amazonia in 1524 and 15 million by Smith (1999, 28) for all of Amazonia in 1500.

assumed to represent maximum possible populations and hence past populations, because known numbers are not systematically projected back to initial contact to account for depopulation, and because many groups which became extinct are not considered at all.

So, what would my mentor on historical demography, Woodrow Borah, say? Thirty-five years of trying to estimate the native Amazonian population in 1492, and now Denevan gives up! Says it can't be done with any meaningful result. Woodrow would probably be disappointed. On the other hand, I have some perspective from all these years of attempting to make estimates; from having examined and reexamined the relevant archaeological, historical, environmental, and agricultural evidence; from being familiar with the methods and estimates of others; and from having conducted fieldwork on several Indian groups in the South American lowlands including research on prehistoric and recent cultivation practices. This perspective, which I believe permits more than just guesswork, gives me confidence that the Indian population in 1492 was indeed *at least* five to six million for Greater Amazonia and at least three to four million for the Amazon basin. These numbers give overall densities of only 0.51–0.61 and 0.51–0.68 per km^2. There were large areas with fewer people, but there were also locations with many, many more.[12]

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2.0 Agricultural Landforms

Introducer: Clark L. Erickson

Abstract Indigenous agriculture of the Americas has a long history as a research topic in cultural geography, cultural anthropology, and archaeology. Native peoples developed a vast range of strategies from intensive agriculture (raised fields, terracing, irrigation) to agroforestry. Over time, human activities transformed much of the environment into highly productive anthropogenic landscapes. William Denevan dedicated his career to understanding indigenous knowledge through research on the complex agricultural landforms. Two early publications explore agricultural intensification, conversion of marginal wetlands and slopes into productive spaces, role of social organization in farming, human response to climate change, and abandonment of fields. In the process, Denevan presents methodological approaches to record, classify, date, and analyze field systems that continue to be relevant.

Keywords Agricultural intensification · Agricultural landforms · Human agency
Raised fields · Terraces

Introduction

Denevan's lifelong interest in everyday farmed landscapes and the people (individuals, families, and local communities) who built and used them have inspired countless scholars to follow in his path. Denevan's interdisciplinary approach provides a means to understand how rural peoples (those "people without history" as used by Eric Wolf 1982), lived, produced, and thrived in the Americas over hundreds and thousands of years in some areas. In his research on pre-European field systems, Denevan asks basic questions of what, where, when, who, how, and why. In many cases, his and that of his student's and colleagues' individual fieldwork on agricultural landscapes answered those questions. In most cases, archaeologists, soil scientists, historians, anthropologists, agronomists, geomorphologists, and hydraulic engineers took up the task of providing answers, often as collaborative teams.

The two publications discussed are *Aboriginal Drained-Field Cultivation in the Americas* (1970) and a chapter in Denevan's 2001 volume based on his 1987 report

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("the Science article" and "the Terrace chapter"). Each represents the conclusion of two important phases of Denevan's research career: his work on raised field agriculture and terrace agriculture. I will focus on a number of important methodological approaches and theoretical frameworks that Denevan introduces and applies in these articles.

Methodological Approaches

Agricultural Landforms

Denevan applies the conceptual framework common in geography of landforms during this period of research and publication. Landforms can be natural or cultural. Geographers use the term "agricultural landforms" to refer to the physical form, character, formation, and function of past or present farming and herding systems, the common categories [forms] of human-made landforms (Golomb and Eder 1964). Other geographers subsume agricultural landforms as examples of anthropomorphology, anthropic geomorphology, anthropogene, or anthropogenic geomorphology (Szabó et al. 2010). Formal imposed, relatively permanent structures such as ridges and furrows, terraces, raised fields, and field infrastructure (e.g., walls, canals, paths, and boundary markers) are the foci (Golomb and Eder 1964; Doolittle 2000, 5–6). Contemporary archaeologists and historical ecologists refer to these physical units as gardens and fields and other agricultural landscape features at the small scale of everyday life and cultural, cultivated, domesticated, anthropogenic, terraformed, engineered, and/or humanized landscapes at the larger, sometimes regional scale (Balée and Erickson 2006). In this chapter, I will use the more modern term "agricultural landscape" (a subset of cultural landscape) rather than agricultural landform.

Denevan's research at this time was influenced by a growing knowledge of the degree and scale of human impact and transformation of the Earth (e.g., Thomas Jr. 1956). Denevan demonstrated that "simple" farming communities, given enough time, were capable of regional-scale alteration of the surface, soils, and vegetation for productive purposes. Because of an interest in pre-European demography and carrying capacity, he also could show that most of the Americas were more densely populated in the past and that farming societies existing on landscapes for millennia provided evidence that what they were doing to the surface of the earth was successful (what we would call sustainable today).

Data Collection, Mapping, and Classification

As stated in the *Science* article, Denevan's goal of investigation of raised fields in the Bolivian Amazon was to determine "the nature, number, distribution, ecological situation, and probable function of the relic ridges, as well as revealing other, associated earthworks, including mounds, causeways, and canals" (1970, 648). As a cultural geographer of the "Berkeley School," Denevan used the approaches and techniques of his time but emphasized the use of aerial photographs in addition to traditional maps to understand raised field and terrace agriculture in the Americas within an integrated system of their specific environmental, geographical, temporal, historical, and cultural context. He learned to appreciate aerial photographs in his earlier raised field research in the Bolivian Amazon (Denevan 1966). Long before GIS was available, Denevan's approach to agricultural landscapes was to assemble, organize, and query past and present maps, cadastral survey, and aerial photographs; read relevant history, archaeology, ethnography, geology, and geography to understand temporal and spatial context; describe and classify forms; and conduct targeted "ground truthing" through repeated fieldwork, often for only short periods of time. The Terrace chapter concisely summarizes this strategy.

A basic approach used in agricultural landform analysis is the mapping and classification of field forms across the landscape, sometimes at the regional scale. Denevan used classification as an initial step of analysis in the *Science* article and the Terrace chapter. Once the types are established, Denevan examines their distribution, context (topographic, ecological, and environmental), and scale to draw inferences about function and sometimes chronology. In the *Science* article, Denevan uses his and colleagues' fieldwork combined with mapping and classification to document the similarities in form and environmental context of "drained" or raised fields in wetlands and highland savannas across Latin America. In the process, he points out significant differences between and within raised field systems that relate to chronology, environmental contexts, and functions. At the same time, Denevan also distinguishes wetland raised fields from the more widespread mounding strategies used by farmers throughout the world. In the Terrace chapter, Denevan emphasizes that we must move beyond the scores of classifications proposed by previous scholars to address "ecology, functions, management, and productivity" of local terracing (Denevan 1987, 2).

Modern landscape archaeologists refer to this approach as "reading" the landscape in terms of palimpsest, associations, disjuncture, integration, boundaries, hierarchy, style, community, and territories in addressing social organization, labor, settlement, land tenure, intensification, expansion, and taskscapes (e.g., Erickson 1993; Miller and Gleason 1994; Walker 2011).

Dating and Chronology

In early research on agricultural landscapes, scholars assumed that fields could not be accurately dated; thus many "big questions" about terraces and raised fields could not be addressed. In the *Science* article, Denevan and colleagues relied on associations with dated archaeological settlements (often diagnostic pottery from the surface), now known to be less reliable than direct dating of fields. Denevan combed literature by chroniclers, explorers, missionaries, historians, and ethnographers for mention of fields to demonstrate their construction and use (and in some cases, abandonment) before the arrival of Europeans. Denevan also reasoned that the recorded colonial and present-day populations of many areas with abandoned fields would not be sufficient in number to create and manage intensive agriculture at the scale of the remains nor require such high production of food. In the Colca Valley research, Denevan and colleagues excavated trenches in abandoned terraces for recovery of stratigraphy and charcoal samples to demonstrate that direct dates of fields could be obtained through radiocarbon dating and associations with diagnostic pottery. The documentation of periods of construction, use, and abandonment were critical for addressing farmer's responses to climate change in late prehistory.

Ethnographic, Historical, and Experimental Analogy

In the *Science* article, Denevan turns to his vast knowledge of the historical, ethnographic, and agronomic literature on *chinampas*, a living although degraded example of indigenous raised field agriculture practiced by the Aztecs in late prehistory and continuing until the present, sweet potato raised fields in Baliem Valley of New Guinea, Sibundoy drained fields in Colombia, Andean lazybeds, Tlaxcala drained fields, Guató mounds in the Pantanal, and traditional mound fields in Amazonia and Africa. In later research on swidden fallow, Denevan and colleagues increasingly turned to ethnography of living farmers in the Amazon (Denevan and Padoch 1988). He also wrote about small-scale contemporary farmers that were still practicing small-scale wetland raised field agriculture (Denevan and Bergman 1975).

Because many systems of raised fields and terracing are abandoned and unused, Denevan encouraged colleagues to use experimental and applied archaeology to understand field function, energetics, and sustainability (e.g., Coolman 1986; Earls 1989; Erickson 1985; Garaycochea 1987; Gómez-Pompa et al. 1983; Puleston 1977; Ramos Vera 1986a, b; Canahua Murillo and Larico Mamani 1992; Kolata 1996; Stab and Arce 2000). Denevan argued that these experimental studies, in addition to understanding abandonment, were critical first steps in evaluating the contemporary and future use of these field systems.

Photography

Although publication at the time limited the inclusion of figures, Denevan effectively used his oblique photographs showing details of continuous raised fields in the foreground with identifiable isolated trees for scale and extending to local horizon taken from low-flying aircraft in addition to maps and aerial photographs to convince readers of the massive scale of landscape transformation. For his Colca Valley Project, Denevan obtained access to the high-resolution aerial photographs of entire valleys filled with terraces, irrigation infrastructure, and terraces from the Shippee-Johnson Expedition in 1931, in addition to later photographs. The aesthetics of highly patterned raised field and terraced landscapes as a human imposition of rigid structure on disorderly nature could easily be grasped by his audience.

Theoretical Frameworks

Political Economy and Social Organization

The level of organization required for the planning, construction, engineering, and maintenance of intensive agriculture at a large scale has long been of interest to scholars. Denevan addressed the social organization of raised fields and terracing in early articles but considered any interpretations as premature based on the information available. In later works, he stressed the agency of the farmers and their farming communities as capable of working and coordinating across generations over long periods of time.

In the *Science* article, Denevan shows that raised fields and their associated societies in the Americas are diverse. He suggests that a variety of relationships may have existed by stating:

> The aboriginal cultures responsible for drainage ranged from the seminomadic Guató, to farm villages and chiefdoms, to the high civilizations. The ridged-field farmers of the savannas were capable people but certainly not comparable to the farmers of the efficient and sophisticated states of the Andes and Mexico. Most of the drained-field systems could have been constructed and managed through small-scale family and community cooperation, as in New Guinea, rather than through central political control. On the other hand, there does seem to be a frequent relationship between degree of agricultural intensification, population size, and complexity of social organization. (Denevan 1970, 653)

Denevan avoids interpreting the mere coexistence of field systems and state societies as evidence of a direct association in terms of social organization of labor and political economy. Although recognizing the deep time and large scale of raised field and terrace agriculture, Denevan never fell into the trap of automatically assuming that the elite planned, implemented, administered, and maintained regional-scale raised fields and terraces as "top-down" state projects controlling the labor of their peasants. Denevan recognized that the periods of field use often overlapped with the presence of powerful states *and* periods after state collapse or absence of state. In the two articles, analogy is drawn from cross-cultural and local case studies to argue that small-scale societies are capable of intensive agriculture. These debates drove much of raised field research (e.g., Graffam 1990; Erickson 1993; Janusek and Kolata 2004; Bandy 2005).

Intensification

Although rarely discussed in his early work, Denevan's *Science* article concludes with a discussion of Boserup's theory of intensification that intensive agriculture is the result of increasing demographic pressure rather than the reverse causation (1965). Agreeing with the prevailing scholarly assumptions of the time, Denevan argued that marginal areas such as wetlands that require increasing labor inputs to raise production were adopted through necessity after available well-drained forest lands were filled by farmers because of higher labor costs for construction and maintenance. He also reasoned that marginal areas such as wetlands with raised fields would be the first to be abandoned with population collapse after European conquest, the condition in which they are most often found today. On the other hand, Denevan noted that land tenure changes imposed by Europeans denied indigenous farmers access to many areas where raised fields and terraces are found and cultivated by their ancestors. He also points out in the *Science* article that wetlands are attractive to early pioneering farmers as a rich source of aquatic protein, irrigation water, level surfaces, and efficient water transportation that may have trumped wetland marginality in the Boserup sense.

Denevan and colleagues realized that data about labor requirements, efficiency, production, carrying capacity, and cultivation intensity for specific field systems were necessary to test Boserup's agricultural intensification theory. Although most case studies showed evidence of large populations during periods of use, he discusses the potential problems in extrapolating population size and density based on the carrying capacity of raised fields.

Denevan explicitly addresses intensification theory in the Terrace chapter. Rather than assume that terrace abandonment means disintensification due to removal of population pressure, Denevan and colleagues' dating of the construction, use, and abandonment of terraces showed that terrace farming (a recognized form of intensive agriculture) practiced in the Middle Horizon was further intensified by retraction of cultivation to lower slopes that could be more efficiently irrigated to raise production than upper slopes as drought conditions increased in the Late Intermediate Period. This fascinating interpretation is a continuing research topic by other scholars (Treacy 1994; Guillet 1987, 1992; Trawick 2003; Gelles 2000; Wernke 2013).

Inspired by Denevan, archaeologists began to question Boserup's assumptions in later research involving experimental archaeology applied to raised fields and terraces. In the early 1990s, Denevan undermined assumptions about the intensification process from extensive to intensive agriculture in arguing that shifting cultivation in tropical forests could only be a widespread productive practice after the European introduction of iron and steel axes to the Americas (Denevan 1993, 2001). Before that, farmers emphasized more permanent field systems with labor and nutrient inputs.

Environmental Determinism vs Farmer Agency

In the 1954, Betty Meggers (1954, 1971) proposed that complex societies did not and could not have developed in the pre-European Amazon or broader Neotropics due to poor soil that limited production to extensive agriculture that could only support low population densities and primitive technology. She and colleagues later added lack of protein resources, tribal warfare, and climate change (regular "mega-el Niños") to the list of limitations. Denevan's early research in the Bolivian Amazon and that of colleagues Donald Lathrap and Robert Carneiro questioned these assumptions. Denevan's documentation in the *Science* article of "at least 170,000 ha

(1700 km^2) of ridged field remnants in Surinam, Venezuela, Colombia, Ecuador, Peru, and Bolivia" (a conservative figure in today's scholarship) helped undermine and later overturn the environmental determinism. Denevan is cautious, careful, and meticulous (one could even say conservative) in his descriptions of landscape features; interpretation of archaeological, colonial, and ethnographic documents; and the estimates of spatial scale and geographic distribution, carrying capacity, and dating of agricultural raised fields and terraces in South America. Through aerial photographs, maps, overflights, ground truth, interviews, and command of the literatures in the Science article and later work, Denevan showed that certain Amazonian peoples were capable of creating, transforming, increasing, and managing resources through active conversion of their environment into productive agricultural landscape.

In the Terrace chapter, Denevan takes on neo-environmental determinism that attempts to explain cultural change, collapse of civilizations, and agricultural abandonments in the Andes and Amazonia by evoking climate change. He shows that terrace farmers actively responded to their changing situation by concentrating and increasing production through selective abandonment and addition of irrigation.

Agricultural Collapse and Abandonment

The Terrace chapter is a short summary of a much larger edited work (Denevan 1986–1988) presenting the research and key conclusions of the Colca Valley Terrace Project directed by Denevan from 1984 to 1986. This publication was followed over the years by numerous dissertations and books and scores of articles and book chapters by Denevan and colleagues. Geographers, soil scientists, ethnographers, archaeologists, ethnohistorians, and others were assembled by Denevan to study the near-continuous 60 km of terracing in the Colca Valley, and as a result the valley became a testing ground of new techniques, interdisciplinary teamwork, and hypothesis testing that continues to the present. In a true multiscalar study, Denevan's investigators worked at the microscale of soil grains, potsherds, and individual fields; the medium scale of integrated field blocks, irrigation canal networks; and the macroscale of region: the middle Colca Valley.

Andean scholars have long wondered why large areas of once productive lands in the Andean coast, valleys, and highlands were abandoned (an estimated 60–80 percent of all terracing), some obviously before European conquest was abandoned sometime in the past. Denevan summarizes traditionally proposed factors or causes of terrace abandonment in the Colca Valley into "socioeconomic and demographic factors" (depopulation and resettlement through Inca and European reorganization, labor availability, land-use change, access to land and water, water management efficiency, and alternative livelihoods) and "natural factors" (climate change, tectonic uplift, water availability, vegetation, and soil change). Neo-environmental determinists assumed failure of the terrace technology and farmer's inability to adapt to the changing environmental situation as the cause.

Denevan first addresses the relation between cultivation intensity and productivity measured by productivity per unit area cultivated and total area cultivated during the later prehistory, the colonial period, and today. He notes that terraces on lower slopes and valley floor are irrigated, while terraces on upper slopes are not. Denevan shows that the process of abandonment and the causes involved were complex and that terrace agriculture is dynamic and nonlinear and involves constant decision-making by their users. After evaluating late prehistoric climate change and assuming that a period of drought and cooler temperatures occurred, he documents a contraction of terrace use during late prehistory and early colonial period rather than wholesale abandonment in the Colca Valley. Denevan argues that farmers intentionally abandoned of the upper slope terraces and began to intensify production on the lower slope and valley-floor terraces that can be irrigated and used year-round. Despite reduced total terraced land used, production remained the same or probably increased through the agricultural intensification of a smaller cultivated area. Rather than respond passively to climate change, farmers actively reorganized their landscape through social and technological innovations to meet the new challenges and expand capacity.

Second, Denevan attributes post-Inca abandonment of many of the irrigated terraces primarily to colonial depopulation mainly from introduced epidemic diseases. This depopulation for the Colca Valley is documented by Cook (1982).

Third, the failure to restore abandoned terraces today, despite population growth in Peru, seems to be the result of high labor costs, ownership issues, and rural to urban migration.

Denevan's Colca Valley Project inspired a wave of new research on terraces and indigenous farming systems more generally. More recent research demonstrates that terrace soils are anthropogenic creations for managing fertility and moisture and preserve remarkable fertility and productivity through time (Sandor 1987, 1992; Treacy 1994; Treacy and Denevan 1994; Goodman-Elgar 2008). Dating of settlements and actual terraces support Denevan's interpretation (Malpass 1987; Vera Cruz Chávez 1987; Brooks 1998). Ethnographic observations of contemporary terrace farmers demonstrate that terracing is sophisticated indigenous knowledge passed down over generations and part of an integrated system of land use, land tenure labor for construction, and maintenance, demonstrating long-term continuity and bottom-up organization of irrigation and terracing (Gelles 2000; Treacy 1994; Trawick 2003). Others show that much of the terrace structure and community organization recorded in the historical, ethnographic, and geographic literature for the Colca Valley is the product of Inca reorganization of this terraced landscape in late prehistory (Gelles 2000). Wernke (2013) demonstrates considerable continuity in terrace ownership, labor, and management between the structures and policies imposed by the Inca and the later Spanish colonial government through GIS, colonial documents, toponyms, and survey.

Applied Archaeology

Although the applied potential of pre-European agricultural research to the contemporary world rarely appeared in his early work, Denevan's publications on raised fields and terraces set experimental and applied archaeology into motion in Peru and Bolivia and were published at a time when scholars, governments, nongovernmental organizations, local communities, and the general public were interested in models of "appropriate" or sustainable technology, traditional Andean agriculture, and indigenous crops. In the late 1980s through the 1990s, nongovernmental organizations such as CARE, PIWA, and Catholic Church and government agencies such as CONCYTEC, the Ministries of Education and Housing, UNESCO, USAID, embassies, and political parties such as APRA supported "make work projects" involving terracing and raised fields in Peru and Bolivia (Torre and Burga 1986; Ramos Vera 1986a, b; Erickson 1985; Canahua Murillo and Larico Mamani 1992; Rivera Sundt 1989; Kolata 1996; Morris 2004).

Although relatively unsystematic and lacking continuity, the raised field and terrace experiments and small-scale development projects provided valuable insights about energetics, social organization of labor, crops cultivated, carrying capacity, functions, and sustainability. Despite the valuable contributions to understanding "abandoned" field systems through experiments in terrace and raised field agriculture, the various development and community projects spawned by this research had less success. The revolt led by the Shining Path, the violent military and police response, general civil unrest, and the poor economic situation throughout the 1980s and 1990s paralyzed scholarly, NGO, and government research on traditional agriculture and associated development projects. In addition, changes in production, markets, land tenure, and crops; lack of local labor; urban migration and rural abandonment; changing livelihoods; and other factors can be blamed for the lack of success as a development project (Erickson 2003; Morris 2004), while others blame farmers, scholars, and traditional agriculture for failure (Swartley 2002; Baveye 2013).

Conclusion

Although trained as a geographer, Denevan and colleagues successfully used archeological techniques such as systematic field survey, targeted excavation of specific landscape features, dating through pottery styles and radiocarbon analysis, pattern analysis, and analogy to understand the creation, structure, chronology, functions and use, maintenance, and abandonment of terraces and raised fields. The two publications helped lay the groundwork for Denevan's thinking about carrying capacity of intensive agriculture and implications for demography of indigenous peoples. At the same time, Denevan was mentoring a large number of motivated graduate students of diverse backgrounds from Wisconsin and other universities who are now professional colleagues exploring and expanding his pioneering work throughout the Americas.

Although framed in what would now be considered a landscape perspective, neither of these two publications use the word “landscape” in its modern sense (the *Science* article uses “landscape” twice to refer to natural environment and the Terrace chapter once). Denevan laid the groundwork for important research about the social organization of labor and taskscapes (Erickson 1993; Walker 2004, 2011), energetics (Denevan 1982; Gómez-Pompa et al. 1983; Erickson 1985), landesque capital (Blaikie and Brookfield 1987; Håkansson and Widgren 2014), agrodiversity (Brookfield 2001), historical ecology and indigenous knowledge systems (Erickson 2000, 2006; Balée and Erickson 2006), and appropriate technology (Renard et al. 2012; Erickson 2003; and detractors Baveye 2013; Lombardo et al. 2011; Swartley 2002). Denevan’s contributions to knowledge about the origins, duration, and scale of human transformation of environment into landscape presage contemporary debates about the existence, origin, and timing of the Anthropocene.

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2.1 Drained-Field Cultivation in the Americas

Original: Denevan, W.M. 1970. Aboriginal Drained-Field Cultivation in the Americas. *Science* 169(3946):647–654. Reprinted by permission of the American Association for the Advancement of Science.

Abstract Pre-European reclamation of wet lands was widespread in the savannas and highlands of Latin America. Large numbers of drained ridges [raised] fields are found in Mexico, Central America, South America, and also in North America. Such intensive agriculture was apparently associated with relatively high population densities and probably complex social organization.

Keywords Chinampas · Drained fields · Garden beds · Indigenous agriculture · Raised fields · Latin America

Introduction

Where population growth begins to exceed the existing carrying capacity, self-sufficient aboriginal farmers may either migrate or increase production through higher yields, more frequent cropping, or the utilization of land which is marginal because of poor soil, steep slopes, lack of water, or excessive water. The denser populations and greater cultural achievements in the pre-European New World have been mainly associated with agricultural expansion by means of irrigation and terracing, and, accordingly, these reclamation methods have received considerable attention. In contrast, cultivation of land having excessive water has been almost ignored, mainly because such agriculture seldom survived into the European period, except in Mexico.

In recent years, extensive ridged-field remains have been found in seasonally flooded plains in widespread parts of lowland and highland South America. (Location and distribution maps can be found in Parsons and Denevan (1967).)

While much remains to be learned, a review of what is known about drained-field agriculture in the New World, and some of the implications, seems worthwhile at this time.

There are different types of poorly drained ground, each presenting different problems, as well as advantages, for reclamation and subsequent cultivation. Most obvious are the large river floodplains which are inundated during periods of high water. Because of the rich alluvial soils, population and cultivation are often concentrated in or adjacent to floodplains, with or without drainage control. However, some of the most extensive areas of inundation are large plains with broad, shallow depressions which fill with rainwater, and here soils may be quite poor, especially in the tropics.

Flooding is also characteristic of the immediate margins of lakes, rivers, and swamps, which fluctuate in coverage both seasonally and from year to year. In addition, there are sites covered by permanent, but shallow, bodies of water which can be cultivated following diking or draining, or through building soil islands. The least severe type of poor drainage is soil waterlogging, with little or no standing surface water, which can occur on sloping land as well as on flatland. Most crop root systems do poorly if waterlogging lasts more than a few weeks. But waterlogged soil can be readily reclaimed by ditching or by building up low crop ridges, if fertility warrants it, or cultivation can be concentrated during the dry periods, as long as there is adequate moisture.

An excess of water is obviously a great handicap to cultivation; however, this is frequently offset, in part, by high-quality soils, level surfaces, the availability of irrigation water, and the richness of aquatic wildlife resources. On the other hand, the labor requirements for constructing and maintaining drainage systems are high, and it is unlikely that drainage would be undertaken so long as other, more easily cultivable land was available.

The following types of wetland cultivation can be differentiated: (i) soil platforms built up in permanent water bodies; (ii) ridged, platformed, or mounded fields on seasonally flooded or waterlogged terrain; (iii) lazybeds or low, narrow ridges on slopes and flats subject to waterlogging; (iv) ditched fields, mainly for subsoil drainage; (v) fields on naturally drained land, including sandbars, riverbanks, and lake margins; (vi) fields diked or embanked to keep water out; and (vii) aquatic cultivation, in which complete drainage is not attempted and plants are grown in water. An agricultural system may employ more than one of these methods. Also, some techniques may serve functions other than drainage alone, such as irrigation, soil aeration, and improvement in the fertility of the soil. All these methods, with the exception of aquatic cultivation, were practiced by aboriginal people in the Americas. Brief descriptions of the major known examples follow. There was nothing comparable to Asian wet rice cultivation, although some wild aquatic plants, such as wild rice and the camus bulb, were harvested.

Chinampas

The one form of pre-European drainage that has long been known and well-studied is the *chinampa* system of the Valley of Mexico (Schilling 1939; West and Armillas 1950). *Chinampas* are rectangular garden plots built up above the water level of shallow lakes. These fields are characterized by the use of seed beds, fertilization with mulches of aquatic plants and bottom muds, and year-round irrigation by means of seepage and with water scooped from the bordering canals. The result is a continuous succession of crops – one of the most intensive systems of agriculture, past or present, in the Western Hemisphere, in terms of total annual production per unit of land. The *chinampas*, which played a major role in feeding the Aztec capital of Tenochitlán, were concentrated in lakes Zumpango, Xaltocán, Texcoco, Xochimilco, and Chalco. They are still being created and cultivated in the southern portions of Chalco and Xochimilco, and these modern survivals provide clues not only to ancient *chinampa* agriculture but also to the functions and technology of other types of drained fields no longer in use.

The first *chinampas* may date back as much as 2,000 years (Coe 1964). Pedro Armillas has attempted to measure the former distribution of *chinampas* on the basis of fossil remnants (*tlateles*) in drained portions of former lakes and suggests a preliminary total of 10,000 ha for just lakes Chalco and Xochimilco (Armillas 1968). While modern *chinampas* are quite broad (5 to 15 m), the fossil fields are often much narrower, suggesting that the present system is not necessarily "virtually unchanged" from the time of Cortes (Moriarty 1968). The narrow fossil fields are closer in form to the ridged fields of South America; also, many of them are found on former lake margins on relatively high land where flooding was not permanent but was seasonal, as is the case with the South American ridged fields.

Ridged Fields of the Wet Savannas

There is no need to elaborate here on the *chinampas* and their important role in Mexican culture history. However, they are not as unique in the Americas as they were once believed to be. Since 1962, numerous remnants of large raised planting surfaces have been found in South America in both lowland tropical savannas and highland basins on terrain subject to seasonal inundation. These features have usually been referred to as "ridged fields," less often as "raised fields" or as *camellones.*

There are at least 170,000 ha (1700 square km) of ridged-field remnants in Surinam, Venezuela, Colombia, Ecuador, Peru, and Bolivia (Fig. 2.1). Some are isolated, but others are near modern settlements and roads. In view of their size and number, the fact that they have been ignored until recently is a puzzle. However, the ridges are not conspicuous on the ground, often being severely eroded or partly buried in sediment (Fig. 2.2). From the air, on the other hand, the ridges form distinctive and obviously human-made patterns (Fig. 2.3), especially at times of the

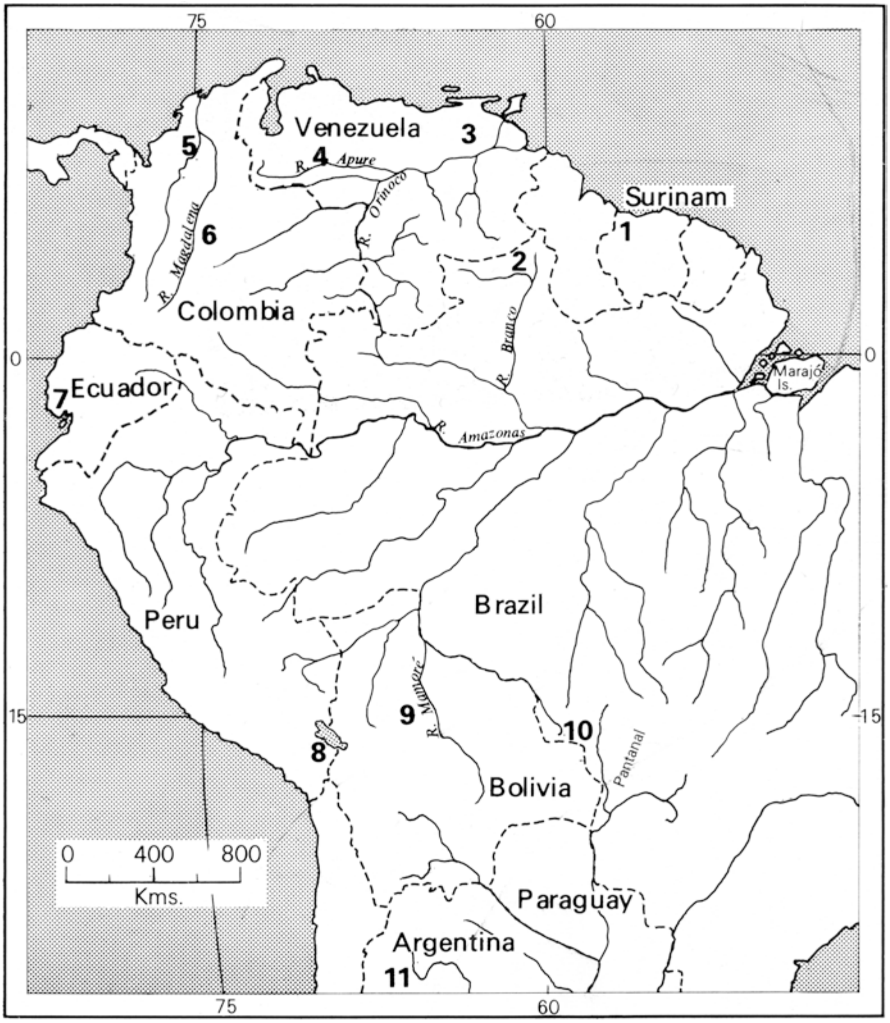

Fig. 2.1 Map of the drained-field areas in South America mentioned in the text. 1, Hertenrits; 2, Makuxí; 3, Karinya; 4, Caño Guanaparo; 5, San Jorge; 6, Sabana de Bogotá; 7, Guayas; 8, Lake Titicaca; 9, Llanos de Mojos; 10, Guato; 11, Lerma Valley

year when contrasting grass tones between ridges and ditches stand out, or when the ridges rise above floodwaters. Also, there has been a general lack of awareness, by both local people and scholars, of what long rows of linear soil features in swamplands might mean. In studying and mapping the ridged fields, aerial photographs have been essential, and these have only recently become available for much of South America.

Except for two very brief reports of obvious ridged fields in unidentified parts of the Orinoco Llanos in the sixteenth and eighteenth centuries (Castellanos 1955, 1:

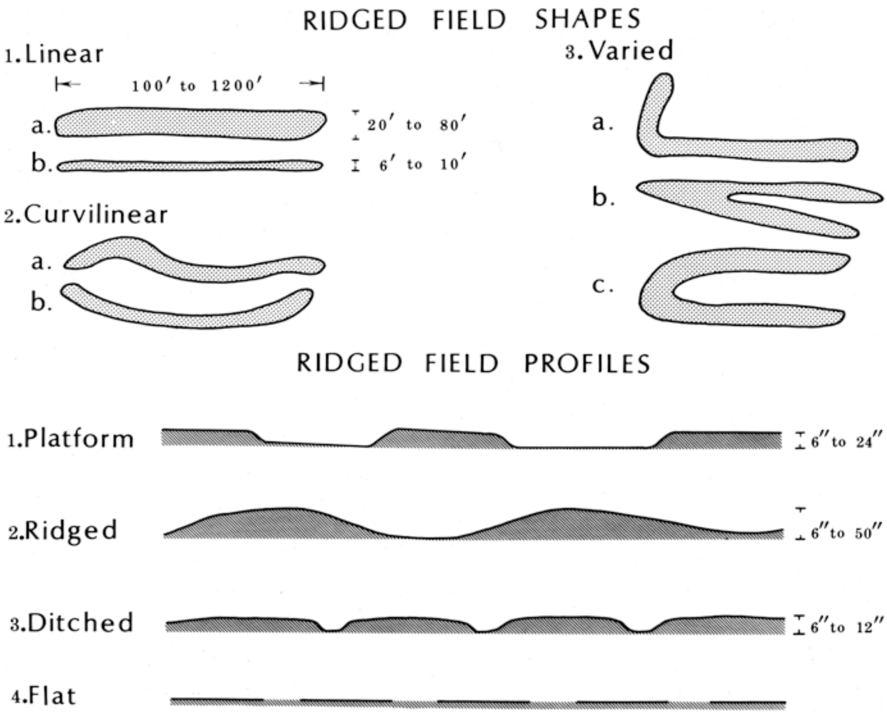

Fig. 2.2 Sketches of ridged-field shapes (above) and profiles (below) in South America. The "flat" profile is one where ridge and ditch relief has been leveled by plowing or sedimentation but with soil differences persisting

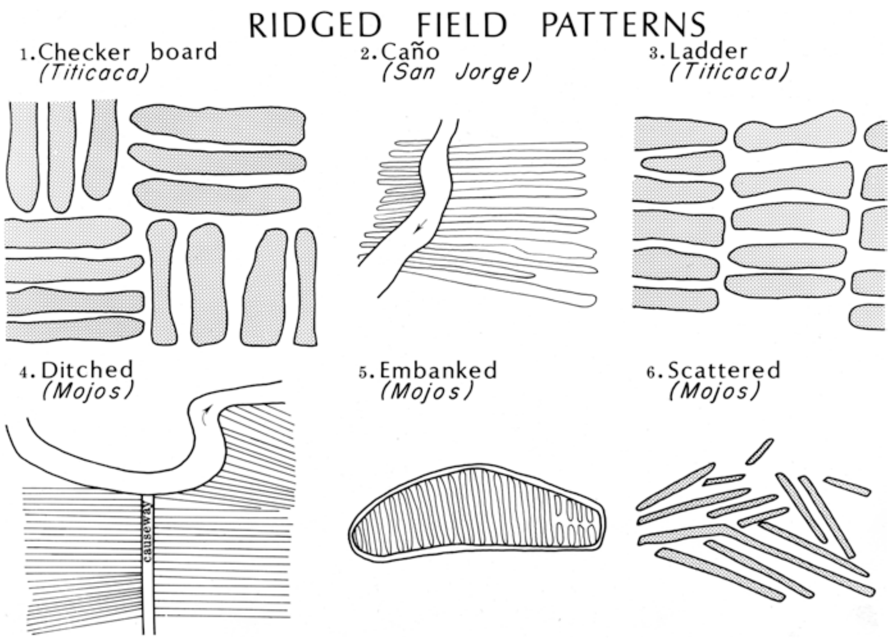

Fig. 2.3 Sketches of ridged-field patterns in South America

539; Gumilla 1945, 430–432), there apparently is no mention of such farming in the literature either of the conquest or of the colonial period. Presumably, the system was given up before the Spaniards arrived or very soon thereafter. In 1916, Nordenskiöld mentioned drainage works in the Llanos de Mojos of Bolivia, but he seems to have missed the largest concentrations and apparently was not impressed. The Reichel-Dolmatoffs mentioned drained fields in the San Jorge area of Colombia in 1953 but gave few details (Reichel-Dolmatoff and Reichel-Dolmatoff 1953, 14). Otherwise, the discoveries and descriptions of ridged fields have come only in the last few years.

A large portion of the vast tropical savannas of South America are subject to seasonal flooding for periods lasting up to several months. With their generally low soil fertility and their excessive water during the growing season, it is not surprising that these "wet" savannas are considered marginal for farming. The tribes of these savannas were agricultural, but they ordinarily confined this activity to the galley forests and forest islands within the savannas. Some of these tribes, such as the Yaruro in the Orinoco savannas and the Siriono in the eastern Mojos savannas, are now seminomadic hunters and gatherers for whom agriculture is of secondary importance (Holmberg 1960; Leeds 1961). Thus, it is some significance that evidence of drained-field cultivation has been found in several savannas.

In 1961, oil exploration geologists in the Mojos savannas of the Department of the Beni in eastern Bolivia came upon large numbers of low, parallel ridges which could only have been made by people (Denevan 1962; Plafker 1963). Independent field study and analysis of aerial photographs and aerial reconnaissance the following year provided a good idea of the nature, number, distribution, ecological situation, and probable function of the relic ridges, as well as revealing other, associated earthworks, including mounds, causeways, and canals (Denevan 1963, 1966, 84–96).

The Llanos de Mojos cover some 180,000 square km in the Beni Basin between the Andes and the western hills of the Brazilian Highlands. For a description of the drainage of the Beni Basin, see Plafker (1964). During high water, the Beni, Mamoré, and Guapore rivers and their tributaries overflow the *llanos*, and as much as 80 percent may be under a few centimeters to a meter or so of water for as long as 6 months. The climate is humid tropical, but the vegetation is open savanna with scattered forests on the higher, better drained ground. Soils are mostly high in clays and low in organic content, so it is not surprising that agriculture today is confined to the better and dryer soils of the forest, while the savannas are used for cattle raising. Nevertheless, there is mute evidence of former intensive savanna cultivation in the form of tens of thousands of raised field remnants which, together with intervening ditches, cover at least 20,000 ha.

West of the town of Trinidad, the old fields consist of narrow, closely spaced ridges (Fig. 2.3), but to the north, near Lago Rogoaguado, the fields are actually rectangular platforms as much as 25 m wide and 400 m long (Fig. 2.3). Elsewhere there are rows of small circular mounds, about 2 m across, similar to the *montones* reported in Hispaniola in the sixteenth century (Sturtevant 1961). Most of the fields are 15 to 50 cm high, wEuropeannough to place them above the level of average floods.

The Spanish explorers and the Jesuits found relatively sophisticated tribes in Mojos (the Arawakan Mojo and Baure) with large villages characterized by plazas, palisades, and moats, and containing as many as 3,000 people, but they made no mention of drained fields. However, the region was not settled and fully explored until the 1680s, by which time the savanna tribes already had been decimated by European diseases, and their cultures disrupted by the direct and indirect effects of initial Spanish contacts. Nevertheless, Jesuit accounts indicate that at least 100,000 Indians still remained in Mojos in the 1690s (Denevan 1966, 116). There has been little archaeology in the region, so the antiquity of the various earthworks is not known, but at the time of first contact in 1580, the Mojos tribes may still have been constructing them in order to provide artificial high ground for agriculture, transportation, and settlement.

The Mojos finds alerted scholars to the possibility of other relic drained fields in South America, and a number of such features have now been described. Particularly spectacular fields were found and photographed in 1965 by Parsons and Bowen in the San Jorge River floodplain, a part of the vast aquatic landscape of the lower Río Magdalena in northern Colombia (Parsons and Bowen 1966). The mapped fields, covering at least 64,000 ha of ditches and raised surface, consist of (i) ridges on, and perpendicular to, the back slopes of natural levees of stream channels (*caño* pattern, Fig. 2.3); (ii) short ridges arranged in checkerboard patterns (Fig. 2.4); and (iii)

Fig. 2.4 Partly inundated pre-European ridged fields in the Río San Jorge floodplain of northern Colombia. (Courtesy J.J. Parsons and W. Bowen)

parallel, but unoriented, ridges. The ridges are as much as 2 m high, 7 m wide on the average, and up to 1½ km long.

In Surinam, prehistoric ridged fields have been reported in the coastal savannas. The main concentration is associated with a large artificial mound called Hertenrits, built about AD 700, near Caroni in an uninhabited wet savanna now being reclaimed as part of a rice project (Laeyendecker-Roosenburg 1966; aerial photographs in Parsons and Denevan 1967). Here the ridges are short and narrow, with no regular arrangement.

In the Guayas River floodplain in Ecuador, there are at least 4,000 ha of old ridges and platforms (Parsons 1969). It is especially surprising that they have been ignored, since they are very close to the city of Guayaquil and since there has been archaeological research in the region (Meggers 1966). The field remnants are associated with *tolas*, artificial burial and house mounds built by the gold-working Milagro culture, which occupied the floodplain from about AD 500 to the arrival of the Spaniards. While ridges are no longer being constructed, some of the old ones are again being cultivated, with rice in the ditches and maize and other crops on the raised surfaces.

The fifth area of relic ridged fields in lowland South America is the Orinoco Llanos of Venezuela. This is one savanna for which there are historical accounts of ridged-field construction (with *macanas*), fertilization (grass mulch), and use (maize, manioc, and peppers) (Castellanos 1955, 539). No locations were given, but the Llanos seemed a likely place to search for ridged-field remnants because of the region's possible role as a major cultural hearth in prehistoric times (Rouse and Cruxent 1963, 88), the environmental conditions being similar to those of the other ridged-field areas, and the known presence of other earthworks (mounds and causeways) probably built for adaptation to floods (Cruxent 1966). Reports of ridges in the Tame-El Yopal area of the Colombian portion of the Llanos have not been verified and may result from confusion with wind-oriented fire scars. However, aerial photographs show an apparent cluster of ridged-field remnants along the Caño Guanaparo in southern Barinas, about 15 km north of the Río Apure and from 125 to 150 km west of San Fernando de Apure. These were first reported by J.H. Terry from the Inter-American Geodetic Survey (pers. comm.). The ground stripes are at right angles to old stream meander scars, similar to some of the San Jorge patterns, and are fairly narrow and as much as 700 m long (Fig. 2.5). The Orinoco Llanos seems to be undergoing very rapid sedimentation, so it is possible that many old fields have been buried.

Undoubtedly more ridged-field remnants will be found in tropical America. The seasonally flooded Brazilian savannas, especially on Marajó Island and in the Pantanal of Mato Grosso, are likely areas. The finding, by A. Siemens (pers. comm.), of a group of apparently pre-European ridged fields in 1969 in the Candelaria area of the western Yucatán peninsula may be one indication of intensive cultivation by the lowland Maya.

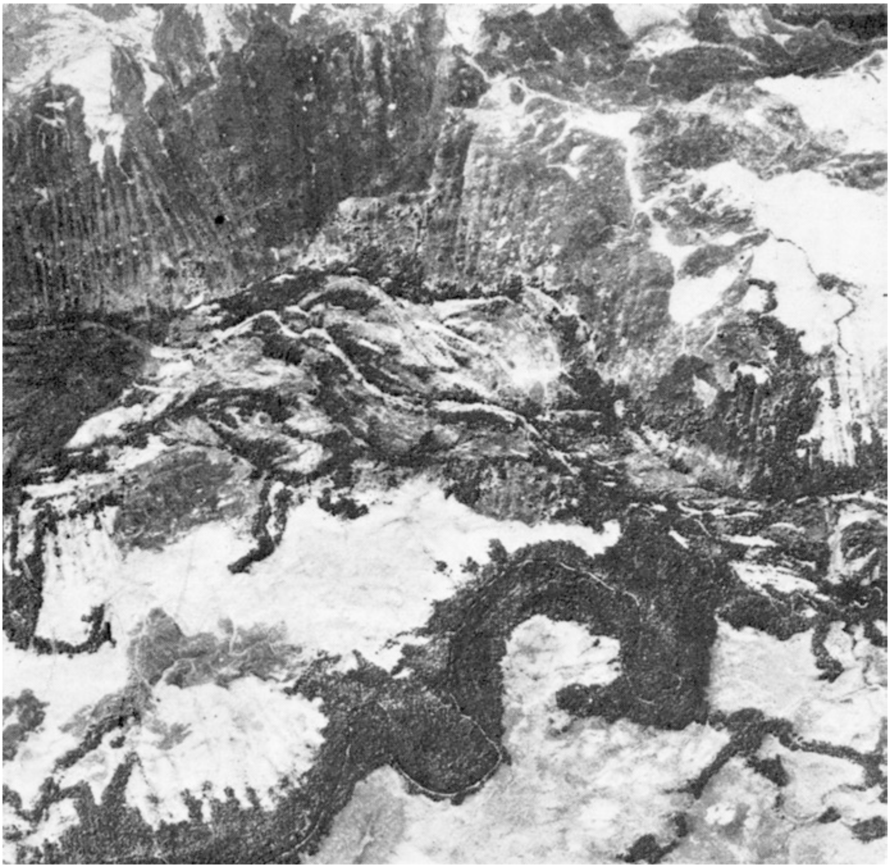

Fig. 2.5 Ridged-field remnants at right angles to streams near the Caño Guanaparo (at bottom) about 130 km west of San Fernando de Apure in the Orinoco Llanos of Venezuela. (Scale: about 1:37,000). (Courtesy J.H. Terry)

Highland Drained Fields

Drained-field systems, relic and contemporary, occur in the highlands of the Andes and Mexico. They are less of a surprise there than in the lowlands, since the highlands contained sophisticated civilizations with other forms of agricultural earthworks. Nevertheless, newly found highland fields add support to the arguments for very dense populations in the Lake Titicaca (Smith et al. 1970) and Bogotá basins, in the same way the *chinampa* system supports similar arguments for the Valley of Mexico.

The prehistoric Titicaca and Bogotá ridged fields resemble those in the lowlands more than they do *chinampas*, since they were built for reclaiming seasonally flooded terrain. The Titicaca fields are the most extensive of all, covering a measured 82,056 ha (Smith et al. 1968). The Titicaca fields were first called to our attention by F. Monheim and J. Dickenson. Nevertheless, they were overlooked until 1966 – even by me when I drove through them between Puno and Juliaca in 1965. They are arranged in a variety of patterns and sizes, but some are as much as 2 m high and 25 m wide and appear as islands along the lake edge during high water (Figs. 2.6 and 2.7). Most are located on the western lake plain in both Peru and Bolivia at elevations of 3,800 and 3,850 m. Crops of potatoes, quinoa, cañihua, and barley are grown here today but mainly on hill slopes. Because of high alkalinity, poor drainage, and night frosts, but also for historical reasons, most of the lake plain has been used for ranching since the conquest, rather than for farming. Ancient fields have thus been preserved, although encroaching plow cultivation in some sectors is destroying what remains.

The Sabana de Bogotá in Colombia is a broad mountain basin, at an elevation of 2,600 m containing seasonally flooded and waterlogged land. With good soil and a temperate climate, the Sabana has been a grain-producing area since at least the sixteenth century, with the aid of subsoil drainage by way of elaborate ditch and drain-off canal systems. Narrow garden beds (*camellones*) are made today by farmers without plows to improve drainage and to handle the heavy clay soils, and such beds are possibly aboriginal in origin (Donkin 1968; Eidt 1959). They do not compare in size with the ridged fields of Titicaca and the lowlands. However, Broadbent has recently discovered the remains of large, ancient drained fields, mostly in the area of Suba, some 20 km north of Bogotá (Broadbent 1968). Many of these are in zones covered until recently by forest and probably owe their preservation to the fact that they, in contrast to most of the Sabana, have not been plowed over for four centuries. In aerial photographs, dated 1940, of Hacienda La Conejera, the former field patterns show up clearly within modern fields recently cleared from forest. On photographs taken of the same site in 1968, after only 28 more years of plowing, the old fields are much less distinct. As seen from the ground, the old fields showed no greater relief than the surrounding land. They are discernible only because of differences in the soil which have persisted, despite the leveling of the ridges and filling of the ditches, so that there is now a differential growth of maize and wheat crops which is expressed in rectangular block form. It seems reasonable to speculate that much of the Sabana de Bogotá was covered by ridged and also ditched fields prior to the arrival of the Spaniards and that these fields were related to the population density and to the cultural achievements of the great Chibcha chiefdom of the Sabana.

Large ridged fields have not been found in the smaller, poorly drained basins in the Andes. However, in Peru I have seen deeply ditched fields in use today (as they have probably been for a long time past) in the Pampa de Anta near Cuzco, the Taraco and Pomata areas of Lake Titicaca, and elsewhere. They are similar to the present Bogotá ditch systems, although smaller and less systematic. Rather than creating raised surfaces, they serve to reclaim waterlogged soils by providing subsoil drainage and by channeling excess water away from crops. The present Sibundoy Indians near Pasto in the Colombian Andes also drain their fields by this means (Bristol 1968).

There is another form of drained field which is very common in the Andes today and is clearlEuropeaniginal origin. This is the lazybed system, locally referred to as

Fig. 2.6 Ground view (July 1966) of one of the large ridged fields at the edge of Lake Titicaca near Requeña, Peru. The pockmarks were made by rooting pigs. Note the eroded edges of the ridge and the alkaline deposits on both sides of the ridge

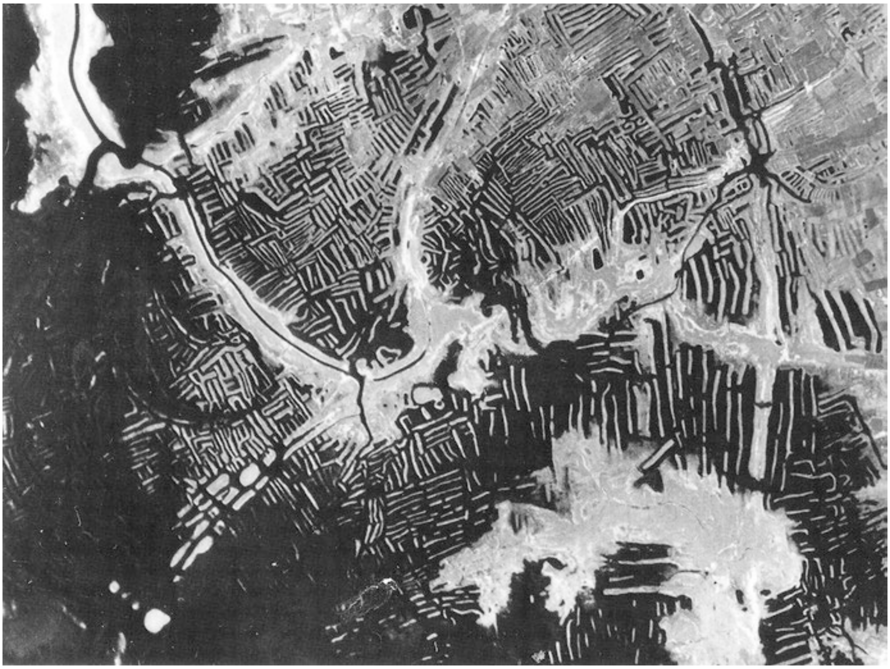

Fig. 2.7 Pre-European raised fields extending into Lake Titicaca northeast of Juliaca in Peru. [This was the cover photo in the original]. (Photo from the William Denevan collection)

huachos or *eras*, which is found from Colombia to Bolivia (West 1959). Potatoes are the main crop, along with cañihua, barley, and arracacha. These fields are commonly made with the Andean foot plow (*chaqui taclla* – similar to the Scottish *cashcrom*), which is used to break and turn heavy grass sods where larger plows are not available or usable, especially on steep slopes. However, lazybeds are also made on poorly drained flat terrain and waterlogged lower slopes in order to improve drainage. Usually they are about 1 meter wide and 30 to 60 cm high. Some of the Titicaca ridged fields have been or are being destroyed by lazybeds built on top of them.

In the Mexican highlands, there are other types of drained fields besides the *chinampas*. The only detailed published descriptions are by Wilken for Tlaxcala (Wilken 1969), but similar fields exist elsewhere – for example, in Puebla, Oaxaca, and the State of Mexico – in poorly drained valley and basin bottoms. In southwestern Tlaxcala contemporary drained fields occur on sites ranging from swampland to land watered only by seasonal rain. In all cases waterlogged soil must be two words swamp land drained before cultivation is possible. The main technique is to dig drainage canals several meters wide and deep, but raised garden plots some 10 m wide may also be built up between the ditches. The ditches are periodically cleaned, and muck and vegetation are spread on the fields to increase fertility.

North American Garden Beds

In North America, remnants of aboriginal raised fields were quite common until most were destroyed by plowing in the nineteenth century (Schoolcraft 1860, 1:58–64; Fox 1959; Peske 1966; Fowler 1969). These fields, usually referred to as “garden beds,” are linear or curved and fairly narrow (1 to 4 m), and they occur in clusters of several hundreds. At least 180 such clusters have been reported in Wisconsin and Michigan, and undoubtedly there were once many more. The primary function of the garden beds may have been to break up grassland sod. However, many if not most of the fields occur in the bottomlands of the Mississippi drainage system; hence they probably also had a drainage purpose. There seem to be no historical descriptions of the garden beds in use. Probably they were a product of the “Mississippian” cultures (about AD 700 to 1500), which were characterized by a dependence upon agriculture and by a village orientation to “the major streams with large alluvial floodplains which provided fertile and easily worked soils” (Griffin 1967).

The occurrence of ridged fields in the mid-latitudes and subtropics is partly confused by what is called *gilgai* topography. This is a landscape crumpled into a complex microrelief of mounds and ridges and depressions. It is known, in North America, India, Australia, and Africa – regions where there are marked wet and dry seasons – usually in lowland savannas and grasslands. *Gilgai* seems to be the result of the “self-plowing” of black, montmorillonite clay soils experiencing severe seasonal swelling and contraction. In some instances, the topography may take the form of parallel linear or curved stripes and depressions somewhat similar to garden beds, lazybeds, and ridged fields (Hallsworth et al. 1955 – see especially plate IV, of "wavy gilgai"). Some features considered to be made by humans such as ridged fields may actually be *gilgai* soils, and some features labeled *gilgai* may really be ridged fields. Certainly, soil scientists and culture historians should be aware of the alternative possibilities.

Other Types of Drained Fields

One of the few contemporary examples of Indian cultivation of wet savannas is in the Rio Branco campos of northern Brazil, where the Makuxí (Macusí) Indians near Serra do Flechal build large manioc mounds some 50 cm high and 1 to 2 m across (H, Sternberg, pers. comm.). These are larger than the small hillocks of tropical forest cultivators and may be comparable to the *montones* of Hispaniola and Mojos. Over 1,000 small mounds in an orchard pattern were found in the Lerma Valley in Salta, Argentina; they seem to have been used for cropping of poorly drained ground (Von Rosen 1924).

Large burial and settlement mounds of aboriginal origin are common throughout tropical America, and many are located on land subject to seasonal flooding, as in Mojos and on Marajó Island. Many of these mounds have had farms on them in the past and still do. One striking example is the Guato Indian mounds reported by Schmidt in 1914 in the savannas between the upper Río Paraguay and the upper Rio Guaporé in western Mato Grosso, one of which, 2 m high, European40 by 76 m (Schmidt 1914). Groves of *Acuri* palms (*Attalea*) were planted on top.

There are a few instances of embanked or diked fields, in which walls were built to keep water out, in Mojos (Fig. 2.3) and the Titicaca plain, but ridges, mounds, and ditches are the more common means of coping with poor drainage.

The cultivation of river land (banks, sandbars, low islands) during low water has been a common aboriginal practice in the floodplains of the Amazon, Orinoco, and other large rivers. While little is known about such agriculture, it was apparently common in the past and is still practiced by both Indians and settlers today. The large Omagua tribe, for example, concentrated cultivation on Amazon *playas* and on islands subject to flooding (Sweet 1969). Crops are planted which have a short growing season and do well in sandy soils – such as peanuts, sweet potatoes, squashes, and some beans.

Seasonally flooded savannas have also been cultivated during the dry season. Padre Gumilla in the eighteenth century reported that the Otomaco Indians between the Orinoco and Río Apure in Venezuela planted crops on the grassy margins of savanna lagoons as the lagoons dried up (Castellanos 1955, 1:539). Today the Karinya in the Orinoco Llanos cultivate *moriche* (*Mauritia*) palm swamps during the dry season with the aid of surface mulches and ash and, in some places, with drainage ditches dug by communal labor (Denevan and Schwerin 1978). Mounding or ridging is minimal, but this system may be a survival of more elaborate earlier practices.

Discussion

There is now evidence, mostly of a reconnaissance nature, of a wide variety of drained-field agricultural systems occurring in a variety of ecological situations in the Americas. However, except for the fields in highland Mexico, we now very little about these systems and the societies responsible for them. The South American field remnants have not been dated or correlated with archaeological materials, and there are few descriptions of the fields in use. That the relic ridged fields were agricultural is implicit, there being no other rational explanation for them (Parsons and Denevan 1967). The fields are sufficiently extensive and impressive to warrant detailed archaeological and ecological studies. Also, additional areas of field remnants may be discovered through careful examination of conventional aerial photographs or with the aid of infrared and other remote sensing techniques. See, for example, Schaber and Gumerman (1969) and Deuel (1969, 40–58) for discussions of photo techniques used in Europe to detect prehistoric "crop marks." Meanwhile, only preliminary conclusions can be drawn concerning causation, techniques, crops, and demographic and cultural implications.

Drained-field agriculture is, of course, common today in the mid-latitudes as increasing efforts are being made to cultivate marginal lands by modern techniques. In Europe, there are classic preindustrial examples, such as the fens of England and the polders of the Netherlands (Darby 1956; Smith 1967, 444–464 and plate 41). But it is to the tropics of the Eastern Hemisphere that we may look for aboriginal drainage techniques that might provide the best clues to pre-European practices in tropical South America. Ridge and mound agriculture is common in the African savannas, but the primary function seems more often to be for aeration, root removal, and concentration of topsoil rather than for drainage (Netting 1968, 58–60; Faulkner 1944). In Melanesia, however, there are excellent contemporary examples of ridged-field agriculture in poorly drained situations. Fields photographed in the Baliem Valley of New Guinea are remarkably similar in pattern and dimensions to some of those in South America. (See Brass [1941] and also Gardner and Heider [1968], especially photograph 24 and also photographs 92–102.) Prehistoric drained fields in the Wahgi Valley of New Guinea have been dated as older than 350 BC plus or minus 120 years by Lampert (1967). Sweet potato fields are constructed by teams of men using simple digging sticks and wooden spades to raise long ridges by throwing earth up from drainage ditches. Fertility is maintained by mixing in a mulch of grass and aquatic plants that make it possible to grow crops continuously for several years before fallowing. The *chinampas* of Mexico are managed somewhat similarly, and the same methods must have been used in South America. It is unlikely that aboriginal people would have developed elaborate drainaEuropean unless each field could have been cultivated numerous times.

But why attempt to farm poorly drained land in the first place, especially when there is more fertile and more cultivable land locally available? For one thing, seasonally flooded areas generally have rich protein resources in the form of fish and other aquatic life such as various turtles, rodents, and birds. Such resources are especially important in the South American tropical lowlands, where the stable crop is manioc, which has a very low protein content. The same reasoning might apply to the Lake Titicaca plain, where the potato is the staple. Proximity to rivers and riverbanks is, of course, also important for transportation and for fertile, easily worked soils.

A second possible reason for the great efforts that were, and are, expended on building and maintaining drained fields is demographic. Boserup (1965) and others have argued that agricultural intensification is a result of, rather than a cause of, increased population, and this does seem to be true for many preindustrial societies. In New Guinea, for example, for groups of essentially the same culture, the ratios of extensive to intensive cultivation vary with local population densities. The argument has been applied to aboriginal agriculture in New Guinea by Brookfield (1962) and Clarke (1966). An increase in productivity per unit of land is resisted as long as there is adequate land available for more extensive systems, mainly because intensification requires greater inputs of labor, at least initially, in terms of productivity per man-hour. In the South American savannas, population must have been initially concentrated on the more fertile soils of the gallery and island forests. Such forest soils now cover only about 10 to 15 percent of the Llanos de Mojos. Once the forest soils were fully in use, surplus populations might have been motivated to cultivate the savannas despite the greater labor required. Probably, as population increased, more and more savanna was brought into cultivation.

That the ridged-field areas in the lowland savannas were relatively densely settled is suggested by historical documents of Mojos (Denevan 1966, 116) and by archaeological evidence for San Jorge (Parsons and Bowen 1966) and Guayas (Parsons 1969; Meggers 1966). The presence of the drained fields provides additional support if the preceding argument is valid. Estimating population densities from the extent of the drained fields is difficult, however, since it is not known how many fields were cultivated at any given time. For San Jorge, if a conservative 0.8 hectare of land and water surface is allowed per person and if only 20 percent of the fields were farmed at once, the population density comes to a substantial 25 persons per square kilometer. Locally, all the ridges may have been in cultivation at once, in which case the densities could have been more than 100 persons per square kilometer. Such densities are not unreasonable in view of present populations of up to 400 per square kilometer in the Baliem Valley of New Guinea, where swamp drainage is practiced (Brookfield 1962).

In the highlands, drained fields were clearly associated with dense populations. These were nuclear areas in terms of population growth and cultural evolution and were apparently characterized by the progressive utilization of ecological niches as population increased and new technology was developed; the most difficult habitats were brought into cultivation last, usually with elaborate reclamation. For discussions of agricultural expansion with population growth in pre-European Mexico, see Flannery et al. (1967) and Spores (1969). Presumably when population and production demands were reduced, as they were drastically in the sixteenth century, the least easily cultivated habitats were abandoned first. Around Lake Titicaca this meant the higher terraced slopes and the drained fields of the lake plain, both of which are still largely uncultivated today. Thus, preconquest populations of the Titicaca area may have been even greater than the present dense populations. Much of the lake plain is hacienda pasture and not available to the

Indians. However, where Indians do control such land, as around the town of Huata north of Puno, there is increasing lake plain agriculture, but with the lazybed system rather than construction of large ridges. Most of the areas of drained-field remnants in Latin America probably supported substantially larger populations than they do toay.

Summary and Conclusion

The three main types of land reclamation in aboriginal America were irrigation, terracing, and drainage. Of these, drainage techniques have received the least attention, probably because they are no longer important and because the remnants are not conspicuous. Nevertheless, drained-field cultivation was widespread and was practiced in varied environments, including highland basins, tropical savannas, and temperate floodplains. Sites ranged from seasonally waterlogged or flooded areas to permanent lakes. Ridging, mounding, and ditching were emphasized, rather than diking. Tools were simple, crops varied, and fertilization was accomplished mainly by mulching.

The presence of drainage agriculture probably indicates that populations exceeded the carrying capacity of the more easily cultivated land. When populations declined, following the conquest by Europeans, the drained fields were probably given up because of the large amount of labor required to cultivate them. Most of these lands are now used only for cattle but could undoubtedly be reclaimed again for agriculture. Advisory programs have yet to reintroduce agriculture to the Mojos and San Jorge savannas, and much of the Lake Titicaca plain having drained-field remnants has been labeled by modern resource surveys as unsuitable for cultivation. However, the cost of such reclamation might be prohibitive where populations are sparse, transportation is poor, and distances to markets are long. The *chinampas* of Mexico are an exception, since there is a major urban market close at hand.

The aboriginal cultures responsible for drainage ranged from the seminomadic Guato to farm villages and chiefdoms and to the high civilizations. The ridged-field farmers of the savannas were capable people but less comparable to the farmers of the efficient and sophisticated states of the Andes and Mexico. Most of the drained-field systems could have been constructed and managed through small-scale family and community cooperation, as in New Guinea, rather than through central political control. On the other hand, there does seem to be a frequent relationship between degree of agricultural intensification, population size, and complexity of social organization (Carneiro 1967). Unfortunately, not much is known yet about the intensification process for drained-field cultivation or for other reclamation systems.

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2.2 Terrace and Irrigation Origins and Abandonment in the Colca Valley, Peru

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Abstract For the Colca Valley of southern Andean Peru, extensive agricultural terracing of pre-European origin is now over 60 percent abandoned. A University of Wisconsin, Madison interdisciplinary project sought to determine the cause of abandonment. This involved mapping the terraces from air photos, archaeological excavation of terraces and associated canals, historical research on colonial censuses, ecological study, and examination of current terracing and canals. We concluded that depopulation resulting from the Spanish occupation of the valley in the sixteenth century was the primary cause of abandonment. We also asked why the abandoned terraces had not now been restored to use, given population recovery in Peru. We concluded that socio-economic factors, primarily the perception of better opportunities in the highland and coastal cities, have resulted in population stagnation or decline in the valley and thus little motivation to restore terraces.

Keywords Andes · Colca Valley · Irrigation · Terrace agriculture · Terrace abandonment

Introduction

> [T]he higher terraces that extend well up the slopes above the main valley floor are abandoned ... [and] show only faintly in contrast to the fresh appearance of those on the valley floor and along the sides of the inner valley. (Lt. George R. Johnson, USN, 1930, 9).

In the Department of Geography at the University of Wisconsin, Madison, in the 1960s and 1970s, the Latin Americanists were interested in the cultural ecology of shifting cultivation, agroforestry, traditional crops, and raised fields. We assumed that terraces and canals, so often photographed, were well-studied; however, Donkin's (1979) terrace survey showed otherwise. Furthermore, the magnitude of terrace abandonment in the Andes was astonishing given current demographic pressures on farmland. We were thus motivated to propose an interdisciplinary project which would examine abandoned terraces and associated irrigation and attempt to determine the cause or causes of abandonment. John Treacy, who had worked with me on raised fields at Samborondón in Ecuador and on Bora agroforestry on the Río Ampiyacu in Peru, had lived for 8 years in the southern Andes of Peru, and he wanted to return to carry out research for his doctoral dissertation. Chatting in my office in Science Hall (or was it over a beer on the lake terrace?), we agreed that a study of Peruvian terraces could be rewarding. Any one of many terraced valleys and basins could have served, but we were particularly impressed by the spectacular aerial photography of the Colca Valley appearing in the reports of the Shippee-Johnson aerial expedition of 1931 (Shippee 1932, 1934; also Johnson 1930). Here was a long stretch of dense and varied terraces, in large part abandoned. The environment, archaeology, and history of the Colca Valley villages had been little studied, but there were detailed colonial *visitas* (censuses) available, recent air photography in 1974, and a demographic history had just been published (Cook 1982). The dirt road from Arequipa to the Colca had recently been improved by the Majes Project which built a cement canal to transfer water from the Río Colca to an irrigation site on the coast.

In the summer of 1983, Treacy and I visited the Colca Valley, where we were assisted by anthropologist David Guillet, who already had initiated a study of water management in the village of Lari. We then assembled our own project (Denevan 1987) with a team consisting of geographers Treacy, Hildegardo Cordova, and mysEuropeaneologists Maximo Neira, Dan Shea, Michael Malpass, and Pablo de la Vera Cruz; ethnohistorian Maria Benavides; and soil scientist Jon Sandor, in addition to North American and Peruvian students. Primary field research was conducted during the summer and fall of 1984, with individual sub-projects continuing over the next decade. Our efforts were initially concentrated in the village of Coporaque, with additional archaeological excavations in Achoma and Cabanaconde and later in Chivay by Brooks (Fig. 2.8).

The main reports of the original Colca Terrace Project are found in Denevan (1986–1988) and in Denevan et al. (1987). Several theses and doctoral dissertations at the University of Wisconsin, Madison, and in Peru resulted directly or indirectly (Treacy 1989; Vera Cruz Chávez 1988, 1989; Webber 1993; González 1995; Femenias 1997; Brooks 1998). During this period there were numerous ecological studies by other US scholars attracted to the Colca, including Guillet (1992), Gelles (1990), McCamant (1986), and Eash (1989). Many studies and reports were also soon produced by Peruvians and Peruvian institutions on history, agronomy, sociology, archaeology, and anthropology. The result has been that in only a few years, the Colca Valley changed from a little-known backwater to one of the best-studied regions in Peru, accompanied by an upsurge in tourism and media attention, with resulting social change for the Colca villagers.

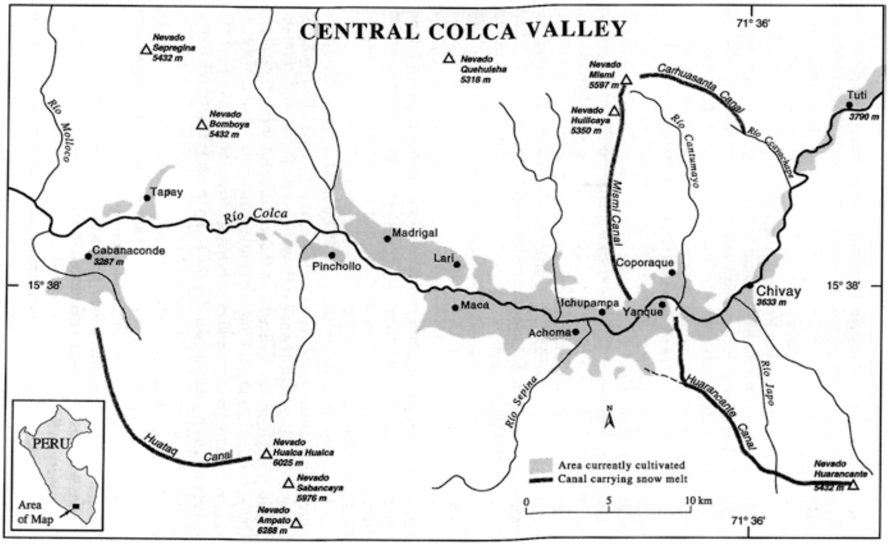

Fig. 2.8 Map of the central Colca Valley, Peruvian Andes, showing Coporaque, other villages, and the Río Japo Basin. Pampa Finaya is located between Coporaque and Chivay. Cultivated land, mostly terraced, is shaded. (From Brooks 1998, 6)

The Colca Valley

The valley of the Río Colca is located in the northern Department of Arequipa in southern Peru on the western slope of the Andean cordillera. It is but one of a series of deep valleys on the southwestern Andean flanks, most of which were terraced in pre-European times. The Colca is incised into a large, flat river terrace (*pampa*). The main cultivable portion of the valley is approximately 40 km long between the villages of Chivay and Cabanaconde at elevations between 3200 and 3600 m. Maize and quinoa were the primary prehistoric crops. Today, maize is planted mainly on terraces, while quinoa, broad beans, alfalfa, and barley appear both on terraces and *pampa* fields. Potatoes are not as common as might be expected. Alfalfa has become a major crop as cattle feed. In addition to cattle, most farmers also have sheep, llamas, and alpacas which are grazed on the *puna* (plateau) above the valley, as well as on fallow fields.

The valley's arid climate has influenced the nature of terrace construction. The agricultural core has a semiarid montane steppe climate, receiving an annual average of 385 mm of precipitation at the principal village of Chivay.

Rains are highly seasonal, peaking in December and declining sharply during late May to early August. For this reason, almost all cultivable land is irrigated by canals bringing water either from natural springs or from streams flowing from mountains topped by glaciers and permanent snow. The roles of irrigation are thus (1) to extend te growing season by 1 to 2 montas,bacowingarmers to plant in August and harvest in May, and (2) to provide supplementary water in case rains slacken during the growing season. Soils are mainly of volcanic origin and are of good structure and fertility. Cultivated and abandoned terraces are located on the *pampas*, on the slopes of the canyon below, and on the steep mountain slopes above to an elevation of nearly 4,000 m.

The Colca Valley has been occupied by terrace farmers since possibly 2,400 BC (Brooks 1998, 270). The immediate pre-Inca culture was the Collaguas which dominated the valley. Little is known about pre-Collaguas people, although Brooks (1998, 313) has identified an earlier ceramic style she calls Japo. Lithic tools in caves indicate that preceramic hunters were long present. The Inca interacted with the valley after 1471; however, there is little evidence of a strong Inca presence in that no Inca administrative center has been located and Inca ceramics, while present, are not common. Also, there is little evidence (a few sherds) of the Wari (Huari) culture, which dominated much of southern Peru, c. AD 650–1000.

The Spaniards arrived in the 1540s and found numerous small villages and dispersed farmsteads which were then converted into *encomiendas* (land and labor grants). Later, as part of the survey of Peru under Viceroy Francisco de Toledo, 1571–1574, the Colca Indians (Collaguas region) were consolidated into 24 Spanish-style villages (*reducciones*) numbering an estimated 62,500 to 71,000 people projected back to 1530. European crops and livestock were introduced, transforming the economy; however, population declined rapidly due to introduced diseases and removal of people to the mining centers. By 1721, the population had been reduced to about 8,600 (87 percent decline). Population increased slowly to 27,534 in 1940 (Province of Caylloma), reflecting out migration to Arequipa and elsewhere (Cook 1982, 83–87). By 1993, the population had only increased to 37,600 (corrected from INEI 1993, 38–39).

In the main terrace sector of the valley from Tuti to Cabanaconde, there are now 12 villages, each numbering between about 1,000 and 5,000 people, the largest being Chivay. Large units of once productive terraces are unused. This is in the Peruvian context of an increase of population from 6.2 million in 1940 to 23 million in 1993, inadequate food production, and apparent scarcity of arable land. Why has there been agricultural stagnation, or an earlier collapse without recovery, in the Colca Valley, as well as elsewhere in the Andes? Are the reasons environmental, such as climatic change or soil depletion and erosion, or are the reasons social? To consider this question, we examined not only terrace abandonment but the history and pre-European history of terracing in the Colca Valley, including the origins of terracing and irrigation.

The Colca Terraces

The ecology of current Colca irrigated terraces has been best studied in the communities of Coporaque (Fig. 2.9) (especially Treacy 1989, 1994a, b; Treacy and Denevan 1994) and Lari (especially Guillet 1987a, b, c, 1992) on the north side of the valley. Most of the Colca terraces were originally constructed in pre-European

Fig. 2.9 Air photo of the Coporaque region in the Colca Valley, Peruvian Andes, showing cultivated and abandoned terraces. See Fig. 2.11. (Photograph by Robert Shippee and George Johnson, 1931. Servicio Aerofotográfico Nacional, Lima, Peru)

times; however, with repairs subsequently, any given wall represents different construction dates and techniques.

The Colca Valley features a variety of terrace types (Fig. 2.10) that may be keyed into the terrace typology described by Treacy and Denevan (1994), Brooks (1998, 126–136), and Denevan (2001, 175–181). The common and most visually impressive type is the contour or bench terrace on valley-side slopes. Bench terraces

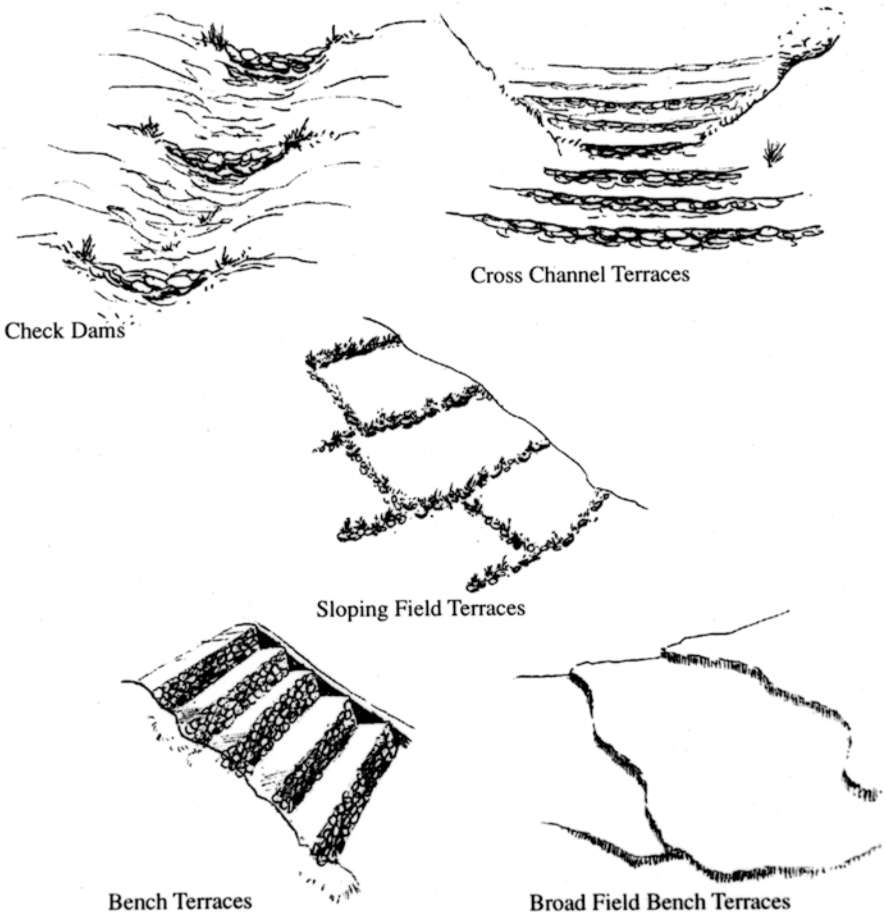

Fig. 2.10 Examples of terrace types. (From Brooks 1998, 2. [Added here from Denevan 2001, 175])

measure between l and 3 m in wall height, between 3 and 7 m in width depending upon degree of hillside slope, and from 40 to 60 m in length. Most Colca Valley bench terraces are irrigated by gravity-fed canals, and terraces often have water drops and channels built into walls and on the terrace platform. Most bench terraces are essentially irrigation platforms on steep slopes; their microclimatic virtues (frost reduction, moisture conservation) allow intensive maize cultivation. Brooks (1998, 192–5) reports finding numerous abandoned, unirrigated bench terraces in the Río Japo Basin between Chivay and Yanque, as well as elsewhere in the Colca Valley. Nearly half of all the terraces in the Japo Basin are of this type. Although not irrigated, the flat cultivation surface of such a bench terrace captures more rainwater than most of a sloping-field terrace, where rainfall runs off the upper surface to the lower surface, and hence, with inadequate rainfall, bench terraces are more productive per unit of slope.

Builders used cut-and-bench methods to construct bench terraces. They excavated through A horizon topsoil into an indurated B horizon to emplace wall base stones ranging in size from 30 to 60 cm in diameter. Stability and interlocking fit were more important than the size of the wall stones; base stones may be smaller than stones placed on top of them. Next, soil was removed from the exposed vertical soil face in order to make the cobble and earthen drainage horizon which most Colca Valley bench terraces have. Builders then moved soil down the slope behind the wall,[1] while stonemasons built the wall higher to retain the soil. Mineralogical examination shows that terrace fill soil is composed of in situ slope soil; no exogenous soils were used (Sandor 1987a, 185). In extensively terraced regions such as the Colca Valley, with steep slopes, the costs in labor would preclude transport of soil fill from elsewhere, at least not in quantity.

Often, a bench terrace is damaged when waterlogged soil bursts the retaining wall and spills forth. Careless wall repair hastens further wall failure. Builders blame poor masonry techniques or dry-soil construction for broken walls. Builders prefer to terrace with wet soil since dry soil is porous and will quickly become saturated following heavy rains, thus provoking sudden, early bursting. Common mistakes cited include not fitting wall stones together tightly or failure to press stones against the earthen fill behind. In the latter case, a gap forms between the wall and fill causing the wall to collapse when the gap fills with water.

Bench-terrace reconstruction duplicates the labor techniques of construction, except that soil spilled from ruptured walls has to be thrown back upslope and re-walled. Workers trench down to the original base to remove the fallen and buried stones from the original wall. The wall is rebuilt and the fill stones and soil are carefully repacked in behind the wall and tamped down. When reconstructing fields, masons replace up to 80 percent of the ancient walls and must laboriously throw spilled soils upslope, which demands more effort than moving soils downslope for new terraces.

An interesting aspect of Colca Valley terracing is the numerous *maquetas* (carved boulders) which depict patterns of irrigated terraces. Brooks (1998, 287–293) identified 14 and describes several. They show primary canals, feeder canals, reservoirs, and end walls between terraces. Different functions have been suggested, but Brooks believes that the Colca *maquetas* served as general plans for terrace irrigation systems and also had a ceremonial role. The famous Sahuite *maqueta* in Cuzco depicts terraces and canalsEuropeaner features. There is a full-scale model in the Museo de la Nación in Lima (Denevan 2001, Fig. 1.1).

For Coporaque, Treacy (1994b, 81–84) described three types of sloping-field terraces: (l) linear, valley side, segmented, some with side walls (124 ha); (2) roughly rectangular, fully walled, near ridge tops (122 ha); and (3) stone-bordered, roughly rectangular, in nearly flat areas (460 ha). All were probably designed to trap

[1] Treacy (1987a, 53; Treacy 1994b, 147) believed that the Coporaque bench-terrace fills were done by hand, whereas sloping-field terraces were self-filling via downslope movement of soil. Sandor (1987a, 185), however, believes that the Coporaque bench terraces were filled by natural slope processes.

runoff. Slope runoff was also directed by diversion walls into reservoirs and into canals which fed irrigated bench terraces. Treacy (1994b, 108) mapped five such sectors in the Coporaque area. Sloping-field terraces tend to measure between 1.3 and 2 m in wall height, 13 to 16 m in width, and between 30 and 50 m in wall length. The broad, sloping catchments of these terraces suggest that they were in part accretional or self-filling fields; they do feature artificial cobble drainage horizons. Most fields of this type are now abandoned in the Colca Valley.

Broad-field, irrigated bench terraces occur in places where steep upper slopes become more gentle, and they are often enclosed by stone walls. Valley-floor broad-field terraces occur on the nearly flat, alluvial terraces (*pampas*) of the Río Colca. Walls are of stone or earth and are only 20–100 cm high, with very wide cultivation sectors between walls, compared to narrow bench terraces on slopes.

Simple cross-channel terraces and check dams, some no more than a rough line of stacked stones, are scattered thinly through parts of the valley. They generally cross gullies or intermittent stream channels on moderate slopes. Examination of aerial photographs shows that barrage-like terraces – stone walls flanked by long, perpendicular water diversion walls – may have been constructed in some areas of the Colca Valley, but field ground checks are needed to confirm them. Cross-channel terraces, like most sloping-field terraces, are today unfarmed.

Non-terraced fields in the valley include large numbers of abandoned, stone-lined plots. There are 460 ha of these east of Coporaque in dissected lava-flow terrain between the Pampa Finaya plateau and the Río Colca. Smaller sectors occur in Tuti, Yanque, and Maca. The walls are of stacked stones 0.5 to 2 m high. They enclose mostly gently sloping fields rectangular or irregular in shape, ranging from 400 to 5000 m^2 in size (Treacy 1994b, 86). The function of the stone walls is not known, possibly a means of moisture conservation.

There are now numerous unterraced, irrigated valley-floor fields of roughly rectangular shape on the *pampas*. They are surrounded by adobe or stone walls which serve to keep cattle out. These walls are rare on the Shippee-Johnson air photos of 1931, when sectorial, open-field grazing was still being practiced. Enclosure took place after 1940 as cattle greatly increased and seriously damaged crops (González 1995, 44–58). Other fields are house and garden plots and recent (since the 1970s), unterraced but irrigated, valley-side sloping fields. The latter fields were made following new canal construction; such unterraced fields probably would have been unthinkable in pre-European times.

The Soil Factor

We have considerable information about the soils of the Colca terraces and associated non-terraced terrain, thanks in particular to the research by Jon Sandor and Neal Eash (Sandor 1987a, 1987b; Eash 1989; Sandor and Eash 1995; Eash and Sandor 1995; also Dick et al. 1994). Most of the Colca terraces still utilized have been in near-continuous cultivation since the early colonial period, given the descriptions of specific fields in the *visitas* (censuses) – fields we can still identify from their names. Cultivation of these same fields undoubtedly extended well into pre-1492, given the dates we have from excavated terraces. Hence, a major question is what was the impact of many centuries of cultivation on the terrace soils? How was soil fertility maintained? Is soil decline related to terrace abandonment?

The region is underlain by volcanic andesitic and rhyolitic rocks, which generally weather into fertile soils. The soils consist of alluvium and colluvium derived from these materials. There are also lava flows and some volcanic ash present. The natural soils on slopes are mainly mollisols, which are grassland soils with abundant humus found in semiarid regions. The degree of soil development in the Colca varies with geomorphic surface and age.

The terrace soils show significant differences from non-terraced soils as a result of long-term cultivation. The thickness of the A horizon is greater, and phosphorus, nitrogen, and organic carbon levels are higher, as are earthworm activity, friability, and water capacity. These differences reflect centuries, possibly 1,000 years or more, of application of fertilizers (manure, hearth ash, garbage, crop residues); terracing which reduces erosion; and tillage, irrigation, crop rotation, fallowing, and use of legumes. These soil characteristics generally pertain to both still-cultivated terraces and long-abandoned terraces, as compared to terrain not terraced, thus indicating persistence of earlier, even ancient, soil management impacts. Phosphorus levels are less, however, in abandoned terraces, indicating partial depletion during the historical period with cessation of fertilization except from livestock manure. High levels of phosphorus appear in the B horizons of terrace soils, suggesting a very long period of downward movement of phosphorus from fertilizer (Sandor and Eash 1995, 177). Thus the “data suggest that soil changes have taken the forms of increased fertility and tilth and that traditional agriculture management practices have conserved soils” (Sandor and Eash 1995, 178). This has made possible near-continuous cultivation of some sectors for as long as 1,500 years (Sandor and Eash 1995, 170).

Terrace Origins

The origins and evolution of the Colca terraces, particularly the irrigated bench terraces, may be related to circumstances that could help explain later abandonment of both irrigated and unirrigated (rainfed and runoff) terraces. Our archaeological excavations at Chijra (Chishra) and Chilacota (Ch’ilaqota)[2] near Coporaque, at Cabanaconde and Achoma, and later in the Río Japo Basin near Chivay (Fig. 2.8) are not conclusive but provide instructive insights. I will focus on Chijra and to some extent the Japo Basin, areas for which we have the best descriptions, ceramics, and carbon-14 (C-14) dates.

[2] These local names are not villages but rather locales, specific habitats with clusters of fields, whose present toponyms can usually be found in the early colonial *visitas*.

The Chijra Area Terraces

The Chijra terraces are located on a ridge just west of Coporaque along a slope extending from 3,752 m down to 3,272 m at the edge of the Río Colca (Figs. 2.9, 2.11, and 2.12). The site of Chijra is an uplifted alluvial fan of moderate slope. There are over 100 abandoned terraces, of which we measured and described 30 (Treacy and Denevan 1986–1988). Of these all are valley-side bench terraces except for six broad-field bench terraces on the gentler lower slope. A large stone aqueduct crosses the terraces at a right angle, splitting into two arms before ending at the sharp break in slope at the lower edge of the broad-field terraces. The aqueduct is 5 m wide at the base and 1 m or more in height in places. On top are the remnants of a stone-lined canal, with a rock gate which could be closed by large stones to force water out of the canal laterally onto the terraces. Water was then dropped, terrace by terrace via stone water-drop grooves, down the faces of the back walls onto spreading stones at the base of the grooves (Denevan 2001) (Fig. 2.13). Small channels of stones across the bench surfaces led the water to the edge of the next wall

Fig. 2.11 Air photo of Chijra, Coporaque ina the Colca Valley, Peruvian Andes, showing abandoned terraces, aqueduct (inverted Y), and prehistoric house walls. The terraces on a steep slope at the lower center and left are in the sector of Alto Cayra. [The terraces in the upper left corner are in cultivation.] (Photograph by Robert Shippee and George Johnson, 1931. American Museum of Natural History)

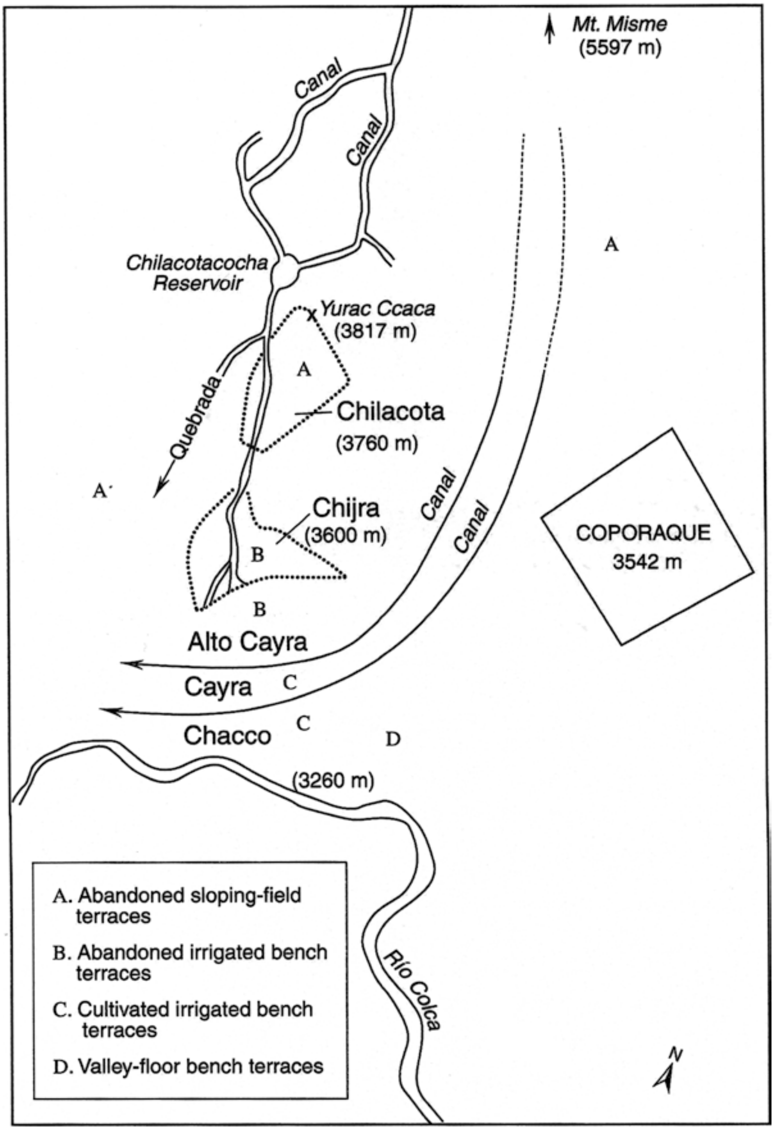

Fig. 2.12 Map of Chijra and environs, District of Coporaque, Colca Valley, Peruvian Andes. See Fig. 2.9. (By W.M. Denevan)

below, or water was allowed to fall all along the edge to the next level, as is done today in the area. Farmers now use hoes to spread water evenly over a cultivation surface ("teaching water" Treacy 1994a, 100) and then move it quickly to the next level to avoid oversaturation. Stepping stones and benches in the walls are common, as are vertical interior drains. The aqueduct was fed by a canal leading from a reservoir above at Chilacotacocha (Fig. 2.12). The bench terraces average 1.7 m in

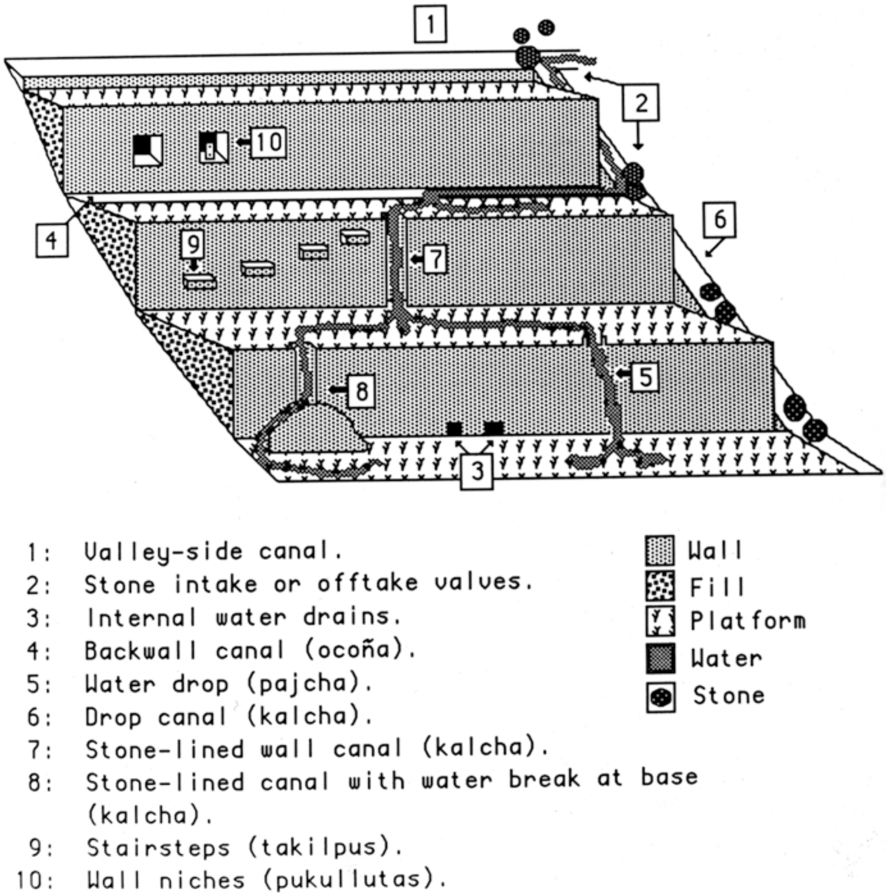

Fig. 2.13 Schematic view of bench terrace and associated features, Coporaque, Colca Valley, Peruvian Andes. (From Treacy 1994b, 111. [Added here from Denevan 2001, 179])

height and 7 m in width, and some were over 100 m in length. Terrace walls are single walls of uncut stone without mortar; they are underlain by cobble-fill to facilitate drainage. Vertical end walls may have served as divisions between household fields. (For details, see Treacy 1994.) Also, there are ruins (walls) of ten Collaguas houses at Chijra, most located on the broad-field terraces (Fig. 2.11).

Above Chijra at about 3,600–3,750 m on a gently sloping plateau is the site of Chilacota (Martin 1986–1988). There is no evidence of any irrigation. There are three massive earthen walls across the plateau that may have served to trap runoff for crop growth. Between the walls there are 15 roughly rectangular smaller walls (double stone walls vertically and single walls horizontally) – a form of sloping – field terracing. The abandoned Chilacotacocha reservoir (3,400 m^2) is just north of the plateau, separated by a *quebrada* (gully). The south wall of the reservoir is 5 m high and has a water gate in it. From the gate a canal leads past Chilacota both to

Chijra and to a *quebrada* which fed canals and terraces west of Chijra (Fig. 2.12). The reservoir was fed by canals receiving meltwater from the snow line well above on the slopes of Cerro Mismi. Also, there is a long wall from a *quebrada* to the northwest which apparently diverted water to the reservoir.

Below Chijra, the slope becomes very steep and is covered with abandoned, highly deteriorated, very narrow bench terraces (Alto Cayra). Some of these were irrigated by the aqueduct, while others were apparently unirrigated. Below this there are two active horizontal canals coming from the east side of the ridge. They irrigate active bench terraces between them at Cayra (Qayra) and bench and broad-field terraces between the lower canal and the Río Colca at Chacco (Chaqo) (Figs. 2.11, 2.12, and 2.14).

At Chijra, three terraces were trenched through sections of bench and wall, and two more terraces were excavated at Cayra and Chacco (Malpass 1987). Neira Avendaño (1990, 155–166) excavated one of the house ruins. We obtained 20 C-14 dates from the terraces, the house floor, and an irrigation canal (Malpass 1987, 61). Ceramics, lithics, animal bones, and pollen were collected (Malpass and Vera Cruz 1986-1988). At Chilacota, seven test pits and trenches were made in and near the sloping-field terraces. The accumulation over well-developed soil was only about 20 cm. No house walls were found, but a possible hearth was trenched near the terraces. Ceramic fragments were found, mostly undiagnostic, along with lithics, including very small (2–4 cm long) obsidian points used in hunting, and substantial

Fig. 2.14 Abandoned terraces (upper) at Alto Cayra and cultivated terraces (lower) at Cayra, oporaque, in the Colca Valley, Peruvian Andes. They are separated by an active, horizontal irrigation canal, which does not show clearly. (From Treacy 1994b)

quantities of deer and camelid bone. The only C-14 dates for the house, AD 300 ± 70 and 1570 ± 80 BC, were from soil below walls and are not good indicators of time of wall construction, but they do suggest a considerable antiquity.

The Japo Basin Terraces

In the Río Japo Basin between Chivay and Yanque, geographer/archaeologist Sarah Brooks (1998) carried out a detailed study of terrace history in the vicinity of the Collaguas ruins of Juscallacta (above Chivay). Here there are a variety of abandoned and still-cultivated terraces. These include crosschannel, sloping-field, and bench terraces, all abandoned and never irrigated, between 3,775 and 4,000 m (Fig. 2.15). About 1,500 irrigated, mostly still-cultivated bench terraces, occur between 3,450 and 3,750 m. Just above the irrigated terraces, there is a lateral canal which feeds the terraces below. There is a total of about 394 ha of terraces, of which 200 ha are currently abandoned (Brooks 1998, 150); thus about 51 percent of the terraces are abandoned, almost all of which are above the canal level. There are only ten or so sloping-field terraces surviving (compared to hundreds in the Coporaque area), and a few dozen cross-channel terraces, so most of the abandoned terraces are unirrigated bench terraces. Most of the lower, irrigated bench terraces are broken up into segments by vertical stone walls, whereas the upper, unirrigated abandoned

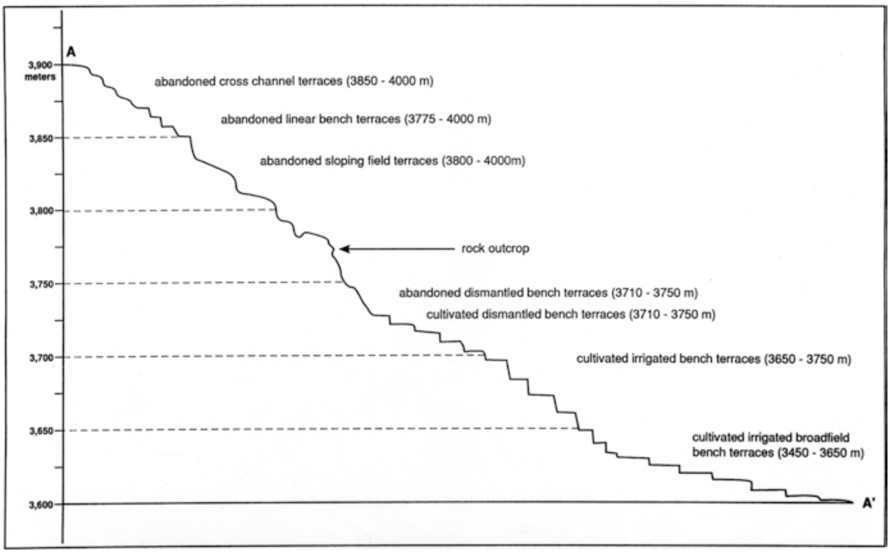

Fig. 2.15 Generalized slope profile showing approximate elevations of terrace types in the Japo Basin, Colca Valley, Peruvian Andes. The linear bench terraces were unirrigated. (From Brooks 1998, 149)

terraces are not. There is much less abandonment of irrigated terraces at Japo than at Coporaque.

Between 3,710 and 3,750 m, there is a band of dismantled (front walls removed) irrigated bench terraces, some cultivated and some abandoned. This destruction is of narrow terraces on steep slopes where the removal of terrace walls facilitates plowing with oxen. It may have begun in the colonial period and is continuing. The result is a probable increase in soil erosion and a reduced control of irrigation water (Brooks 1998, 170–176). Water can still be released from the horizontal feeder canal above onto the now unterraced sloping fields. Elsewhere, terraces are removed to plant forage for livestock (Benavides 1997).

Other terraces or locales in the Colca Valley were excavated at Achoma (Shea 1987, 1997) and Cabanaconde (Vera Cruz Chávez 1987, 1988), and ceramics have been collected elsewhere (Vera Cruz Chávez 1989); however, no terraces or anything else were dated from these areas.

Terrace Chronology

We obtained several C-14 dates for terraces and canals from our excavations at Chijra near Coporaque (Malpass 1987, 61), as did Brooks (1998, 286) for the Japo Basin. These dates are from buried soil and charcoal from within terrace fills and beneath terrace walls. A word of caution is necessary before looking at these dates. Terrace walls are continually being rebuilt or repaired, and the soil fill behind the walls is disturbed by cultivation and also by both erosion and soil movement from above. Hence, anything dated in a terrace wall or fill, or an original soil surface beneath a fill or wall, may not be a reliable date for cultivation of that horizon or surface.

Brooks dated a sloping-field terrace at 3,790 m in the Japo Basin to 2350 BC. A date of 1570 BC for a sloping-field terrace at 3,760 m at Chilacota (Fig. 2.12) suggests that this type of terrace was also in use very early in the Colca Valley. There are no dates available for cross-channel terraces, but they are also undoubtedly very early. Probably both sloping-field and cross-channel terraces are older than unirrigated bench terraces, as the former are less complex and less labor-intensive but have a lower portion of cultivated space than do bench terraces. Probably all three types of unirrigated terraces continued in use until the development of irrigated bench terraces, and some continue in use today in the Colca Valley. Brooks dated an unirrigated bench terrace at 3,790 m the Japo Basin to AD 1510 and one at Pampa Finaya [between Coporaque and Chivay] at 3,880 m at AD 655.

For irrigated bench terraces at Chijra (Fig. 2.12), the most reliable early dates are for the lowest levels of three excavated terraces: AD 510 and 660 (Trench I), 550, and 570. For Trench I there are upper horizon level dates of AD 940, 1020, and 1370, and at Cayra there is a terrace date of AD 1340. In the Japo Basin, Brooks has dates for irrigated bench terraces of AD 780, 1690, and 1847. There seems to have been terrace cultivation throughout the colonial period.

Thus, the pattern of terrace types, elevations, and dates in the Japo Basin and in the Coporaque area indicates early unirrigated terraces (2350 BC or older) above 3,790 m (Japo) and 3,760 m (Chilacota) and a shift downslope to irrigated bench terracing by at least AD 500–600. Most of the irrigated bench terraces in the Japo Basin continued in use to the present. In the Coporaque area, the terraces at Chijra and Alto Cayra were abandoned at some time, whereas the lower terraces at Cayra and Chacco have continued in use (Fig. 2.12). Settlement also moved downslope, from Juscallacta to the area of present Yanque and Chivay and from Chilacota and Chijra to the area of Coporaque.

Changing Terrace Forms

The shift of terrace cultivation downslope, with irrigation added, can be related both to changing climatic conditions and to increasing population pressure which necessitated dependable production in a region of undependable precipitation. Rainfall for Yanque at 3,417 m currently averages 419 mm Brooks 1998: 65), which is inadequate for rainfed (unirrigated, non-runoff, watered) maize (c. 600 mm minimum) and potatoes (c. 500 mm minimum); however, quinoa and cañihua, when well-spaced, need as little as 300 mm and were probably important crops on nonirrigated terraces (Treacy 1994b: 107). Rainfall in the Colca is somewhat higher at higher elevations, an estimated 518 mm at 4,000 m (Brooks 1998, 390).[3] Certainly there is sufficient moisture today for runoff fields (cross-channel terraces and sloping-field terraces), and even unirrigated bench terraces planted in quinoa. However, very few of these terraces are cultivated now, only 9 percent in the Colca Valley, mostly in the community of Achoma and none in Coporaque or in Chivay which includes the Japo Basin (Denevan 1988: 22).

There were relatively dry periods in the central Andes of Peru during AD 540–560, 570–610, 650–730, and 1040–1490 with precipitation reduced c. 5–20 percent, based on ice core measurements at Cerro Quelccaya east of the Colca Valley (Thompson et al. 1994; Brooks 1998, 74). Such a decline in rainfall could have contributed to the development of irrigated bench terraces at lower elevations where slopes are gentler and more easily irrigated. In addition, there is a frost factor. While there is greater precipitation above 3,750 m, there is also a greater frequency of frost and hence crop failure. The upper limit of agriculture, based on frost risk, was depressed as much as 150 m during the periods AD 650–850 and especially 1240 or 1255 to 1850 or 1880 AD (Little Ice Age) (Brooks 1998, 71, 384). The shift to irrigated terraces at lower elevations may relate to dry or cold periods or both. However, with sloping fields as high as 4,200 m above Chilacota and irrigated bench terraces being below 3,760 m at Japo and Cayra, a decline in the frost line would be of limited significance.

[3] Based on Winterhalder (1994, 47, 60). Winterhalder demonstrates why irrigation is required in the Colca Valley on the western side of the southern Peruvian Andes, whereas in the Sandia Valley on the eastern side, rainfed cultivation of terraces is possible due to greater total rainfall and other factors.

Thus, there were two climatic stress periods during the past 1,000 years: first, AD 540–730 (dry) and 650–850 (cold), and second, AD 1040–1490 (dry) and 1240–1880 (cold). The dates for irrigated bench terraces suggest that they were first being constructed during the earlier period. They may have been elaborated and expanded during the latter period. Malpass (1988) believes that the presently utilized irrigated terraces at Cayra and Chacco were not developed until the Inca period, but this seems much too late given such terracing in many parts of Peru well before then. The abandoned irrigated terraces at Chijra and Alto Cayra, fed by vertical feeder canals, probably date to the earlier dry period, whereas the still-cultivated terraces lower down at Cayra (a single date of AD 1340) and Chacco, fed by horizontal canals, may date to the later dry period (Fig. 2.12). Also, fewer animal bones and lithics (obsidian arrow points) were found in the lower terraces compared to Chijra and Chilacota, indicating a greater emphasis on agriculture and less on hunting and grazing at the lower terraces.

In parts of the areas of irrigated bench terraces, there already may have been sloping-field and unirrigated bench terraces that were either destroyed by the new irrigated terraces or were converted into the new terraces. This is suggested by the sharp divisions between abandoned unirrigated terraces and irrigated terraces, the division marked by a lateral canal constructed to irrigate the lower terraces. Such a sharp lower boundary for the unirrigated terraces would not have existed prior to the canal, so unirrigated terraces may have extended below the level of the present canal. Our excavation of Trench 1 at Chijra did expose a buried stone rubble wall, dated AD 940, suggesting the construction of the present terrace over a previous one (Malpass 1987, 54–56). Both Treacy (1994b, 102–105) for Chijra and Brooks (1998, 392) for the Japo Basin argue for the conversion of unirrigated terraces into irrigated terraces below 3750–3760 m. The two terrace types could have coexisted for a time until the superiority of the irrigated bench terraces was clearly recognized. Some seemingly abandoned sloping-field terraces in Coporaque are cultivated today when adequate rainfall occurs or is anticipated.

This scenario, drawing on Brooks and Treacy, is an argument that the development of complex labor-intensive irrigated terraces resulted, in part at least, from climatic stress.[4] This is contrary to some popular and academic thinking that climatic deterioration (dryer, colder) was the cause of agricultural and social collapse at various places in the world. In the Colca Valley, climatic stress instead seems to have motivated the development of a more efficient, more productive, and less risky method of food production. There is another dimension to this story, however. Even without long-term climatic change, there was regular climatic variation, especially in precipitation, and thus a great risk to crops during very dry years. For example, at Yanque with an average of 419 mm rainfall over 31 years, the wettest year had 708 mm, and the driest year had only 213 mm. Intermittent crop failure during dry

[4] Cardich (1985) argues that shifts of cultivation to lower slopes in the Andes were due to climatic changes toward colder temperatures. He believes it was warmer from 300 BC to AD 500; colder from AD 500–1000, warmer from AD 1000–1350; and colder from AD 1320 through the Inca period. This might explain an abandonment of the Chilacota fields after 1320; however, temperature would not explain the often sharp division between irrigated and unirrigated terraces elsewhere.

years became progressively more of a problem to people as population grew. Irrigation is not fully dependent on immediate precipitation, but rather can also draw on reservoirs, springs, and meltwater from snowcaps. Irrigation can provide a more secure supply of water in terms of quantity and predictability, and it can extend the growing season. Thus, it is a great advantage even when there are not decades or centuries-long periods of reduced rainfall. Irrigation continues during wet periods, as we see in the twentieth century.

Finally, an expansion of terracing and a shift of cultivation downslope during late pre-European contact may have been associated with a greater interest in maize, which requires reliable moisture, a long growing season, and minimal frost. An important role of irrigation for Andean valleys, such as the Colca, is to lengthen the growing season for maize (Mitchell and Guillet 1994, 6–7).

Thus, shifts in terrace form and elevational level could have involved more than shifts in precipitation patterns and frost lines. This question is of more than local concern. The pattern of abandoned, unirrigated terraces at higher elevations and cultivated, irrigated terraces below them is widespread in the Andes (Malpass 1988). Firmer dating of climatic, agricultural, demographic, and other events in the Colca and elsewhere is essential if we are to better understand the history of Andean agricultural fields.

Terrace Abandonment

We have considered the abandonment of unirrigated terraces; however numerous irrigated terraces have also been abandoned. Explanation is important given plans for restoring abandoned terraces.[5] There are actually two issues involved. One is the cause or causes of abandonment. The second is the reason or reasons why terraces are not restored to cultivation, given apparent land shortages in the Andes. Terrace restoration projects in Peru have paid Indian farmers to rebuild terraces and associated canal systems without knowing why the farmers themselves do not rebuild and use old terraces. It is quite possible that once terraces are rebuilt, they will not be cultivated because of lack of sufficient irrigation water, conflicts over land rights, excessive labor costs, excessive distances from villages, or for other reasons. The situation is complex, involving environmental change, technology, social and economic organization, demography, and markets. Conditions change over time and from village to village, both for the Colca and for other terraced regions of the Andes (Treacy and Denevan 1994).

[5] Proponents of the restoration and expansion of traditional terracing in Peru include Peruvian ecologist Marc Dourojani (1983, 66, 70) and several contributors to the volume on *Andenes y camellones* (Torre and Burga 1986). The United States Agency for International Development had a major project to improve traditional terrace agriculture and to construct new terraces in Venezuela, Guatemala, and Peru (Williams 1986).

Mapping Abandonment

In order to determine the degree of terrace abandonment, cartographer Laura Hartwig and I mapped and measured both cultivated and abandoned fields for the Colca Valley (Denevan 1988). We used the 1974 overlapping vertical air photos which were taken for the Majes irrigation project.[6] At a scale of 1:17,000, they are quite suitable for differentiating cultivated from abandoned terraces.

Our study included 10 of the 12 Colca communities, Tuti and Tapay lacking complete photo coverage. Seven categories were mapped: upland (sloping-field terraces) cultivated and abandoned, terrace (irrigated bench terraces) cultivated and abandoned, bottomland (valley-floor bench terraces and enclosed non-terraced fields) cultivated and abandoned, and not cultivated land.

For the mapped sectors of the valley (some of the upper zones are not on the air photos), we determined that the total area of fields was 14,356 ha, of which 6,071 ha, or 42 percent, were abandoned. The total area of bench terraces was 8,962 ha, of which 5,426 ha, or 61 percent, were abandoned. Of the sloping-field terraces, 91 percent were abandoned, and of the valley-floor non-terraced fields, only 7 percent were abandoned, possibly in fallow.

For the community of Coporaque, using the 1974 air photos at 1:17,000 scale, we measured 1,399 ha of fields, of which 726 ha were abandoned, or 52 percent. Initially, Treacy (1987a, 153, 155), using the 1955 air photos at 1:55,000 scale, obtained a total of 1,445 ha, of which 970 ha, or 67 percent, were abandoned in 1983–1984 when 475 ha were reported in cultivation or in fallow. Using our 1974 air photo mapping units plus ground checking to revise our categories of field types and cultivated versus abandoned fields, we measured a new total of 1,456 ha, of which 784, or 54 percent, were abandoned (Denevan 1988, 22). This was then adjusted by Treacy to give a total field area of 1,367 ha, of which 847 ha, or 62 percent, were abandoned or semi-abandoned (Treacy 1989, 75–76; 1994, 66). His 62 percent abandonment for Coporaque, compared to our 52 or 54 percent, suggests that for the entire valley our 42 percent based on air photo analysis alone, without ground checking, may be too low.

In Coporaque, of the valley-side bench terraces (270 ha), 43 percent are abandoned or semi-abandoned; 10 percent of the valley-floor bench terraces (267 ha) are abandoned or semi-abandoned; 100 percent of the sloping-field terraces (246 ha) and stone-bordered fields (460 ha) are abandoned; and none of the valley-floor walled, non-terraced fields are abandoned (Treacy 1994b, 66). Most of the bench terraces are or were irrigated; however at least 20–30 ha of the valley-side terraces are semi-abandoned and may never have been irrigated. The distribution of the various types of terraces and other fields in Coporaque is mapped by Treacy (1994b, 87). Figure 2.16 shows cultivated and abandoned terrace areas for most of Coporaque and Chivay and part of Yanque.

[6] The 1931 vertical photos at a scale of 1:13,000, taken by the Shippee-Johnson Expedition (Denevan 1993), are of excellent quality, and we used them to determine the change in extent of terracing for the community of Coporaque between 1931 and 1974. The difference was very small – an increase of overall cultivated area of 30 ha or c. 4 percent. This increase was on sectors of *pampa* (river terrace) land which had been uncultivated in 1931.

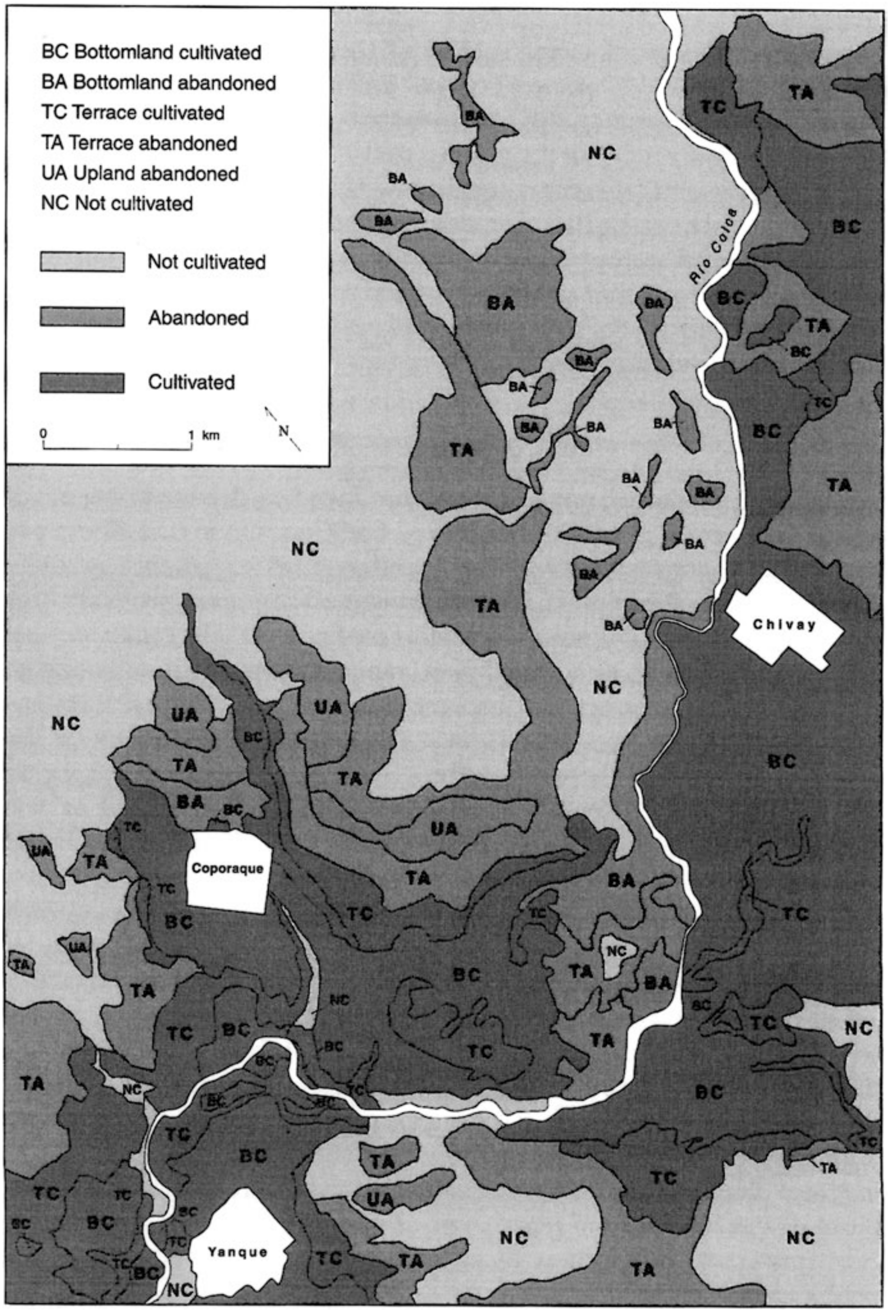

Fig. 2.16 Map of abandoned and cultivated terraces, Chivay, Yanque, and Coporaque, Colca Valley, Peruvian Andes. Based on 1974 air photos. By Laura Hartwig: corrected by John Treacy from ground observation. (From Denevan 1988, 27)

Our mapping and measurements from air photos demonstrate the following: (1) The degree of abandonment is considerable. This supports undocumented speculation that over half the visible terraces in the Andes are abandoned. (2) The abandoned terraces tend to be on the high slopes and more distant from current villages than the now cultivated bench terraces and bottomland fields. Accessibility is clearly a factor. (3) Most of the unirrigated, sloping-field terraces are abandoned – 91 percent overall and 100 percent in some communities. Sloping-field terraces still cultivated may be irrigated from springs. (4) The least abandonment is on the *pampas* near the Río Colca – only 7 percent overall and 0 percent for some communities. Here, many of the fields are now enclosed by adobe walls; they are irrigated, but many are not terraces.[7] (5) Within the mapped sectors of the valley, which total 18,128 ha, 46 percent is now cultivated, 33 percent consists of abandoned terracing, and only 21 percent is neither. Thus, a very high portion (79 percent) of the landscape is cultivated or was cultivated in the past.

Time of Abandonment

To understand why bench terraces were abandoned, we need to know not only where abandonment occurred but when. For Coporaque, the sloping-field terraces at Chilacota must have been abandoned before the Inca period (AD 1470–1530), given the absence of Inca-influenced ceramics, probably during one of the preceding prolonged dry and/or cold periods. The bench terraces at Chijra and Alto Cayra, now abandoned, were undoubtedly in use during the Inca period, given the presence of Inca ceramics. The *visitas* for 1591, 1604, and 1616 give the place names for terrace irrigation sectors, with data on crops grown in each. Many of these names (toponyms) persist today. From the three *visitas*, Treacy (1994b, 259–62) located 74 sectors for Coporaque of which 23 are not listed for 1616, suggesting their abandonment was between 1591 (or 1604) and 1616.

The presently cultivated sectors of Cayra and Chacco were in cultivation in 1591, 1604, and 1616 (also in 1931). Chijra does not appear in the *visita* lists and apparently was abandoned by 1591. Other Coporaque terrace sites listed in the *visitas* were not identified and located by Treacy and probably represent now abandoned fields. While we cannot be more precise about the dating of terrace abandonment, much of it certainly occurred during the early colonial period. Unirrigated bench terraces were abandoned earlier, at least in the Japo Basin; Brooks did not find Collaguas sherds on most of the unirrigated bench terraces above 3,800 m. Undoubtedly, terraces went in and out of cultivation from place to place and from time to time depending on circumstances, in addition to short-term periods of fallow and long-term abandonment.

[7] Most of the enclosure process occurred between 1930 and 1980 in order to prevent animal infringement into fields, thus eliminating open-field, communal grazing (González 1995).

Theories of Abandonment

The causes of irrigated terrace abandonment in the Andes have been the subject of considerable discussion and debate. Field (1966, 484–489) emphasized population decline and land-use change rather than climate. Wright (1963, 73–74) mentioned a "drying landscape" and also out migration. Donkin (1979, 35–38) gave priority to colonial depopulation, but he also discusses climatic and other theories.

A major discussion of abandonment appears in the journal *Current Anthropology* (Guillet 1987c). Guillet's article, focusing on Lari in the Colca Valley, is followed by 11 commentaries, including by Andean cultural ecologists William Mitchell, Jeffrey Parsons, Jeanette Sherbondy, Gregory Knapp, and John Treacy; Guillet provides a final response. Guillet (1987b, 409) believes that terrace expansion and contraction are closely related to water availability, with abandonment in the short term due to "constraints in the system of water distribution" and in the long term due to periodic droughts. "During periods of relative water abundance, constraints are relaxed, allowing new terraces to be constructed and abandoned ones rebuilt." The commentators elaborate on this thesis, raise objections, and suggest other significant factors. Treacy (425) pointed out that "one need not invoke climatic shifts alone to account for water scarcity." J.R. Parsons (423) says there needs to be "a systematic consideration of the cultural forces that may be casual factors in terrace expansion and contraction." Mitchell (422) emphasizes that terrace farmers today are involved in a market economy, so that there is not a direct relation between population and amount of land in cultivation. Knapp (421) reminds us that "deintensification" is a more accurate term than "abandonment," since no-longer irrigated terraces may still be grazed or dry farmed from time to time.

Factors of Abandonment

Prehistoric Abandonment We have mentioned that the unirrigated, sloping-field terraces and bench terraces were abandoned, either because of reduced rainfall or because of the greater reliability of irrigated terraces. There was a second period of prehistoric abandonment of some of the higher irrigated bench terraces such as at Chijra and Alto Cayra, for reasons unknown. There was a concomitant shift of settlement downslope, during one or both periods, and probably a shift from a staple of frost-tolerant and low moisture-tolerant quinoa to less tolerant maize. Thus, there was considerable abandonment of terraces prior to the third period of abandonment during the early colonial period.

Colonial Depopulation We know from the ethnohistorical study by Cook (1982) that the Colca Valley suffered a major population decline in the sixteenth century due primarily to introduced epidemic diseases. For the Colca villages, the estimated decrease from 1530 to 1721 was 87 percent. For the community of Coporaque, the population changed from an estimated 5,957 in 1520 to 1,956 in 1604 (Treacy 1994b, 167), a 67 percent decrease in less than a century. Commonly, under conditions of depopulation, the most intensive forms of cultivation and the least accessible agricultural lands are abandoned. In the Colca Valley, with Spanish-controlled Indian settlements being located on the *pampas* near the river and the population reduced, the higher terraces were indeed abandoned. Population has partly recovered, but the 1981 total for the Collaguas villages was still only 68 percent of the 1530 estimate. This compares with at least a 42 percent reduction in land cultivated; however, much of the land now cultivated is in alfalfa for livestock for market; in addition, some food staples are brought in from outside the valley.

Climatic Change The relationship between terrace construction, use, and abandonment and climate is complex and inconsistent. Less precipitation (both rain and snow) would presumably mean a reduction of water available for irrigation and hence a reduction of terrace cultivation. On the other hand, decreased rainfall could have made rainfed agriculture impossible and labor-intensive irrigated terracing feasible. Farmers today say they would restore old, unused terraces to production if they had more water. However, abandoned canals reaching *quebradas* fed by snowmelt on the high slopes, such as Cerro Mismi, could be restored and irrigation water thus increased, but this has not happened. At Coporaque, there are three sectors of abandoned terraces, including Chijra, totaling 112 ha, that could be restored, each linked to unused canal systems (Treacy 1994b, 227).

I have already suggested that the shift to irrigated bench terracing first occurred during the dry period between AD 540 and 730, with later expansion during the later dry period AD 1040–1490. Large portions of both periods experienced colder temperatures which would have forced cultivation downslope where precipitation is even less, but only down c. 150 m or less. The subsequent period, AD 1500–1720, was wetter than average, but this was the time of probably the greatest abandonment of irrigated terraces.[8] The twentieth century has also been a wetter than average period, but without significant terrace restoration. (There has been some expansion of valley-floor *pampa* fields.) Thus, climate change would not seem to be a primary factor in post-conquest terrace abandonment or in the lack of much restoration of old terraces today.

Springs Many terraces on slopes are watered from springs rather than from irrigation canals leading from *quebradas* fed by rainfall and snowmelt. These springs stop flowing or dry up due to unclear events, including depleted aquifers and tectonic activity; periodically they may revive and new springs may appear. Terraces are abandoned or restored accordingly.

Tectonic Activity Stanish (n.d.) has suggested that in the Moquegua Valley, tectonic uplift may have disrupted terrace/canal connections resulting in terrace abandon-

[8] This is true for most of the Colca Valley, but not for the Japo Basin where very few irrigated terraces have been abandoned.

ment due to the cutoff of irrigation canal intakes from stream channel sources which have downcut. David Keefer (pers. comm. 1997) reports that in the Moquegua area, landslides caused by earthquakes buried a section of a prehistoric canal resulting in terrace abandonment. Some terraces have been destroyed at Lari by landslides due to undercutting by the Río Colca.

Canal Water Loss Stanish (1987, 360) believes that in the Moquegua region of southern Peru, prehistoric settlement may have been relocated up river in association with shortening of irrigation canals in order to reduce water loss from seepage and evaporation. This could have resulted in terrace abandonment in the original sectors irrigated downriver. This does not seem to be a factor in the Colca Valley where most canals have always been of relatively short length.

Canal Abandonment When canals are abandoned, the associated irrigated terraces will, of course, also be abandoned. Canal abandonment may be due to a reduction of the amount of water available but also for social and political reasons. Water may be diverted from one community or farming sector to another. In the Colca Valley, such conflicts are of long-standing and have led to bloodshed. Also, the labor required to maintain and repair a canal may become excessive in relation to productivity, especially on high, steep, and distant slopes. Figure 2.12 shows abandoned canals and a reservoir and aqueduct that once provided water to the terraces at Chijra and Alto Cayra. The abandoned 10 km long Carhuasanta Canal north and east of Cerro Mismi (Fig. 2.8) [apparently] brought water from the Río Apurimac drainage (Amazon) to the Río Colca drainage (Pacific Ocean) on the western slopes of Pampa Finaya, east of Coporaque, via another 30 km of *quebrada* and canal (Treacy 1994b, 116–120).

Water Availability and Management Efficiency How much water is actually available at the present time in canal systems in relation to crop needs? The study of the hydrology of the irrigation system in Coporaque by Waugh and Treacy (1986–1988) suggests that there is nearly three times as much water available for crop production as is used by the present crops. This would seem to indicate that in terms of water availability, the area cropped could be considerably expanded. We must keep in mind, however, that during some periods of the growing season, there is more irrigation water available than can be used and the surplus is vented into the main *quebradas* and the Río Colca and thus lost to the crop system. Furthermore, no system of water management for crop production is 100 percent efficient in capturing all the water brought into an irrigation system. Nevertheless, the loss of two-thirds of the water available seems excessive and raises the question of possible change in water management efficiency since prehistoric times.

Variation over time in water management efficiency would mean increased or decreased water supplies for irrigation. For example, if water is dropped too rapidly through a terrace system, more water runs off rather than being absorbed by the soil and being available for greater crop production. Indications are that water management technology in pre-European times differed from current technology. For one thing, the Spaniards destroyed a social organization that probably contributed to efficient management of complex terrace/irrigation systems.

Maria Benavides (1997) points out that in the Colca Valley, there is insufficient irrigation water available to irrigate both the existing abandoned terraces and the currently cultivated terraces. Thus, she believes, when terraces were developed at lower elevations, where irrigation is more feasible, the terraces at higher elevations had to be abandoned. However, the higher terraces were not irrigated but were fed by rainfall and runoff. The moisture captured by the high terraces, which otherwise could have ended up in the canal system for the irrigated terraces below, would have only been a portion of the total water available for irrigation.

Vegetation and Soil Change Agriculture and overgrazing have drastically modified and reduced the natural vegetation cover, making slopes more susceptible to rapid runoff rather than water retention in the soil. Hence, the moisture available to crops from rainfall and runoff is reduced. Soil analyses by Sandor (1987a, b) indicate that soil fertility was improved with terracing and thus was not likely a factor in terrace abandonment. Soils are mostly derived from volcanic and alluvial material and are relatively fertile. Furthermore, they are now maintained by the application of manure and compost and by periodic fallowing, and both were likely true in the past. Sandor surprisingly found fertility of the surface soils to be comparable in both cultivated and abandoned terraces. The latter are not actively fertilized, but do receive manure from grazing livestock.

Loss of Terrace Knowledge An anonymous reviewer has queried whether terrace abandonment could be the result of post-Columbian loss of knowledge of how to construct and maintain terraces effectively. Ancient terraces are still maintained and repaired and some new bench terraces are being constructed. The quality of terraces has indeed deteriorated in some places, resulting in premature wall collapses and washouts (Treacy 1987b, 53–54). A higher frequency of breakdowns occurs where previous breeches were not adequately repaired. However, terrace farmers are well aware of good versus poor practices, so deficiencies are not necessarily a matter of loss of knowledge. And regardless of their knowledge about maintenance and rehabilitation, farmers today know little about how their terraces were initially developed, most present terraces having been first constructed hundreds of years ago (Williams 1990, 91). Involved is not only information about construction but also decision-making as to location, dimensions, soil and water management, etc. Knowledge which may have been lost could have been replaced by adequate similar or different techniques. So, we do not know if loss of traditional knowledge has contributed to terrace abandonment, but this seems unlikely.

Labor Inputs Today these are clearly larger for terraces on higher slopes compared to those for lower slopes. This is due to (1) greater distances from settlements to fields and hence travel time; (2) the difficulty of working on high, steep slopes; and (3) the higher level of maintenance necessary for terraces on steep slopes. Also, soils may be poorer on higher, steeper slopes, resulting in lower crop yields per investment of labor. Nevertheless, farmers in Coporaque say that high labor input is not the reason why they do not restore and farm abandoned terraces.

Changes in Land Use The introduction of European crops and cattle caused the breakdown of a fragile agroecology. Cattle rapidly damage terraces, increasing maintenance and at times leading to terrace abandonment. The walls of bnch terraces, both abandoned and used, have been ripped out in some areas in order to facilitate both plowing and grazing. The Spaniards brought about a greater emphasis on pastoralism, which has low labor requirements, at the expense of agriculture. The recent shift toward planting alfalfa for cattle feed has reduced the land in subsistence crops. Thus, the relation between land terraced, food production level, and number of people supported is less direct than in the past. Finally, segments of terraces, abandoned and cultivated, have been destroyed by the road and canal-building activities of the Majes water diversion project.

Social Access to Land and Irrigation Water Communities as well as individual households do not have equal access to irrigation water. (This can result in violent conflict or in efforts to renegotiate access to water.) One community or some farmers, as a result, can have sufficient water to cultivate all the land available, whereas adjacent communities or neighbors do not have enough water to irrigate all their terraces. Furthermore, if more water were available, it would be very difficult for people in a community to agree on who would have access to it as well as to the new land which would be irrigated. Additional land for additional irrigation would not likely come from private holdings but rather from unused land that is almost all communal land used for pasture. J. Treacy (pers. comm. 1988) indicated that in Coporaque in the 1980s, there were prospects of obtaining additional water and opening up an abandoned canal (the "Inca" Canal east of the village), but agreement could not be reached on who would get the abandoned terrace land for restoration and the new water to irrigate it, and hence the plan was dropped.

As Treacy (1989, 342) pointed out, farmers are most concerned with the timing of water availability, "making sure that supplies are distributed in an orderly fashion among many users during the time scarce supplies are available to seed crops. The total amount of water [available] is an abstraction of little interest to many Coporaqueños." Timing has to do with crop needs in relation to climate, water storage capacity, the irrigation system, and the social allotment of water. "Few would be willing to give up their water rights and privileges to risk watering new lands under unfavorable conditions." Water security is more important than maximizing water supply in order to increase land area cultivated. For a discussion of the problems with attempts to restore abandoned terraces at Coporaque, see Treacy (1987b).

Alternative Livelihoods Under conditions of population growth and even with population stability, there may be attractive alternatives to farming terraces under difficult conditions as well as to subsistence farming in general. These alternatives include seeking wage labor locally and migration in search of jobs in Arequipa or on the coast. The attractions may be food, or cash, or urban excitement and opportunities, or hopes of a better life for the next generation. Low market prices for farm staples is another factor. Rural to urban migration is occurring throughout the South American Andes for these reasons.[9] For the 12 central Colca villages, the population increased from 15,201 in 1940 to only 21,517 in 1981[10] (Denevan 1987, 17), a period during which the population of Peru nearly tripled. The young people of the Colca Valley have chosen to go elsewhere rather than restore and farm abandoned terraces.

Conclusion

By the completion of our Colca terrace project, the focus of research had shifted from explaining bench-terrace abandonment, which was primarily related to sixteenth-century population decline, to explaining why abandoned terraces had not been restored and brought back into production. We realized that in our emphasis on ecological research, we had neglected socioeconomic matters which are critical to understanding why abandoned terraces stay abandoned – resource management policy (especially water rights), land tenure, and alternative livelihoods. These conditions, of course, are themselves affected by the expansion and contraction of land under cultivation. Subsequent studies of the Colca Valley gave greater attention to water management as the key to land use, particularly terrace use: Treacy (1989, 1994b) on Coporaque, Guillet (1987b, 1992) on Lari, and Gelles (1990, 1994) on Cabanaconde, as well as elsewhere in the Andes (Mitchell and Guillet 1994).

The short- and long-term variability in the environment, especially climate, creates conditions which the Colca people have more or less adapted to by changing the form and location of terracing. However, demographic growth, or lack of growth, or decline seems to be the most important reason for [either] terrace expansion or contraction. Today, there is demographic stagnation without terrace restoration. There is demographic stagnation because people perceive better opportunities elsewhere. As the population continues to grow and external opportunities decline, there may be an indigenous movement back to the semiarid Andean mountain valleys such as the Colca. In the Colca, at least, environmental conditions do not seem to preclude the restoration of now abandoned irrigated terraces. What was done in pre-European times should be able to be done today and without resorting to energy- and capital-expensive modern technology.

[9] Depopulation or stagnation has occurred in many Andean villages in recent decades, despite higher fertility and survival rates (Preston 1996).

[10] The 1993 Census gives a lower total of 18,344 (Brooks 1998, 104).

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3.0 Cultural Plant Geography

Introducer: Karl S. Zimmerer

Abstract Cultural plant geography has undergirded the research career of William M. Denevan. Rooted in human-environment geography, Denevan's contributions provided landmark advances. He first utilized cultural plant geography in research on the region-scale anthropogenic influences and human use of tropical pine forests in Central America. Denevan subsequently directed this approach to research on the landscapes and adaptive strategies of the tropical lowland agriculture of indigenous groups. Subsequently it was central to his comprehensive analysis of Amazonian and Andean landscapes, global-scale synthesis, and interpretation such as the anti-Pristine Myth project, and the current interdisciplinary subfields of historical ecology, social forests, and agrobiodiversity.

Keywords Plant geography · Culture · Amazon · Agrobiodiversity

Introducing Denevan's Cultural Plant Geography: From Biogeographic Imprints to Adaptive Strategies and Beyond

Cultural plant geography launched the research and writing of William M. Denevan by providing the framework for his first major publication, *The Upland Pine Forests of Nicaragua: A Study in Cultural Plant Geography* (Denevan 1961), based on his Master's thesis (1958). The monograph opens by stating "This is a study in cultural plant geography, a study concerned with the relation of human activities to the occurrence and distribution of the genus Pinus…." The opening sentences are anchored with a footnote defining this freshly minted field as "the study of vegetation changes resulting from man's occupance of the earth…" (Robison 1958).

Denevan's focus on cultural plant geography evolved significantly in his ensuing works that traced an arc increasingly directed in the 1970s and 1980s toward the adaptive strategies of the tropical lowland agriculture of indigenous groups. The study of vegetation change as cultural plant geography was central to this research

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that became synopsized in "Fields of the Mojo, Campa, Bora, Shipibo, and Karinya" (Denevan 2001). Crafted as a chapter in his volume on *Cultivated Landscapes of Native Amazonia and the Andes*, the design of "Fields" utilized the theme of cultural plant geography even as the term itself had become uncommon.

Drawing on the aforementioned publications, the goal of this essay is to trace Denevan's research on cultural plant geography from its early foundation of biogeographic imprints to subsequent interest in adaptive strategies. Initially contributing to the discipline of geography, it later influenced the vibrant interdisciplinary studies of historical ecology, social forests, and agrobiodiversity (including agroforestry and agroecology). This trajectory is crucial to the insights that Denevan marshalled in debunking the Pristine Myth of Nature in the Americas (Denevan 1992 and section "The Pristine Myth: The Landscape of the Americas in 1492" in Chap. 8) and advancing the awareness of pre-European Amazonian landscapes. I first provide a brief analytical review of each publication in sections "The Upland Pine Forests of Nicaragua: A Study in Cultural Plant Geography (1961)" and "'Fields of the Mojo, Campa, Bora, Shipibo, and Karinya,' in *Cultivated Landscapes of Native Amazonia and the Andes* (2001)". I then examine the theme of cultural plant geography in the broader context in Denevan's *oeuvre* as well as its major interlocutors from the past to the present (section "Discussion: Cultural Plant Geography in Context, Oeuvre, and Influences"). The essay concludes with a synthesis of this influential legacy (section "Conclusion: From Imprint and Adaptation to Historical Ecology, Social Forests, and Agrobiodiversity").

The Upland Pine Forests of Nicaragua: A Study in Cultural Plant Geography (1961)

The Upland Pine Forests of Nicaragua appeared in the University of California Publications in Geography, a series that had been founded by Carl Sauer, and denoted the scope of Denevan's early career success. "Upland Forests" furnished a geographic examination of the relations of pine forests and species distributions to human fires and settlement in northern Nicaragua. Denevan selected the country's Segovian highlands as part of "the southern geographical terminus of the genus in the Western Hemisphere" (Denevan 1961, iii). Interest in these pine forests was further justified by their value as important sources of timber and resin, with the latter valued for shipbuilding, fuel, and medicinal purposes (Denevan 1961, 297–299).

Denevan's focus benefitted from the growing number of studies of geographers, foresters, and ecologists investigating the role of fire in the distributions of pine species in tropical and subtropical regions. These studies had begun in the early twentieth century with the pioneering work of O.F. Cook that influenced cultural plant geography (Gade 1970). Research on pine forests and fire swelled in the 1950s with works by leading scientists and scholars such as James J. Parsons, who served as Denevan's graduate and thesis adviser at the University of California at Berkeley, G. Budowski, F. Egler, L. Holdridge, and C. Johanessen in addition to the pine specialist N. Mirov at the Berkeley station of the US Forest Service.

Denevan's monograph begins by reviewing the geographic distributions of the Central American pines (13 species in the genus *Pinus*) and the temperature-precipitation ranges of the species triad that occurs in Nicaragua (Denevan 1961, 254). It then amasses multifaceted evidence to argue that the upland pine forests of Nicaragua are dependent on human burning and, correspondingly, this vegetation formation occurs in conjunction with long-continued permanent settlement (Denevan 1961, 300).

"Upland Forests" integrates research in biogeography and ecology with the use of cultural and historical techniques. One core of Denevan's approach applied then-current ecological theory, in particular the concept of climate-driven climax vegetation, to understanding the distribution of these pine forests. It is the exceptions to the climax forest communities, Denevan deduced, that are created by edaphic conditions and the human activities of settlement and burning. Undertaking this approach required Denevan to integrate fieldwork with the mapping of plant and physical geographic distributions, the observation of human burning and other vegetation impacts, and the evaluation of historical records of human settlement.

"Upland Forests" devotes ample attention to the description of physical geographic parameters, especially geologic parent materials and soils, topography, and climate. It compares these distributions to biogeographic results based on the mapped occurrence of pine forests using air photos. Denevan then describes the role of fire, especially the regular practice of light burning by local inhabitants. His treatment of fire incorporated insights based on what are now termed natural experiments, such as the de facto fire exclosures created by crossing trails. In addition, Denevan created historical geographic estimates of the frontier of the denser settlement of human population at the eastern edge of the Nicaraguan highlands in the years of 1603, 1752, and 1957. He concludes the monograph definitively: "upland pine forests of Nicaragua are a deflected climax, a man-made formation resulting from clearing of broadleaf forest and repeated burning" (Denevan 1961, 300).

Overall, Denevan singles out the role of human fires as the major determinant of the distribution of upland pine forests in the Nicaraguan highlands. Human settlement and soil types influenced by geologic parent material also facilitate the success of these upland pines. It contrasts the absence of pines east of the settlement frontier in Nicaragua's central mountains, as well as the settled southern uplands. Denevan completes the conclusion by calling for more studies on the cultural plant geography of pines in the tropics, reflecting his deep-seated commitments as an empirical and comparative researcher. More broadly, this publication initiated Denevan's sustained analysis of anthropogenic landscapes.

"Fields of the Mojo, Campa, Bora, Shipibo, and Karinya," in *Cultivated Landscapes of Native Amazonia and the Andes* (2001)

Vegetation change related to human activities is critical to the expanding focus that Denevan developed on the agricultural field systems of the lowland tropics of South America. This research culminated in his publication of "Fields of the Mojo, Campa, Bora, Shipibo, and Karinya" in 2001. It provides the comparison of several distinctive swidden agricultural systems of indigenous groups in the Amazon and Orinoco basins from Bolivia and Peru to Venezuela. Drawing on field research and earlier publications (e.g., Denevan 1963, 1971; Denevan and Schwerin 1978; Denevan et al. 1984), "Fields" offers descriptions of the tropical lowland agricultural systems of the colonial Mojo (Bolivia), the extensive system of the Campa (Peru), the managed fallows of the Bora (Peru), the floodplain cultivation of the Shipibo (Peru), and the multiple-biome fields of the Karinya (Venezuela) (Denevan 2001, 75). Important to note in the present volume is that the description of the managed fallows and agroforests of the Bora is omitted in the reprint of "Fields" herein (section "Fields of the Mojo, Campa, Shipibo, and Karinya") since it overlaps with the material contained in section "Indigenous Agroforestry in the Peruvian Amazon: Bora Indian Management of Swidden Fallows" in Chap. 6.

Denevan's immediate goal was to assess the temporal and spatial variation of vegetation and related human activities in swidden field systems. By innovating the application of cultural plant geography, Denevan was able to describe this variation and, more broadly, the adaptive strategies of lowland tropical agriculture. Denevan extended this innovative deployment of cultural plant geography to the community-level properties of food plants, notably the diversity of cultivated species, as well as the importance of non-crop vegetation, with particular attention to the utilized tree flora that was culturally important and biodiverse.

Denevan's treatment of the swidden agricultural system of the Campa people is exemplary. The Campa or Asháninka reside in the Gran Pajonal ("Great Grassland") in the central Amazon (*selva* central) of Peru. Pajonal vegetation comprises tropical forests, scattered patches of savanna grassland, and agricultural systems based on extensive-style swiddening. Population of the Campa, who are the largest Amazonian indigenous group, was estimated at 30,000–45,000 in 2001 (Denevan 2001, 78), whereas the original 1971 publication surmised 24,000–26,000 persons (Denevan 1971, 498).[1] Denevan detailed how the characteristics of vegetation (mature forest) and soil (more fertility) are important siting factors in the vegetation and edaphic changes that occur with forest clearing for the purpose of swidden agriculture. He enumerated the diversity of Campa crop domesticates and semidomesticated plants as 48 species (1971, 505).[2] Still, it was the ample and irregular variation in agricultural vegetation and management, which Denevan synopsized as "unstable swiddens" (Denevan 2001, 78), that was considered most characteristic.

Conceptually this research employed three distinct applications of cultural plant geography. First was its emphasis on the variation of vegetation over time that is a distinguishing feature of swidden clearing, cropping cycles, and managed forest fallows. Second was the focus on adaptive strategies that enabled Denevan to refer to the food and cultural ecological functions of swidden by rooting this analysis in the distribution and occurrence of swidden plants (see also Denevan 1983 and section "Adaptation, Variation, and Cultural Geography" in Chap. 8).

[1] On the recent demographic growth of indigenous people in the tropical lowlands of Latin America, as well as the challenges of demographic estimates, see such works as McSweeney and Arps (2005).

[2] This richness of Campa crop diversity at the species level (a component of agrobiodiversity) was significant but overshadowed by the 101 cultivated plant species and other plants cared for that were documented several years later among the Karinya in the Venezuelan Llanos (Denevan and Schwerin 1978, 42–45).

Third was the role of human activities in relation to vegetation changes across space. Distinctive spatial variation became increasingly important in Denevan's descriptions, such as the Shipibo people's multiple food-producing biotopes consisting of floodplains, mudbars, and topographic positions on levees and backslopes. Each of the nine local biotopes was associated with distinct arrays of crop vegetation and cultivation techniques (Denevan 2001, 93–95). Similarly, Karinya cultivation in the Orinoco Llanos was found to correspond to nine biotopes, each distinct with regard to topography, vegetation, crop types, and field system (Denevan 2001, 98–100; see also Denevan and Schwerin 1978). The term "variable adaptation" aptly encapsulated this spectrum of diverse Karinya agricultural systems.

The "Fields" research and writing "falls within the conceptual discourse of cultural ecology, which is concerned with how people relate to, or interact with, their environment" (Denevan 2011, 10). This cultural ecology takes in geographical ecology, political ecology, ecological anthropology, and historical ecology. Notably omitted is the systems ecology perspective, perhaps for reason of its tendency to assume equilibrium-trending conditions (Zimmerer 1996), whereas the Campa findings had impressed on Denevan the variability and lack of systemic regularity that are characteristics of swidden agriculture in tropical lowlands (Balée and Erickson 2006).

Denevan's cultural plant geography in this reading must be seen as focused on the relation of human activities to vegetation distribution, in particular the temporal and spatial variation of vegetation in swidden agricultural systems. Denevan saw this variation as crucial to the realm of cultural adaptations to the environment that he positioned as the central topic of his research and scholarship (Denevan 1983). It also leveraged his capacity to comment briefly on implications regarding a then vociferous argument on protein deficit and the so-called Counterfeit Paradise of Amazonia that was being researched and debated by a host of leading environmental archaeologists and ecological anthropologists (Denevan 1971, 524).

The "Fields" reading also evidenced the influence of Denevan's faculty appointment. He drew extensively on the cadre of excellent graduate students investigating lowland tropical agriculture and resource systems at the University of Wisconsin, Madison, as well as elsewhere. "Fields" was also propelled through the inputs of collaborating faculty and research scientists, many themselves top scholars. Finally, financial support from the university and the Geography and Regional Science program of the National Science Foundation included grants for his field research projects in Peru and Venezuela in the 1970s and 1980s. These influences were parlayed into Denevan's growing interest in the "big questions" of the relations of people and land to the rise and fall of societies (Denevan 2001, 10) that complemented his earlier call for additional quantitative data collection (Denevan 1971, 528).

Discussion: Cultural Plant Geography in Context, Oeuvre, and Influences

The cultural plant geography illustrated in this pair of readings was crucial to the ensuring emphasis in Denevan's research on the mixed landscapes of Latin America that included, but were not limited, to the indigenous agriculture of native and pre-

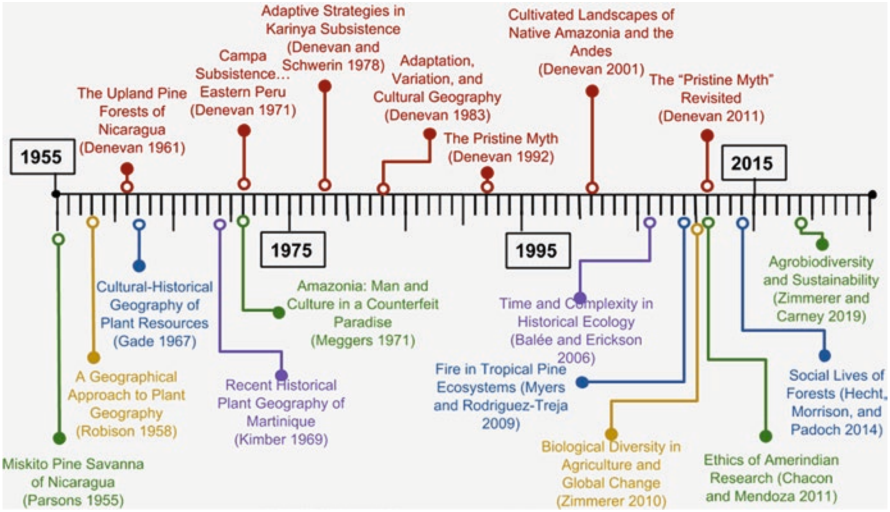

Fig. 3.1 Cultural plant geography in publications of William M. Denevan and related works

European peoples in South America. For instance, Denevan and Schwerin investigated the current indigenous management of vegetation change in contemporary Karinya agriculture in order to reflect on the extensive remains of raised-field complexes in Venezuela. Human burning related to vegetation change and patterns similarly was an enduring focus. Denevan integrated these insights to support his influential debunking of the so-called Pristine Myth of Nature in the Americas (Denevan 1992, 2011, 2016; Fig. 3.1). He drew heavily on the insights of cultural plant geography to argue against the claims of nominal or nonexistent levels of the demographic presence and environmental impact of indigenous people prior to Columbus' voyages and subsequent European colonialism.

Denevan's debunking of the Pristine Myth of Nature in the Americas bolstered new, enhanced awareness of the sophisticated indigenous agricultural systems of pre-European peoples as the basis of extensive areas of humanized landscapes in the Amazon and elsewhere (Denevan 2011). Denevan marshalled the concepts and tools of cultural plant geography to ground his growing engagement with the big questions of pre-European populations and environmental impacts in the Americas. While the Pristine Myth had been propelled by proponents as varied as the journalist Kirkpatrick Sale and literary and artistic figures of the nineteenth-century Romantic Movement, the new awareness triumphed by Denevan subsequently became propagated through influential, well-informed public figures such as the popular science author Charles Mann (Mann 2005).

Scholarly and scientific influence on the cultural plant geography of Denevan owed primacy of place to the work and guidance of his graduate thesis adviser and mentor, James Parsons (Denevan 1989), and Sauer's landscape approach (Denevan and Mathewson 2009). Parsons' field studies in spring 1953 had led to the publication of his article on the "The Miskito Pine Savanna of Nicaragua and Honduras" in the *Annals of the Association of American Geographers* (Parsons 1955). It argued elegantly for the causative role of human burning in the genesis of this pine savanna in the rainy Caribbean lowlands of Central America. Parsons highlighted the probable global scope of the influence of human burning on tropical pine forests (e.g., in Southeast Asia, Parsons 1955, 36). Later he extended his study of fire in arguing for its influence on the savanna grasslands of northern Colombia, the Colombian and Venezuelan Llanos, and the pastures of Central America.

The early cultural plant geography of Denevan, as well as some of Parsons' works in this domain, reflects the influence of Sauer's well-known 1925 treatise on *The Morphology of Landscape* (Sauer 1925; Leighly 1965). This influence is evident in the designs that Parsons and Denevan chose for their research on Central American pine forests, the organization of findings within their publications, and the contours of their reasoning and argumentation. Parsons and Denevan began their publications with information on the biogeography, taxonomy, and ecology of pines followed by accounts of regionally specific physical geography proceeding from topography and geologic parent material to climate and soils. Each then laid out the evidences for edaphic influences and, most importantly, the role of human fires and settlement in determining the region- and site-specific occurrence and distribution of pines in the tropics.

The Morphology of Landscape, it should be recalled, had propounded the case for studying and understanding the formation and functions of landscapes as the common ground of geography. This formulation of the landscape concept began with physical geographic factors and then overlaid the activities of cultural groups. Settlement counted as one of the principal factors of the cultural landscape. Fire too figured prominently, especially when it became etched in Sauer's contribution to the 1956 Wenner-Gren Conference that produced *Man's role in shaping the face of the earth* (Sauer 1956; Thomas 1956). In short, Denevan closely followed Parsons, and both were influenced to a significant degree by Sauer's concept (albeit not exclusively), in their applications of cultural plant geography to tropical landscapes.

Denevan's cultural plant geography benefitted from departmental strengths and campus-wide programs at the University of Wisconsin, Madison, in the fields of Latin American studies, biogeography, and historical approaches that were echoed in Gade's framings of "cultural-historical geography" and later "cultural biogeography" (Gade 1975) and Kimber's use of the term "historical plant geography" (Kimber 1969, 1988). Their works highlighted cultural and historical approaches combined with biogeographic and ecological concepts and methods. Other overlaps crucial to Denevan's own evolving approach to cultural plant geography were rooted in anthropology and archaeology.

The early phase of Denevan's cultural plant geography also reflected the widespread albeit mostly implicit influence of the tenets of cultural-historical ecology. The umbrella of cultural-historical ecology (Zimmerer 1996), which was associated with the works of James Parsons and others, had become a mainstay in human-environment geography of the mid-twentieth century and was distinguished by the characteristic use of ecological concepts integrated with biogeographic, cultural, and historical analysis. Subsequently the shift of Denevan's interests toward adaptive strategies became the basis of the research that was encapsulated in "Fields of the Mojo, Campa, Bora, Shipibo, and Karinya" that grew out of such individual case studies as those of the Campa (Denevan 1971), Karinya (Denevan and Schwerin 1978), and Bora (Denevan and Padoch 1988; Denevan et al. 1984). Denevan's vibrant circles of graduate students and colleagues, which extended to other departments and universities (Erickson 1988), helped propel this group's continuing interest in understanding adaptive strategies (Knapp 2017).

The evolution of Denevan's later cultural plant geography also contributed perspectives that subsequently expanded and gained momentum as distinct fields, in particular historical ecology, social forests, and agrobiodiversity (Fig. 3.1). In the case of historical ecology, for example, Denevan's contributions fueled the pioneering systematic investigations of anthropogenic and humanized landscapes across geographic regions, cultures, and time periods (Balée and Erickson 2006; Erickson 1988). Denevan's cultural plant geography also helped fuel research on the social dimensions of forests (Hecht et al. 2014) and the human-environment interactions of the biodiversity of food systems and agriculture that is referred to as agrobiodiversity (Zimmerer 2004; also termed "agrodiversity"; see Brookfield and Padoch 1994). Expanding attention is now also probing the regional and global geographic dimensions, cultural dynamics, and historical depth of agrobiodiversity (Coomes 2010; Zimmerer 2013; Zimmerer et al. 2018; Zimmerer and Carney 2019). Denevan and his collaborators continue to advance the understanding of historical ecology, social forests, and agrobiodiversity from a landscape perspective in the Amazon (Clement et al. 2015).

Conclusion: From Imprint and Adaptation to Historical Ecology, Social Forests, and Agrobiodiversity

Cultural plant geography has been integral to the career trajectory and contributions of William M. Denevan. The readings paired with this essay illuminate this central axis and how it has guided the growing spectrum of interest and activities devoted to human-environment interactions in general and, in particular, newer studies in historical ecology, social understandings of forests, and agrobiodiversity. In *The Upland Pine Forests of Nicaragua*, Denevan crafted a landmark study integrating the use of ecological concepts, observations of cultural practices, and historical analysis. It was rooted in the influential cultural-historical ecology of Parsons and others that traced to Sauer's framework for understanding the morphology of landscape.

Denevan subsequently broadened the scope of cultural plant geography to encompass the temporal and spatial variation of vegetation in the swidden, forest-fallow agriculture of the lowland tropics. This innovation became the foundation for his subsequent status as a leading scholar of the adaptive strategies of tropical swidden agriculture and the landscapes and earthworks of pre-European Amazonia.

Denevan's innovative turn in the use of cultural plant geography marked his transition from the ardent pupil in the field applying the lessons of Parsons and the Sauerian Berkeley School to his ascent and prominence as a leading scholar and scientist advancing new insights on indigenous agricultural adaptations, demography, landscapes, and environmentalist concerns. Denevan marshalled these insights in debunking the Pristine Myth of Nature in the Americas and supporting the alternative view of intensive agriculture amid humanized landscapes, dense human populations, and complex societies in the tropical lowlands. More recently, Denevan's cultural plant geography has inspired the initiation, growing interest, and expanding prospects of the interdisciplinary fields that focus on historical ecology, social forests, and agrobiodiversity.

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3.1 The Upland Pine Forests of Nicaragua

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Editor's Note: This is an excerpt, consisting of the original Preface, Chaps. 4, 5, 6 and 8, plus references and relevant figures and plates (herein relabeled as figures and incorporated into the text).

Abstract The genus *Pinus* in the Western Hemisphere extends from Canada to the Caribbean coast and interior mountains of Nicaragua. The highland vegetation is mainly mixed tropical broadleaf hardwoods, with sectors of *Pinus oocarpa*. The highland pines occur on poor acidic soils derived from granitic rock, whereas the hardwoods are on soils derived from both granitic and volcanic (basic) rocks. The question raised is why pines, usually a temperate climate tree, are found in the tropics, at their southern-most limit. Observation, historical information, and mapping indicate that the pines mostly occur (1) on acidic soils and (2) where there has been a long prehistory and history of human settlement involving forest clearing and frequent burning. The *oocarpa* pine is resistant to fire, whereas the hardwoods are not and thus are unable to compete with the pines on acidic soils. On the more fertile volcanic soils, however, the hardwoods dominate, even with burning. It is unlikely that extensive and pure stands of pines existed in Nicaragua before human arrival.

Keywords *Pinus* · Biogeography · Nicaragua · Forests

Preface

> There is a frequent correlation between actual vegetation and remote human occupations. The rate of succession towards the original forest community, the establishment and maintenance of certain plant communities and the dominance of certain species, can in many

cases be attributed to past human interventions, some of them dating several centuries back. Budowski (1959a, 262)

This is a study in cultural plant geography,[3] a study concerned with the relation of human activities to the occurrence and distribution of the genus *Pinus* in Nicaragua – the area of the southern geographical terminus of the genus in the Western Hemisphere.

In the humid tropics, the climatic climax vegetation is a mixed forest containing a great variety of species; the nature of any other type of plant community is largely determined by the selective effects of nonclimatic conditions, especially edaphic factors but also human activities such as forest clearing, grazing, and burning. Students of both people and vegetation have been giving increasing attention to the correlation between the existence of many tropical savannas and grasslands and human burning. There has been much less concern, however, for the possible correlation between fire and tropical forest communities dominated by a single woody species; such a relation has been suggested, for example, for certain casuarina forests in the East Indies, eucalyptus forests in northeastern Australia, teak forests in Burma and Thailand, oak forests in Central America, and pine forests in Southeast Asia and Middle America.

Species of the genus *Pinus* extend well into tropical latitudes in the Western Hemisphere. On the Caribbean coast of Nicaragua near Bluefields, a *Pinus caribaea* – grassland association – extends to 12° 10′ N latitude (Fig. 3.2), and in the northwestern highland area known as the Segovias, discontinuous stands of pine, mainly *Pinus oocarpa*, extend south of Matagalpa to 12° 45′ N. Studies of the ecology of the lowland pine savannas of eastern Nicaragua have previously been made by Parsons (1955b), Taylor (1959a), and Radley (1960), but the upland pine forests (Fig. 3.3) have received little attention in the literature.[4]

Most pine species readily invade open sites and thus may be part of an early successional stage rather than a climax plant community. Pine seeds are known to germinate best in exposed mineral soil, and young seedlings of many species, including *P. oocarpa* and *P. caribaea* in Nicaragua, are intolerant of shade. How then can the Nicaraguan pines, all of which occur in the warm and humid climates of elevations below 5000 ft (Fig. 3.4), compete with much faster growing tropical plant species so successfully that they form large and pure stands?

My observations indicate that in highland Nicaragua, pines grow mainly on sites with thin soils and acidic parent materials, in an area having a long history of human settlement which involved forest destruction, periodic burning, and soil erosion; however, regardless of soil conditions, the upland pines seem incapable of competing successfully – except, possibly, in localized "natural habitats" – with broadleaf vegetation for a long period of time without the aid of at least occasional fire.

[3] The term "cultural plant geography" has been proposed by Robison (1958b, 286) "…as a designation for the study of vegetation changes resulting from man's occupance of the earth…."

[4] For brief discussions of the highland pines, see Taylor (1959b, 207–209) and Food and Agricultural Organization (1950, 48–52).

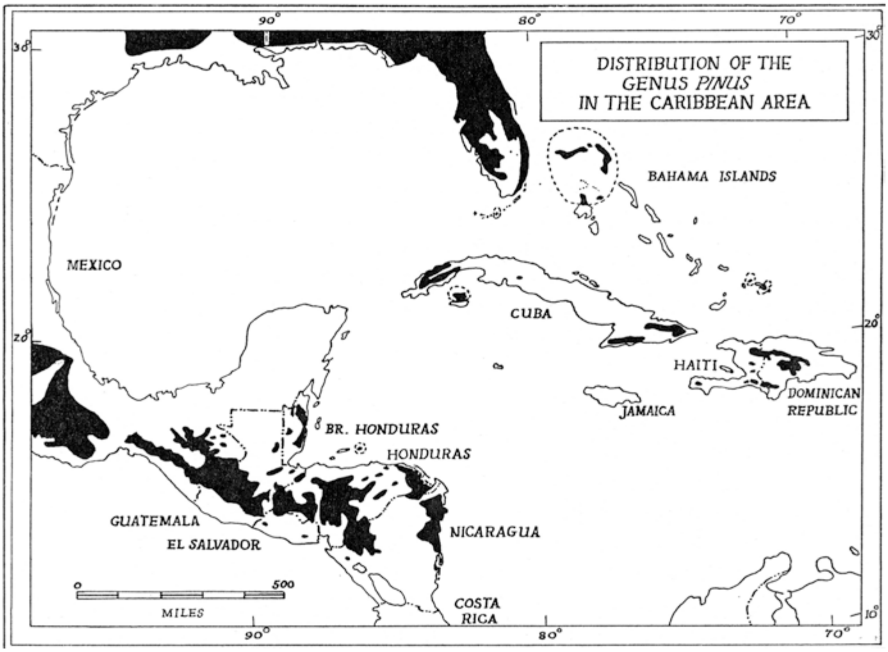

Fig. 3.2 The distribution of the genus *Pinus* in the Caribbean area

In this study climatic, edaphic, and biotic relationships as well as economic and historical human factors are examined to provide a basis for understanding the biogeography of the upland pine forests of Nicaragua. Special attention is given to the role of fire as an ecologic agent. Relief, parent rock materials, soils, climate, and vegetation are considered in some detail, primarily as they relate to the distribution of the pines, but also as they relate to settlement and land-use practices in the pine area.

It is important to point out that the physical and cultural geography of the interior of Nicaragua is very poorly known; consequently, this study was in many ways a reconnaissance. For most of Nicaragua, there is need for basic research in all fields of the physical and social sciences.

Fire and Pines

"These hills were covered with long-leaved pines, and the proportion they bear to the hard wood is said to have been increased by the Indian practice of burning the grass; the bark of the oak and other kinds of hard wood being more combustible, and more easily injured by fire, than that of the fir tribe." This little-known statement, made by Sir Charles Lyell in 1849 (Lyell, 69), concerned an area near Tuscaloosa, Georgia, but is equally applicable to many of the pine forests in the tropics.

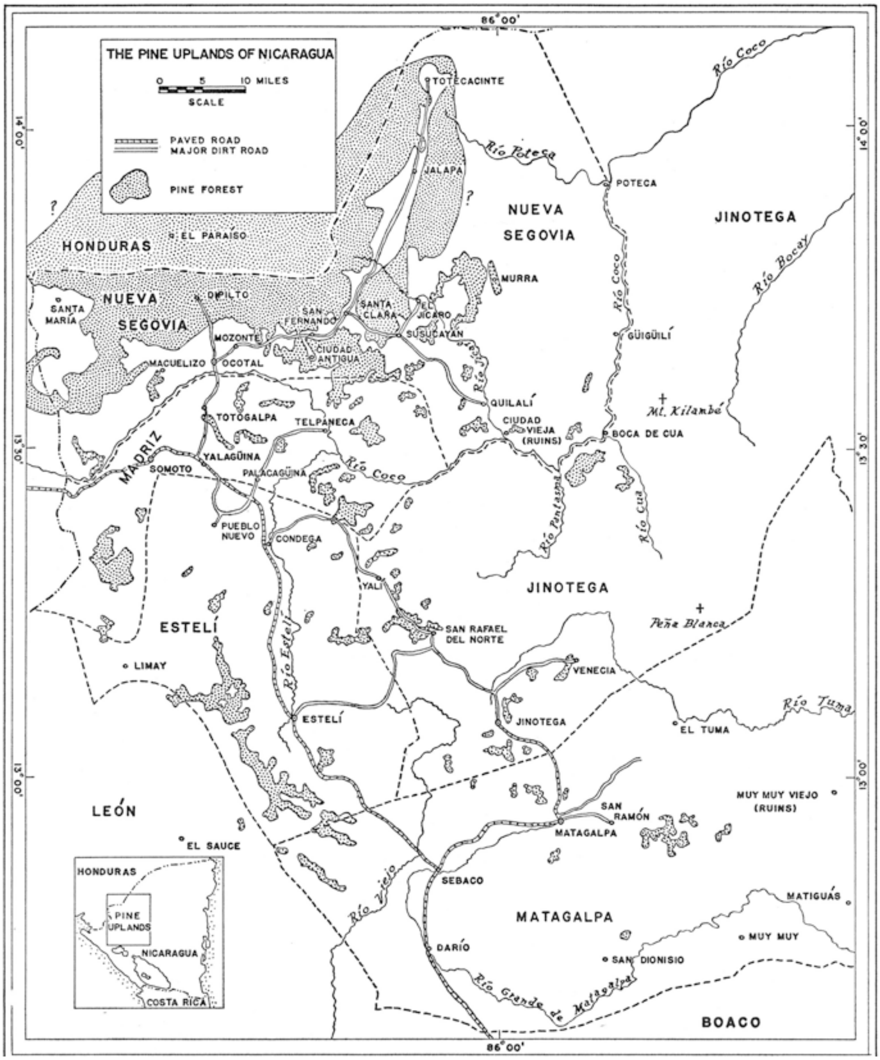

Fig. 3.3 The pine uplands of Nicaragua. (Sources for pine distribution: Aerial photographs 1956 and Taylor 1959b)

The Role of Fire in the Ecology of Tropical Pine Forests

O.F. Cook (1909) was probably the first to suggest that many of the Central American pine forests had succeeded mixed broadleaf forest on impoverished soils following shifting slash-and-burn agriculture. He argued that the pines were able to survive because of periodic, mainly human set fires, which kept out competing hardwoods. A number of reliable observers have since reached similar conclusions about various pine lands in tropical and subtropical regions of the Americas: for Central

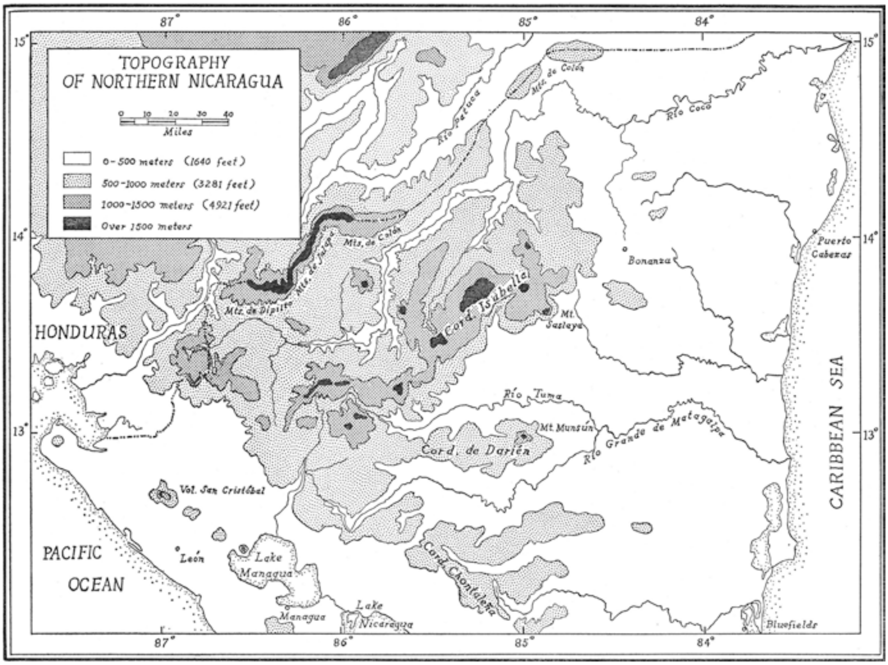

Fig. 3.4 Topography of northern Nicaragua. (Source: Tegucigalpa sheet. American Geographical Society, 1:1,000,000, 1942)

America, Allen (1955, 254), Budowski (1959b, 275–276), Johannessen (1959, 35–37), Parsons (1955b, 45–47), Taylor (1959a, 78–80; 1959b, 207–209), and Vogel (1952, 4–6); for the Caribbean islands, Beard (1953, 167), Ciferri (1936; see Bartlett 1957, 291–295), Durland (1922, 217), and Holdridge (1947, 64–68); and for the southeastern United States, Egler (1952, 232) and many others.[5]

Bartlett (1957, 284, 293), in reviewing and summarizing the studies of Ciferri (1936) and Chardón (1941)[6] on the pine forests of the Dominican Republic, commented:

[5] Comprehensive studies, including observations made over a period of years by Wahlenberg (1935, 1946) and others, indicate that the southern longleaf pine (*P. palustris*) is not only unusually fire tolerant, but is largely dependent on periodic fires. Chapman (1932, 330) expressed the opinion that lightning fires can occur frequently enough to account for longleaf pine forests. It has been suggested that pines elsewhere in the United States are dependent on periodic fires; see, for example, Weaver's article "Fire as an Ecologic Factor in the Southwestern Ponderosa Pine Forests" (1951).

[6] Chardón (1941) argued that the pines of the Dominican Republic were unrelated to human activities because they were geologically ancient and occupied high uninhibited mountains and because the abandoned *conuco* clearings he had observed had been invaded by *Cecropia* and other plants rather than pines. Bartlett (1957, 284) acknowledged the geologic relation, but added: "That lower-altitude *pinares* may not have been extended by human agency, just as prairies have, by clearing and fire, does not seem as yet to have been sufficiently investigated."

> It is hard to formulate an acceptable hypothesis for the persistence of pure pineland at low altitudes without involving fire as a factor. Fire, if it occurred frequently enough, would protect pine forests against replacement by deciduous [species] providing the climatic and edaphic conditions were such as to maintain the pine-forest floor in a seasonally combustible state … primitive man may well have greatly aided pine reproduction and extended pine by burning over land in hunting … [but] preparation of habitats for pine may conceivably result not only through the agency of man, but from natural causes such as wind-falls and fires caused by lightning.

The last qualification by Bartlett, the natural preparation of habitats, is frequently suggested, and although such preparation has been only rarely documented, natural phenomena, including landslides, blowdowns, floods, lightning fires, sterile soils, and rocky surfaces, have certainly provided favorable sites for the establishment of pines in tropical areas. It is my contention, however, that people's modifications of vegetation and soils by forest clearing and burning have made possible a considerable expansion of the tropical pine forests.

It is important to note that in Nicaragua *P. oocarpa* and *P. caribaea* are intolerant of shade: they are unable to regenerate beneath other trees, tall grass, or even in their own shade. The shade tolerance of the least common Nicaraguan pine, *P. pseudostrobus*, is unknown. During 6 months in the pine uplands, I did not once observe regeneration of any pine beneath heavy shade. This intolerance, substantiated by Taylor (1959b, 208) for *P. oocarpa* and (1959a, 79) for *P. caribaea*, is characteristic of many pine species. In the humid tropics, periodic fires are by far the most widespread means by which open stands of woody vegetation with little shade are maintained.

In the pine uplands of Nicaragua, light burning by the inhabitants takes place every year. In some areas it has probably been practiced since long before the Spanish Conquest. The importance of burning to the survival of pine forests in Nicaragua is partly shown by the existence of mixed hardwood stands, which do not burn, near pines and on comparable sites but with no young pines among them. Where mature pines do grow with a hardwood stand, the pines are apparently older; presumably they grew on an open site, and the hardwoods, no fires occurring on the site, came in later. This situation has been observed, for example, where intersecting trails block off fires.

The initial reaction to protection from fire in an open pine stand is a vigorous growth of young pine, but these shade out further pine regeneration. On some sites in the pine uplands which are protected from fire, there are very dense stands of pines, but with hardwoods exclusively, no pine seedlings, in the understory (Fig. 3.5). If protection from fire continues and the trees are not thinned, the hardwoods will probably take over such sites as the mature pines die. There have been several reports, from Nicaragua and elsewhere in Central America, of pine logs and buried pine roots amidst tall mixed broadleaf forest. Cook (1909, 20) noted that in Guatemala, in supposedly virgin forests east of Cobán in the bottom lands of the Polochic valley near Panzós and in the coffee district north of Senahu at 3000 ft elevation and above, dead pine roots were dug up by the Indians, who used them for torches. Cook was of the opinion that pines were once much more abundant when native populations were larger and there was more clearing of the forest. In Chiapas, Miranda (1953, 289–290)

Fig. 3.5 Dense stand of *Pinus oocarpa* east of Matagalpa which has been protected from fire for 2 or 3 years. There is no regeneration of pine in the dense understory of grasses, ferns, and leguminous weeds. If burning is not resumed, hardwood trees will grow up out of the moist shaded understory

reported buried pine logs south of Monte Libano in mature rain forest at an elevation of 900 m and at a distance of 15–20 km from the nearest pine ridges. He considered pines to be quite alien to the site and explained the logs as being the remains of either an extinct mesophytic pine species or an existing species which grew when the climate was drier than at present; neither suggestion seems tenable. Buried pine roots have also been observed in Honduras north of the junction of the Río Patuca and Río Guampu in a rain forest that contains tall cedar and mahogany, and buried pine stumps and logs occur in broadleaf forest in the pine uplands of Nicaragua just east of the Río Cua and also between Yalí and the Río Coco. It is surprising that fallen dead pines can resist decomposition under humid tropical conditions for an indefinite length of time. Where buried pine stumps occur north of Yalí, the present broadleaf forest is said to be at least 30 years old and possibly much older. The heavy bark or the resin content of pines may aid in their preservation.

Although burning can encourage pines at the expense of other plant groups, fire can also limit regeneration of pine if it is so frequent that pine seedlings do not have time to develop a fire-resistant bark and to grow high enough that their needles are above the flames of ground fires (Figs. 3.6, 3.7, and 3.8). (*P. oocarpa* can survive partial but not complete defoliation by fire.) Even if burnings are infrequent, a fire in accumulated debris and dead grass can be severe enough to destroy saplings and [even] older pines. In the pine uplands, a young pine requires from 3 to 7 years to become fire-resistant, depending of course on the severity of the fires. Early in the dry season, fires are less severe than they are at the end of the dry season when there

Fig. 3.6 Burned-over pine slope near Dipilto, Nueva Segovia. Although no pine seedlings have survived the fire on this site, the mature pines have been protected from serious damage by their thick layers of bark

Fig. 3.7 *Pinus oocarpa* seedling about 24 in. high near Matagalpa. If the surrounding grass were to be burned, this seedling would almost certainly be killed

Fig. 3.8 Stand of *Pinus oocarpa* on a steep slope near Dipilto, Nueva Segovia. Note the deep gully erosion on the left. All the young pines on the right were killed by fire a few weeks after this photo was taken

is more and drier fuel. If fires are very frequent, only grass survives, and if they are very infrequent, hardwoods begin to take over a pine stand. Thus, both the survival and the density of the pine forests are related to the frequency, severity, time, and extent of burning. Several Central American foresters have commented on the effects of different frequencies of burning in pine forests. Holdridge (1953, 43), writing of Middle America in general, noted:

> Pines also can be extended, as a result of burning, to occupy areas previously covered for the most part by broadleaf forest. With fires spaced at intervals of 5 to 20 years, the pines maintain themselves as dominants; with fires almost every year, the pines change in time to savannas; without fire, the larger part of the pine areas become covered with associations of broadleaf trees. [WMD translation from Spanish.]

After approximately 10 years without fire, however, not only do many deciduous species grow large enough to resist infrequent fires, but an evergreen hardwood formation can become so verdant that fire cannot penetrate it.

Vogel (1952, 4–6) stressed the harmful effects of too-frequently recurring fire on the pine forests of Honduras:

> During the half year from January to June, the pine forests in Honduras suffer annually from hot forest fires that rage unchecked over almost every square mile of the Central Region… Honduran pines seed well, and the climate is favorable to reproduction. The only factor which prevents an excellent stand of young pine for future generations is *frequently recurring fire*. We say "frequently recurring" because it is quite probable that pine would be succeeded on large areas by various other forest species in a century or two if the forests remained without any fire at all for that period. As a rule, pine does not reproduce well in a

heavy ground cover of grass and brush. Only when the ground cover is opened by fire, water, or by mechanical means does the seed germinate and take hold.

But, once germinated and rooted, the seedlings must be protected from fire for the next five to seven years, or they will be lost as surely as if they had never germinated. That is what has happened to pine seedlings everywhere that reproduction is absent today; it has started but has been killed by fire.

It is surprising, considering the amount of burning, that extensive pine forests still exist in Honduras and Nicaragua; apparently rough mountain relief breaks up fires sufficiently so that a few seedlings escape burning long enough to become fire-resistant and thus perpetuate an open pine stand. On the other hand, in the pine savannas on the gentle terrain of the Miskito Coast, though seeding is profuse, few seedlings survive the sweeping and unimpeded fires except where a close network of trails break the fires. Wind-blown upslope fires in the mountains are, of course, more severe than the slow-moving fires on the flats, but the former burn out rapidly and their coverage is spotty.

All these observations on the relations of fire to the establishment and survival of pine apply to *P. oocarpa* in the northern mountains of Nicaragua, where pines grow up to an elevation of about 5000 ft, but they do not necessarily apply to all the pines in Middle America. At high elevations, especially where frost occurs (above approximately 6000 ft), pines may maintain themselves without much aid from fire, although disturbances of sites may still be necessary for initial germination. Most of the Middle American pine forests actually do occur at fairly high elevations. As elevation increases, low temperature reduces the variety of plant species and rate of plant growth, i.e., the competition with pines which must contend. Yet except at very high elevations, there is much dense hardwood forest vegetation above the frost line in the humid tropics; consequently, generalizations about the relation of low temperature to occurrence of pine are not easy to make. Probably the combination of temperature and edaphic factors provides more natural habitats for pines at higher elevations than at lower elevations. But slash-and-burn agriculture in the high mountains would undoubtedly have increased the potential area there for the establishment of pine that was previously dependent upon natural disturbances and unusual localized conditions. Clearing and burning would also make it possible for pines to invade the warm lower mountains and coastal plains and survive. Conclusions similar to these have been reached by a number of observers, including Beard (1953, 167), Ciferri (1936, 259), Durland (1922, 217), and Holdridge (1947, 66), all with reference to Hispaniola, and Cook (1909, 19) for Guatemala. Holdridge, in his study of the pine forests of the Morne la Salle Mountains of southern Haiti (1947, 65–66), commented:

Since a great number of tropical and subtropical tree species are not able to survive under the colder conditions at higher altitudes, the pine would not have been subjected to severe competition and would have been able to exist in pure stands or in mixture with some hardwood trees. Where soils are poor at high elevations, there is little reason to doubt that the pines occurred in pure stands in pre-Columbian days in a very similar state to that found at present.

At lower altitudes below the frost line, the history of the pine distribution may have been quite different. By observations of the rapid invasion of hardwood species on richer soils where fires have been excluded for several years, one becomes conscious of the tremendous population pressure which a pine forest would have to exert to hold its own. Apparently, the pine stands below the frost line were less extensive in pre-Columbian time and occurred as

> patches or scattered trees mixed with hardwoods in the frost hollows, on areas of poor soil and along the crests of the ridges. As soon as man entered the area, occasional fires began the process of pushing back the hardwoods from sections which the pines were able to invade because local sources of seed were available and to hold because the pines are quite fire hardy except when young.

The Causes and Effects of Fires in the Segovias

The upland pines of Nicaragua are all found at relatively low elevations in a subtropical environment where their establishment and survival seem particularly favored by fire. The question remains whether natural fires occurred frequently enough before the coming of people to account for large and pure stands of pine.

There is very little documented evidence of lightning fires in Nicaragua or elsewhere in Central America (Budowski 1959b, 265). One lightning fire, which burned through 5 mi^2 of pine forest, was reported in British Honduras in 1954 (British Honduras, Forest Department 1955). In Nicaragua most electrical storms take place during the rainy season when the cover of vegetation is verdant and hardly combustible. A few storms do occur during the dry season, when fires could be ignited by lightning if it were not accompanied by heavy, steady rainfall. The frequent occurrence of lightning fires in the Florida Everglades, where the climate is comparable to the tropical *Aw* climate of Central America, has been documented by Robertson (1955). Lookouts observed 12 lightning fires in 1951 and 11 in the first half of 1952. These were grass fires, however, and it is doubtful that there was much grass in the Nicaraguan mountains before Indians first appeared and began clearing the forest and burning. Areas now being converted to grassland are first manually cleared of forest and undergrowth and then burned. Present savanna fires generally do not invade the forests of Nicaragua unless the forests are open and have a grassy ground cover. However, during an unusually dry season, the margins of a mature closed forest might be destroyed by fire, thereby extending savanna. Gallery forests in eastern Nicaragua were invaded by fire during the severe dry season of 1958 according to Taylor (personal communication 1961).

What is known about the causes, nature, and ecological effects of fire in the American tropics has been well summed up by Budowski (1959b, 270–272). He noted that crown fires are not known in primary tropical forests but may occur in very dry secondary forests and that recurring savanna fires may gradually penetrate the understory of a dry forest and encourage grasses and fire-resistant trees. The ecologist Dansereau (1957, 275) wrote about the burning of tropical vegetation: "Although it is too much to say that climax vegetation is immune to fire, … humid plant cover (from tropical rain forest to summer-green deciduous forest) hardly ever burns if it has not been previously disturbed, in such areas it is the jungle, the second growth forest, and the scrub or grassland that burn."

Thus, although both the Florida fires and the fire in British Honduras [today's Belize] show that lightning does start fires in the tropics, the frequency and long-term effects of such fires in tropical forests seem minor. However, secondary vegetation, usually a product of human activity, is often very susceptible to either natural or human fires. The dominant role of clearing and burning in extending the pine area in the Nicaraguan highlands is suggested by several additional circumstances: human activities have been the known cause of regular fires in the pine uplands since long before the germination of the oldest pine tree standing today, and existing pine forests are limited to parts of the highlands long settled and long burned-over. Possibly there have been pines in Nicaragua only within human times. A recent entry of pines into Nicaragua from northern Central America is certainly suggested by the presence in Nicaragua of only three pine species, none of which are endemic; by the small area of the country covered by pine in contrast to the large areas of pines in Honduras and Guatemala; and by the fact that the southern limit of the genus *Pinus* in the Western Hemisphere is in Nicaragua, even though there is no major climatic barrier to further advance.

The native people of tropical America, past and present, have always used fire to clear and prepare land for crops. The Indians of Nicaragua must have also set fires intentionally to drive game in hunting (a common practice all over the world), by accident, and for amusement. Where the same site was often fired, grass and fire-tolerant trees would be encouraged. Many grass fires undoubtedly burned over former *milpa* sites on the forest boundary, preventing regrowth of mixed forest and adding new clearings to the general area of regular burning.

The extent of slash-and-burn agriculture and the frequency of hunting fires would be expected to depend on the density of native populations, and because the upland pine forests in Nicaragua were in existence when the Spaniards arrived,[7] the size of the Indian population at that time should be meaningful. Within a few years after the entry of the Spaniards into Nicaragua in 1522, the expeditions they sent into the northern highlands in search of gold and to secure pine pitch for use in ship construction found an ample supply of settled Indians, probably mainly of the Matagalpa group, that could be put to work. The number of inhabitants in the pine uplands in the early sixteenth century is not known, but there is evidence[8] that the population of the entire country was at least half of the 1,300,000 estimated in 1956. Most of the present 37 towns and pueblos of the pine uplands were Indian villages at the time of the Spanish Conquest, and many other villages have since vanished. (Cuadra Cea 1957, identified at least 28 villages as existing in 1524.) That numerous settlements existed in the pine uplands long before the Conquest is indicated by the uncovering of artifacts, including, pottery, tools, and stone statues, that antedate the sixteenth-century cultures of Nicaragua. If the present 350,000 population of all the [five] Segovian departments (Nicaragua 1954, 22–26) is, like that of the country as a whole, slightly more than twice that at the time of Conquest, the population density at that time would have been about 20 per square mile.

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[7] Pedrarias Dávila, first governor of Nicaragua, in a letter to the Emperor of Spain in 1525, mentioned pine forests north of León, where pitch was collected (Dávila 1954, 130). Oviedo, writing in 1547, mentioned pines in the "sierras" of Nicaragua (Oviedo y Valdes 1944, II:57, 270).

[8] Bartolomé de las Casas (1822, 68–69) wrote in 1542 that from 1523 to 1537, the Spaniards shipped over 500,000 Indian slaves from Nicaragua to Panama and Peru and killed another 500,000–600,000, leaving only 4000–5000 in the country. These figures are undoubtedly excessive but are indicative of a rapid demise of a large native population in Nicaragua. Lic. Diego de Herrera, in a letter to the Emperor in 1545, reported 600,000 Indians in Nicaragua at the time of the Conquest, of which, as a result of slave exports, disease, and killing, only 30,000 remained in 1545 (de Herrera 1875, 398–399).

There is reason to doubt, however, that even a fairly dense population is needed to bring about great modifications in vegetation through clearing and burning, especially in regions with a marked dry season, where grass fires, once started, could easily maintain themselves. For example, the Miskito pine savannas of eastern Nicaragua are currently almost completely burned over every year by a population having a density of between 1 and 2 per km^2, with most of the people concentrated in a few coastal towns. As far as is known, the main population centers in Nicaragua have always been on the rich volcanic soils of the Pacific coast, the population thinning out eastward through the mountains. Theoretically a small but fairly well-distributed population could have cut into the original broadleaf forest of the highlands, section by section, from the west. Each abandoned plot behind the agricultural frontier might subsequently have been burned over every year or every few years, thus favoring such pyrophytic plant associations as grasslands, scrub savanna, and pine or oak forests. In contrast to tropical forest destruction from shifting agriculture along an expanding frontier is the situation in many of the tropical forests of the world, where either land pressure is so great that the native people [disperse] their shifting farms throughout their territory as part of a planned rotation or forest-fallow system or slash-and-burn farms are dispersed simply because the population is small and very dispersed as is true of the present-day Sumo Indians in the rain forests of eastern Nicaragua.

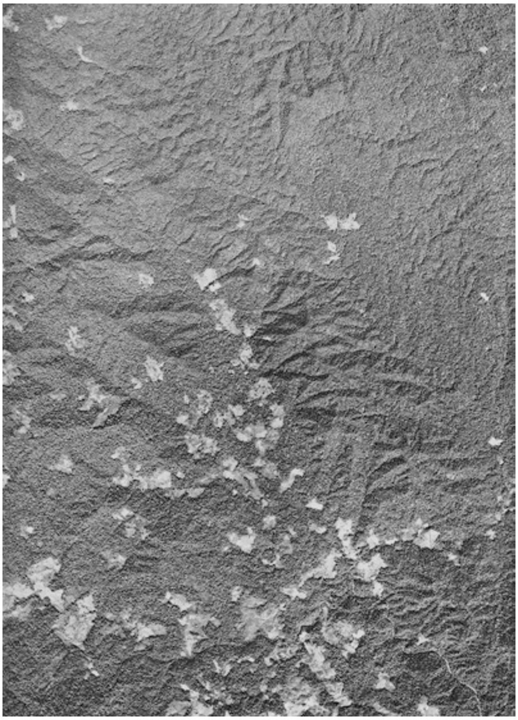
Where a population is expanding from a nucleus into an area of unlimited new land, little attention need be given to the land abandoned behind the frontier, although it may be burned over freely in hunting or to improve pasture for livestock. Such abandoned land would burn much more readily than the forest ahead of it. Grasslands and savannas have probably been produced in various parts of the tropics as often by this means as from the extensive cultivation by a population too dense to allow abandoned plots to return to forest fallow.

In 1874, Thomas Belt (1928) reported section-by-section cutting into the eastern forest in the southern highlands of Nicaragua in the Department of Chontales. Belt showed an early awareness of how the actions of people determined the boundary between the eastern rain forest and the grassy upland savannas. He observed a sharp break between the two without a significant corresponding change in soil, bedrock, or climate. He attributed this break to the practices of the Indians of cutting and burning the forest for planting maize and beans. These farm plots were then abandoned, he wrote, not because the soil was worn out, but because cutting a new clearing was easier than weeding out the invading grasses and shrubs:

> Should the brushwood ultimately prevail and cover the ground, the Indian or Mestizo comes again after a few years, cuts it down, and replants it with maize. But as most of his old clearings get covered with grass, he is continually encroaching on the edge of the forest, beating it back gradually, but surely, towards the north-east. As the process has probably been going on for many thousands of years, I believe that the edge of the forest is several mi nearer the Atlantic than it was originally (Belt 1928, 187).

The process of cutting back the forest from the west has been extensive in the eastern parts of Nueva Segovia, Jinotega, and Matagalpa, where pine forest, savannas, and pasture lands end abruptly before a wall of broadleaf forest, which forms the boundary line of regular burning. Aerial photographs reveal that beyond this boundary there is a checkerboard pattern of clearings and forest, the clearings becoming fewer and fewer eastward (Fig. 3.9).

Fig. 3.9 Aerial photograph (1956) of a mountainous area about 20 mi east of San Rafael del Norte and near the Río Tuma. The settlement frontier and area of regular burning lie further west; here there are only scattered *milpa* clearings, which thin out to the east. Annual rainfall is about 80 in., and the vegetation is seasonal evergreen rain forest. Scale: 1:61,000. Photo courtesy of the Inter-American Geodetic Survey



Indian fires on any given site would probably have the irregularity that does not completely destroy pine regeneration. Furthermore, many of the Indian fires must have been light fires occurring at the beginning of the dry season when grasses can be burned but are not so tall, dry, and combustible as at the end of the dry season, when hot fires would be most destructive of pine seedlings.[9] Since the establishment of a large number of cattle herds in Nicaragua,[10] the people have burned their lands every year to encourage and improve annual growth of pasture and to destroy ticks and other harmful insects. Almost all this burning is done in the last few weeks of the dry season just before the first rains bring up new grasses. Jaragua (*Hyparrhenia rufa*), which often grows 6 ft high, is the main pasture grass in the pine uplands. By the end of the rainy season, it is usually too coarse to be palatable for cattle, and within a few more weeks, it is completely dried out and without food value. Consequently, the pasture grasses are not grazed low, even with overstocking, and hot ground fires that are very

[9] Egler (1952, 225–228) discusses the time of occurrence and the effects of Indian fires in comparison with subsequent types of burning in the pine areas of southern Florida.

[10] The size of the cattle herds of colonial Nicaragua is unknown, but Sofonias Salvatierra (1939, II, 209–210) commented, without documentation, that during the seventeenth and eighteenth centuries, Nicaragua was the leading cattle producer of Central America and regularly provided beef and hides for the other provinces. He wrote that at the start of the eighteenth century, 52,000 head of Nicaraguan cattle were sent each year to the fair at Chalchuapa (El Salvador). Lévy (1873, 477) reported that there were 1,200,010 cattle in Nicaragua in 1872. In 1952, a total of 1,182,000 head of cattle were reported by the Ministerio de Economía (Nicaragua 1956, 35).

Fig. 3.10 *P. oocarpa*. One of the southernmost outliers of upland pines, located southeast of Matagalpa overlooking the valley of the Río Grande de Matagalpa. Burning is regular here, and there is no pine regeneration. The mountains of Chontales are in the background

destructive to pine seedlings and young pines up to 6 or 7 years old are possible. The native grasses, on the other hand, are almost all short and burn less vigorously, even when allowed to accumulate for several years. In either short or tall grass, the fires in the pine uplands are relatively slow-moving ground fires, which seldom race through the tree tops or damage property. Most observers agree, however, that burning practices effectively limit the regeneration of pine in the pine uplands. Still, rugged relief in many sections tends to break up fires, and enough seedlings escape fire long enough to become fairly fire-resistant and so maintain an open stand of pines. In addition, a single logging operation may leave behind a system of fire-breaking trails that helps protect new pine regeneration. Pines are disappearing completely only where regular burning is accompanied by persistent logging (Figs. 3.10 and 3.15).

Human Settlement and the Distribution of Pines in the Segovias: The Eastern Limit of Upland Pines and the Settlement Frontier

The Eastern limit of the upland pine forest in Nicaragua lies near the eastern boundary of permanent settlement. This boundary has not changed significantly since pre-Conquest times (Fig. 3.11). From Totecacinte in Nueva Segovia to San Carlos at the southeastern corner of Lake Nicaragua, a frontier line can be traced beyond which there are no settlements, other than a few scattered river villages, in a vast expanse of tropical rain forest that stretches over 100 mi to the savannas of the Caribbean low-

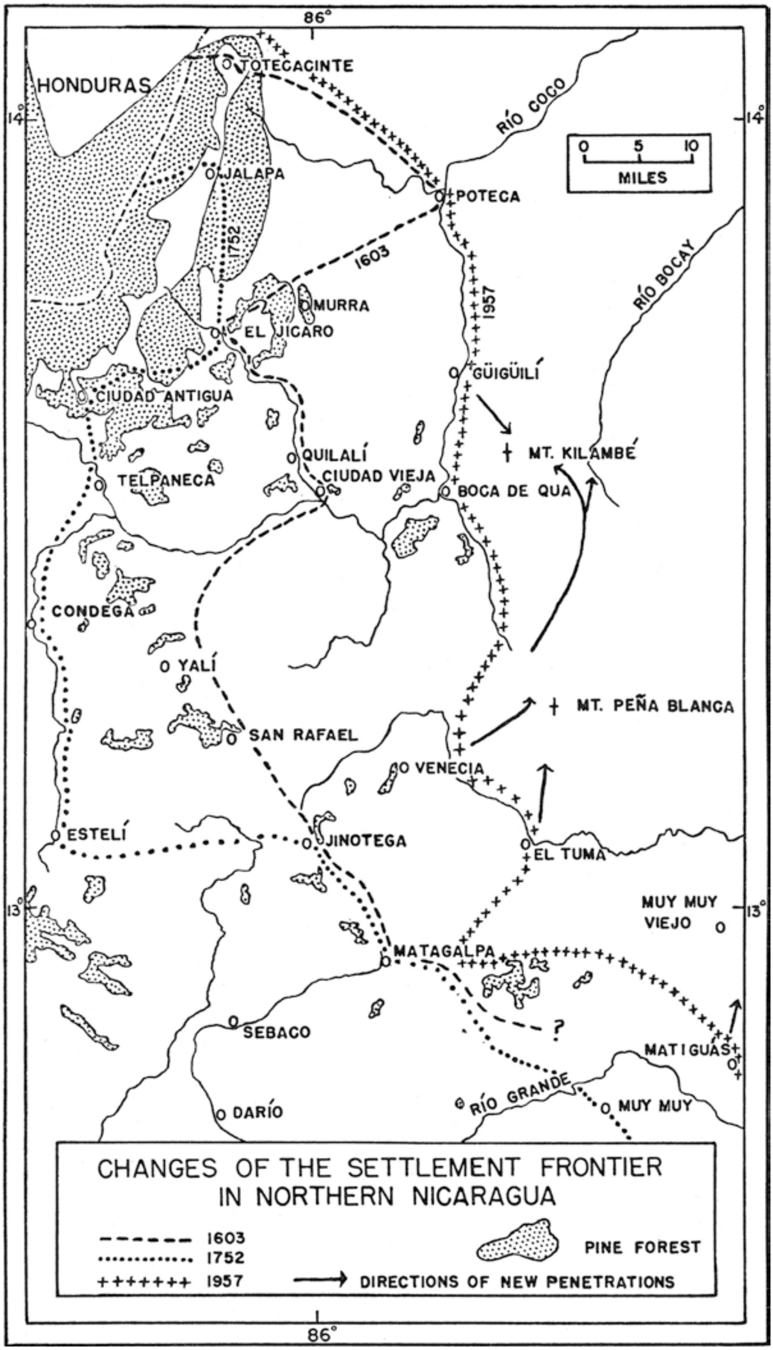


Fig. 3.11 Changes of the settlement frontier in northern Nicaragua

land. The upland pine forests are all west of this line, which marks no sudden change in the physical environment, but does roughly mark the eastern limit of regular burning. There is no sharp transition in parent materials or climate, and lateritic soils commence at variable distances east of the pines. The granitic, schistose, and acidic volcanic rocks on which most of the pine stands are found all extend east of the upland pine area (Fig. 3.12). It is true that annual rainfall is high east of the pine zone,

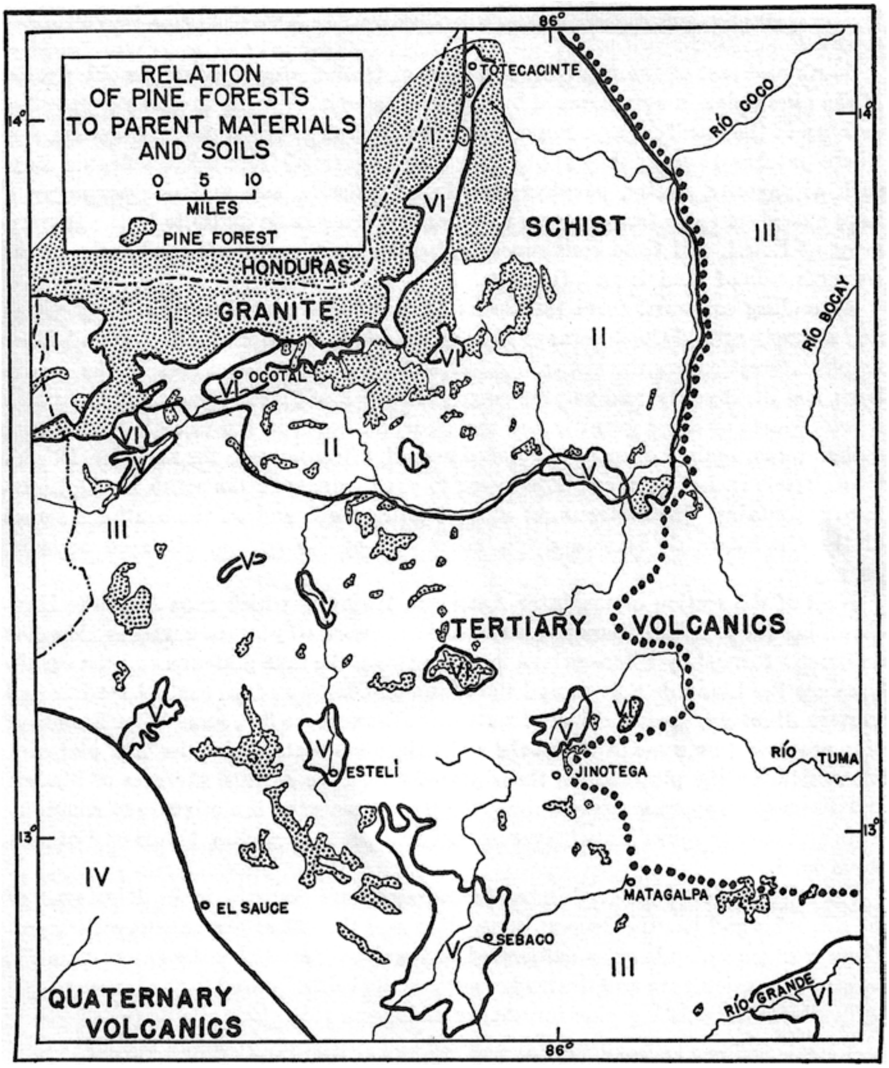


Fig. 3.12 Relation of pine forests to parent materials and soils. I, granite; II, metamorphics (mainly schist); III, Tertiary volcanics; IV, Quaternary volcanics (mainly ash); V, recent alluvium (mainly black clay); VI, Tertiary sediments. The dotted line indicates the approximate boundary between shallow upland soils (west) and deep tropical brown and reddish-brown latosols (east). East of the line, rainfall is over 70 in. annually and the vegetation is rain forest; to the west vegetation is mainly secondary deciduous forest, scrub savanna, and pine forest. (Main source: Taylor 1959b)

usually exceeding the 80 in. that is about the maximum known for *P. oocarpa*; nevertheless, *P. caribaea* is adapted to the rainy lower eastern mountains of Nicaragua and *P. pseudostrobus* to the cool and humid high eastern mountains of Isabella and Darién.

Soils change from tropical brown soils to reddish-brown latosols as rainfall increases east of the pine uplands, but this change in itself does not seem to prevent growth of pine. The pines grow on soils that are thin with little organic topsoil remaining, while mature broadleaf forest grows on a fairly fertile topsoil, although one rapidly leached of nutrients upon exposure to heavy rains. Today the thin, eroded soils on pine-covered slopes are unsuited for crops, but the same slopes may once have been covered with heavy broadleaf forest on more fertile soils that would have made possible the shifting slash-and-burn agriculture that theoretically prepared the way for colonization by pines. That *milpa* farming was possible is evidenced by the fact that the poor soils derived from granite and schist in Nueva Segovia – soils that are very infertile under pines – are deep and rich enough where broadleaf forest does exist to support some coffee and food crops.

The pine uplands are an area of thin soil and regular burning. Both of these are conducive to establishment and survival of pine, and neither are characteristic to the east of the settlement frontier. The fact, then, that fires, soil degradation, and extensive replacement of broadleaf forest by secondary and deflected climaxes, including pine forest, are all limited primarily to the area of long permanent settlement suggests that human activities have been a major reason for the present distribution of upland pines in Nicaragua.

No corresponding frontier exists in adjacent Honduras except south of the Río Patuca. High mountains bordering the Atlantic coast have produced local rain shadows in central and eastern Honduras, and relatively low rainfall, along with fertile valleys, have made much of interior Honduras more suitable for permanent settlement than are the dense and humid rain forests of central Nicaragua. As a result, clusters of settlement are dispersed throughout Honduras. Extensive pine forests are also found throughout the country (Fig. 3.2), generally in areas of thin, stony soils, acidic parent materials (schist, granite, sandstone, andesite), and regular burning. Somewhat analogous associations exist in Guatemala and Mexico; there are few areas of pine anywhere in the tropics or subtropics that are not subject to periodic burning.

The easternmost pine stands in highland Nicaragua are usually found with other deflected or secondary plant associations, but often there is a sharp transition from pine to tall evergreen broadleaf forest. I have seen areas where the tension zone between pine and broadleaf forest shows no relation to climate, topography, or parent materials, although the contrasting plant communities suggest two completely different environments. On the 4500 ft range east of Murra in Nueva Segovia, for example, *P. oocarpa* and *P. pseudostrobus* cover the western slope and the uppermost eastern slope, while all visible vegetation eastward is submontane rain forest. At another site on the Río Tapalchi, a small tributary of the Río Poteca in eastern Nueva Segovia, an open forest of *P. caribaea* ends abruptly at the bottom of a small valley. Here, in May 1957, I observed the burning of dry grasses beneath the pines (Fig. 3.13), but the adjacent broadleaf forest undergrowth was too verdant to ignite (Fig. 3.14). The local inhabitants claimed, however, that if new patches were cut out of the margin of the *montaña* and abandoned to grass and brush, then the edge of the fire line would tend to advance. A number of settlers had huts among the pines, but in order to clear land

Figs. 3.13 and 3.14 *Top*: Open stand of *P. caribaea*, "inland form," with the understory being burned, located near the Río Tapalchi, a tributary of the Río Poteca in eastern Nueva Segovia. Annual precipitation here is over 70 in. The view is toward the west. *Bottom*: Edge of mixed hardwood forest less than 100 ft from the pine stand shown in Fig. 3.13. A single pine appears at the left. The view is toward the east

for *milpas*, all went into the broadleaf forest where there was a dark organic loam topsoil instead of the reddish subsoil exposed by erosion beneath the pines. The parent material throughout the region is schist containing numerous quartz veins.

In other sites farther south, such as the regions around Ciudad Vieja (Nueva Segovia), northeast of Yalí (Jinotega), and near the Río Cua (Jinotega), patches of pine are interspersed with pasture and tall secondary semi-evergreen forest. Some of these areas, all in the frontier zone, were completely abandoned by both native

Fig. 3.15 Slope near Santa Clara, Nueva Segovia; originally pine covered, but now used for pasture. A single pine stump is in the foreground, while pines line the ridge in the background

and Spanish inhabitants during the colonial period because of attacking Indians and English pirates from the east. During the past hundred plus years, revolutions and bandit uprisings have led to further abandonment. East of the Río Cua and between Yalí and the Río Coco, buried pine stumps and logs are found in the midst of tall secondary broadleaf forest. These seem to be explained only by the fact that burning ceased when settlers were gone. The extent of such replacement cannot even be guessed at. Replacement of pine is also indicated by occasional individual mature pines unaccompanied by seedlings in mixed broadleaf forests on Finca La Trampa east of Venecia in Jinotega and in the mountains south of the town of Matagalpa. In both instances the pines are several mi from the nearest pure stand of pines.

A review of the settlement history of the Segovias shows that the frontier line has not been stable, but until recently has failed to advance significantly without subsequent retreat. The eastern limit of upland pine forest may likewise have fluctuated, but pines have failed to maintain themselves where people have not maintained strong pressure on the broadleaf vegetation by clearing and burning.

The History of Settlement

The present settlement frontier in northern Nicaragua, as represented by the easternmost highland towns, does not differ greatly from that established by Spanish gold seekers in the sixteenth century (Fig. 3.11), which, with the exception of the town of Nueva Segovia,[11] was based on already well-established Indian villages. These

[11] The original site of the town of Nueva Segovia, which is now the ruins of Ciudad Vieja, was the first permanent Spanish settlement in the Nicaraguan highlands. It is believed to have been founded in 1536 by Rodrigo de Contreras, the third governor of Nicaragua (Guillén de Herrera 1945,

villages include the original sites of Totecacinte, Poteca, Jalapa, El Jícaro, Telpaneca, Jinotega, and Matagalpa (Cuadra Cea 1957). The early Spanish accounts mention no large permanent Indian villages east of these, all of which are located near the present eastern margin of the pine forests. There were, however, numerous roving river and forest tribes (Sumo and Miskito) farther east.

Very little is known about the nature and background of the Indians who occupied the pine uplands at the time of the Spanish Conquest. The Matagalpas were the main tribe; however, they had been overrun by one of the several waves of Nahuatl peoples from Mexico who swept into western Nicaragua between the twelfth and sixteenth centuries, introducing their language but otherwise adopting much of the existing culture. Many of the place names in the pine uplands, including all those on the frontier listed above, are of Nahuatl origin, while most of the others are Matagalpan (e.g., Estelí, Güigüilí, Dipilto, and Yalagüina).

At the time of the Conquest, the Matagalpas were a sedentary people with a subsistence economy based on maize farming, as were most of the tribes of western Nicaragua. The Matagalpas lived in large villages in the river valleys or on river terraces where they apparently cultivated alluvial soils but also relied strongly on slash-and-burn *milpa* farms on the surrounding mountain slopes. The same pattern is still followed by the modern Lenca Indians of Honduras, the Lencas having originally occupied much of the pine lands of central Honduras. Doris Stone (1948, 205), in describing the Lencas, wrote: "The male members of a family generally spend five to six days a week in a straw hut built near the field. They return bringing food to the village at the end of a week." Such a system made permanent settlement possible, but the surrounding forests, once cleared, were given little opportunity to reestablish themselves because of burning and frequent reclearing.

The Spaniards apparently experienced little difficulty in establishing control over the area occupied by the Matagalpa Indians. The hostility later encountered came from the eastern forest tribes, which are of the Sumo-Miskito language stock. Almost all of the place names east of the Spanish frontier are Sumo-Miskito.[12] These people were migratory hunters, gatherers, and fishermen. Although they did grow such crops as manioc and plantains, cultivation was (and still is) a less important part of their subsistence economy than it was among the tribes of western Nicaragua. The Indians of eastern Nicaragua seem to have always confined their activities, including farming, to the low valleys and consequently must have had little effect on the vegetation of the high eastern cordilleras. Archaeological investigations, of course, may someday reveal a pre-European people who inhabited the mountains of Isabella and Darién.

36–38). As the major non-Indian Spanish settlement in the Segovias, the town of Nueva Segovia remained the center of Spanish gold-mining activities throughout the colonial period and was long the third city of Nicaragua after León and Granada. Lévy (1873, 164) wrote: "La Segovia fué, durante mucho tiempo, la parte más rica, populosa y lujosa del país."

[12] The numerous Sumo-Miskito place names that are found near the margins of the original Spanish frontier include Bocay, Matiguás, Pantasma, and Murra. East of the Spanish frontier line, there are only a few Matagalpan names, including Munsún and Güina (a tributary of the Río Bocay). See Alfonso Valle (1944) for translations and origins of the Indian place names of Nicaragua.

Thus, the Spaniards occupied that part of the Nicaraguan highlands controlled by permanently settled farming Indians. This was the general pattern of the Spanish Conquest throughout Latin America; rain forest areas with wandering tribes received little lasting attention. The Spaniards made almost no concentrated effort to penetrate farther into the interior of Nicaragua, partly because no gold was known east of the pine uplands.[13] They were also discouraged by the lack of rich soils such as are found on the Pacific coast of Nicaragua and in the highlands of Costa Rica; by the denseness of the tropical rain forest, which makes movement much harder than in the pine forests; and by the extreme hostility of the independent eastern Indian tribes.

Soon after occupying the Segovias, the Spaniards found native opposition so intense that several times the frontier settlers and their conquered Indian slaves were forced to flee. In some instances the entire villages were abandoned or transferred westward to more secure sites. The main early encounters seem to have been with Honduran tribes, the Lenca, Jicaque, and Paya, who frequently attacked the Spanish outposts in Poteca, Totecacinte, Jalapa, and the large town of Nueva Segovia. In 1611 Nueva Segovia, the regional capital city, had to be moved from its original site at the junction of the Río Jícaro and Río Coco (Ciudad Vieja) to what is now Ciudad Antigua, and in 1617 Poteca was abandoned when its inhabitants fled to the Llanos de Totecacinte (Guillén de Herrera 1945, 113–115).

Possibly even more feared than the Indians were the Dutch, French, and English pirates who frequently raided the cities and fleets of Spanish Middle America during the seventeenth and eighteenth centuries. Hearing of the wealth of Nueva Segovia, buccaneers sought out the town from both the Atlantic and Pacific coasts. Nueva Segovia and Matagalpa were both sacked in 1654, and in 1688 Nueva Segovia was completely destroyed (Guillén de Herrera 1945, 170–174). In the eighteenth century, the raids on the Segovias took on a new character as the British government sought to undermine Spanish control of Nicaragua. Miskito Indians and Zambos (mixed Miskito and Black), allied with the British and often led by British officers, repeatedly moved up the Río Coco and Río Grande de Matagalpa to harass the highland Spanish settlements. The towns of Nueva Segovia, Telpaneca, Matagalpa, Jinotega, Sebaco, and Muy Muy were attacked many times between 1704 and 1781. The gold camp of Muy Muy (the present site of Muy Muy Viejo) east of Matagalpa was forced to retreat to its modern site in the valley of the Río Grande following a Miskito attack in 1749. In 1789 the town of Nueva Segovia was again moved, this time from what is now the site of Ciudad Antigua to the present site of Ocotal (Guillén de Herrera 1945, 175–183). Miskito raids ceased in the nineteenth century, but then the Matagalpa Indians turned against their masters. There were major outbreaks in Matagalpa and Jinotega in 1824, 1827, 1844, and 1881 (Gutierrez Castro 1954, 10).

[13] The very productive Bonanza and La Luz gold-mining districts of eastern Nicaragua, discovered by rubber gatherers in the 1880s, were apparently not known by either the Indians or the Spaniards (Parsons 1955a, 51). Spanish gold mining in Nicaragua was largely confined to river placers in Nueva Segovia, and it was mainly after independence (1821) that a large number of hard-rock mines were opened up in the Segovias and in Chontales.

The colonial period, then, was one in which the settlement frontier did not advance, but retreated, as is best evidenced by the two westward transfers of the city of Nueva Segovia. Bishop Morel de Santa Cruz, who visited and described the Segovias in 1752, found that Totecacinte had also been abandoned by that time, leaving Jalapa, El Jícaro, Nueva Segovia (then at Ciudad Antigua), Telpaneca, Yalagüina, Palacagüina, Condega, Estelí, Jinotega, Matagalpa, San Ramón, and Muy Muy as the outposts of Spanish settlement. He mentioned that despite armed garrisons in most of these towns, the people lived in constant fear of the frequently attacking Miskito Indians.[14]

The approximate retreat of the Spanish frontier in the Segovias from 1603 to 1752 is shown in Fig. 3.11. It can be assumed that most of the Christianized Indians retreated with the Spaniards; moreover, it is certain that all of the native population of the Segovias was greatly diminished under Spanish rule, as it was elsewhere in Nicaragua. The effects that this retreat of settlement and native depopulation had on vegetation can only be surmised. Much of the originally well-populated land east of Jalapa and El Jícaro, some of which is now in pine, could well have returned to tall broadleaf forest. Behind the frontier, slash-and-burn agriculture undoubtedly decreased appreciably, but vegetation continued to be under attack because of the establishment of large herds of livestock. The Nicaraguan historian Salvatierra (1939, II:209) wrote that Nicaragua was the leading cattle-raising country of Central America during the seventeenth and eighteenth centuries, thanks to suitable "condiciones naturales" including "grandes pampas en Chontales y Segovia." The grasslands necessary for stock raising must have been in existence when the Spanish arrived, but they could well have been created by the slash-and-burn agriculture of pre-Conquest native people. Nevertheless, the Spanish ranchers maintained and improved pasture by intentional burning, much as the country people do today. With large herds of cattle, it is possible that burning became more regular than it had been among the Indians, even though the human population had greatly declined. The pine uplands having become range land subject to burning, there is no reason to believe that pines would have been replaced by broadleaf forest during the colonial period within the area of Spanish settlement. Burning by the Spaniards may either have promoted the extension of the upland pines or have seriously retarded the reproduction of pine, as regular burning often does today. Only speculation is possible, because almost no descriptions of the Segovias under Spanish rule are available.

Since independence was gained from Spain in 1821, general national disorganization and an almost continuous series of revolutions have hindered new settlement in the interior of Nicaragua. The eastern tribes remained troublesome, and the Nicaraguan armies were too occupied elsewhere to attempt to pacify them. However, the discovery and exploitation of gold and silver veins in the Segovias, mainly by North Americans, led to the founding or reestablishment of several towns, including

[14] According to Morel de Santa Cruz, as summarized by Ayon (1956, II, 389–398), the other important settlements in the Segovias in 1752 were Sebaco, Somoto, Totogalpa, and Mozonte. Morel found only a few settlements in the southern highlands of Nicaragua. He noted that at least six former towns in Chontales had been abandoned by the time of his visit.

Macuelizo (1801), Dipilto (1840), Santa Maria (1849), Quilalí (1880), and Murra (1900), all in the Department of Nueva Segovia. Then, toward the end of the nineteenth century, political and economic events sent thousands of Nicaraguans into the central highlands and eastern rain forests. The first economic incentive was the demand for rubber, and the *Castilla elastica* tree of eastern Nicaragua soon became the leading source of rubber north of the Amazon Basin.

More recently, coffee has been the main cause of the expansion of frontier settlement in the Segovian highlands. In the 1890s North American, German, and English colonists planted millions of coffee trees in the mountains just east of Matagalpa and Jinotega. Penetration any farther into the interior from these towns was long hindered by the almost complete lack of transport facilities and roads, but motivated by high coffee prices and the successes of the European- and American-owned fincas, hundreds of small farmers pushed eastward from the long-established frontier settlements. This pioneer thrust, the first of significance since the Spanish Conquest, was mainly confined to the period of peaceful and progressive rule by the dictator José Santos Zelaya between 1893 and 1911. Several new towns were founded, including Güigüilí, Yalí, San Fernando, and Matiguás, while Telpaneca, Muy Muy, and San Ramón were revived by the coffee boom. After Zelaya was ousted, however, Nicaragua found itself in a revolutionary turmoil that lasted until 1934 and at times involved the United States Marines. Most of the peasants in the country were conscripted into the armies, with the result that small farms were abandoned and the coffee crops of the large *fincas* went unpicked. The Segovias suffered especially from its own infamous rebel leader, Sandino, who turned against all Segovians who did not come to his aid. He killed hundreds of people and destroyed dozens of farms and small villages. The frontier coffee lands were almost totally evacuated as the settlers fled to Ocotal, Jinotega, Matagalpa, and even Managua. In a few years' time, thousands of coffee trees and extensive pasture lands were swallowed up by tropical forest.

Once stability had been returned to Nicaragua under the regime of Anastasio Somoza (1933–1956), people began filtering back to the frontier area. The lack of a program of road construction, together with World War II, hampered new settlement, however, and only in the late 1940s did a major modern pioneer push get under way. The stimulus again was rising coffee prices, but drought and shortage of land in the heavily populated valleys of Estelí and Sebaco also helped encourage thousands of new settlers to move into the Cordillera de Darién east of Matiguás, the Cordillera Isabella northeast of Jinotega, and the Mt. Kilambé area (Figs. 3.4 and 3.11). New settlements are as yet only beginning to form, but previous outposts such as Quilalí, Güigüilí, Yalí, and Matiguás (Fig. 3.11) are now boom towns where, during the harvest seasons, long mule trains loaded with sacks of coffee meet convoys of supply trucks. Since about 1950 the settlement frontier has been pushed eastward more persistently than at any other time during the past 400 years.

The eastern limits of the permanently settled part of northwestern Nicaragua, then, have changed little since the Spanish Conquest. Between 1603 and 1752, the frontier actually retreated, and only in the last 70 years, and particularly in the last 10, has it advanced beyond that initially established by the Spaniards and by the Matagalpa Indians before them. The eastern limit of pines corresponds fairly closely with the eastern limit of relatively dense settlement up to the present century. Pine forest is found today only where human activities have brought about a fairly continuous disturbance of the vegetation. It is possible that the eastern limits of upland pines have fluctuated with the frontier. The pine trunks, stumps, and roots in the tall tropical forest of the Río Cua and Yalí areas prove that former pine lands have been invaded and the pines replaced by broadleaf species on at least a few sites. Such areas may have been abandoned by settlers and occupied by tropical forest during the recent revolutionary period or possibly earlier.

The Southern Limit of Upland Pines

The failure of the genus *Pinus* to extend its natural range south of Nicaragua has generally been attributed to the marine breaks between Panama and Nicaragua and in southern Nicaragua (the San Juan Depression); the former apparently existed until the Late Cretaceous and the latter from the late Eocene to the late Miocene, both acting as barriers to the spread of South American plants northward and North American plants southward. Both marine breaks, however, were bridged by many plants, largely by means of temporary land connections and by permanent connection since the early Pliocene (Schuchert 1935).

The barrier to the further southward spread of the southernmost North American conifers seems to be largely climatic, based on the absence of high mountains with low temperatures. Between central Honduras and northern Costa Rica, there is a distance of approximately 300 mi with no elevations above the 6560 ft Mt. Saslaya in the Cordillera Isabella in Nicaragua. The frost line in Central America being between 6000 and 7000 ft, a large area for the most part below the frost line could well retard further migration of many northern cold-climate species,[15] including most but not all pines. Several North American conifers have their southern limit in the high mountains (up to 9300 ft elevation) around Lake Yojoa in west-central Honduras; this is apparently the limit of *Abies guatemalensis*, *Cupressus lindleyi*, *Taxus globosa*, *Pinus ayacahuite*, and *Pinus montezumae* (Allen 1955). Certain pines have been able to migrate farther south than other conifers partly because of their adaptation to the high temperatures of low elevations.

If edaphic conditions and natural fires alone were sufficient to open the way for the migration of pines into tropical and subtropical habitats of Central America, then there seems to have been enough time since the last sea barrier was closed during the Pliocene for pines to spread into southern Nicaragua, Costa Rica, and even farther south. On the other hand, if clearing of broadleaf forest and human fires are responsible for the extensive spread of pines into Nicaragua from the high mountains of northern Central America, then the period that people have been on the

[15] The absence of high mountains in Nicaragua and southern Honduras also seems to have been a barrier to the northward spread of tropical American mountain plants, many of which have their northern limit in the Costa Rican highlands (Lauer 1954, 344).

scene, possibly 30,000 or 40,000 years, might be short enough to explain at least partly why pines migrated no farther south than they have.

The actual southern termination of upland pines in central Nicaragua can be attributed to a number of factors, none of which, however, seem adequate to form a permanent barrier to expansion. Parent materials and soils are very important. The parent materials in the southern mountains of Nicaragua are mainly basalts, with a minimum of the acidic rocks that form the soils occupied by most of the Nicaraguan pines.[16] The larger areas of acidic parent materials in the southern highlands, which would be most receptive to invasion by pine and could thereafter serve as centers for further dispersal, as do the granitic rocks of Segovia, seem to be mainly at elevations lower than the observed range of *P. oocarpa*.

Climate is a factor. The valley of the Río Grande de Matagalpa probably forms an altitudinal or temperature obstacle to *P. oocarpa* (Fig. 3.4). South of the pine uplands, this valley is 10–15 mi wide, with an elevation of 1000–1700 ft; temperatures are probably too high for *P. oocarpa* and *P. pseudostrobus*, while the tropical *P. caribaea* is not found anywhere in the southern region of the upland pines. A leap over the valley to the mountains of Chontales by *P. oocarpa*, whose seeds can be carried by the wind, might be possible, however, if there were suitable soils or the other side.

New invasions by pine, even when competing species are suppressed and climate and soils are suitable, have been further prevented in recent times, or at least hindered, by excessive burning by cattlemen, as well as by heavy grazing and the wide distribution of tall introduced grasses. These are all hazards to the survival of young pine seedlings.

Climate, elevation, and parent materials do not establish a permanent southern limit for pine in Central America, but together they may have been able to at least temporarily stop the southward march of the upland pines. This barrier is strengthened by present human activities, which make it very difficult for pines to advance farther south, even on a small scale, by natural seeding. It is interesting to note, however, that pines have been planted successfully on the volcanic plateau of Masaya in southern Nicaragua, in Costa Rica, and in various parts of South America.

The southernmost stands of pines in the upland pine area of Nicaragua are widely scattered but form two southward-pointed prongs separated by the low plains of Sebaco (elevation 1700 ft); one ends west of Sebaco and the other southeast of Matagalpa. Figure 3.10 shows two solitary pines overlooking the valley of the Río Grande de Matagalpa. These trees are surrounded by grasslands on which there are no young pine trees or seedlings, apparently because of the severity of recurrent fires. I saw no young pine trees anywhere along the margins of the southern stands, and the local people say that these stands have existed with little areal change for as long as they can remember. The pine forests east and southeast of Matagalpa descend to about 2500 ft elevation on moderate slopes with thin soils derived mainly from andesitic parent materials. On Cerro Padre, a steep pinnacle rock south of

[16] In Mexico and northern Central America, pines generally occur on parent materials (granite, schist, and rhyolite) similar to those in the pine uplands of Nicaragua. Pines also grow on sandstone, on serpentine, and possibly on basalt in the Sierra Occidental of Mexico. *P. caribaea* occurs on limestone in some areas, especially in the Caribbean islands.

Matagalpa and a few mi northeast of the village of San Dionisio, there is a patch of mature pines that constitutes the southernmost stand of pines known in the highland area of Nicaragua (12° 45′ N.).

The southernmost stands of *P. oocarpa* do not occur in the pine uplands, however, but slightly farther south, some 30 mi west of Cerro Padre, on the upper slopes of the adjacent volcanoes San Cristóbal (or El Viejo) (elevation 5545 ft) and Casita (elevation 5085 ft) (Fig. 3.4); Casita is the farther south (12° 44′ N, 86° 59′ W). These pines were observed by Taylor (personal communication 1958), who commented:

> I have climbed Volcán Casita which is immediately south of San Cristóbal to see the pines. These stands are very small but are somewhat bigger than those on San Cristóbal. They are formed on the typical soils of the volcano, i.e., shallow but fertile silty loam. I suspect that they entered since the last eruption and that the successions were never permitted to mature but were burnt regularly for grazing thus permitting the pines to persist… The trees concerned are young, mostly less than 60 ft [in height] and 10 in. in diameter, and the stands only cover a few ha each; elevation is from 2,000 to 4,000 ft but could be higher on San Cristóbal.

Pines do not grow on any of the other high volcanoes of western Nicaragua, although they do grow on volcanoes in Guatemala (e.g., Volcán Fuego). On San Cristóbal and Casita, the pines may have been planted; and if not, then they indicate how pine seeds can be carried long distances by the wind or by biotic carriers to seed new areas where conditions are suitable for pines. The prevailing winds are from the northeast and therefore from the pine uplands. In general, however, wind direction probably has had little bearing on the distribution of pines in the Nicaraguan highlands.

From time to time, there have been rumors that there are pines in the Departments of Boaco and Chontales in southern Nicaragua, but such rumors have never been verified. I searched without success for pines in the mountains of Boaco and Chontales, and Taylor reports (personal communication 1958) that he could find none during several weeks of plant collecting in the same departments. A plant called *pino* (unidentified) is common in the hills on the southeast side of Lake Nicaragua, and "pino" means pine in Spanish, which may explain the rumors of pines in southern Nicaragua. The Nicaraguans commonly use the Nahuatl Indian term "ocote" for the true pines of the Segovias.

The southernmost pines in the Western Hemisphere, which occur not in the Nicaraguan highlands but on lowland savannas in eastern Nicaragua, where stands of *P. caribaea* extend to 12° 10′ N near Bluefields, have a distribution and southern termination closely related to the presence of quartz gravel sediments in eastern Nicaragua; these sediments would probably support a low hardwood forest, however, if it were not for annual burning (Taylor 1959a, 80).

Conclusions

The thesis presented here is that the upland pine forests of Nicaragua are a deflected climax, a formation resulting from human clearing of broadleaf forest and repeated burning, and it is unlikely that extensive and pure stands of pines existed before the appearance of Indians. The evidence contained in this paper, which is supported by the literature on many of the pine forests elsewhere in the tropics, suggests that, in general, the degree of disturbance of site and frequency of fire necessary to establish and sustain a tropical or subtropical stand of shade-intolerant pine is greatest in areas with rich and deep soils, basic parent materials, and high average temperatures and high precipitation and is less in areas with poor soils, acidic parent materials, low rainfall, and especially increasingly lower temperatures. Because people are the main disturbing element in the environment, the presence of pines on a given site can in part be related to the extent and nature of their activities on the site.

There is little doubt that the present upland pines of Nicaragua germinated and matured while being subjected to periodic fires. The question remains whether or not similar pine forests could have existed before the coming of humans. Restrictive edaphic conditions may well have originally provided habitats for pines which served as centers of dispersal to receptive sites provided by such natural events as blowdowns, landslides, lightning fires, and volcanic eruptions. Natural habitats were probably only local, however, and natural disturbances do not seem to occur frequently enough on any given site in highland Nicaragua to suppress mixed broadleaf forest continuously.

Of significance is the occurrence of the upland pines only in areas of long-continued, permanent settlement. There are no pines in the central mountains of Nicaragua east of the settlement frontier, although parent materials and climate often appear to be suitable. On the other hand, neither are there pines in the settled part of the southern uplands of Nicaragua, where the history of clearing and burning is similar to that of the pine uplands, but where parent materials generally are better suited to the growth of broadleaf forest than pine.

The conclusions for the upland pine forests of Nicaragua seem to be relevant to the problem of the occurrence and distribution of pines elsewhere in the tropics and subtropics. Many, if not most, of the pine forests of Middle America do occur on acidic parent materials and thin impoverished soils and are subjected to at least occasional fires. There is a danger, however, in attempting to apply ecological interpretations in one region to others, especially with reference to pines. Not only may climatic, edaphic, biotic, and human factors differ in other areas, but pine species differ widely in their requirements and tolerances. Individual studies of the various tropical pine species in different environments are needed in order to understand more fully the distribution of the genus *Pinus* in the tropical world and people's relation to that distribution.

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3.2 Fields of the Mojo, Campa, Shipibo, and Karinya

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Abstract Presented here are four case studies of indigenous cultivation in Greater Amazonia based on Denevan's own field work, plus information from others, and the use of historical material. They provide examples of different types of agricultural systems: relatively intensive swidden or shifting cultivation (colonial Mojos); extensive swidden (Campa); floodplain cultivation (Shipibo); and multiple biome use (Karinya). A fifth type, managed fallow (Bora), included in the original but removed here to avoid duplication, is examined in Chapter 6.2.

Keywords Campa · Floodplain cultivation · Indigenous cultivation · Karinya · Mojo · Shifting cultivation · Shipibo · Swidden

The Mojo: A Colonial Reconstruction

For the colonial period, seldom is there enough information available to reconstruct agriculture for a specific tribe or region. Following are some early descriptions of forest cultivation in the Jesuit Province of Mojos in northern Bolivia (Fig. 3.16) (Denevan 1996), a region better known for pre-European drained, raised fields in seasonally flooded savanna. Shifting cultivation was and continues to be practiced in gallery forest and in forest islands within savanna, both habitats tending to have semi-deciduous forest relatively easily cleared with stone axes. The main Indians involved were the Mojo and the (related) Baure chiefdoms.

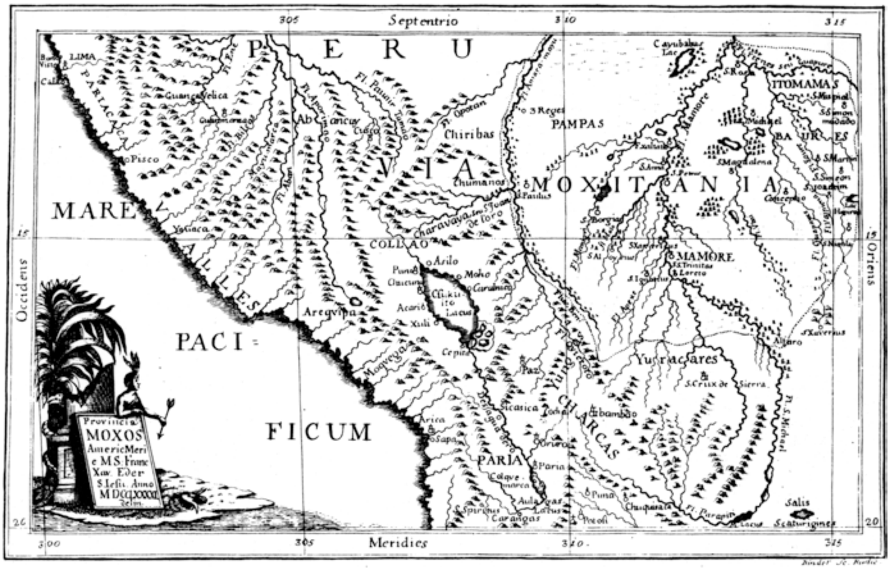

Fig. 3.16 Map of colonial Peru c. 1767 showing Mojos (Moxitania). From Eder (1985, 428)

Swidden (shifting cultivation) fields were called *chacras* (or *chácaras*) during the colonial period and are now referred to as *chacos*. There are many early references to forest cultivation but few details. Several of the descriptions of Mojos by members of the Solís Holguín expedition in 1617 mention fields: "*chácaras* in carefully cleared *montaña*" and "great *chácaras* of maize and other vegetables" (de Lizarazu 1906, 149, 195). The Jesuit Diego Altamirano (1891, 30) wrote in 1710 that the Mojo first felled the trees and rooted out the useless weeds and grasses. Coronel Aymerich, Governor of Mojos from 1768 to 1772, wrote that because of flooding, crops were grown in the forests on *alturas* (high ground) next to the rivers (gallery forests on natural levees), where transport was easy to villages that were sometimes 2 or 3 days away (René-Moreno 1888, 68). Padre Francisco Eder (1985, 75), in 1791, said that cultivation of the *campos* (grasslands) was impeded by flooding but that they were not inferior to the forests because the ashes of burned grasses provided good fertility.

The process of clearing forests for fields in Jesuit Mojos has been described by Métraux (1942, 59, based on Eder 1985, 73–74):

> The Mojo and Baure cleared fields in the forests, which were not flooded during the rainy season. At the end of August they first destroyed the underbrush, then cut the base of large trees by alternately charring and hammering the wood with stone axes. They waited until a strong wind blew down the undermined trees, or else felled selected trees, which knocked down all the others. The dry trunks were burned and their charred remains left on the field to protect young maize stalks. The Spaniards who penetrated the country with Solís Holguín were amazed at the size of the Mojo fields, which were crossed by wide roads.

The Mojo and Baure used digging sticks for planting, and presumably so did the other tribes in Mojos. Stone axes were used by the Mojo to clear forests. Alonzo Soleto Pernia, a member of the Solís Holguín expedition, said that "we found trees cut, as if by stone axes; they have mines where they obtain stone for axes for cutting trees, and their edges are like iron" (de Lizarazu 1906, 211). Rock outcrops may not have been far from the area visited by the expedition; however, in the central savannas, most stone tools were obtained in distant trade and were a valuable commodity. Possibly in much of Mojos, there were not enough axes to clear sufficient forest to support large numbers of people. The Mojo had cutting and sawing tools made from bone, teeth, and chonta palm wood, but these would not have been very effective for clearing mature forest. The change to metal tools undoubtedly had revolutionary effects for shifting cultivation. By 1676, and probably much earlier, Mojo Indians were traveling south to Santa Cruz to trade cotton goods for "machetes to cut and clear their *chacras* [fields]" (Marbán 1898, 148). The Jesuits used axes and machetes as major gift items in gaining the friendship of the native people.

In the early seventeenth century, the Indians of southeastern Mojos had large farms producing great quantities of food. These must have been forest fields, assuming that most raised fields in the savannas had been abandoned by that time. This is indicated by the reports of the members of the Solís Holguín expedition. The relation of Juan de Limpias (de Lizarazu 1906, 170) states that a Captain Diego Hernández Vexarano [Verarano] saw a large number of *percheles* of maize and other legumes and told Juan and another soldier to count them. Juan de Limpias counted over 700 *percheles* of maize in 1 group which formed one of the granaries (de Lizarazu 1906, 158).

Métraux (1942, 59) translated *percheles* from these accounts as "probably the forked sticks used to support maize." This usage is apparently based on one of the meanings of *percha* as perch or pole and *perchonar* meaning to leave shoots on a vine stock. However, it would be impossible for a single "forked stick" to support a harvest of 30–45 bushels of maize. A unit of land is a possibility, but most likely *perchel* refers to a maize crib on pilings for protection against flooding and animals (the term has been so used in Portugal). Such cribs were still being built by the Chácobo in Mojos when visited by Nordenskiöld (1920, 3) in the early twentieth century. Seven hundred *percheles* each holding 30–45 bushels of corn would total 21,000–31,500 bushels – a sizable amount for what was presumably 1 village.

Padre Joseph Castillo (1906, 309–310), in 1671, listed the following Indian crops: manioc, maize, beans, squash, sweet potato, peanuts, papaya, chili pepper, cotton, arracacha, tobacco, and plantain. Most of these were also listed in the reports of the earlier Solis Holguín expedition in 1617. The Old World plantain was apparently pre-Jesuit, being cultivated in 1677 according to Castillo. The staple seems to have been manioc. Padre Pedro Marbán (1898, 139) wrote that "yuca [sweet manioc] is the common bread of the land," and Padre Antonio de Orellana (1906, 13) in 1687 said that "yuca is their principal food." On the other hand, the numerous maize *percheles* suggest that maize was at least locally important. In the reports of the Holguín expedition, maize is mentioned ten times and manioc only once. But Marbán (1898, 138) later said that "there is not much maize because these Indians do not use it for *chicha* [maize beer], except once in a while."

The Jesuits introduced rice, and they said that rice produced as many as five crops a year and that harvesting was done in canoes (Eder 1985, 75–76). This indicates that the Jesuits were growing wet rice on the savannas; however, the Indians may also have been harvesting a wild rice.

The colonial fields contained both indigenous and Jesuit techniques and crops. Raised fields in the wet savannas were largely abandoned. Land use today is quite different, with subsistence fields being smaller. Steel axes and machetes are now used for clearing and weeding. *Percheles* no longer exist. Manioc is the dominant crop, and maize and rice are rare. The savannas are used for livestock. Food is imported.

Thus, it has been possible to extract considerable information on Mojo forest agriculture from sixteenth- and seventeenth-century exploration and missionary accounts. However, our information on the Mojo is from different places and times and may pertain in part to tribes other than the Mojo. And we are told little or nothing about field types, cropping/fallowing patterns, and agroecological techniques. For most other Indian groups in Amazonia, even less early information is available, and we have to rely on twentieth-century reports long after initial contact, or on what can be learned from archaeology.

The Campa

Unstable Swiddens

> I wouldn't want to live in a world without lions. These people [Campa] are like lions. (Herzog 1982)

The Campa (Asháninka) live in the eastern foothills of the central Andres of Peru. They number at least 30,000 and possibly 45,000, making them the largest indigenous group today in Amazonia (Hvalkof 1989, 128).[17] In the 1960s I made a field study of Campa agriculture in the Gran Pajonal, a dissected plateau with scattered patches of savanna (Fig. 3.17) (Denevan 1971).[18] These Campa are best described as extensive swidden farmers with a strong emphasis on hunting and minimal gathering and fishing. While males spend most of their working time hunting, most household food by weight and calories comes from cultivated plants. Manioc is the staple, but each field is initially planted in intermixed manioc and maize (Fig. 3.18).[19]

Fields are usually cropped for only 2 years, with a fallow of 10 years or more. In a typical year, a new field is cleared and planted, the crop in the previous year's

[17] It has been incorrectly claimed that the Yanomami, with about 20,000 people (Chagnon 1992, 1), is the largest Indian group today in Amazonia.

[18] Studies also have been made of the agriculture of the nearby and linguistically related Machiguenga (Johnson 1983) and Amuesha (Salick 1989; Salick and Lundberg 1990).

[19] A minor crop of interest is the Amazonian potato, *Solanum hygrothermicum*, which I encountered in a few Campa villages at elevations of around 1200 m (Ochoa 1984).

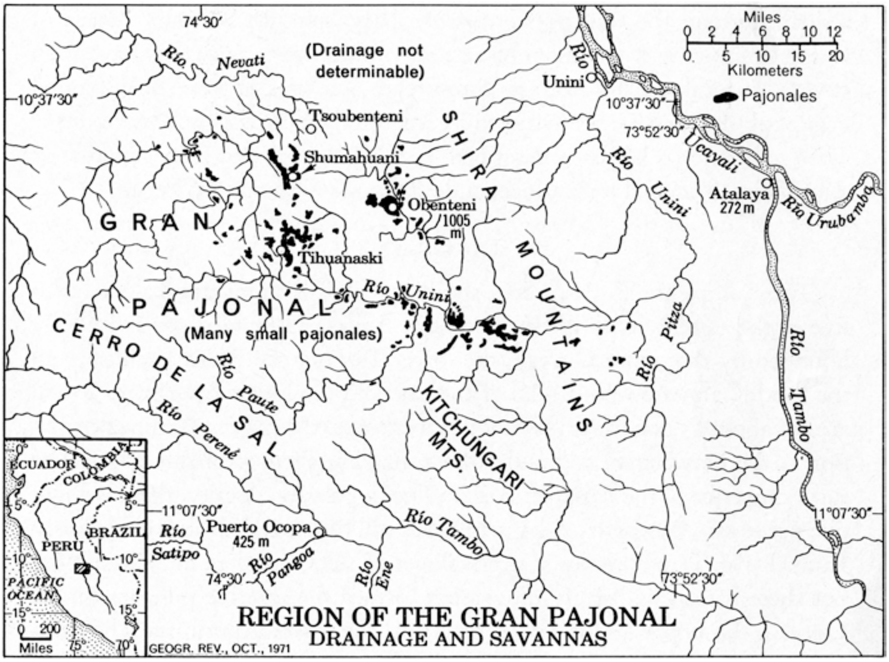

Fig. 3.17 Map of Gran Pajonal, Peruvian Amazonia. (From Denevan 1971, 497)

Fig. 3.18 Campa boy planting maize with a digging stick on a recently burned swidden, Peruvian Amazonia. (From Denevan 1971, 506)

clearing is harvested, and residual crops in a 2-year-old field are also harvested. There is relatively little management and use of either swidden-fallow fruit trees or house gardens, given frequent movement. This contrasts with the Río Pachitea Campa to the north, who have longer cropping and shorter fallows (Allen and de Tizón 1973, 143), and the Bora, who have managed fallows (see section "Indigenous Agroforestry in the Peruvian Amazon: Bora Indian Management of Swidden Fallows" in Chap. 6).

I was impressed that many Campa are not systematic land-use planners. Clearing and burning can take place well into the wet season, with a poor burn resulting, leaving little fertilizing ash and much debris. Planting is done immediately after burning, regardless of the season, even weeks before the rainy season begins. Resulting yields, therefore, may be poor for most crops, although manioc does fairly well. The Campa are certainly aware of the ideal timing of clearing, burning, and harvesting, but other factors intervene.

A field that is still producing may be abandoned prematurely because of a house shift so that a family is located too far away to be able to utilize an old field. Relatives may move in and help to deplete a field. Because of bad weather, an extended trading or fishing trip, or sickness or an injury, a farmer may not clear a field every year. As a result, some families have plenty of food and their future needs are assured, whereas others are caught short with a field depleted before another comes into production. Then they either move in with relatives or live off the land, hunting and gathering, until the new field is producing. Such instability of production seems to be common in the Gran Pajonal and helps explain why the Campa frequently clear, burn, and plant out of season. They fully realize that they will not get a good crop, but a poor crop planted in the wrong season is better than no crop at all. Such seasonal and managerial irregularity in swidden systems may be more widespread than we realize (Brookfield 1968, 421; Salick and Lundberg 1990).

A new field may be adjacent to a current one or as much as tens of kilometers away. Given plentiful land, the reason for distant moves of houses and fields can possibly be explained (not always) by game depletion rather than by soil depletion, pest invasion, or social factors, since game is the primary source of protein. Game is rapidly hunted out or scared away around a settlement, hence the frequent moving to new locations. This so-called protein thesis of Amazonian settlement is discussed below.

Seminomadic tribes with a strong emphasis on hunting, such as the Campa, are often thought of as being incipient or primitive agriculturalists. However, the large number of crops cultivated[20] and the many varieties of each, plus the utilization of a wide range of microecological conditions, suggest considerable agricultural sophistication, more so than that of the average nonindigenous colonist in tropical Peru. The strong emphasis on manioc cultivation is misleading. It is a labor-saving way to provide calories, while the Campas' greater physical efforts are directed toward hunting for the more basic food element, protein. Although a labor/time study was

[20] The Gran Pajonal Campa cultivate at least 49 crops; however, fields are dominated by manioc.

not made, agricultural productivity seems to be a high in terms of time expended, whereas hunting productivity is low for time expended.

The Protein Thesis

It has been postulated that the greater population densities and more developed cultures have been located along the large Amazonian rivers, in contrast with sparsely populated interfluves, because of the availability of protein-rich aquatic resources which supplement the protein-poor root crops that dominate the diet of Amazonian people (Lathrap 1962, 547–549; 1968; Carneiro 1970; Denevan 1966; Gross 1975). Away from the rivers, game is the main source of protein, but it does not seem to exist in quantities large enough to support large social units, nor is it permanent enough to support long-enduring settlements. Hence, the aboriginal pattern in the upland forests is usually one of small seminomadic social groups with a limited material culture.

One of my purposes in studying the Gran Pajonal Campa was to test the above argument with a brief dietary study (Denevan 1971). These Campa clearly fit the non-riverine model. Their total protein intake is low (less than 50 g most days), and they seem to go to great efforts in hunting, with small returns, to stay above the minimum. The result is settlement instability. The Gran Pajonal Campa move about once every 2 years, to average distances of about 8 km.[21] This is not to say that other factors besides game depletion are not important, and a careful sociological study is needed to define these factors, their causes, and their relative significance compared to the dietary argument. In contrast with the Gran Pajonal Campa, the riverine Campa are much less nomadic.

Certainly diets can change, and a greater use of protein-rich maize and beans would reduce the need for animal protein.[22] The emphasis on protein-poor root crops in Amazonia is rational given their great productivity and ease of ground storage, so long as protein-rich fish and game are available. It is interesting to note that the missionaries have been able to establish much larger and more permanent Campa villages along the lowland rivers. On the other hand, the Seventh Day Adventists prohibit meat eating, or at least restrict the varieties of game and fish that the Campa in their missions can eat. As a result, severe nutritional problems arose in the past at some missions, as on the Perené (Paz Soldán and Kuczynski-Godard 1939); elsewhere, the Campa have often ignored the restrictions. The Adventists have made major efforts to shift the Campa diet from traditional manioc toward maize, beans, peanuts, and other crops with a relatively high protein content.

[21] Bodley (1970, 36) documented four long-distance shifts of houses of from 20 to 80 km; in each case the reason given for moving was depleted game or fish resources.

[22] However, the Amahuaca of eastern Peru, one of the few Indian groups in the Amazon Basin for whom maize is the staple rather than root crops, are still seminomadic despite the apparently relatively high protein content of their vegetable diet (Carneiro 1964).

Robert Carneiro (1960) and others have shown that quite substantial settlements and population densities can be supported by indigenous shifting cultivation in Amazonia. On the other hand, if shifting cultivation does not supply sufficient protein, the availability of unevenly distributed animal protein becomes a limiting factor. Such a limitation, apparently applicable to the Campa, must be viewed as culturally determined in so far as the dietary pattern responsible for it is culturally determined.

The Campa obtain at least 90% of their food, by weight eaten, from agriculture and at most 6% from hunting, 3% from gathering, and 1% from fishing. The evidence for the Campa, though not precise, indicates that a group consistently thought of as being hunting-oriented actually may obtain no more than 5–10% of its total food (by weight consumed) from hunting.

The argument that the availability of protein from game in Amazonia not only influenced shifting cultivation but determined low population density, small and unstable settlement, and cultural development became a major debate in cultural anthropology in the 1970s well after my Campa research and Carneiro's 1970 paper, and it has continued (Denevan 1984a; Hames 1989). The catalyst was an essay on "Protein Capture" in the *American Anthropologist* in 1975 by Daniel Gross. Numerous field studies followed which attempted to support or disprove the thesis, one direct result being the stimulation of research on Amazonian cultural ecology.

Following Gross, the topic was picked up by Marvin Harris (Harris and Ross 1987) and the cultural materialists as a prime example of how human behavior can be explained as invariably functional in some way in terms of survival, in contrast to the structuralists who argue that much behavior can be explained in other ways or is inexplicable. The materialists further claim that limited game availability can also explain other aspects of behavior in Amazonia, such as warfare (competition for game territory), food taboos (restrictions on large game animals), and sex (greater availability for successful hunters).

Many of the leading Amazonian scholars became involved in the debate. Opponents of the thesis include Beckerman (1979); Chagnon and Hames (1980); and Diener et al. (1980). Beckerman and Diener argue that there is little evidence of protein scarcity. Brokers include Johnson (1982) and Sponsel (1983). Renewed support has come from Baksh (1985), Good (1987), and Frank (1987), even though Chagnon (1992, 96) maintains that "the protein debate has now pretty much been laid to rest." Johnson emphasizes perceived versus actual protein scarcity. Good emphasizes greater labor inputs to procure adequate protein from progressively smaller game as population size grows, rather than absolute scarcity.

Thus, we have a situation today in the Amazonian interfluves where Indian settlements, almost without exception, are small and moved often, with low population densities (mostly 1.0/km^2 or less; Beckerman 1987, 86). Why? Limited game seems to be more critical than poor soils. Despite evidence that game is more plentiful than previously thought, there is some threshold where it will not be adequate, possible at 2 persons/km^2 or even 20, but certainly before 200. However, this is not a direct environmental limitation. It is the result of a diet dominated by manioc with most protein coming from animal sources, and this particular diet is a cultural choice given options available, pressures present, and history. A diet based on maize and legumes, as in tropical Mesoamerica, is nutritionally well balanced and not dependent on protein from fish and game. The Amazonian Indian diet works because population densities are low and because labor inputs are low. A denser population could be supported without game if the diet were changed to emphasize seed crops and legumes, but the labor inputs would be high; there are numerous examples in Asia and Africa.

In my opinion, protein scarcity is one but not the only explanation for the consistently extremely low densities of historical *terra firme* Indian populations (Denevan 1984b). The argument is less valid, however, for prehistoric times when shifting cultivation was limited by stone axes and cultivation and settlement were probably more permanent.

The Shipibo: Multiple Biotope Use

The Indians of the major floodplains of the Amazon, such as the Tapajó and Omagua, were rapidly destroyed by European contact, and few survive today. One exception is the Shipibo along the central Río Ucayali in Peru; they number over 15,000 and have much of their culture still intact. The economy of the village of Panaillo provides some idea of aboriginal floodplain subsistence, since the sustaining area lies entirely within the floodplain of the Ucayali. The cultural ecology of the village was studied in 1971–1972 by a research team led by my student, Roland Bergman, and I spent a brief period with them in July 1971. The information here is drawn mainly from Bergman (1980; also see Myers 1990, 25–36).

Panaillo in 1971 was a small village located near the juncture of the Río Panaillo and the Río Ucayali, one of the major tributaries of the western Amazon in Peru (Fig. 3.19). The Shipibo there had regular contact with the river port city of Pucallpa, spoke Spanish, and had a resident Peruvian schoolteacher. Their subsistence system remains largely traditional, being based on plantain cultivation and fishing.

Typical of Amazonian meander-type rivers, the central Ucayali forms a floodplain that is some 30 km wide, an unstable biotope (microhabitat) network of natural levees, side channels, backswamps, sandbars and mudbars (*barriales*), islands, and lakes (Lathrap 1968). During low water, extensive *playas*, which may be wider than the river itself, are exposed. Tributary streams, such as the Panaillo and Aguaytia, add their own levees and backswamps to the complex floodplain landscape. The village is situated along the top of the levee (*restinga*) of the Río Panaillo a few hundred meters from the Ucayali. Facing the river (foreslope), the levee breaks sharply down to the water. On the other side (backslope), the levee grades down more gently into a large backswamp (*tehuampa*), which is bounded by a former levee of the Río Aguaytia, and behind that there is a permanent lake (*cocha)*. Continuing westward, the area to the Río Aguaytia consists of lakes, backswamps, levee remnants, and *playas*. To the north lie the large *playas* and levees of the Ucayali (Fig. 3.19).

The Shipibo farmer is thus faced with a varied environment but with repetitive sequences of biotopes and an annual sequence of rise and fall of water level. This

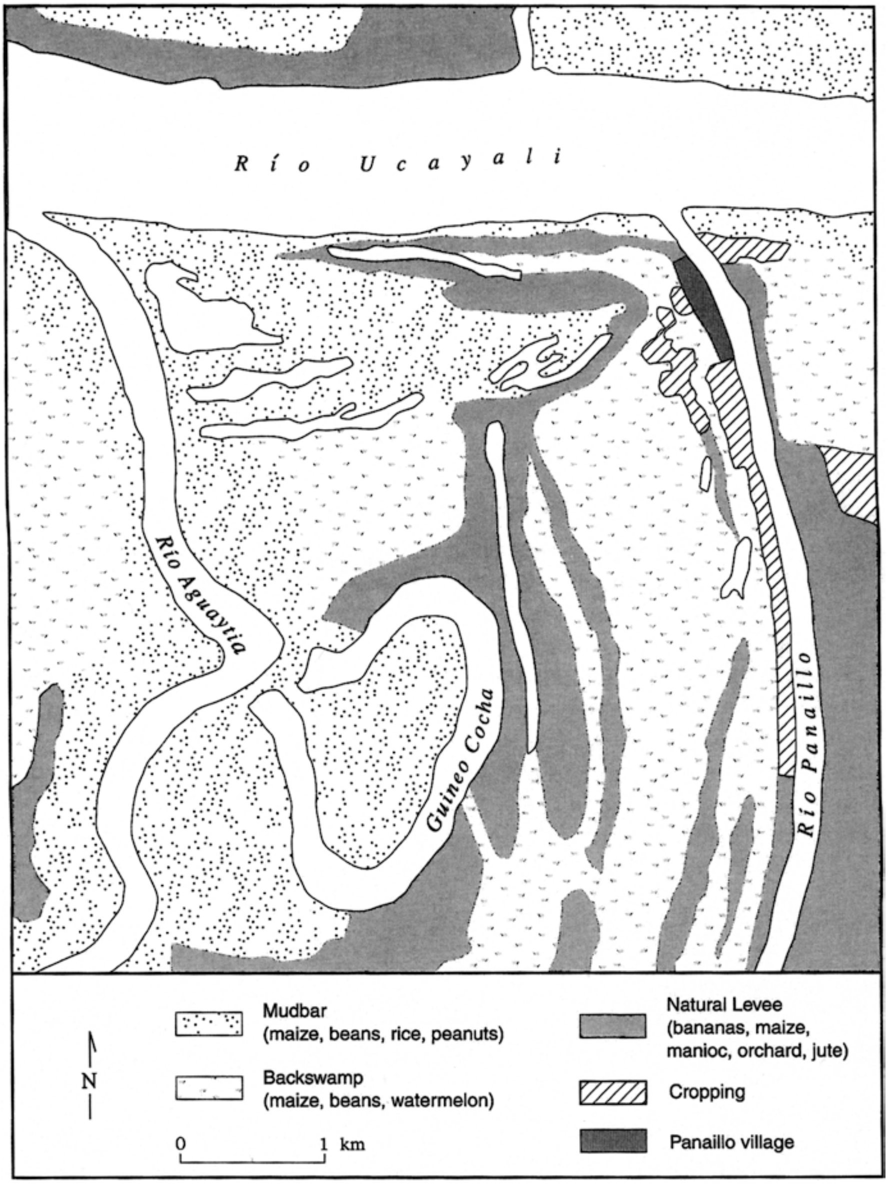

Fig. 3.19 Map of land types and major crops at Panaillo (Shipibo) on the Río Ucayali, Peruvian Amazonia. Most of the area shown is also used for hunting and fishing. (Adapted from Bergman 1980, 90)

zonation is matched by a horizontal zonation of crops. However, the size of individual biotopes and distances between biotopes vary considerably. Major advantages are diversity and good soil. The major disadvantages are periodic early or unusually high floods. Success in subsistence is dependent on knowledge of changing river levels and the related availability of biotopes for different lengths of time and hence for different specific crops.

Bergman (1980, 60, 90, 93) mapped and diagramed the land use of Panaillo, recording houses, individual fields, and crops (Fig. 3.20). The crops associated with the principal biotopes there are as follows:

Mudbar In the Río Panaillo; flooded annually, silt loam soil; short growing season crops, including maize, beans, rice, peanuts, sweet potato, watermelon; potentially farmed every year, but only small portions are above water long enough to be cropped

Levee Foreslope Silt loam soil; sugarcane

Levee Top, Riverside Sandy soil; orchard and garden crops, including star apple, guava, cotton, shapaja, mammee, soursop, hog plum, and the Old World crops mango, lemon, grapefruit, sugarcane, and tangerine

Levee Top, Center Houses, dooryard plants, similar to the riverside biotope above, but only a few scattered crops

Levee Top, Backswamp Side Dominated by permanent bananas; a few short-fallow swiddens with maize and manioc; scattered guava, sapote, star apple, genipa, cacao, pejibaye, breadfruit, shapaja palm, tangerine; some commercial jute

Levee Backslope Silt loam soil; sugarcane

Higher Backswamp, Lower Levee Silt loam soils; swamp forest, cecropia, wild cane; maize, beans, watermelon, manioc, rice; potentially farmed every year

Lower Backswamp Swamp forest, no cropping

Playas of the Río Ucayali Enormous sand- and mudbars; exposed as long as 6 months; flooded annually; beans, maize, rice, and peanuts (Fig. 3.21)

Thus, the Panaillo Shipibo make use of nine biotopes for several distinctive forms of agriculture – permanent, annual, and short fallow – each with a different group of crops.

Plantains (including bananas) were the principal crop, with manioc, maize, beans, and rice all secondary. These were supplemented by a variety of orchard and garden crops. Fish was the main source of protein except during the highest water (February–March), when fishing was poor. At that time, game, which tends to be trapped and concentrated on the levees, is important. Of the food crops, there were 10.75 ha of bananas, all on the levee. There were only 0.031 ha of manioc and 1.17 ha of maize on the levee, but 7.29 ha of jute for market. The low-water *playa* and backswamp crops of maize (4.15 ha), watermelon (1.87 ha), beans (1.28 ha), and peanuts (0.10 ha) totaled only 7.40 ha, even though there was considerable unused backswamp land

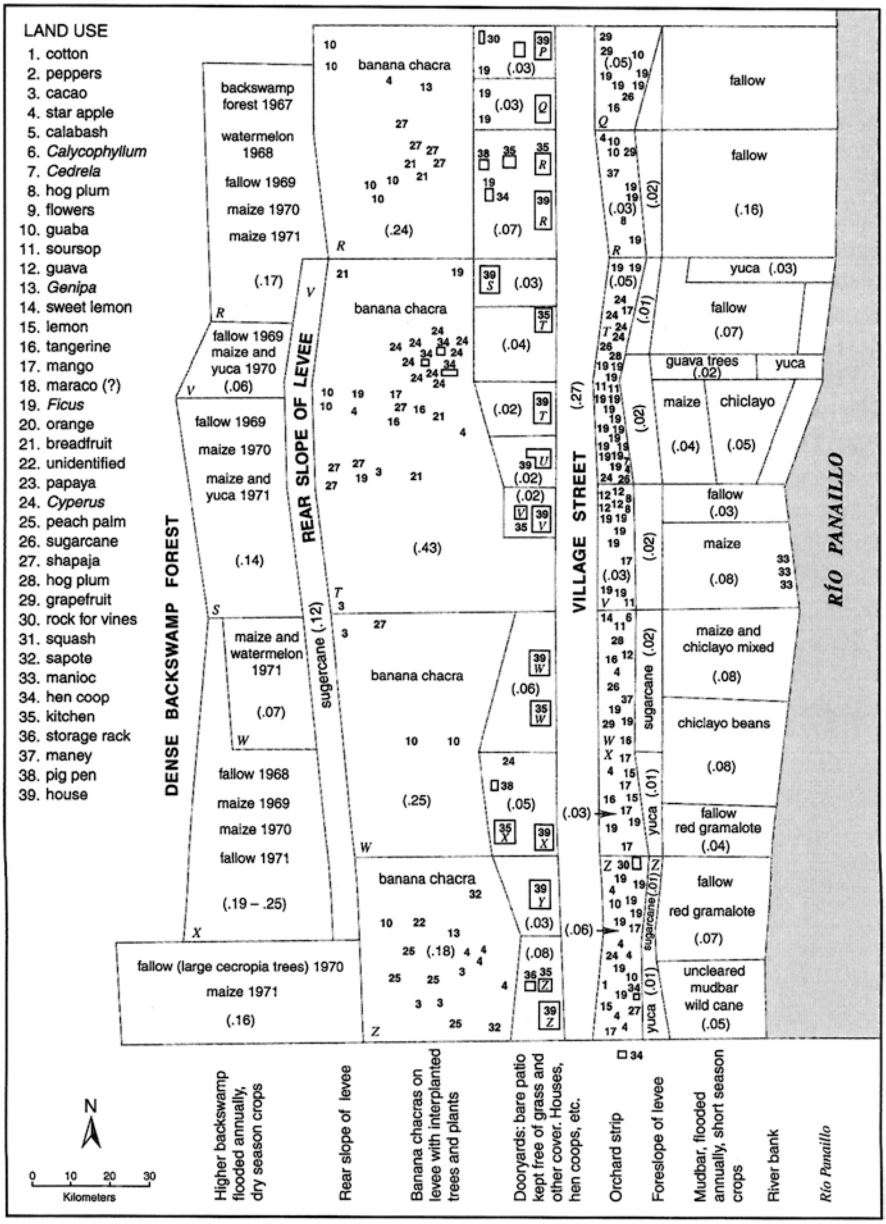

Fig. 3.20 Map of fields and crops at Panaillo, Río Ucayali, Peruvian Amazonia. According to Bergman, "The trees and plants shown are economic, whether deliberately planted, as most are, spontaneously seeded, or wild but cared for. In the banana *chacras* [fields] useful species grow surrounded by dense stands of bananas. An orchard strip runs the length of the village between the street and the front edge of the levee. Only a few scattered banana plants are grown here amidst the fruit trees. In the orchard strip of [household] X manioc grows on the steep foreslope of the levee. On the rear slope of the levee [household] V grows sugarcane in a long narrow strip that is too low for reliable banana growing. Numbers in parentheses indicate areas in ha. Capital letters indicate households. Crops are for 1971 unless otherwise indicated." (Adapted from Bergman 1980, 93)

Fig. 3.21 Shipibo woman planting maize and rice on *playa* of the Río Ucayali at Panaillo, Peruvian Amazon. (Photograph by Roland Bergman, 1971)

available. The low-water crops, however, cannot provide year-round food, as can levee crops, hence the traditional underutilization of the low-water biotopes despite their potential for high levels of annual production. The Panaillo Shipibo do not have much mudbar land on the *playas* of the Río Panaillo that are exposed long enough to plant rice for commercial production. Most of the large nearby *playas* of the Ucayali, where there is good rice land, were controlled by non-Indians.

There is also an ecological zonation of fish and game resources, based on types of water body, vegetation, and seasonality. The patterns of actual catches reflect the ecological zonation in combination with distance from the village. These patterns are mapped and described by Bergman (1980, 135–66).

Biotope agricultural patterning at Panaillo is representative of the kind of zonation that occurs throughout the floodplains of the Amazon Basin. However, it is not necessarily typical, given the wide variation in the environmental factors discussed earlier, in crop orientation, in accessibility to land or in land ownership, in population pressure, and in availability of commercial outlets. Variation from region to region is considerable, but there tends to be local consistency in agricultural zonation, as we see at Panaillo.

Most years the levees stand above flood levels. The highest local levees flood about every 10 years, but the slightly lower Panaillo levee floods every 5–7 years

(Bergman 1980, 53–60). Houses are on raised platforms and people move about in canoes, but crops are vulnerable. In the 1971 flood, the river rose 9 m above the dry season low-water level, although this was only a meter or so above normal floods. Staple crops, especially plantains, were mostly destroyed. Where the water covering the plants exceeded 60 cm for over 30 days, the crop loss was total. At lower durations more plantains recovered. Prehistoric crops of maize or manioc on the levees would have been more vulnerable than the post-conquest plantain. *Playas* are exposed for up to 6 months; however, unusually high-water levels during the dry season can result in destruction of *playa* and also backswamp crops (maize, beans, watermelon, peanuts, and rice today). When crop loss is severe, the Shipibo travel to distant swiddens located on high levees and in upland forest to harvest plantains and manioc.

Plantains, the staple, are a European introduction that replaced manioc on the better drained soils. Fish is the main source of protein except during brief high water when maize and game are more important (Blank 1981). This system of permanent (levee top plantains), short-fallow (levee top manioc), and seasonal (*playa* and backswamp maize, beans, etc.) cropping supported a population of 107 people in a permanent village (over 25 years old in 1971); the resource area exploited had a population density of about 4 per km^2 (Bergman 1980, 203).

The present Shipibo village of Panaillo is similar to other Shipibo villages in utilizing multiple microhabitats for cultivation. However, many Shipibo villages, both past and present, were located on bluff tops, in contrast to Panaillo, and thus also had direct access to the high-forest habitat where the poor weathered oxisols of the *terra firme* could be used for long-fallow shifting cultivation emphasizing manioc. This is true of the present Shipibo village of San Francisco de Yarinacocha (see Fig. 8.4), as well as prehistoric occupations on the same site (Lathrap 1968, 74).

The Karinya: Multiple Biome Use

Indian farmers commonly utilize multiple microhabitats (biotopes), as we have seen with the Shipibo. Most, however, are still thought of as specialized to a particular macrohabitat (biome), such as rain forest, savanna, or floodplain, but some cultivate in more than one biome. In Chapter 8.3 "A Bluff Model of Riverine Settlement in Prehistoric Amazonia", I argue that prehistoric riverine people farmed both the floodplains and the adjacent bluff forests. For both multiple biome and multiple biotope exploitations, the objectives are to maximize and diversify production and to minimize the risk faced if only one habitat is utilized, at the cost of increased travel time.

The Karinya of the Orinoco Llanos are a particularly good example of a society with multiple resource strategies. Farmers in the same community farm four biomes – the Orinoco floodplain, forest (*monte*), savanna, and palm swamp (*morichal*), comprising at least nine different biotopes.

The Karinya were studied by anthropologist Karl Schwerin (1966) in the early 1960s. When I was examining pre-European raised fields in the Venezuelan Llanos at Caño Ventosidad in 1972 (Zucchi and Denevan 1979), I took the opportunity to also observe the Karinya ditched fields, assisted by Roland Bergman (Denevan and Bergman 1975). Schwerin and I later collaborated on a broader treatment of Karinya adaptive strategies (Denevan and Schwerin 1978).

The Karinya are a Carib-speaking group widely dispersed throughout the eastern Llanos west of the Orinoco delta in some 30 communities, including a few villages south of the Orinoco (Fig. 3.22). The total population in 1962 was 3828, most Karinya having regular contact with regional gas and oil industries and with urban centers. Our study focused on the communities of Cachama and Mamo.

The Orinoco Llanos, which I consider part of Greater Amazonia, cover a vast area north of the Orinoco in Venezuela and Colombia. Much of this plain is low lying and poorly drained; however, there are extensive tablelands of Tertiary age north of the river forming what are often called the Llanos Altos.

Despite a long dry season, the streams of the eastern Llanos flow year-round, dissecting the terrain into large *mesas*. The *mesas* are covered by savanna – open grassland with a few trees, with very infertile, sandy soils. The stream channels, however, are lined with *moriche* palm (*Mauritia flexuosa)* gallery forests, and swamps (*morichales*), with fertile alluvial soils. The intervening region between

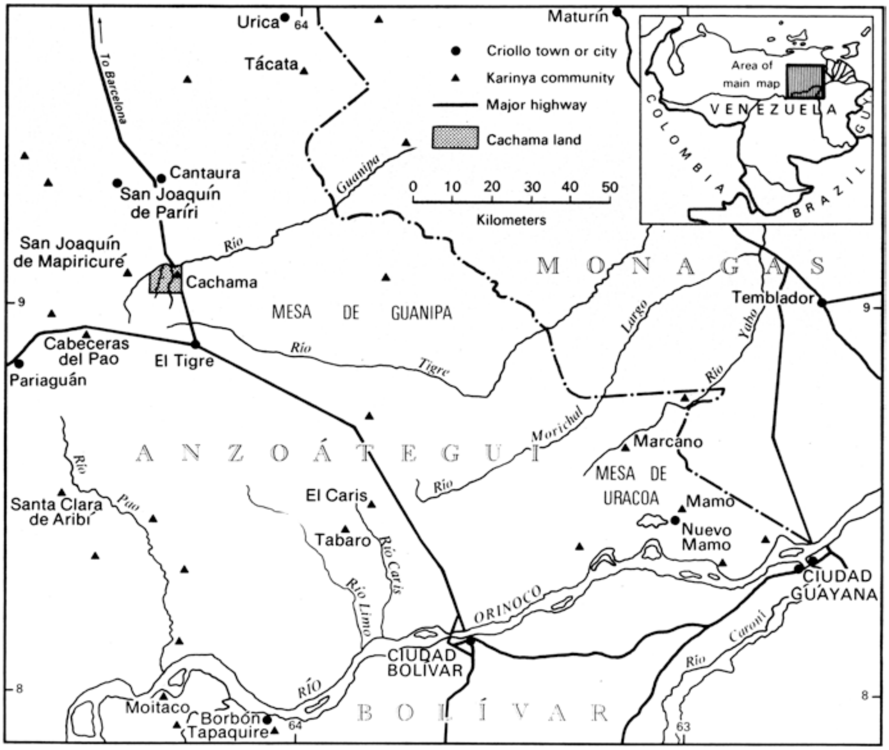

Fig. 3.22 Map of the Karinya region, Orinoco Llanos, Venezuela. (From Denevan and Schwerin 1978, 6)

the *mesas* and the Orinoco is a low-lying floodplain. During the rainy season, the great river overflows its banks, flooding much of this area and replenishing the many lakes, lagoons, and backwaters which are found there, often at considerable distance from the main stream. At the same time, there are high places in the floodplain which are rarely or never flooded, including natural levees, river terraces, and islands within the Orinoco channel. This region of sharply contrasting savanna, *morichales*, and floodplain comprises the Karinya territory (Fig. 3.22).

The Karinya have adapted their agricultural techniques to the exploitation of a range of macro- and microenvironments. Seven types of cultivation are practiced, some occurring in several different habitats, while some habitats are utilized for several different types of cultivation. The forms of agriculture are as follows: swidden mixed cropping, *playa* mixed cropping, river-bottom drained fields, floodplain drained fields, house gardens, *playa* monoculture, and savanna monoculture. The locations of these systems are indicated in the profile shown in Fig. 3.23, and each is briefly described in Table 3.1; for more detail see Denevan and Schwerin (1978). Drainage of the palm swamp (*morichal*) fields was undertaken by means of a network of ditches.

The Karinya not only exploit a range of ecological situations for cultivation, but raise domesticated animals (chickens, pigs, cattle, goats, burros), and engage in fishing in small streams (Cachama) and in the Orinoco (Mamo), as well as some hunting (rabbits, iguana, deer, birds, capybara). Most men also participate to some degree in wage labor in the petroleum industry, ranching as field hands, or as workers in the nearby cities.

The Karinya have adapted not only to a varied physical environment but also to a changing socioeconomic milieu (Denevan and Schwerin 1978, 59–66). The result is a flexible and opportunistic economic system, one that is now being recognized as common for rural Indians and peasants in the Andes, Amazonia, and elsewhere.

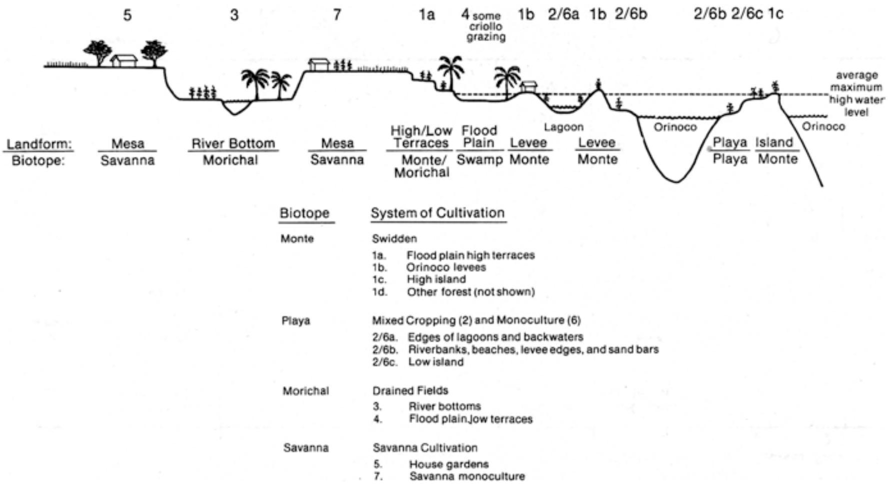

Fig. 3.23 Biotopes and Karinya cultivation, Orinoco Llanos, Venezuela. (Adapted from Denevan and Schwerin (1978, 14; See Table 3.1)

Table 3.1 Systems of Karinya cultivation

System[a]	Biotope	Types of crops	Tools used	Technological improvements	Communities
1. Swidden mixed cropping (short to long fallow)	Primary and secondary forests of river levees, high terraces, and islands	Subsistence; esp. tubers	Axe, machete, *garabato* (wood hook)	Minimal	Mamo, others
2. Playa mixed cropping (annual)	Levees, beaches, lake margins, islands	Subsistence; esp. grains, legumes	Axe, machete, *garabato*, *chícora* (digging stick), shovel	Minimal	Mamo, Tapaquire, others
3. River-bottom swamp (*morichal*) drainage (continuous or short fallow)	Gallery swamps along rivers draining the *mesas*	Subsistence; esp. tubers, bananas	Axe, machete, *garabato*, *chícora*, shovel	Swamp drainage, some irrigation	Cabeceras del Pao, Tabaro, Cachama, Tácata, S. Joaquín, Santa Clara, Morichal Largo, Marcano, others
4. Floodplain swamp (*morichal*) drainage (continuous or short fallow)	Low terraces of the Orinoco floodplain	Subsistence; esp. tubers, bananas	Axe, machete, *garabato*, *chícora*, shovel	Very long drainage channels, efficient drainage, local crop specialization	Mamo, others
5. House gardens (continuous)	Savanna and elsewhere next to houses	Supplementary crops; fruit, herbs, medicinals, vegetables	Machete, *garabato*, *chícora*	Individualized care	All
6. Playa monoculture (annual)	Levees, beaches, lake margins, islands	Cash crops; esp. grains, fibers, legumes	Axe, machete, hoe, *chícora*	Minimal, but with crop specialization, large fields	Mamo, others
7. Savanna monoculture (annual)	Savanna	Cash crops, esp. grains, fibers, legumes	Tractor-drawn machinery	Mechanization, fertilizer, large fields, crop specialization	Cachama

[a]Numbered cultivation systems are keyed to Fig. 3.23. Crops are listed in Denevan and Schwerin (1978, 42–5)

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4.0 Human Environmental Impacts

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Abstract It is difficult to overstate how influential William Denevan's research and writings have been. His work, derived from the school of landscape analytics at Berkeley, was profoundly influenced by forms of interdisciplinarity that were unusual for the time and provided the empirical research that would give rise to alternative positions to the functionalist cultural ecological theories, under development ideologies, and the ahistorical views of the Amazonia that placed the region as a *tabula rasa* and knowledge void. His own work on historical demography provided the archival impetus, and his experience as a journalist in Peru gave him initial experience in the field, which if one were not enchanted by ideas of Amazonian primitivism could provide ample evidence of the engineering and ecological cultures that had been annihilated in the great dying. His own research embraced indigenous agricultures of multiple types, and his master work *Cultivated landscapes of native Amazonia and the Andes* (2001) provides an indispensable overview. At a time when analytic focus lays firmly in annual cropping systems, his sweep was remarkable and paradigm breaking. His attention to wetlands, terraces, lake cultivation, and fallow stimulated research into what became a recognition not just of the diversity of systems but of the management within them. His studies of fallow management opened up the reality of studies of the anthropogenic Amazon that have recast its history and landscape.

Keywords Amazon · Andes · Cultivated landscapes · Interdisciplinary paradigm-breaking

Introduction

I actually made William Denevan wail when at the Association of American Geographers (AAG) annual meeting session in his honor I called him a closet theorist. Most scholars would have blushed, shuffled, and been flattered. Given the contortions in theory from the mid to the end of the twentieth century, its problematic

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effect on understanding Amazonian history and lands still fuels a continuing debate (Levis et al. 2012; McMichael et al. 2012; Bush et al. 2015; Clement et al. 2015a, 2015b) and its obtuse writing (highly irritating to someone whose prose is as limpid as his), one can understand why he howled. Denevan, who began his career as a journalist in South America, perhaps sees himself as much a reporter – which means getting the story – as a careful scholar.

However, Denevan certainly is a theorist and an excellent one, and it is very hard to think of anyone whose work, insights, and influence – indeed theories – have been as profound in recasting the way Amazonia has been studied and understood. Part of it is that it has been "stealth" inductive theory development rather than the realm of deductive "a priori" of all-encompassing frameworks. A scientific theory, after all, is a well-substantiated explanation of some aspect of the natural world, based on a body of facts that have been repeatedly confirmed through observation and experiment. Such fact-supported theories are relatively reliable accounts of the real world (Hacking 1999; Machamer et al. 2000) at least within the paradigms that can embrace them. Amazonia, in the 1960s and 1970s was seen as wild and untrammeled, leached of any history, even that as recent as the rubber period, including that of the Second World War (Garfield 2010; Garfield 2013; Hecht 2013). Amazonia, in this imaginary, became a place where complex civilizations could never have evolved due to theories about soil constraints (Meggers 1971a), protein limitations (Gross 1975), and other sociobiological features that undermined cultural development (Chagnon 1968), thus condemning the original inhabitants to a relatively brutal stone age existence and using nature rather than history to explain the low population densities. Meggers and Chagnon, in particular, had books that were academic best sellers and which were required texts for students in anthropology, geography, and, often, Latin American Studies. This made their views especially influential and is an optic that still reverberates in understanding Amazonia today (McMichael et al. 2012; Piperno et al. 2015), although the factions fairly clearly segregate along the lines of those engaged in extensive field work versus short sampling visits (Hecht 2017). This can be understood in part through the optic of Kohler's idea of understanding the field via labscapes versus landscapes, which at the debate level came down to the Meggersians versus the Denevanians.

Denevan was a field-based geographic empiricist – a materialist – and he reasoned inductively. His method involved working from specific observations to broader generalizations and insights. Thus, resource, or bioconstraint, theories about Amazonian civilization impoverishment and sparse settlement are to be contrasted with an edifice of empirical work which provides a countertheory rooted in an analysis of upper Amazon archaeology, historical resources, and native technology and knowledge systems that informed land use. Denevan's "countertheory" was that native populations had a range of production technologies (not just annual cropping in swidden systems) that could indeed support complex societies and that these technologies in their "hardware" (engineering and other forms of physical practices) and software (knowledge systems) were variable, complex, and widespread. Furthermore, historical demographic data supported more densely populated Amazonia (Denevan 1992). This was a substantive paradigm shift between Denevan and most other mid-century Amazon historical and settlement theorists, as well as modernization theorists who held that archaic traditions and non-western understandings of the world were inefficient, atavistic, and produced in the economic plight of the third world. Thus, environment, functionalist ecology, and culture in the mainstream view conspired to maintain Amazonian primitivity.

What Thomas Kuhn (Kuhn 2012) argued in his *Structure of scientific revolutions* was that scientific knowledge and the theories that frame them do not proceed in linear ways, but advance through "paradigm shifts" that open up new approaches to understanding that would have "fallen outside the frame" or simply not been considered valid by the older criteria. Competing paradigms may not be comparable – that is, they are contending accounts of reality – say soil constrained, mostly empty, primitive Amazonia versus occupied pre-European Amazonia with complex civilizations and elaborate agroecologies. One central question on the virtues of theories is whether they open up new arenas of research or close them off. The "Denevan paradigm" needed a lot of empirical work to support its bold – indeed visionary – assertions, and it required (and acquired) unforeseen research directions. It is for this reason at a recent Amazonian archeological meeting held in Trinidad, Bolivia, a place where Bill Denevan pioneered his understanding of pre-European landscapes (Denevan 1963), when the question was put to the table, who was the researcher who had most influenced them, the answer was a resounding chorus of "Denevan." His dissertation was published in Spanish in La Paz in 1980.

Although the deeper Malthusian bias always saw native populations running up against strict environmental limits, Denevan, with his attention to historical demography as well as native science, viewed tropical forests as both human artifact and as sites of landscape innovation. Moreover, soon his students, allies, and admirers were carrying out the scientific but embedded field work necessary to support or refute the "inhabited Amazonia" hypothesis. While he may not have entirely agreed with economist Ester Boserup's technologically determinist solutions to Malthusian limits (Boserup 1965), he certainly was a believer in human creativity, as well as the intellectual limits of tropical analysis framed within the cultural ecological contexts of the time (Denevan 1966). He suggested that historical demographers had vastly underestimated human numbers and that "theorists" ignored the magnificent engineering feats that abounded in Bolivia, Venezuela, Colombia and in the Guyanas (and are highly visible). The emphasis on village case studies at the time obscured the questions of scale and did not really understand the production systems, whether in forest, savanna, or tropical mountains. Concepts of Amazonian food production were based on agrotechnologies in the Andes, Mexico, Europe, and Asia, rather than on intensive swidden, forest management, and raised fields in wetlands – the Western binary only saw "wild" and "tame."

From subtle manipulations of forest agriculture, secondary succession, and wild diversities to massive causeways, palisades, and vast raised field (Fig. 4.1) infrastructure and ringed ditch structures, the human imprimatur is still everywhere. Fire, agriculture, and human settlements and mobilities shaped the "wild" in ways that ranged from the barely perceptible to massive and extensive earthworks (Erickson 2000; Erickson 2006; Rostain 2012; Politis 2007; Hecht 2009). Indeed,

Fig. 4.1 Large raised fields in the Llanos de Mojos, Bolivian Amazonia. (Photo by Clark Erickson)

Heckenberger describes the impact of the Kuikuru on an area the size of Belgium (Heckenberger et al. 2007). Likewise, Denevan for Western Amazonia (Denevan 1970; Denevan 1971; Denevan 2001; Denevan and Chrostowski 1970).

A filigree of human influences could be discerned if one had eyes, openness, and methods to see it, rather than just looking for the confirmation of some theoretical principle and swanning in to dig a few soil pits and back to the lab to prove it, thus proceeding to ignore tree crops, all modern ethnography, and archeological data on the basis of a poorly articulated bench scientism (Hecht 2017). Denevan had seen the human mark first hand because he had traveled widely in his early journalist life all over the Andes and the Western Amazon, by horseback, foot, and boat. He had seen things from the air and from the ground and later as a wide-ranging scholar. Unlike many – indeed most – tropical scientists, who often tethered themselves to their research stations and their self-reinforcing intellectual communities, he had gotten around and he had hung around. Denevan talked to people and was not afraid of them. He also carefully read Amazonian history and reportage from other eras (chronicles and travel narratives) not for the "jungle boy" thrill of the masculinity and the colonial knowingness that infuses them, but rather for the relatively fleeting views of the inhabitants and human landscapes which were there but had to be read between the lines. His own collection of traveler's books is legendary, and woe be to anyone – students especially, who were not up on their Amazon history as well as the latest field research. Furthermore, he was generous with this information from the past or present. I feel I have been getting reading advice from him my entire life.

His yowl at that AAG meeting might usefully be understood as a response to the time he developed his thinking. His early professional life was in the late 1950s and the 1960s which in retrospect might be viewed as the "age of functionalist theory," especially as applied to the developing world and environmental concerns. It was a period of immense theoretical confidence allied to rather flimsy empirical evidence and clunky early modeling. This was the time of dependency theory, a raft of Marxist models ranging from world systems theory, theories of imperialism, articulation theory, as well as the right wing modernization theory to the domino theory, just to name a few that were knocking about seminar rooms as well as galvanizing development and political and revolutionary practice (Edelman and Haugerud 2005; Escobar 2008; Grandin et al. 2010). Environmental theory was awash in catastrophist population bomb exercises (Ehrlich 1968; Hardin 1968; Meadows et al. 1972) and helped give Malthusian inflections to the cultural ecological theories of the day that were applied in Amazonia and tropical Latin America more generally (Durham 1979). Highly deductive and deterministic approaches with often rather limited field support often characterized these efforts,[1] so it is not surprising that Denevan maintained a distrust of often careless application of deductive logic... especially in Amazonia, which was relatively short on empirics. In many ways, the systems approach stifled ways of "seeing" as scholars truffled about for data that would support their views. In Amazonia, this involved ignoring the complexity and often richness of regional soils including anthropogenic ones and turning a blind eye to one of the most massive fisheries on the planet and indifference to the complex usage of landscape attributes at regional landscapes. The critique of this narrow focus became far more prevalent as the "constraints models" began to lose ground (Posey and Balée 1989; Redford and Padoch 1992; Padoch 1999; Lehmann et al. 2003a; Woods et al. 2006) and as Amazonian research began to include more social history (Hecht and Cockburn 1989; Schmink and Wood 1992). The simple theorizing from abstract models was really not to his way of thinking, especially in places where at the time people did not really know what was going on. Because Denevan did not work in Brazil, due in part to what seemed to be a Smithsonian embargo of certain kinds of research,[2] he was not initially part of the juggernaut of Brazilian Amazon studies, but this distance provided him with more intellectual freedom and placed him in the center of the massive earthworks and anthropogenic systems of the Western Amazon. This unfortunately initially marginalized his insights more than they deserved to be, in part because Brazil so dominated the idea of the Amazon, and the development and conservation politics were especially intense and publicized there. However, at the end of the day, he was a consummate Amazonist (rather than Brazilianist, Peruvianist, etc.) and inspired scores of Amazonian field scholars:

[1] Meggers used the Kayapó Indians for her arguments, whom she had never visited.

[2] Roosevelt and Lathrap maintained profound analytic disagreements with Meggers, and felt that research permits, funds, and affiliations were blocked by her.

geographers, archaeologists, ethnobotanists, anthropologists, ecologists, historians and demographers, and geneticists (Astier et al. 2011; Zenteno et al. 2013); see, for example, Denevan et al. (1984), Padoch (1988), Posey and Balée (1989), Padoch and DeJong (1991), Brookfield and Padoch (1994), Balée (1998), Erickson (2006), Posey and Balick (2006), Heckenberger and Neves (2009), Woods et al. (2009), Zimmerer (2010), Fraser et al. (2011), and Clement et al. (2015a, 2015b) among many, many others including the compilers and section introducers of this volume and this author. Denevan's work was largely "deviant," and while not exactly ignored within mainstream Amazonian framings for more than a decade, his research was outside the model, one of those places where the "yes but" questions were posed, emerging from an ever more overwhelming body of evidence. These insights would also serve as platforms for rethinking modern forms of alternative land uses (WinklerPrins 1999; Glaser 2007; Hecht 2009; Novotny et al. 2009; Factura et al. 2010; Junqueira et al. 2010; Barrow 2012). His papers that follow in this collection serve as a quite complete literature review of human impact in pre-European times on Amazonian landscapes. This array of complex impacts seems to have supported a great deal of forest ecological resilience even as most of the population died (Hemming 1987), permitting "a great forgetting," and the re-inscription for a time at least, of Amazonia as the great untrammeled wilderness.Denevan and his battalions of students and affines showed that there was much that was sustainable, durable, and transferable in traditional agricultures and forest management and that many of the practices that supported livelihoods also maintained forests. We now call this "forest dependence" and integrate this far more into our understanding of production systems and rural economies (Astier et al. 2011; Zenteno et al. 2013; Agrawal et al. 2008; Wanger et al. 2009; Larson et al. 2010). Increasingly, this has become a topic integrated into issues of decentralization and forest governance (Bray et al. 2005; Merry et al. 2006; Pacheco et al. 2012; Stickler et al. 2013). This literature on indigenous and traditional management stands now as a counterpoint to agroindustrial and extensive production systems (Brondízio 2008; Clement and Junqueira 2010; Padoch and Pinedo-Vasquez 2010; Altieri and Toledo 2011; Levis et al. 2012; Hecht et al. 2014; Clement et al. 2015a, 2015b). Denevan's work inspired as "agroecology" writ large, and it is very difficult to find papers on indigenous knowledge systems that do not cite him.

Imminent Demise?

The smoldering and desolated landscapes that define conservation ads have now become so commonplace that one forgets that there was a time when the alarms about deforestation had yet to be sounded. Clearing was seen as the emblem of conquest and mastery – a sort of terrestrial "space shot" of those last, "new" frontiers. Current destructive human impacts on Amazonian forests – and its problematic consequences – are now so much a part of our understanding of the region that it is easy to forget that most of this literature mostly comes after Denevan's influential 1973 paper about the pace and consequences of Amazonian deforestation: "Development and the Imminent Demise of the Amazon Rain Forest" (also see Denevan 2007). This was a time when the Trans-Amazon in Brazil, the Carretera Marginal in Peru, development forays in Bolivia and Ecuador, and countless colonist projects captured headlines, even as large-scale fiscal incentives, credit schemes, and speculation triggered massive deforestation for cattle ranching – what was to become ultimately the real occupation strategy (Hecht 1985; Hecht 1993). Amazonia exploded into low level insurgencies as traditional and indigenous peoples were displaced, and new migrants often found themselves in the devil's snare of low agriculture prices, insecure tenure, precarious infrastructure, poor agricultural advice and powerful land grabbers, and competing claims over land. The "law of the jungle" was roughly what it had been since the rubber period, the Winchester 44 – hence the humorous and rueful references to "Law 44."

The article ("Imminent Demise") is an excellent example of the problems of extrapolating early clearing patterns to the present day: the models had Amazonia as more or less over – completely gone – by now. This does not diminish in any way the "shock of the new" in the 1973 article. In the 1960s, most of Amazonia was still forested, but a few years later any one on a road or a flyover was treated to a panorama of massive clearing, as roads punched into the region, and modernization policies designed to wrest the region from its tropical torpor into the modern day were applied with little attention to the regional context. Anyone who had known it before – and Denevan was one of these – could barely believe the pace and scale. In general, models for understanding it relied on transposition of US Western Frontier imaginaries of a noble national trajectory to inhabit an "empty" land through colonist policy (indeed Brazil used the slogan invoked for the annexation of Palestine: "People without land for a land without people") and framed by models that had population spilling over from its nodes of increase (northeast Brazil or Andean zones) to ravish the tropical lands, even though it was large-scale production systems for cattle that were the drivers of clearing. The deeper concerns from the military governments in power at the time had no small portion of geopolitical concerns at their heart. The place had had fuzzy boundaries ever since its incorporation into the European ambit, and the upper Amazon was especially conflictive – and indeed the most recent border skirmish between Peru and Ecuador was only adjudicated in 1998. *Integrar o entregar* – loosely translated as "use it or lose it" – became a military motto that reflected the deeper territorial anxieties of Amazonian states (Hecht and Cockburn 1989; Schmink and Wood 1992). It surely was a frontier in the sense of one economic and social form confronting another and very explosive. The main arena Amazonia occupied was development analysis which at the time had little concern for and virtually no expertise for understanding environmental impacts since regional development (and development assistance) was largely understood as a "Marshall Plan" for the tropics and focused on economic metrics and regional planning exercises (Goodland and Irwin 1975; Barbira-Scazzocchio 1980; Becker 1982; Mahar 1989; Peattie 1990). What Denevan observed, because it was everywhere, was the vast extension of poor pasture as part of the "development trajectory," not the vibrant rural communities dreamed of by planners.

Denevan was prescient in his concerns: besides disquiet over the regeneration of forests and their biodiversity (which remains highly controversial), foremost on his list were the impacts on climate. Amazonia is critical in global carbon dynamics, and future climate may well hinge on it (Saatchi et al. 2007; Fearnside et al. 2009; Numata et al. 2011; Anadon et al. 2014; Aragão et al. 2014; Alencar et al. 2015; Coe et al. 2017). Next, the impact of clearing on soils of these poorly managed pastures and other kinds of occupation was central in the calls for understanding what constituted sustainability and indeed what was driving a land use so degrading in the frontier stages (Hecht 1982; Hecht et al. 1988; Walker et al. 2009). While the term sustainability did not come into wide use until the 1990s with the Bruntland report which catapulted the term "sustainable development" into global usage, the alarms over a pointless destruction coming from students in the field were just beginning when Denevan wrote this 1973 piece (Smith 1982; Moran 1983; Schmink and Wood 1987). These books with their 1980s publication dates reflected the growing apprehension about what development would actually mean in Amazonia.

The loss of wildlife and the use of charismatic mega fauna as the ecological ambassadors for forest conservation were still on the horizon. The need for bush meat and the value of beautiful animal pelts still drove animal exploitation which had had an explosive period in the immediate postwar time. The CITES (Convention on the International Trade of Endangered Species) agreement only came into force in 1975, and rampant overhunting for food and for sale was the norm. The impact of these transformations has kept battalions of zoologists at work chronicling the complexity of wildlife dynamics under the current conditions of tropical change.

Denevan, along with botanist Paul Richards, was one of the first to call attention to the genetic value of these rainforest systems. This utilitarian optic had hardly been addressed at the ecological or pharmaceutical level at the time of his writing. Moreover, the astonishingly rich work of Harvard ethnobotanist Richard Schultes (1956, 1979, 1987; Schultes and Raffauf 1990; Smith and Schultes 1990; Davis 1996) reviewing tropical rain forest genetic value largely comes after this article. Schultes was well aware of this value having spent a considerable time prior and during WWII in collecting the *Hevea as* well as documenting almost every other kind of latex plant in Amazonia in response to the truncated supply as Asian rubber plantations – the source then of most of the world's rubber – fell to axis powers. As a discipline and research line however, the situation was incipient: there were a few comments on economic botany in the annals of almost every Amazonian explorer, and the dull but necessary reports: "Useful Plants of…" compendiums--a sort of colonial style of useful plant listing that crammed the shelves on imperial botany, but the biodiversity boom (and biopiracy fears) emerged more than a decade later with Merck's bioprospecting in Costa Rica, and Shaman Pharmaceuticals work in Amazonia, and with it the general alarm over the pirating and patenting of indigenous cures and plant substances (Conklin 2002). These triggered significant activism about the intellectual property rights of native populations (Elisabetsky and Posey 1994; Argumedo et al. 1996; Posey and Dutfield 1996; Ellen et al. 2000). The implications of Amazonian biodiversity for regional history, for social history, and for planetary history still lay in the future.

At the time of Denevan's early writing, climate issues were barely on the table. The studies of environmental consequences of nuclear war concentrated the climate questions around the pressures of nuclear winter as the main impact of human interference with climate. Still in the future were climatologist James Hansen's testimony to the US Congress and the birth of the IPCC with its relentless compendium of human effect impacts. Other question raised in the article – such as the effects on what we call now non-timber forest products – were barely on the radar and certainly not the center of a raft of policies and economic incentives to support economic forests that they later became (Grimes et al. 1994; Baraloto et al. 2014; Cotta 2017; Ribeiroa et al. 2018).

The work of Bill Denevan stands quite remarkably ahead of the pack, in his empirically rooted, historically inflected rigorous attention to how people might engage tropical forests. Through rigorous field research, historical geography, environmental history, and engaged writing, he provided the theoretical basis and the inspiration for the studies of a humanized Amazon, an Amazon that was certainly our past, and one which might have formed the basis for a justly inhabited Amazonia, instead of the one that is currently falling under the onslaughts of cattle and soybeans (Oliveira and Hecht 2018).

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4.1 Development and the Imminent Demise of the Amazon Rain Forest

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Abstract Given current [1971] and projected rates of deforestation in Amazonia, most of the undisturbed forest will soon be gone. It will be replaced by grassland, scrub savanna, crops, and second growth forest. The causes are development (cattle ranching, logging, monoculture, mining) and population growth from massive migration facilitated by extensive road building. "A disaster of enormous proportions" is imminent.

Keywords Amazon · Climate · Deforestation · Economic development

Introduction

Within one hundred years, probably less, the Amazon rain forest will have ceased to exist. Some Brazilian planners speak of the year 2050, but the tropical ecologist Paul Richards suggests that if present trends continue most of the undisturbed Amazon rainforest "may vanish" by the end of the current century (Richards 1970, 194). It will have been replaced, for the most part, by grassland and scrub savanna, with some second growth forest. The prospect seems inconceivable to those of us who have flown over the endless vastness of Amazonia, but it is just a matter of time given current and projected trends in Amazonian development. What are the causes, and what are the consequences of what may be the most intensive destruction of extensive forest in the history of the world?

All the South American countries with Amazonian territory now have major programs for developing their tropical lowlands. The western nations are "marching" to the east, and Brazil is “marching” to the north and west. Plans exist or are being made to move hundreds of thousands of families into the Amazonian fringes and interior, to convert tropical forest to pasture, and to develop commercial agriculture, logging, and mining. At great cost, roads are being built, and the infrastructure for colonists and settlements is being established. The reality of what is occurring is dramatized by ambitious transcontinental highway projects: the Carretera Marginal de la Selva, partially completed, along the eastern Andean slopes and piedmont from Venezuela to Santa Cruz, Bolivia; the completed Brasília-Belém and Brazilia-Acre highways; and the east-west Transamazonian route, begun in 1970, from Recife to the Peruvian border, with major branches to Santarém and Cuiabá. The function of these and many lesser roads is not so much to connect two distant points as it is to open up new land to agricultural settlement. The FAO predicts that in the year 2071 the world will be able to support 36 billion people at the level of a North American diet, based mainly on breakthroughs in food production via desert irrigation and “intensive arable farming [of] some seven million square km in the Amazon Basin” (Pawley 1971, 22–27).

Newspaper, magazine, and television accounts abound on these projects and projections as public interest has been caught up in the excitement of the conquest of “the last great frontier.” A solution to overcrowding on the coasts and in the Andean highlands is seen. Armchair development planners promote grandiose, but questionable schemes such as that of constructing dams on the Amazon and its tributaries to create enormous lakes. Furthermore, there may be more geographers working on frontier colonization in tropical South America than on any other single topic. Certainly, more of us should be concerned about and be engaged in research on the possible long-range human, ecologic, and economic effects of Amazonian development.

How much of Amazonia has already been affected? There are no reliable calculations, but probably at least 5 percent of the non-riverine tropical forest has been cleared in the past 20 to 30 years. Before that, most settlement was restricted to the rivers because of inaccessibility to the *terra firme*. Some idea of how fast tropical slash-and-burn settlers can work their way through a forest is contained in a report by Hilgard Sternberg indicating that two million ha were cleared in the Magdalena Valley of Colombia in just 10 years (Sternberg 1968, 423). The Carretera Marginal is expected to influence about seven million ha (Snyder 1967, 88). The Brazilian government has set aside a 12-mile (20 km.)-wide strip for colonization along the Transamazonian (5000 km) and Cuiabá-Santarém (1500 km) highways, a total of 13 million ha, an area larger than New York state (Brazil Ministry of Transportation 1970, 20).[3] The potential magnitude of future settlement can be seen from the reported increase in population from 100,000 to 2,000,000 and from 10 to 120 settlements along the Belém-Brasília highway (2123 km) between 1960 and 1970 (Brazil Ministry of Transportation 1970, 22).

This observer has not been over the new Brazilian highways but is familiar with many of the colonization areas opened up by cross Andean or lateral foothill roads

[3] For a discussion of the enthusiasm, as well as the controversy, within Brazil over the Transamazonian Highway, see Pereira (1971).

in eastern Peru and Bolivia and along the Venezuelan piedmont. Flying over or driving through such areas, one is impressed by the tremendous amount of forest clearing that has taken place.

Within a decade or two, the foothill forests will have been destroyed, except for some very wet sections, since soils and climate are generally more favorable than in the low selva, and distances to highland markets and populations are relatively short. During the same period, most of the available good alluvial agricultural land will be taken up on the levees and floodplains of the major lowland rivers. Subsequently, more and more feeder roads will radiate off the transcontinental Amazonian highways and out from the river towns, as they already do from Manaus, Belém, and Pôrto Velho, into the interfluve forests with their fragile soils.

The ecological results of relatively uncontrolled shifting cultivation by a concentrated population of settlers can be seen in many parts of western Amazonia. Farm plots cleared along penetration roads, for both spontaneous and planned colonies, have not evolved, after 10 years or so, to a stable short-fallow shifting cultivation or to a form of more intensive or permanent cultivation, nor have they reverted back to forest. Instead, we find non-agricultural grazing land in the form of open-to-dense scrub savanna, varying with local soil and climate, frequency of fire, and livestock management practices. Few if any agricultural settlers can be seen. Most of the original small holdings have been consolidated into small- to medium-sized ranches. The extensive agricultural system of shifting cultivation has been replaced by livestock grazing, an even more extensive land-use system, utilizing relatively little labor. Most of the original settlers have moved on to clear new land where labor inputs for fighting weeds and maintaining fertility are much less and crop yields higher than for older farms.

In Venezuela, the massive conversion of tropical forest to scrub savanna is well known, as for example, along the new highway from San Cristóbal to Barinas in the southern Andean piedmont. There is continuous pasture but seldom a farm and seldom mature forest. Henry Sterling (unpublished) using aerial photographs and 35 mm slides has carefully documented the transition from forest to pasture near Santa Barbara in the same region between the years 1952 and 1966. The deforestation of the southern Maracaibo lowlands is described by Carl Lindstrom (1972). In the upper Amazon, Ray Henkel has demonstrated the same trend in the Chapare region of Bolivia, where he has mapped a three-stage economic/settlement sequence: Pioneer Fringe, Commercial Core, and Zone of Decay (Henkel 1971). Many other examples could be cited, such as along the Tingo María-Pucallpa highway in eastern Peru. The pattern seems to be similar in Brazilian Amazonia; the potential for eventual ecological and agricultural disaster is particularly well exemplified by the 90-year-old Bragantina (Bragança) colonies east of Belém, where the once forested land has been described as now barren and the population as no longer able to feed itself (McNeil 1972, 604–605).

Thus, present-day land use, including frequent burning and even the use of defoliants, brings about a rather permanent degradation of tropical rain forest land in terms of its ability to support quantity and diversity of plants and animals, in marked contrast to long-fallow aboriginal cultivation, which generally permitted forest and wildlife recovery. Briefly, then, what are some of the ramifications of massive forest destruction in Amazonia?

Climate Several scholars have speculated on possible climatic effects of deforestation in the Amazon Basin. Wilford Portig notes that "the evapotranspiration of a dense evergreen tropical forest is practically equal to the evaporation of an open water surface of the same area," and if the forest is replaced, the regional climates of central Brazil or even a larger area could be significantly affected, with possibly lower rainfall (Portig 1968, 376). Reginald Newell believes that tropical forest removal may significantly affect latent heat, cloudiness, water vapor transport patterns, horizontal heat and momentum transport, and convergence-divergence patterns (Newell 1971, 457–459). Sternberg points out the possible effects on temperature and rainfall of extensive, long-lasting smoke haze from swidden and grass fires (Sternberg 1968, 434–435).

Soils The effect on soil structure, moisture, and organic content of the conversion of tropical forest to savanna is succinctly described by Gerardo Budowski (1956, 23–33). The considerable chemical and physical differences between forest and savanna soils are explained in detail by Nye and Greenland (1960). With a few exceptions, the natural fertility of savanna soils is considerably inferior to that of tropical forest soils.

Hydrology Deforestation will certainly affect rates and quantities of water runoff and sediment movement, and river characteristics and behavior could be modified on a continental scale with effects that have yet to be considered. Likewise, there is little knowledge about the results of direct human reshaping of drainage systems and the creation of lakes.

Wildlife Many animal species in Amazonia, such as the tapir and manatee, are already seriously threatened by settlers hunting for food. Other threatened species are hunted for their hides (jaguar, caiman), or for sale to laboratories, zoos, and individuals (birds, monkeys). Game is seldom seen now near permanent settlements; long stretches of river are deficient in turtles and certain fish; and even in the interfluve forests, animals are often less numerous than expected (Heltne 1967, 134).[4] Even though national and international restrictions are placed on the export and import of skins and live bodies of endangered species, many forms of wildlife will vanish if the Amazon forest is destroyed.[5]

Genetic Material Paul Richards and others have stressed the role of the tropical rain forest as a genetic reservoir for future evolution and as a source of surviving or new forms of plant life, many of which could be of use to people. Destruction of the rain forest and adjacent formations is closing off "the invasion of the subtropical and temperate regions by plant lineages evolved originally in the tropics" (Richards 1957, 405). There is, of course, a tremendous diversity of life forms in the Amazon rain forest, but most species, especially of flora and insects, have yet to be identified and described. "With the present rate of destruction of the tropical rain forest throughout the world, there is great danger of mass extinction of thousands of species" (Gómez-Pompa et al. 1972, 764).

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[4] Also, see the follow-up letters in *Science:* 157 (1967), 991–992; 158 (1967), 717; 159 (1968), 147; 159 (1968), 1052–1053; 160 (1968), 251–252; 161 (1968), 520–522; 162 (1968), 53–55; 162 (1968), 1432–1433.

[5] Also, see Bennett (1971, 33–40).

Scientific Values Another point often made is that the rain forest is worth saving for its great value as a scientific laboratory. "In the past, the tropics have contributed an immense amount to man's knowledge of nature. Much of what we know about the evolution of plants and animals and even of humans was learned by naturalists working in tropical forests… If the jungle is destroyed before we have had a chance to study it, whole chapters in biology may never be written" (Richards 1970, 199).

Esthetics And finally, to return again to Richards: "…the jungle is a place of mystery and beauty. To visit the rain forest is to be overwhelmed by the variety and complexity of a unique living world of nature that has flourished for millions of years. Why save the jungle? Come with an observant eye and the question will answer itself" (Richards 1970, 199).

Someday, most of Amazonia may look like Madagascar, where, out of a total area of 228,000 square miles, apparently all are originally forested, only 12,000 square miles of mature forest survive, with 16,000 square miles of secondary forest; most of the remainder is now savanna (Gourou 1966, 73–74). Sections of mature rain forest may survive in Amazonia in a few national reserves, and there will always be some secondary forest. However, many plants and animals do not live in such forests, nor will such forests return to an original form if primary rain forest does not exist nearby to serve as a source of seeds and wildlife. Most species of the mature rain forest are incapable of recolonizing extensive areas cleared for agriculture, because they are not preadapted to disturbance conditions, nor are their seeds capable of long-range dispersal (Gómez-Pompa et al. 1972, 762–765). Forest has been cleared so long and so extensively in much of Southeast Asia and Africa that it is doubtful the original forest could ever return, even if the soils did recover.

The ecological prospects disturb many of us, but putting the above arguments aside, what about the oft-argued primacy of people, of economic development? "Development" is the cause and justification of the destruction of the Amazon rain forest. The Brazilians, as made clear to the rest of the world at the United Nations Conference on the Human Environment at Stockholm in 1972 (Wolff 1972, 44–46), are resentful when environmentalists speak of preserving Amazonia for trees, jaguars, and Indians,[6] when there is a national need for increased food production and resource development. A rationale often expressed is that a certain amount of pollution must be tolerated in order for development to occur and that environmental protection cannot be economically or socially afforded until after there has been development (Ludwig 1971). This is not to say that Brazil is unconcerned about ecological dangers. The government officially supports the study of ways to preserve the existing ecology of Amazonia, such as through the research program of the Instituto Nacional de Pesquisas da Amazônia centered in Manaus. The Institute believes in a rational development policy which would stress the use of alluvial soils for agriculture, existing grasslands for livestock, and a controlled use of the interfluve forests for tree crops, timber, and wildlife resources (Machado, Pers. comm.

[6] Current concern about aboriginal "genocide" and "ethnocide" in Amazonia is reflected by numerous publications and meetings, such as the Barbados Symposium in 1971.

1972).[7] Nevertheless, the trend remains one of massive forest destruction. A strong argument can be made that such destruction may serve the interest of immediate development but will be self-defeating in the long run.

The ecosystems replacing the rain forest are less productive biologically and generally are of lesser economic value. If the rain forest is destroyed, a great source of actual or potential raw materials (timber, cellulose, resins, gums, nuts, fruits, fibers, drugs) will be lost. Furthermore, the soils of the scrub savannas, compared to those under forests, are characterized by low fertility owing to a low level of organic input and nutrient storage and by an initial high level of leaching; runoff and erosion are greatly accelerated following forest clearing (Nye and Greenland 1960, 135; Watters 1971, 31–36). Laterite and hard pan formation is common. At the Iata colony in Rondônia, Brazil, for example, laterite formation turned fields into virtual pavements of rock within five years after clearing (McNeil 1972, 605). Consequently, it takes much greater technological, chemical, and/or human inputs to make the savannas produce reasonable crops and pasture. There may be, nevertheless, some degree of settlement and economic success; however, we can generally expect extensive ranching of mediocre quality cattle, with relatively high living standards but few people benefiting. Furthermore, there may be labor-intensive forms of agriculture supporting fairly dense rural populations, but at a bare subsistence level, as in much of Africa and Asia today. Physical survival in large numbers may be possible on marginal land because of high individual labor inputs, whereas the land is so marginal that labor-saving and yield-increasing devices are simply uneconomical. Moreover, we should be wary of economic development schemes for the savannas. Many have failed in Africa, and other apparent successes, such as the Guarico project in Venezuela, turn out to be highly subsidized.

In conclusion, road building and colonization in Amazonia probably should be restricted for now, and other solutions to overpopulation and poverty in South America should be sought more vigorously. The rain forest can be no more than a temporary safety valve in any case. It is unlikely that the Amazon Basin will become a desert, as sometimes claimed (Anderson 1972, 60–64), but it could become a wasteland with greatly reduced opportunities for plants, animals, and people.

Answers are not suggested here to the obvious ecological problems of economic development of the tropical rain forests,[8] but Latin Americanist scholars, planners, and governments need to give much more attention to the negative and long-term aspects of wide-open tropical colonization than they heretofore have – while there is still time for finding and implementing sound alternatives. There is little indication of a slowing of the pell-mell, destructive rush to the heart of Amazonia, but rather the opposite. In the words of geographers at the Instituto Brasileiro de Geografia, "a disaster of enormous proportions" is imminent.[9]

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[7] A similar point of view was expressed by the former director of the Servicio Nacional de Pesquisas Agronomicas in Brazil, F.C. Camargo (1958).

[8] See, for example, Tosi Jr. and Voertman (1964); and Dickinson III (1972).

[9] This quote appears in Betty Meggers (1971, 154) in a chapter discussing the impact of the modern world on Amazonia. For recent general discussions by geographers of human influence on the ecology of tropical Latin America, see Sternberg (1968) and James Parsons (1971). For a review of geographic research on Amazonia, see Edmund Hegen (1971).

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4.2 Pre-European Human Impacts on Neotropical Lowland Environments

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Abstract The pre-European New World lowland tropics (Neotropics) in South America, mainly Amazonia, were long believed to have contained sparse human populations with minimal impact on the physical environment. Recent studies, however, have shown that this is a misconception. Populations were relatively large in some places, and even low densities of people had some impact. Changes over a considerable period of time occurred in vegetation, wildlife, soils, land surfaces, and river patterns. The evidence is from archaeological excavation and from paleoecology including soils, pollen, charcoal, and plant and animal remains. Ethnohistorical and ethnographical information can be projected backward if reasonable. Forests were altered in composition. Savanna was created by clearing and burning. Wildlife was depleted. Fertile black soils were developed. Earthworks were constructed, including raised and drained fields, causeways, canals, mounds, wells, and fish traps. The degree of environmental change resulting from human activity varied from minor and not noticeable to significant and still visible today.

Keywords Human impacts · Landscape modification · Pre-European South America

Introduction

An ecology without Man... is true only for an environment without Man. Sauer (1958, 107)

The topic of early human impacts on New World environments, including Amazonia, is controversial as to degree and extent (Balée 1989; Cleary 2001;

Denevan 1992b; Gómez-Pompa and Kaus 1992; Hayashida 2005; Krech III 1999; Stahl 1996; Vale 2002). Certainly, whenever and wherever people were present, even sparse populations, there was some change in the landscape. People cannot live on the land and use plants and animals for subsistence and other needs without changing that land and the plants and animals present. Human-induced changes may involve equilibrium in which the natural ecosystem basically recovers, even though composition is changed, or in which the human ecosystem is sustainable; or the changes may involve disequilibrium in which the regenerative capacities (i.e., biological diversity and productivity) of either system are retarded or destroyed (i.e., degradation) (Sponsel 1992). These changes may have been intentional or not, ephemeral or long lasting, localized or regional, one-time events or cumulative, and highly visible or not readily apparent.

Vegetation is the most widespread focus of change; other impacts have been on wildlife, soil, river patterns, microclimate, and the land surface itself. The forces of change are settlement, cultivation, grazing, hunting and gathering (foraging), burning, and various earthworks and river works. All of these impacts and forces were present in pre-European South America. Furthermore, native people "recognize that human beings, past and present…have affected the distribution of the biota and the formation of the landscape" (Balée 2003, 285–286). "Far from being a wild world… the forest is perceived as a superhuman garden" by the Achuar in Ecuador (Descola 1994, 324).

Lacking written records, it is difficult to reconstruct early indigenous impacts, and any attempt to do so involves speculation and inference. Some alterations have persisted to the present, but most have been masked by either landscape recovery or by human destruction. Furthermore, nature and culture merge, the dichotomy being artificial and conceptual (Descola 1994, 1–6). Furthermore, some human disturbances that seem old are actually recent, but the reverse is also true. The distinction may be difficult to ascertain.

Archaeology and landscape analysis can be informative, but they are nevertheless limited in spatial coverage. Ethnohistorical and ethnographical information can be projected backward, and this is done here; however, we do not know for certain whether such information is representative of pre-1492 people (Erickson 1995; Roosevelt 1989). There is paleoecological evidence of human impacts: soils, pollen, charcoal, and plant and animal remains. Finally, reconstruction of past human impacts is complicated by the fact that natural environmental change (especially climate) has occurred along with anthropogenic change. As result, separation of human from natural factors in explaining past change is difficult. Also, fire is a major cause of vegetation change, but often it is not clear whether the source of ignition is natural (lightning) or human, even today. Geographers have been particularly concerned with sorting out the role of humans in environmental change; however, the topic is interdisciplinary.

There is considerable awareness now of prehistoric human impacts on the Neotropical environment, but there are earlier antecedents. The pioneer botanist, Carl F. P. von Martius, who was in Brazil in 1817–1820, "noted that in the Amazon nature had already been transformed by humans for several millennia" (in Barreto and Machado 2001, 243). Furthermore, Alexander von Humboldt (1869, 193) in 1808 said that it was a "misconception" that tropical forests were "primeval."

This chapter focuses on the Amazon basin, the area I know best, and the Orinoco region, the Guyanas; parts of Colombia and Venezuela, Panama; the humid Pacific coast of Ecuador; and the Brazilian coastal region.

Amazonia, scene of great human activity today, is generally perceived to have had sparse pre-European populations, with widely dispersed, small, temporary villages, with extensive forms of subsistence and with a simple stone-tool technology. This image has undergone a radical transformation in recent years (Denevan 2001, 130–131; Heckenberger 2005; Roosevelt 1999a; Stahl 2002; Viveiros de Castro 1996).

Early populations of course were small and scattered. The oldest known human presence in Amazonia, around 11,200 BP (before present), is at Pedra Pintada in the upland near Monte Alegre just north of the lower Amazon River (Roosevelt 1999b). Here, subsistence was initially based on the collection of fruits, fish, shellfish, and small game. Trees with edible fruits were undoubtedly spread by Paleo-Indians (pre-agricultural). By 8000 BP, these foragers had become specialized fishers and shellfish collectors, leaving enormous shell mounds along the rivers. In the uplands, subsistence was broad-spectrum foraging for wild plants, fish, birds, and small animals. Projectile points were fine and rare, probably used for large fish and aquatic mammals. Big game was of minor importance, as confirmed by the bone assemblages. There is no indication of overhunting or severe depletion of fish or shellfish by Paleo-Indians or subsequent hunters and gatherers.

Hunting and gathering people were fully capable of having an impact on the distribution of useful plants and animals, as evidenced by groups that still survive, such as the Nukak: "The foragers of the past were without doubt builders of their environment" (Politis 1999, Politis 2001, 48).

In Panama and in Colombian Amazonia, several domesticated crops were probably being cultivated by 7000–10,000 BP, including yams *(Dioscorea* spp.), lerén *(Calathea allouia),* arrowroot *(Maranta arundinacea),* bottle gourd *(Lagenaria siceraria),* squash *(Cucurbita* spp.), and sweet potato *(Ipomoea batatas).* In Panama, manioc *(Manihot esculenta)* was present at 7000 BP and maize *(Zea mays*) between 5500 and 7000 BP (Piperno and Pearsall 1998, 203–227; Piperno et al. 2000). By 4000 BP, there is evidence of major forest clearance in several regions, apparently for maize agriculture (Piperno and Pearsall 1998, 312–313).

The population of Greater Amazonia in 1492 is now believed to have numbered at least five and one half million (Denevan 1992a). There were large permanent villages stretching for kilometers along the bluffs of the Amazon River and major tributaries, with some settlements probably totaling 5000 or even more people (Denevan 1996). In the interior interfluves or uplands *(terra firme),* there were both small villages and scattered, large semi-permanent settlements, in contrast to historical villages which are invariably small and shifting. In some of the savannas, there were also substantial populations (Denevan 1966; Roosevelt 1991, 38–39). In the floodplains, cultivation was seasonal and very productive. In the uplands, where shifting cultivation dominates today, it is apparent that intensive, semi-permanent forms of cultivation were practiced in the past in some areas (Denevan 2001, 115–127). Given such populations, settlements, and intensive cultivation, an argument can be made that in 1492 much of Amazonia was not pristine but had been modified in various ways, ranging from insignificant and subtle to complete transformation. Even sparse hunting, gathering, and fishing people had some effect.

Forest Modification

"The indigenous populations of Central and South America have been using, manipulating, and managing tropical forests for several thousand years ... [but] to the untrained eye, the managed and the pristine can easily merge into one" (Peters 2000, 203–204). Amazonian forests occupied by native people today are anthropogenic in various ways, but even more so in pre-European times when rural populations were in some places greater than they are now. Current forms of biotic modification are indicators of what may have taken place in the past – change in species presence, distribution, and density, as well as in forest biomass, age, structure, and other characteristics (Balée 1994, 16–165; Peters 2000; Rival 2002, 68–93; Roosevelt 2000; Smith 1995). Some of these changes can persist in some form for hundreds of years. In abandoned fields, full recovery of forest biomass can take 190 years in the upper Río Negro region; recovery of species diversity can take up to 80 years (Saldarriaga et al. 1988; Brown and Lugo 1990).

Fallows as successional vegetation may actually have a species diversity comparable to or greater than that of a mature forest, including a number of rare species, although a different kind of diversity (Balée 1993, 1994, 136–137; Brown and Lugo 1990). Fallows may be actively managed and utilized for several decades by maintaining a high proportion of useful plants (Denevan and Padoch 1987; Peters 2000). After a plot is last harvested, residual crops may be protected, fruit and other useful trees may be planted, and certain secondary species may be favored. Examples from the Bora fallows in Peru are uvilla *(Pourouma cecropiaefolia),* peach palm *(Bactris gasipaes),* guaba *(Inga* spp.), caimito *(Pouteria caimito),* umarí *(Poraqueiba sericea),* and cashew *(Anacardium occidentale)* (Denevan and Padoch 1987, 41).

Mature fields are also manipulated. Unwanted saplings are replaced by weeding or felling, and new useful trees are introduced by planting or by protection of desirable volunteers that otherwise might be shaded out. Useful plants may be established at tree-fall openings, which are more frequent than realized (Denevan 2001, 125–126). The result is to "increase the density of desirable species by decreasing the density of undesirable ones" (Peters 2000, 211). The early successional vegetation in clearings and at burns tends to have a higher concentration of edible plants than does mature forest (Piperno et al. 1992).

Some abandoned plots enriched by one or a few species may be able to maintain that dominance for centuries (Smith 1995) or indefinitely (Peters 2000). Old Ka'apor village sites often contain dense stands of tucumá palms *(Astrocaryum vulgare)* and babassu palms *(Orbignya phalerata),* which may also dominate burned clearings (Balée 1992, 47–48). The Kalapalo manage groves of piqui *(Caryocar brasilensis),* which extend for miles around abandoned villages (Basso 1973, 35). Various other types of dense stands of palm and other fruit trees occur on archaeological sites (Balée 1988; Peters 2000). Numerous forest trees (wild orchards) develop from discarded seeds around settlements, camps, trails, and activity areas (Politis 1999, 2001). Other examples of trees providing prehistoric cultural signatures, including the Brazil nut *(Bertholletia excelsa),* are given by Smith (1995).

In addition, there may be intentional planting of cultivated seeds and tubers carried from fields and gardens to forest areas adjacent to villages, camps, trails, and fishing sites. Aside from settlements, there are thousands of kilometers of indigenous trails today in central Brazil, and undoubtedly, there were many more in the past. Posey (1985) along just 3 km of a Kayapó trail found 185 planted trees, about 1500 medicinal plants, and about 5500 food plants. Also, there are small hidden patches in the forest consisting of both cultivated and semi-wild plants that serve for emergency food supplies. These may occur at tree falls or where a tree has been felled to get at honey or fruits.

Foraging for useful wild plants can affect species presence and distribution. Undesired plants are removed, but other selected plants may be protected and even planted. "More than 76 percent of the species used by the Kayapó that are not domesticates are nonetheless systematically selected for desirable traits [genetic manipulation] and [are] propagated" (Posey 1992, 28–29).

Hunting is another factor. Some hunted species disperse plant seeds, so that plant patterns are affected by game depletion. Various mammals, especially monkeys and rodents such as agoutis *(Dasyprocta* spp.), are dispersers of fruit and nut seeds. Likewise, bird populations, important for pollination as well for dispersing seeds, are affected by hunters. In addition, there is an abnormal seed dispersal by animals attracted to human settlements and fields, thus influencing fallow compositions (Balée 1993).

A good example of the creation of a partly humanized rainforest comes from the Huaorani (Jivaro) of the Ecuadorian Amazon (Rival 2002, 68, 70, 80, 83, 91–92):

[M]en, women, and children spend hours "cruising" in the forest…collecting food within a radius of 5 km.... They explored the forest systematically, looking for ... evidence of previous occupation [where useful plants are concentrated] ... [the forest] is conceptualized as a patchwork of successional fallows … numerous plant species are encouraged to grow outside cultivated areas. ... [They] exploit plants where they find them in the forest. As a result, they actively manage the forest ... by altering the natural distribution of plant and animal species... Past people are thought to [have] provide[d] in abundance for their descendants…the forest, far from being a pristine environment, is the product [or "historical record"] of the life activities of past generations that have transformed it into an environment rich in resources.

Other excellent studies of the impact on tropical forests by foraging activity are for the Hotï in the Guayana region of Venezuela (Zent and Zent 2004) and for the Nukak in Colombian Amazonia (Politis 1999, 2001).

Paleoecological data from the rainforests of Darién in southern Panama provide a 4000-year record of human disturbance. The forest and wildlife of today have recovered from that disturbance, following the Spanish conquest and the subsequent demise of the native population. However, early disturbance "may have had a profound effect upon the modern forest associations" (Bush and Colinvaux 1994, 1761). In central Panama, major forest disturbance and deforestation had taken place by 3000 BP, if not earlier. "That prehistoric hunter/gatherers and shifting cultivators using 'primitive' technologies exerted considerable influences on the structure and species composition of tropical ecosystems [in Panama] no longer seems in doubt" (Piperno et al. 1992, 123). Although separating human impacts from climate-induced change is difficult, pollen studies indicate that human agency in rainforest disturbance in western Colombia probably has been "significant from the [early] Holocene onwards" (Marchant et al. 2004, 833).

The overall impacts of these types of activities may seem to be minor, but extent and cumulative effect can be significant. Fallows are managed, not natural, regrowth. In a study of young Bora fallows, 645 plants were collected along transects, and of these 207 (32 percent) were identified as being useful (Denevan and Padoch 1987, 52). Peters (2000) reports small tracts of forest with as many as 300 useful species present. Cultural surveys of forest trees in Amazonia indicate proportions of species that are useful for several indigenous groups: 82 percent for the Chácobo, 77 percent for the Ka'apor, 61 percent for the Tembé, and 49 percent for the Panare (Balick and Cox 1996, 183). The use of any kind means a degree of human impact on the plants involved. Furthermore, human presence increases the density of useful plants. In Yucatán, botanist David Campbell (Pers. comm. 2000) found that in a 10-km radius of Mayan villages today, 70–90 percent of the trees present have utility, compared to only 50 percent or less in more distant areas. Also, traditional pharmacopoeias (medicinal plants) in the Neotropics mostly occur in disturbed forest, not in primary forest (Voeks 2004).

Balée (1989) estimates that at least 12 percent of the current Amazonian forest in Brazil is mostly anthropogenic, consisting of babassu forest, bamboo forest, liana forest, and Brazil-nut forest. This does not include smaller patches of artificial forest or extensive areas of manipulated forest. These anthropogenic forest modifications involve recent impacts by indigenous people; however, they are indicative of human impacts that undoubtedly occurred in preEuropean times.

Amazonian forests, even where semi-deciduous with a long dry season, or during occasional severe droughts, are infrequently ignited from natural causes, major fires "perhaps occurring only once or twice per millennium on average" (Cochrane and Laurance 2002, 321). This is because lightning without heavy rain is rare during dry periods. However, early Indians burned their clearings and burned for other reasons. During droughts, these fires could escape, causing forest fires that could be extensive, as we know from recent conflagrations. Forest recovery of biodiversity from hot forest fires can take many years (Fearnside 1990), varying with fire frequency and extent (Cochrane and Schulze 1999).

Savanna from Forest

Savanna (tropical grassland with or without scattered or clustered trees) occupies about 30 percent of Greater Amazonia. The largest savannas are the scrub grasslands *(cerrados)* of central Brazil, the Pantanal of Mato Grosso, the Orinoco Llanos, and the Llanos de Mojos of northeastern Bolivia. Some are well drained *(cerrados)*, and others are subject to seasonal flooding (Mojos, Pantanal), while the Orinoco region has sectors of both.

Grasslands are explained in the wet tropics as being the result of extremely weathered (senile) soils *(cerrado* landscapes), alternating flooding and desiccation (Mojos, Pantanal, Orinoco), clearing and burning, or a combination of these in conjunction with long dry seasons. There has been a long and continuing debate over the extent to which the South American savannas are natural versus anthropogenic, with the consensus now being that most are natural (Sarmiento 1984). They have clearly been expanded, however, by forest clearing in association with savanna burning, and some small savannas are primarily the result thereof.

Today, all the South American savannas are burned frequently, even annually, by both natives and other people, and there are historical reports of past indigenous burning. It has been assumed that such fires will gradually push back a forest-savanna boundary at the expense of the forest. However, observations by myself in Mojos and by others elsewhere (Eden 1986) suggest that boundaries may be fairly stable and that in some places the forest is even advancing. This is because the forest is moist and relatively fire resistant, so that grass fires do not penetrate. The forest/grassland boundary in Mojos can contain fire-tolerant plants, such as Bromeliaceae, *Heliconia* spp., certain palms such as motacú *(Attalea princeps),* and species of *Acacia, Mimosa, Cassia,* and *Curatella.* Where clearings occur at a forest edge, however, savanna fires can move into abandoned fields, thus expanding the savanna. The extent of such expansion over thousands of years is difficult to estimate. On the other hand, recent research in eastern Brazilian Amazonia indicates that anthropogenic fire frequencies of less than 20 years, common in savannas, can by themselves eradicate rainforest trees for a distance of 500–2500 m from the forest edge during a short period of time (Cochrane and Laurance 2002).

Regardless of how much savanna fires can cause forest retreat, fires do have an effect on the woody plants and grasses within savanna, favoring fire-tolerant species and affecting biodiversity. Moreover, while natural fires do occur, their frequency is far less than human burning, which, as result, has a different type of impact on vegetation.

There are examples of savannas created by indigenous people at various times. Figure 4.2 shows forest clearing from 2200 to 9000 BP, based on a long-term study of sediment cores in central and western Panama (Ranere 1992; Piperno and Pearsall 1998, 290–297). Over half this region was cleared of primary forest by 2200 BP. In eastern Panama, pollen data for the past 4000 years show a surge of *Gramineae* (*Poaceae*), indicating grassland, and disturbed forest taxa with charcoal influxes indicating burning. The forest recovered after AD 1600, which would be after native depopulation (Bush and Colinvaux 1994). By about 3300 BP, “significant forest modification was certainly underway” on the north coast of Ecuador (Stahl 2000, 254). In the sixteenth century, Spaniards described open, unforested sectors, including in Darién at the time of Balboa and in adjacent Dabeiba in present Colombia, as well as in western Panama. Soon after, much of this reverted to forest, except on cattle ranches (Sauer 1966, 285–288; Bush and Colinvaux 1994).

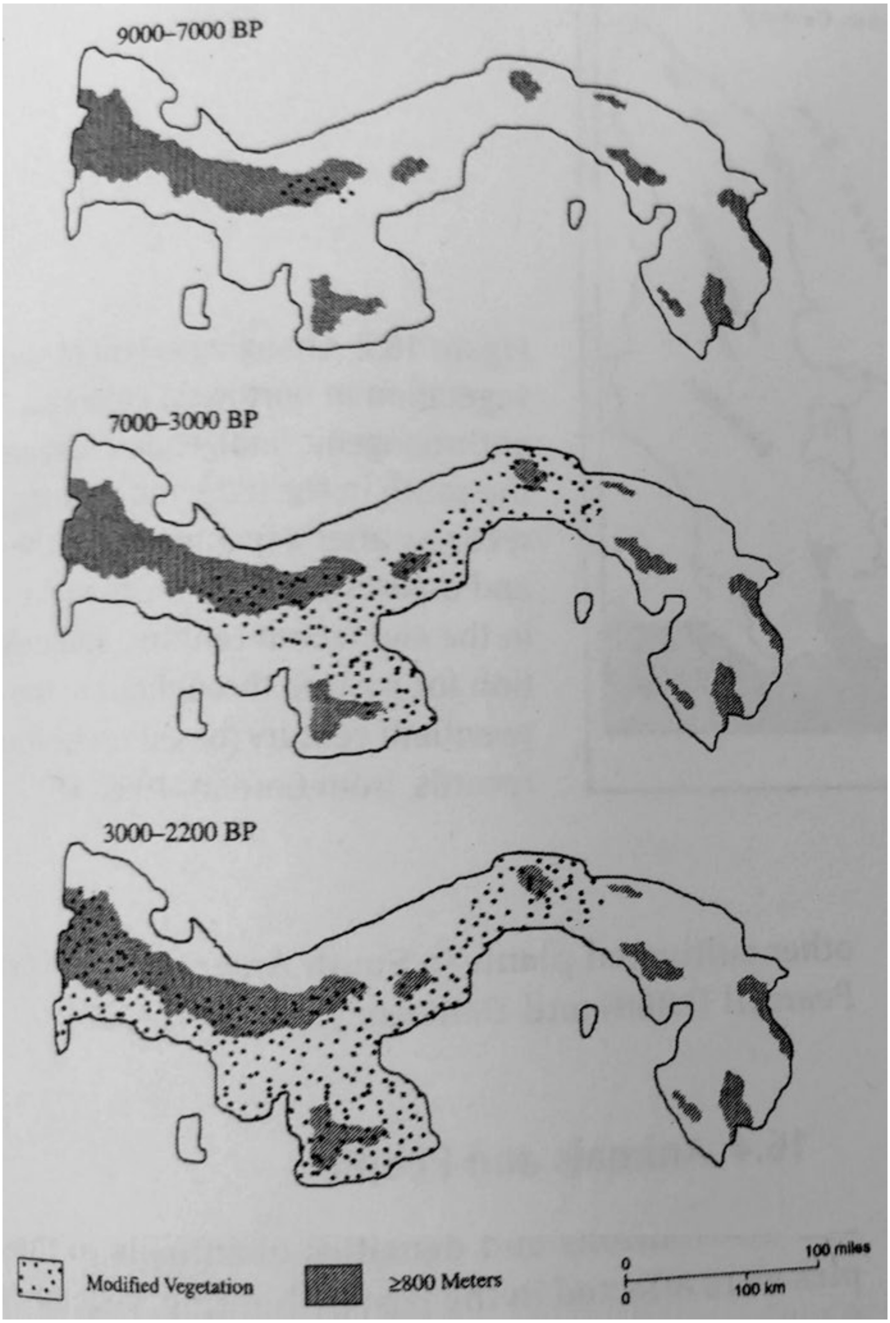

Fig. 4.2 The known distribution of disturbed vegetation in Panama, including deforestation and conversion to savanna, for three periods prior to 2200 BP based on pollen, phytoliths, and other indicators. (From Ranere 1992, 41)

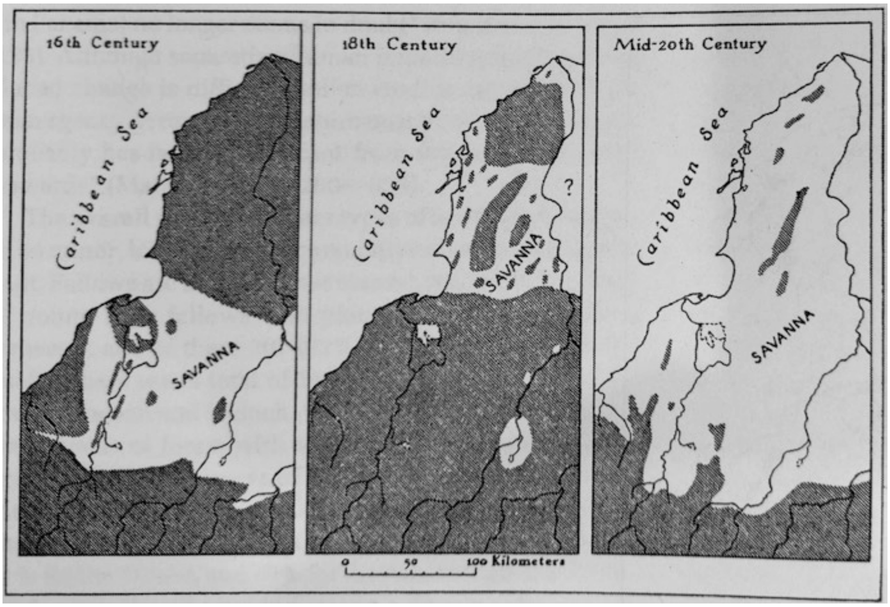

Fig. 4.3 Changing extent of savanna vegetation in northwest Colombia: anthropogenic (indigenous) savanna in the south in the sixteenth century; forest recovery after depopulation in the south and deforestation (Spanish) in the north in the eighteenth century; and deforestation for pasture throughout in the mid-twentieth century. (Based on historical records, from Gordon 1957, 69)

In the northern Colombian lowlands, west of the Río Cauca, a large sector was in anthropogenic savanna in the sixteenth century (Fig. 4.3). With subsequent native depopulation and land abandonment, this region had recovered to forest by the eighteenth century, while much of the area north to Cartagena had been cleared for pasture. By the mid-twentieth century, most of the remaining northern forest had been cleared, and the southern forest had once more reverted to savanna (Gordon 1957, 57–78).

In the central-eastern Peruvian foothills, there is a low-lying plateau called the Gran Pajonal or Great Grassland. Actually, it consists of dozens of small *pajonales* on the hillsides. Study of vegetation, soils, and climate indicates that these savannas originated as Campa (Ashánika) Indian swiddens, which were continuously burned after crop abandonment, with coalescence into larger patches. The anthropogenic origin of specific *pajonales* is confirmed over a period of 24 years by field observations, interviews with the local Campa, and aerial photographs. Historical records show that savannas were in existence in this remote region in the early eighteenth century when Franciscan missionaries first arrived and gave the Gran Pajonal its name (Scott 1978).

The open, white sand *campinas* (scrub savannas) of the lower Rio Negro in Brazil are believed to have once been closed *campina* or *caatinga* (scrub forest) that had been cleared and cultivated by Indians (Prance and Schubart 1978). Ceramics and radiocarbon ages of charcoal indicate that these places were occupied about AD 800–1200. Subsequently, limited succession back to closed *campina* has taken place, and only very slowly due to the extremely poor soil, except on patches of fertile *terra preta* soil (see below) that now contain closed *campina* forest.

Where landscapes were deforested and converted to savanna, whether gradually or rapidly, there would have been significant local and regional changes in water runoff, soils, microclimate, and wildlife.

Domesticated Plants

Indigenous management of tropical vegetation not only affected forest and savanna structure and composition but also led to genetic alteration and creation of new species in the form of domesticated crops.

The most important food crops domesticated in Amazonia, northern Colombia-Venezuela, and southern Central America are manioc and sweet potato. Others include squash, peanut *(Arachis hypogaea),* arrowroot, chili *(Capsicum* spp.), jack bean *(Canavalia* spp.), cocoyam *(Xanthosoma sagittifolium),* lerén, and yampee *(Dioscorea trifida)* (Piperno and Pearsall 1998, 164–165).

Tree crops are a large and somewhat unclear category. The one true Amazonian tree domesticate is the peach palm or pejibaye *(Bactris gasipaes).* However, there are dozens of others, many of them palms, used for fruit, nuts, fibers, and medicinals, which can be categorized as semi-domesticates. These were modified to varying degrees by pre-European people and are now cared for and may be planted. For Amazonia and lowland northern South America, Clement (1999) lists 138 cultivated or managed plant species at European contact, of which 52 are domesticates; 68 percent are trees. For discussion, lists, and identifications of domesticated, semi-domesticated, and other cultivated plants in South America, see Piperno and Pearsall (1998) and Denevan (2001, 307–323).

Animals and People

The distributions and densities of animals in the Neotropics were affected in the past by hunting, vegetation modification, and transfers from one place to another (Grayson 2001). The game animals hunted by Amazonian Indians for both food and other products include large mammals such as monkeys, tapir *(Tapirus terrestris), peccary (Tayassu* spp.), deer *(Mazama* spp.), and smaller animals including paca *(Agouti paca),* agoutis (*Dasyproctidae* spp.), armadillo *(Dasypus* spp.), and capybara *(Hydrochaeris hydrochaeris),* as well as numerous birds, aquatic mammals, and fish.

Most hunting is done today within a 3–10 km radius of a village, with some sectors favored over others and with more infrequent distant hunts that last for several days. For Ache hunters in Paraguay, most hunting is within 8.5 km of their village, with normal game densities beyond (Hill et al. 1997). In the upper Río Napa region of Ecuador, 81 percent of the hunting days occurred in a territory of 590 km^2 (one day's travel or less) around a Siona-Secoya village, but intermittent hunting occurred over an additional 1910 km^2 (Vickers 1991). Thus, game patterns can be influenced over large zones by a single small village or camp. Particularly within the closer zones, large game can be depleted rapidly, followed by a greater concentration on small game. Around permanent villages, depletion is accordingly severe (Hames 1980). As foraging efficiency declines, either travel time increases or villages are relocated. Also, there is species selectivity in hunting, which varies culturally.

Some idea of the total game killed per year by indigenous people can be derived from data for the Waorani in Ecuadorian Amazonia (Yost and Kelley 1983). A population of 230 people in four villages killed 3165 mammals and birds in 11 months. This is equivalent to 3453 in 12 months, which equals only 15 per person and is likely conservative. However, if this is extrapolated to Greater Amazonia for the year 1492, given an estimated population then of 5,664,000 people (Denevan 1992a), the annual game kill would have been 85 million. Of course, this is based on only one sample and on a crude estimate of the 1492 population. Redford (1992) estimates that for Brazilian Amazonia today, the annual kill of mammals is 14 million per year.

Game recovers where villages are abandoned or shift location. A zone farther distant may be hunted, allowing a zone closer in to recover. Thus, we can envision a patch pattern of stable, depleted, and recovering zones of wildlife density related to population density and to village size and permanence or shifting (Hames 1980; Robinson and Bennett 2000; Vickers 1991). Some species are able to "maintain population levels and genetic diversity in a patchwork landscape and across a heterogeneous region through metapopulation processes"; however, other species are threatened by habitat fragmentation (Young 1998).

Game depletion and densities are also affected by habitat modification, such as conversion of forest to savanna or open woodland, or of mature forest to secondary forest. It has been suggested that the dispersal and numbers of white-tailed deer *(Odocoileus virginianus)* and cottontail rabbit *(Sylvilagus floridanus)* in Panama and adjacent Colombia were influenced by prehistoric forest disturbance and savanna expansion (Bennett 1968, 40–41). Indigenous activities that increase the frequency of fruit-bearing trees increase the frequency of animals feeding on those fruits, including tapir, peccary, deer, rodents, monkeys, and birds (Piperno and Pearsall 1998, 62).

Another aspect of the alteration of natural wildlife patterns is the attraction of animals to the crops in gardens or swiddens, those animals then being hunted by the farmers (Linares 1976). Data from Tambopata in southeastern Peru indicate that large ungulates (tapir, peccary), primates, and felids are rare in swiddens and nearby, whereas small game species such as rodents and armadillos thrive and provide important game meat without being depleted (Naughton-Treves 2002).

While zones may be depleted of certain species because of human presence, there is no evidence, apparently, of any definite, pre-European animal extinctions in Amazonia. Bennett (1968, 28, 49–50) has suggested that in lower Central America-Colombia, pre-agricultural people may have contributed to regional extinctions of large game (bison, horse, mastodon, mammoth) and that, by AD 1500, brocket deer and tapir were absent in densely populated western Panama. Regional depletions and extinction of game result in those animals no longer being able to form critical ecological functions such as seed dispersal and interaction with other animals (Redford 1992). The only animal domesticated in the tropical lowlands of South America is the Muscovy duck *(Cairina moschata),* possibly in the Caribbean region of Colombia-Venezuela (Donkin 1989, 70–71).

Anthropogenic Soils: *Terra Preta*

Wherever cultivation and settlement activity take place, soils to some degree are altered physically and chemically, and erosion may occur. In Amazonia, there is a soil that was completely altered by human activity, hundreds or even thousands of years ago, and is still present and fertile today. This is *terra preta* or *terra preta de índio* (dark earth or Indian black earth) (Lehmann et al. 2003; Glaser and Woods 2004). These soils occur in long strips along the river bluffs and in patches in the interior uplands, ranging in extent from less than a hectare to several hundred hectares and in depth up to 2 m. Dates of ceramics in *terra preta* are as old as 2360–2450 BP (Petersen et al. 2001). Because of their high fertility, these soils are sought out today by local farmers, as undoubtedly they were in the past.

Terra preta, compared to surrounding reddish Ferralsols and Acrisols, has higher levels of pH, potassium, phosphorus, calcium, organic matter, and nutrient and moisture holding capacity. This has been attributed to high concentrations of charcoal (black carbon residues of incomplete combustion), along with intense, associated microbiological activity (Glaser et al. 2001).

While the blacker form (*terra preta*) of dark-earth soil seems to have been formed from pre-European village kitchen fires and middens containing large quantities of bones, ceramics, ash, and charcoal, there is a lighter, brownish form (*terra mulata*), which is much more extensive, usually surrounding the black patches. This brown soil is believed to have resulted from intensive cultivation, involving frequent in-field burning of logs, branches, and leaf litter within or brought to a field, along with crop remains and weeds, plus the incorporation of organic material through mulching and composting (Woods and McCann 1999). Thus, if this thesis is correct, intensive cultivation in pre-European times, rather than depleting soil fertility, actually created a persisting fertile soil. Such soils are seldom formed today, given the frequent shifting of villages and fields.

Dark-earth soils tend to have a greater frequency of certain plant species than adjacent soils, including economic plants such as babassu palm, Brazil nut, cacao (*Theobroma cacao),* hog plum *(Spondias mombin),* papaya *(Carica candicans),* and guava *(Psidium guajava),* as well as more vines and thorny plants. After clearing, biomass accumulation is more rapid on dark earths, and weed growth in general is more rapid; some animals and birds are more common in dark-earth vegetation (German 2003; McCann 2004, 116–144).

Cultivated Fields

There is evidence for the cultivation of both tubers and maize in Panama by 6000–7000 BP (Piperno et al. 2000) and for maize in Ecuadorian Amazonia by 5300 BP with dated charcoal indicating forest burning for fields as early as 7000 BP (Piperno and Pearsall 1998, 258–261). The practice of agriculture, of course, completely modifies the natural environments where fields and associated features are located. Cultivated fields may be short lived, as in shifting cultivation, with their impacts being ephemeral with relatively rapid habitat recovery. More permanent cultivation has long-term impacts on soil, and full vegetation recovery can take centuries. In addition, cultivation can include significant and highly varied landform-altering features such as terraces, canals, raised surfaces, excavations, and water control embankments and ditches (Denevan 2001). These features can survive hundreds and even thousands of years following field abandonment, thus continuing to exert an environmental impact.

In Amazonia today, native cultivation invariably consists of short-cropping/long-fallowing shifting cultivation (Meggers 1996, 19–23), and the common assumption is that the same was true in the past, even though there are few reports of shifting cultivation in the early colonial records. An argument can be made, however, that because of the inefficiency of stone axes, compared with metal axes introduced after 1492, a field once established (especially at tree falls, forest burns, and tree blowdowns) would be farmed more or less continuously, utilizing soil-enhancement techniques (Denevan 2001, 116–119). Such fields can have long-lasting effects. The anthropogenic *terra mulata* soils discussed previously are a good example of this. In the interior, permanent villages must have been surrounded by large zones of disturbed vegetation consisting of rotations of fields with house gardens, orchards, and managed secondary forest. Heckenberger (2005, 96, 98, 118, 122) shows aerial photographs of pre-European village sites in central Brazil, each surrounded by secondary forest of unknown age. Along the main rivers, most settlement was on the edges of bluff tops, not in the floodplains where annual flooding could be destructive. *Terra preta* soils and cultural middens extend, in places, for several kilometers along the bluffs, evidence of intensive disturbance in the past (Denevan 1996).

Two archaeological studies confirm ancient permanent settlement and cultivation. The first is for Araracuara on a bluff overlooking the Río Caquetá in Colombian Amazonia (Herrera et al. 1992). Here, at two of the sites excavated, settlement and cropping were more or less continuous from AD 385–1175 and from AD 1–1800. Agriculture consisted of permanent fields and agroforestry systems with fruit trees. Soil fertility was apparently maintained with additives of river silt and algae and terrestrial organic material. The two sites total 20.5 ha of black and brown anthropogenic soils.

Pre-European village sites have been examined in the uplands of the upper Rio Xingu basin in southern Amazonia (Heckenberger 1998; Heckenberger 2005). Two of these cover 40 and 50 ha, and each may have contained over 1500 people (villages nearby today may only cover 2–5 ha, with a few hundred people or less). These sites were occupied from AD 800–900 to at least AD 1590. The central plazas of these villages are surrounded by multiple defensive ditches or moats. Between the plazas and the outer ditches there are anthropogenic soils, apparently created by both villages, refuse and intensive cultivation over many years.

In the dry savannas of South America *(campo cerrado* of Brazil and the higher-lying Orinoco Llanos), soil fertility is extremely poor, and industrial cultivation today uses heavy inputs of chemical fertilizers. Native cultivation, past and present, is rare except in gallery forests and forest patches *(islas).* However, the Kayapó create small (<4 ha) artificial scrub and forest patches *(apêtês)* within savanna and manage them (Anderson and Posey 1989; but see Parker 1992 for a dissent). These are initiated from compost heaps in nearby forest, consisting of sticks and leaves plus soil from termite and ant nests. The rotting material is used to make small mounds in the savanna, which are enlarged over time by natural and human processes. Most of the species present can be planted or managed. Whether completely artificial or not, the vegetation of *apêtês* is clearly human influenced and, in some instances, may be of considerable antiquity.

Raised Fields

In the seasonally flooded savannas of South America, pre-European people made cultivation possible by digging drainage ditches and constructing raised platforms, ridges, and mounds. Some measure over 350 m in length, up to 20 m in width, and a meter or more in height. Not only did such features alter drainage, vegetation, soil, and wildlife when constructed and used, but hundreds of thousands of them have also survived to the present, thus maintaining anthropogenic landscapes in now sparsely inhabited regions.

Water both accumulates in and is drained from the ditches between the fields. Aquatic vegetation and wildlife are concentrated in the ditches. Muck from the ditches was transferred to the raised surfaces to improve fertility. Today, the vegetation, wildlife, and soils of the ditches and fields continue to be different from those of surrounding areas.

Raised field landscapes are not rare, localized, or minor in extent. They occur in the Llanos de Mojos of Bolivia (Fig. 4.4), the San Jorge savannas of northern Colombia (Fig. 4.5), the coastal savannas of the Guianas, the Orinoco Llanos of Venezuela, and the Guayas coast savannas of Ecuador, as well as in the Andes (Denevan 2001, 215–277). Remains of pre-European-raised fields cover at least 150,000 ha (1500 km^2) in the tropical lowlands. This is only a portion of what once existed, the greater part having been buried under sediment or destroyed by erosion or by cattle and other modern land use.

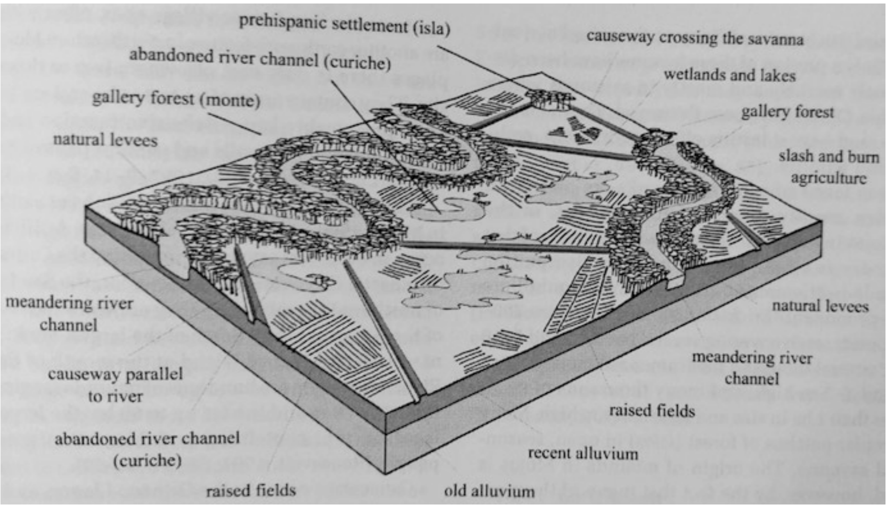

Fig. 4.4 Model of prehispanic hydraulic systems in the Llanos de Mojos in Bolivian Amazonia, showing raised fields, an artificial mound (*isla*), and causeways and water control walls or dikes with canals adjacent. (From Erickson 2001, 28)

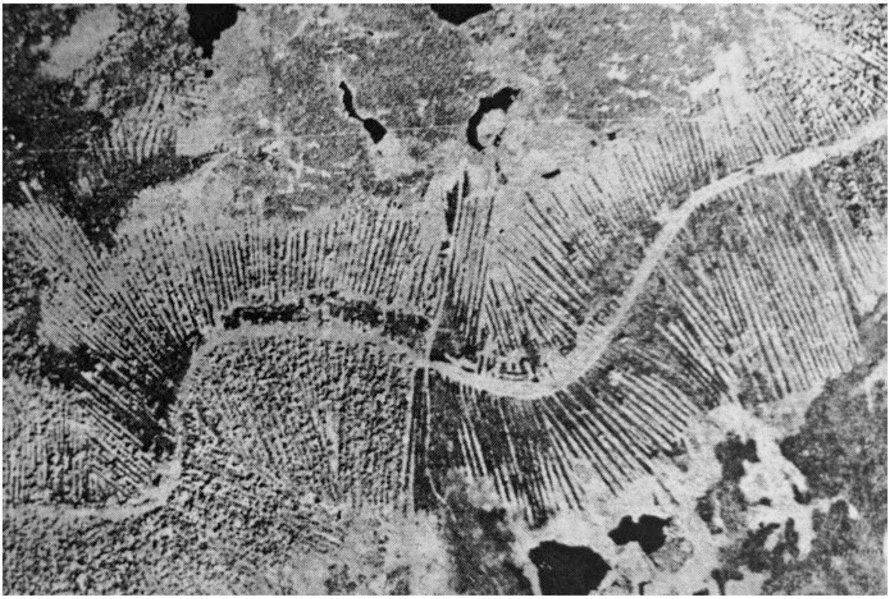

Fig. 4.5 Prehispanic raised fields in the Río San Jorge floodplain, northern Colombia. (From Parsons and Bowen 1966, 331)

Other Earthworks

Throughout the tropical lowlands of South America, there are many pre-European earthworks of various types that have survived to the present–mounds, embankments, causeways, and ditches, in addition to the raised agricultural fields. Only a portion of these features have been discovered, mostly recently and mostly in savannas where they are visible. Others have been destroyed. They alter the land surface and have a lasting effect on drainage, soils, vegetation, and wildlife.

All of these forms of early earthworks are present in great numbers, readily discerned from the air, in the Llanos de Mojos in Bolivian Amazonia. Thus, much of that landscape today is a relic pre-European landscape (Fig. 4.4). Mounds (settlement, burial, and ceremonial) are particularly prominent (Erickson 2000b). There are 200–300 large mounds, each covering several ha and 3–18 m in height; several thousand medium-sized mounds, 1–2 ha in size and 1–3 m high; and many thousands of small mounds, less than 1 ha in size and less than 1 m high. Many occur as circular patches of forest *(islas)* in open, seasonally flooded savanna. The origin of mounds in Mojos is complicated, however, by the fact that many of them are natural (clustered termite and ant mounds, remnants of river levees) (Langstroth 1996). It is not easy to differentiate these from artificial mounds without careful examination; both may contain human bones and artifacts.

Causeways are elevated earthen roads. In Mojos, they measure up to 10–12 km in length, 4–6 m in width, and 0.5 to 1.5 m in height (Fig. 4.4). Aerial photographs of the region reveal causeways at least 1500 km in total length (Denevan 1991), but this may be too low (Erickson 2000c). Most are in poorly drained savanna and connect artificial mounds, but some are within or cross through forest. One mound has at least 40 causeways radiating from it, or nearby (Erickson 2000c). The causeways served for foot travel during floods. Adjacent and parallel to many are the ditches from which the excavated earth was removed and that served as canals for canoe travel. Some causeways may have served as field boundaries and as dikes to control floodwaters in zones of raised fields, either to keep water out for drainage or to keep water in for irrigation, or both; there also may have been political and ritual functions (Erickson 1995, 2000c, 2001).

A curious form of zigzag embankment exists in northeastern Mojos (Baures), which is neither causeway nor dike (Erickson 2000a). Some of these are up to 3.5 km long, and a sampling from aerial photographs indicates that they total an estimated 1515 km in length. They are 1–2 m wide and about 0.5 m high. They change directions frequently, and at the angles, there are funnel-like openings that could be easily opened or closed, with small artificial ponds adjacent. There is no evidence of associated crop furrows or raised fields. Erickson believes these features are fish weirs and that the ponds served for fish storage. Also, enormous quantities of edible snail shells *(Pomacea gigas)* are found nearby. The zigzag walls probably also diverted water and affected water depth and duration, whether intentional or not.

Moats circling former village sites, often with mounds, are another earthwork feature in northeastern Mojos. In some places, there is only one, elsewhere two or three around a site. They contain areas of up to 3–4 ha and are 1–3 m deep. They presumably had a defensive function, and some are associated with low walls and perhaps palisades (Denevan 1966, plate 7; Erickson et al. 1997, 9–14, Figures 3–33).

Although the greatest concentrations of earthworks are in Mojos, they are also found elsewhere. Artificial mounds occur in other wet savannas, including the Orinoco Llanos, the coastal Guyanas, coastal Ecuador, and the San Jorge region of northern Colombia, and they are scattered in the forests of the Amazon Basin. Some of the largest are in the savannas of eastern Marajó Island at the mouth of the Amazon River. Here there are hundreds of mounds ranging in height from 3 to 20 m and in area up to 90 ha; the largest had villages on top, containing possibly a thousand or more people (Roosevelt 1991, 30–31, 38–39).

Causeways occur in the Orinoco Llanos, and a few survive in other regions but nowhere as numerous as in Mojos (Denevan 1991). Sunken trails or roads (from foot traffic and erosion) of possible pre-European origin have been reported in the Amazonian forest. Carvajal, on the first European voyage down the Amazon in 1541, observed roads leading from the interior to the river (Denevan 1996). Nimuendajú (2004, 123) in 1939 reported old sunken roads near the Rio Tapajós that were 1–1.5 m wide and 30 cm deep, "running almost as straight as arrows from one *terra preta* to the next." Today, the numerous indigenous trails in Amazonia usually become rapidly overgrown with vegetation after abandonment, and we do not know how many of them may be pre-European. There was a major Inca road through the Guayas Basin of coastal Ecuador, and stone-paved Inca roads reached the eastern foothills of the Andes. Roads and trails are corridors for adjacent plant and animal disturbance.

Old moats (circular, semicircular, rectangular) are also found in parts of Amazonia other than Mojos. They have been described in particular in the upper reaches of the Rio Xingu (Heckenberger 2005, 75–112). Associated are circular ring mounds with and without moats, mounds, linear walls and ditches, and roads. Some of the moats enclose as many as 20 ha. Moats and other earthworks [geoglyphs] have also been described and photographed recently in deforested areas in Acre in western Brazil (Pärssinen and Korpisaari 2003, 91–172).

Water way, as in "Waterway" Management

Less known in Amazonia than artificial earthworks are various fluvial modifications. A 1916 paper by Erland Nordenskiöld [and Denevan 2009] discussed such features in Mojos and elsewhere in Amazonia. He even suggested that the beginning section of the Casiquiare "Canal," which connects the Río Orinoco with the Río Negro and the Amazon, may not be natural but rather was cut by pre-European people.

The Casiquiare thesis may not be valid, but there are documented instances of such artificial canals between river branches. Again, the best examples come from the Llanos de Mojos in the Bolivian Amazon, and these still show up on aerial photographs (Denevan 1966, 74–77, pl.10). None have been dated, but most seem quite old. Around 1710, Padre Diego Altamirano said that "canals made by hand" could be used for small boats (Denevan 1966, 74). The Jesuit missionaries maintained and used these canals, and they may have constructed some of them. Pinto Parada (1987, 233, 269) mapped several canals, including one complex 120 km in total length, connecting lakes and streams. The large canals are up to 7 m wide and several meters deep. In addition, there are hundreds of smaller canals crossing the savanna from one forest *isla* to another, some adjacent to causeways (Erickson et al. 1997, 15–17). All of these canals would have easily served for canoe transport. Some are simply paths cut through the grass for use during floods (Denevan 1966, pl. 9b).

Canals may also have served to divert water away from or drain water from raised-field complexes. Causeways, whether intended or not, may block the movement of flood water, thus creating deep water on one side and little or no water on the other, as can be seen today.

Another feature is the *corte* or river shortcut. These are dug out where river meander necks are narrow and would eventually breakthrough naturally. They may be initiated by paths being cleared across necks for canoe portages to reduce travel time. They are gradually deepened, allowing water to fill them. They are cut today in Mojos, ranging in length from tens of meters to several kilometers (Denevan 1966, 76–77; Erickson et al. 1997, 14, Figure 34). *Cortes* of unknown age have been reported throughout Amazonia (Raffles and WinklerPrins 2003).

In the Amazon estuary, on Ituqui Island near Santarém, at Careiro Island near Manaus, and in the Orinoco floodplain, some waterways thought to be natural are actually anthropogenic. They were created to provide better access to fields and forest products and to concentrate sedimentation in back swamps for cultivation (Raffles and WinklerPrins 2003; Sternberg 1998, 98). These canals *(cavados)* are up to several kilometers long and a few meters deep. Over time, they are enlarged by natural processes that erase evidence of their origins. All of these canals were made recently; although native people likely dug similar ones in the past.

Ponds and Wells

Ponds or reservoirs dug for fish and for storing water, and also wells, occur in several parts of Amazonia. In northeastern Mojos, shallow, circular depressions *(pozas)*, up to 50 m in diameter and 1.5 m deep, are associated with pre-European causeways, zigzag walls, and artificial forest islands. The depressions may have been the sources of earth for these features. When they fill with rainwater, they contain fish and attract aquatic birds, deer, and other game (Erickson et al. 1997, 19, Figure 55).

Wells are smaller excavations that fill with water from shallow water tables during the dry season and provide a source of drinking water. In northeastern Mojos, they are concentrated within 100 m of pre-European settlement mounds and thus seem to be associated with them (Erickson et al. 1997, 19). Old wells, about 2 m in diameter and 2 m deep, occur within and near *terra preta* sites in the uplands east and west of the lower Rio Tapajós in Brazil. These sites are distant from flowing water (Nimuendajú 2004, 123).

Conclusion

La Selva Humanizada. (Correa 1993)

Human impacts on South American environments have been continuous, but highly variable in form and in intensity, from pre-agricultural hunters and gatherers to the present. Europeans entered the lowland tropics with the discovery of the Amazon River by Cabral in AD 1500. From slavery and epidemics, the native population was rapidly reduced. Fields and villages were abandoned and hunting declined. As a result, the environment recovered in many respects. Fields were replaced by second growth, villages were grown over, and anthropogenic savannas reverted to forest, as we have seen for Panama and western Colombia. However, by the late nineteenth century, with the rubber boom, another cycle of destruction had begun, diminished after 1915, and then intensified after 1960.

There has been considerable debate over whether prehistoric indigenous land use conserved or depleted resources. The best response is that sometimes there was intentional conservation; other times there was unintentional conservation; and at other times practices were destructive and not sustainable of resources and habitat.

Today, with deforestation for agriculture and pasture, logging, settlement, roads, and mining, human disturbance is massive, obvious, and often irreversible. Much of this is by corporate entities and large estates, as well as by new settler farmers with little experience with rain-forest living. However, surviving Indians and long-time residents of mixed background are also present. Although details differ, land management and effect on the environment by these people are often comparable to those of pre-Europeans, use with recovery, sustainability within change, and major impacts mostly being concentrated and lesser impacts being dispersed.

Traditional societies in the past were not few and environmentally insignificant, but rather comprised vital components of the landscape.

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5.0 Indigenous Agroecology

Introducer: Charles R. Clement

Abstract The two book chapters in this section synthesize William Denevan's thinking about how Native Amazonians created and managed agroforestry systems for food production and in the process created an anthropogenic soil called brown earth (*terra mulata*). The practices involved were labor intensive, because of the use of stone and wooden tools, allied with burning of semi-dry organic wastes, resulting in continual additions of charred biomass to the soil, which fertilized the food plants and created these brown earths. William Denevan did not use the term agroecology, but many of these practices are resurgent today in agroecological farming methods.

Keywords Agroforestry · Amazonia · Anthropogenic soils · Historical ecology

Introduction

Many of William Denevan's contributions to the historical ecology of South America have changed the way we think about the indigenous past, as is clear from articles and book chapters in this reader. The two chapters that follow are parts of the rethinking of this past. They are the logical outcome of a series of other contributions, especially Denevan et al. (1984), Denevan and Padoch (1988), and (Denevan 1992a, b). In his magisterial synthesis of the *Cultivated landscapes of native Amazonia and the Andes* (2001), he fully integrates these ideas into a hypothesis of preconquest agroecological management of food production systems that contrast clearly with ethnographical projections from modern swidden-fallow systems, as pointed out by Christine Padoch (Chap. 6, this volume). By chance, both of these book chapters were first presented at meetings in 2002: the First International Workshop on Terra Preta Soils in Manaus in July and at the Symposium on Neotropical Historical Ecology in New Orleans in October. Both meetings included considerable discussion of preconquest agroecology.

At the end of the second millennium, Wim Sombroek, a soil scientist at the University of Wageningen, stimulated an international effort to research what became

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known as Amazonian Dark Earths (ADE). During Sombroek's (1966) research near Santarém, Pará, he described two types of ADE, *terra preta de índio* (Indian black earth) and *terra mulata* (brown earth), and affirmed that both were of anthropogenic origin. Over the next decades, archaeologists and soil scientists became sure that *terra preta de índio* was anthropogenic and had originated in middens (Neves et al. 2003). *Terra mulata* was less well understood, since there were seldom ceramic fragments and the phosphorous, calcium, and magnesium levels were much lower than in *terra preta de índio*. However, Denevan (2001) had synthesized the available information about *terra mulata* and how it might have been created by preconquest agroecological management. His contribution (in absentia) at the Manaus meeting became part of the second major book on Amazonian dark earths (Glaser and Woods 2004), bringing *terra mulata* into the mainstream. Soon thereafter, Manuel Arroyo-Kalin (2008) used geoarchaeological methods to very carefully examine *terra mulatas* near Manaus and supported Denevan's hypothesis (Arroyo-Kalin 2012).

In his presentation at the Manaus meeting, Denevan (2004) described a set of management practices within agroforestry plots that today are called agroecological practices (Altieri and Toledo 2011), although Denevan did not use this term. This set of practices is the logical extension of his observation that stone axes are extremely inefficient at felling trees, hence forests, which have the logical implication that labor-intensive short-fallow short-cultivation systems were more likely than the long-fallow shifting cultivation so common today (Denevan 1992b, 2001). Within this set of management practices, the frequent burning of still humid slash and other in-field sources of organic matter yields larger amounts of charcoal than of ash. The gradual incorporation of this charcoal enhances nutrient cycling (Glaser and Birk 2012), thus favoring crop yields while gradually creating *terra mulata*. Importantly, Denevan hypothesized that this type of agroforestry management is much more productive than long-fallow shifting cultivation and can support the large villages observed along the Amazon River by the first European adventurers.

Historical ecology became an important interdisciplinary research program in the 1990s (Balée 2006), involving anthropology, archaeology, geography, history, sociology, demography, and the physical and biological sciences. Many of the human-environment relations that are studied by historical ecologists involve long-term changes due to management of the various agroecologies that human groups create in landscapes at varying distances around their settlements. Denevan's contribution to the Neotropical Historical Ecology Symposium focused on his hypotheses about the implications of stone versus metal tools before European conquest and continued with the discussion of agroecological management practices that created *terra mulata*. Although he focused his attention on Amazonia, his hypotheses are valid worldwide before the advent of metal tools. Without metal tools, preconquest groups relied upon stone and wooden tools, which are much less efficient than metal. Appropriate stone is a rare resource in Amazonia, certainly reducing even further its use. In addition to stone and wooden tools, human groups have relied upon fire as a tool since *Homo erectus* (Goren-Inbar et al. 2004), and Denevan examines how this was used for managing different agroecologies around settlements. Importantly, there is ethnographic evidence for these management practices in Amazonia (Denevan 2001, 2006).

There is an ongoing debate about the degree of landscape domestication across Amazonia today (Tollefson 2013), with some arguing for extensive modification

(Clement et al. 2015; Levis et al. 2017; Roosevelt 2013) and others for minor modification, especially in the interfluvial regions (Piperno et al. 2015). Some of those who argue for smaller degrees of domestication seem to be worried that the confirmation of larger degrees will give free reign to developers who want to bulldoze the forest for soybean and pasture (Meggers 2001). Since his "pristine myth" (1992a), it is clear that Denevan believes that a larger degree is more likely than a smaller degree, but this must be understood within his hypotheses about agroecological management in preconquest Amazonia. Because this agroecological management is labor intensive and opportunistically takes advantage of tree falls, blowdowns, and other natural events that open the forest periodically across Amazonia, Denevan hypothesized: "The productive landscape probably consisted of semi-intensive fields and managed bush fallows, surrounded by zones of modified forests manipulated by hunting and gathering and other human activities" (2006, 153). As he also points out in this contribution, villages were established both along the bluffs of major rivers and along the tributaries that extend into the interfluvial regions. Levis et al. (2014) observed that preconquest villages (seen as *terra preta de índio* sites) along tributaries in various parts of Central Amazonia are found every few km. The combination of Denevan's hypothesis and Levis et al.'s observations allows the inference that there was not much unmodified forest between villages, especially given the dense network of tributaries in Amazonia that represent the majority of the 30 percent of the region with at least seasonal waterways (Junk et al. 2011). Even thouvgh Denevan did not cite Junk et al. when he revisited the "pristine myth" (2011), it is clear that he believes that the majority of Amazonia was modified to some extent.

Denevan is careful to point out that Native Amazonian management practices are qualitatively different than modern management practices, even modern agroecological practices, and contributed to the resilience of landscapes that permitted the rapid resurgence of mature forests across Amazonia after the decimation of native populations that began in 1492 (Denevan 2014). Modern practices do not build resilience into agroecologies like Native Amazonian practices did. Unfortunately, it is unlikely that modern societies' agricultural research services will change direction and learn from Native Amazonian practices (Clement 2006), although Denevan hoped they would (2006, 159), even though this appears to be increasingly essential to modern efforts to adapt to climate change.

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5.1 Pre-European Forest Cultivation in Amazonia

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Abstract Traditional cultivation in Amazonian forests today is dominated by long-fallow shifting cultivation. However, there is little evidence for it prior to about AD 1600. A new model of pre-European agriculture has been suggested based on the inefficiency of stone axes, the evidence of anthropogenic soils, and archaeology. The productive landscape probably consisted of semi-intensive fields and managed bush fallows, surrounded by zones of modified forest manipulated by hunting and gathering and other human activities. Not only was soil fertility apparently maintained artificially by organic inputs and in-field burning, but also some soil was altered in the form of dark earth, which continues to be visible and fertile. Thus, pre-1492 cultivation practices and landscapes seem to have been considerably different from those of current traditional people, a perspective gained from a historical ecological approach to stability versus change in people-environment interactions.

Keywords Amazonia · Historical ecology · Semi-intensive agriculture · Soil fertility

Introduction

Historical ecology is "the study of changing human-environmental relations" (Crumley 1996, 560; also see Balée 1998; Balée and Erickson 2006, 1–6). It is an interdisciplinary subject involving anthropology (including archaeology), geography, history, sociology, demography, and the physical and biological sciences. The expression "historical ecology" became current in the 1990s, but it has been used from time to time since at least the 1970s. The topic subsumes some of the previous and ongoing research in cultural and human ecology, cultural and historical geography, environmental history, and landscape ecology. However, cultural ecology has often lacked a historical dimension; environmental history focuses on historical documents; and landscape ecology may minimize or ignore human agency.

Within my field of geography, people-environment research has been a major concern since at least the time of Alexander von Humboldt (1769–1859). Three benchmark historical volumes dominated by geographers or geographical approaches are *Man and nature* in 1864 (Marsh 2003), *Man's role in changing the face of the earth* (Thomas Jr. 1956), and *The earth as transformed by human action* (Turner II et al. 1990). Particularly important have been Carl Sauer (1938, 1950, 1956, 1958) and his students at Berkeley. Sauer was instrumental in turning early American geography away from environmental determinism (1927, 165–175). Today, "nature and society" is one of the four principal subfields of geography.

Historical ecology includes prehistory. Sauer's influence long ago turned me toward early human impacts on environment in Latin America (Denevan 1961) and to native cultivated landscapes (Denevan 1966). I have argued that indigenous alterations of nature were present to some degree wherever native peoples were present (Denevan 1992b, 2007). People cannot live on and from the environment without changing it. However, this perspective is not without controversy (Vale 2002). Regarding cultivation, I became more and more convinced that pre-European agricultural practices were sophisticated and productive. Many of these practices were lost after 1492, but their vestiges have persisted on the landscape in various places. Here I briefly present some of the evidence and thinking about native cultivation in Amazonian forests.[1]

The prevailing image of pre-European agriculture in upland Amazonian forests (*terra firme*) is that of shifting cultivation (swidden) with periods of short cropping and long fallowing. This is the dominant pattern today for both native peoples and settlers. However, a revisionist model has been suggested, a landscape of semi-intensively cultivated fields[2] intermingled with fruit orchards, managed fallows, house gardens, and brief bush fallows, with semi-permanent settlements, some numbering thousands of people, surrounded by zones of modified forest manipulated by hunting and gathering activities (Denevan 2001, 102–132).[3] This complex

[1] For indigenous cultivation in floodplains and savannas in Amazonia, see Denevan 2001.

[2] "Semi-permanent cultivation," "semi-intensive cultivation," and "intensive shifting cultivation" are more or less synonymous terms for "crop/fallow rotations" in which both the cropping and fallow periods are brief (from 1 to 10 years or so). In contrast, a long-fallow system can be considered extensive, and permanent (annual harvests) or nearly permanent cultivation can be considered intensive.

[3] This view of semi-intensive cultivation is part of a revised (or new) paradigm (or model or synthesis) of Amazonian prehistory, which argues for complex societies (see Denevan 2001, 130–131; Heckenberger et al. 1999; Neves 1999; Roosevelt 1994; Stahl 2002; Vivieros de Castro 1996). The older, standard paradigm of egalitarian societies, low populations, small temporary settlements,

system of integrated land use both created and exploited fertile, anthropogenic soils (dark earths, anthrosols), known in Brazil as *terra preta de índio* (Indian black earth), or known more broadly as Amazonian Dark Earths (ADEs).

Semi-permanent Cultivation and the Inefficiency of Stone Axes

In Amazonian forests, the vegetation must first be cleared before forms of cultivation of annual crops are possible. Light materials could have been cut using machetes made of hard wood, but trees were chopped down mainly with stone axes. However, stone axes are so inefficient for removing large trees, compared to metal axes introduced by Europeans that long-fallow shifting cultivation was probably difficult, even with the girdling and burning of tree trunks (Denevan 1992c). Experimental research with both types of axes indicates that up to 60 times more energy and time is required to clear forest with stone axes, with an average of about ten to one, depending on tree diameter and hardness, axe type, cutting technique, arm strength, and use of auxiliary methods (Carneiro 1974, 1979a, b; Hill and Kaplan 1989, 331). Stone axes not only cut poorly but also dull and break, and the hafting comes undone, requiring frequent polishing, repair, and replacement. Moreover, in Amazonia, suitable stone may be hundreds of miles away.

The historical short-cropping/long-fallowing shifting cultivation system has been made possible by labor-efficient metal axes. In pre-European times, there must have been much less frequent forest clearing with stone axes. Once an opening was established by clearing or by a treefall, natural burn, or blowdown from violent wind shear (Nelson et al. 1994), the opening could have been cultivated semi-permanently, possibly with gradual enlargement at the edges (Denevan 2001, 120–127). Fertility could have been maintained by organic inputs of household garbage, ash and charcoal, mulches and composts, and by the frequent in-field burning of weeds, crop residues, logs and branches, leaves, and palm fronds, from both within a field and carried from adjacent forest. Labor inputs would have been high, especially for controlling weeds, which are more aggressive with intensive cultivation compared to clearings from mature fallows or primary forests. However, even short fallows of a year or two can reduce weeds; hence, the likelihood of semi-permanent rather than permanent cultivation–a few years of crops rotating with a few years of bush fallow. Dark-earth farmers in Brazil today do this to reduce the labor costs of weeding (German 2003a, b, 326).

Ethnographic examples of semi-intensive cultivation can be found in Amazonia. The best studies are for the Kayapó in central Brazil (Hecht 2003; Posey 2002, 165–218). Cropping for up to 5 or 6 years and fallows as short as 8–10 years are

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and long-fallow shifting cultivation (reviewed in Stahl 2002, 39–42) is still persistent (Meggers 1957, 1996, 2001).

possible because of site-specific plantings, soil-fertility management, polycropping, and crop zonation. Organic material is collected and burned periodically within fields. These small burns are managed for frequency, location, extent, and biomass, as well as for seasonal and diurnal timing and thus burn temperature. These burning techniques are important for specific crops, crop clustering, field architecture, and fertility characteristics. Small mounds are created from compost and are planted (*apête*), and rich top soil is placed in cracks in rocky sites and then planted.

Pre-European cultivation was probably often, if not usually, more intensive and more productive than post-contact cultivation. After 1492, metal axes became available, a technological revolution in terms of tools and forest-clearing efficiency. However, there was actually an agricultural de-evolution toward long-fallow shifting cultivation, a simplification and reduction in productivity both per unit of land and over time, which has continued to the present. In addition, some interior Amazonian societies abandoned cultivation completely for a foraging economy (Balée 1992, 37–41; 1995, 98–102). Both changes can be termed "agricultural regression." Such regression also resulted from forced migration away from the attractive riverine zones onto the poor upland soils in the interior (Lathrap 1970, 186–190).

Iron and, later, steel axes became the primary trade items for native people in colonial times and in some remote areas until recently because of their extraordinary value for clearing forest (Métraux 1959). Descriptions of shifting cultivation are rare throughout the Americas prior to about AD 1600 (Denevan 2001, 115). However, with little or no evidence, scholars frequently have assumed that long-fallow shifting cultivation was the dominant form of pre-European agriculture in Neotropical forests (Meggers 1957, 80–83; still unchanged 40 years later, see Meggers 1996, 19–23) and that "intensive cultivation is an impossibility" (Roosevelt 1980, 87, later reversed in Roosevelt 1999, 381–382). Historical, ecological, and archaeological research and analysis cannot be too strongly emphasized in order to counter such assumptions, which are based primarily on ethnographic (recent or present-day) analogy.

Dark Earths

Anthropogenic dark earths are widespread in Amazonia, probably covering at least 0.1–0.3 percent (15,500–20,700 square km) of the forested area (Sombroek et al. 2003, 130). Black dark-earth soil, or *terra preta* proper, usually contains high concentrations of ceramics, kitchen waste, and bones, which identify the soil as a former settlement site.[4] Soil scientists now consistently explain the soil color and fertility as the result of sustained accumulation of organic refuse in middens, including ash and charcoal from domestic fires, over a long period of time. The lighter or brownish form of anthropogenic dark earth, usually called *terra mulata,* is much

[4] For recent studies and discussions of anthropogenic dark earths in Amazonia, see Glaser et al. 2001; Glaser and Woods 2004; Lehmann et al. 2003b; Mann 2002; McCann 2004; McCann et al. 2001; Petersen et al. 2001; Woods and McCann 1999.

more extensive and usually surrounds patches of darker *terra preta*. A section of both soils on the bluff of the Tapajós River near Belterra south of the city of Santarém was mapped long ago by the late Wim Sombroek (1966, 175), a pioneer of Amazonian soil research. He was apparently the first to maintain that *terra mulata* was produced by "long-lasting cultivation."

The key to *terra mulata* formation and persistence seems to have been a burning practice that leaves intact charcoal, which is not degradable, in contrast to ash. This "cool" burning could have been a form of "slash and char," with incomplete combustion, in which moist slash is burned, in contrast to the slash and burn today, in which a "hot" burn is a more complete burn, accomplished after a long period of drying out. The resulting soil carbon from cool burning, along with high levels of soil microorganisms, apparently created the self-sustaining high fertility of these soils. The stone axe thesis helps explain why long-term, semi-permanent cultivation could have taken place instead of long-fallow shifting cultivation, which does not produce dark earth. Frequent organic inputs and in-field burning could have made semi-permanent cultivation possible as well as contributing to the creation of *terra mulata*.

Terra preta contains up to 70 times more carbon than does surrounding soil. Carbon itself is not a direct nutrient, but it retains nutrients and makes them available, stabilizes soil organic matter, raises pH level, raises soil microbial activity, maintains soil moisture, helps repel insects, reduces nutrient leaching, and thus maintains and improves soil fertility and hence crop-production level and sustainability (Lehmann et al. 2003a; Steiner et al. 2004).

The largest known extents of dark earths are on the forested bluffs along the main Amazonian rivers (Denevan 1996; Denevan 2001, 104–110). It is often believed that riverine settlement in Amazonia was located primarily below the bluffs in the floodplains. For example, archaeologist Anna Roosevelt states that late prehistoric people "were very densely settled . . . along the banks, levees, and deltas of the major floodplains" (Roosevelt 1987, 154–155). Furthermore, historian John Hemming says that "In the sixteenth century the native population was very dense in the flood plain or *várzea*" (Hemming [1978] 1995, 191). However, the floodplains are a high-risk habitat for both settlements and crops because of periodic extreme flooding of even the highest natural levees. Houses can be built on pilings, but crops will be destroyed.

Exploration, Cultivation, Settlement, and Dark Earths

The first descriptions of the Amazon River were in the mid-sixteenth century by Gaspar de Carvajal ([1542] 1934) from the Orellana voyage and by the surviving men of the Pedro de Ursúa-Lope de Aguirre disaster (Mampel González and Escandell Tur 1981). These accounts clearly indicate that most of the large Amazonian settlements were located on the bluff edges, not within the floodplain. Long, linear settlements were reported, which extended continuously for several leagues (a sixteenth-century league was 5–6 km). For example, Captain Altamirano and Francisco Vázquez, who were with Aguirre in 1561, mentioned a settlement that was two or three leagues long or more, with the houses touching one another (Mampel González and Escandell Tur 1981, 225; Vázquez de Espinosa [1628] 1948, 387). Carvajal gave similar information for 1541 ([1542] 1934, 198–212).

How credible are these early reports (see discussion in Denevan 1996, 661–664)? First of all, the descriptions from the two expeditions are similar, and they were only 20 years apart. Such accounts of large bluff settlements are absent by the mid-seventeenth century, by which time there had been massive population reductions.[5] Moreover, although Carvajal and the men of Aguirre told some fanciful stories, these stories seem to have come mostly from poorly understood natives, not from direct observations by the Spaniards. Exaggeration is possible. Regardless, dark-earth soils full of potsherds, evidence for large, linear bluff settlements, cover bluffs for estimated distances of as much as 2–6 km (Myers 1973, 240; Petersen et al. 2001, 97; Smith 1980, 560). However, these stretches of dark earth were not necessarily fully occupied at a point in time given probable local (short-distance) shifting of houses and fields.

Large bluff settlements could not have been supported by seasonal *playa* and natural-levee cultivation. The bluff soils are the same poor Oxisols that dominate the interior upland forests. There was likely a complimentary system of bluff cultivation and hunting combined with seasonal floodplain cultivation and fishing (Denevan 1996, 671–672). Bluff cultivation for large, semi-permanent settlements would have had to have been productive and sustainable, as I have suggested. Evidence for this assumption comes from the Araracuara sites on the bluff of the Caquetá River in the Colombian Amazon (Herrera et al. 1992). Radiocarbon dating and analyses of soils, pollen, phytoliths, plant remains, and ceramics indicate nearly continuous human occupation of one site for 800 years; the creation of fertile, brown anthropogenic soils; and intensive agroforestry systems with maize, manioc, and fruit trees.

Most large archaeological sites along the major rivers are located where the primary river channels, navigable year-round, impinge against the bluffs, rather than where the channels are in the middle of the floodplain and less accessible from the bluffs. These bluff-channel junctures are the locations of pre-European dark earths and settlements, colonial missions, and most towns and cities today, with relatively empty lands in between (Denevan 1996).

If semi-intensive cultivation and dense settlement were located on the bluffs, then both were certainly also possible in the interior forests. Indeed, there is dark-earth, archaeological, and historical evidence for large permanent settlements in the

[5] Populations in the lowland Amazon Basin proper circa 1492 have been roughly estimated at between two and a half and five million and may have been considerably higher (Denevan 1992a, 1996, 656, 672–674; 2003, 186–187; Hemming 1995, 505–521). By 1900 the total indigenous population was reduced by disease, war, and slavery to a few hundred thousand, rose to about 500,000 in 1972, and subsequently has risen somewhat above that despite near extinction for some groups (Denevan 1992a, 232). For Brazilian Amazonia only, Hemming estimates about 350,000 in 1910, decreasing to 100,000 in the 1950s and then increasing again to 350,000 still living in tribal communities at the end of the twentieth century (Hemming 2003, 636–637). Recent genetic study indicated 45 million people in Brazil with some Indian ancestry (Hemming 2003, 812).

interior forests where soils are similar [to the bluff soils]. Oitavo Bec, a dark-earth site south of the city of Santarém, covers more than 120 ha (Woods and McCann 1999, 12), and the Comunidade Terra Preta site located between the lower Rio Tapajós and the Rio Arapiuns covers 200 ha (Smith 1999, 26). However, there are numerous small dark-earth sites of only 1 hectare or so, which must have been created by a few people (Smith 1980, 563). Historical [Indian] settlements in the interior mostly had 100 people or less, but there are reports of some with several thousand inhabitants.[6]

Conclusions

Thus, several lines of evidence and reasoning suggest that in pre-European Amazonia cultivation was often semi-permanent rather than long fallow. Such cultivation could well have created *terra mulata*, and, if so, then *terra mulata* soil, wherever it now occurs, may be indicative of former semi-permanent cultivation. Once established, self-perpetuating dark earths could have made possible ongoing semi-intensive cultivation to the present day, with further organic additives being unnecessary or minimal, although other factors would have caused periodic field abandonment (Petersen et al. 2001, 92; Woods and McCann 1999, 10–12). The implications for understanding both the Amazonian past and future agricultural development in Amazonia and elsewhere are dramatic. Uncertainties remain, but the necessary field and laboratory research is now under way (see, e.g., Lehmann et al. 2003b).

A historical ecological approach to people-environment interactions involves examination of change over time: change in society, change in technology, change in the environment (both natural and human induced), and consequently changes in the interactions involved. For pre-European Amazonia, I suggest a very different form of indigenous cultivation from that of today, one often (not always) based on the human formation of fertile soils and their utilization, with a concentration of land use and associated settlement rather than dispersal, as is characteristic of long-fallow extensive cultivation in recent times.

The present is not necessarily an extension of the past. Although there are continuities, there are also dramatic disjunctures. For Amazonian cultivation and the associated landscape, such a disjuncture seems to have occurred after 1492. Just as some agriculturalists regressed to foragers, others regressed from semi-intensive cultivators to long-fallow shifting cultivators.

[6] Examples of large archaeological settlements (estimated) occur in the upper Xingu River basin (1000–1500) and in Goiás (1043–1738). Historical examples reported (varying accuracy) include settlements of the coastal Tupinambá in the sixteenth century (up to 8000); in central Brazil, the Bororo in the early twentieth century (1500), the Kayapó in 1896 (1250 or 5000 in four settlements); and in 1900 (3500–5000), the Paresi in the early eighteenth century (1200), the Xarae in the sixteenth century (7500), and the Apinayé in 1824 (1400) (for sources, see Denevan 2003, 182–183).

Finally, if pre-European cultivation was often semi-permanent or permanent, then the disturbance patterns of distribution, kind, quality, and diversity of natural resources would have been different from those produced through post-1492 long-fallow swidden; that is, patches of intensive modification were interspersed with less-intensive disturbance based on hunting and gathering activity, in contrast to a more uniform disturbance based on the constant shifting of cultivation, foraging, and settlement.

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5.2 Semi-intensive Pre-European Cultivation and the Origins of Anthropogenic Dark Earths in Amazonia

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Abstract Anthropogenic dark earths of pre-European origin are widespread in Amazonia, both along river bluffs and in the upland interfluves between rivers. Although there are gradations in color and characteristics, the two main forms are *terra preta* proper, which is black, and *terra mulata*, which is dark brown. The former is clearly the result of the accumulation of household waste (midden). The latter, which is of primary concern here, apparently was produced by semi-permanent cultivation involving the application of ash from in-field burning and from organic amendments. How this could have taken place is examined, based in part on current practices.

Keywords Amazon dark earths · Anthropogenic soils · Amazon · Human agency

Introduction

Anthropogenic dark earths are widespread in the uplands (*terra firme*) of Amazonia, in patches covering a hectare or less up to several hundred ha. The blacker form (*terra preta*) seems to have developed from pre-European village middens consisting of ash and charcoal from kitchen fires, cultural debris, feces, human and animal bones, and house/garden waste (Woods and McCann 1999). The lighter, dark brown form (*terra mulata*), which is much more extensive, is believed by some soil scientists (Sombroek 1966,175; Glaser et al. 2001), archaeologists (Herrera et al. 1992; Petersen et al. 2001), botanists (Prance and Schubart 1978), and geographers

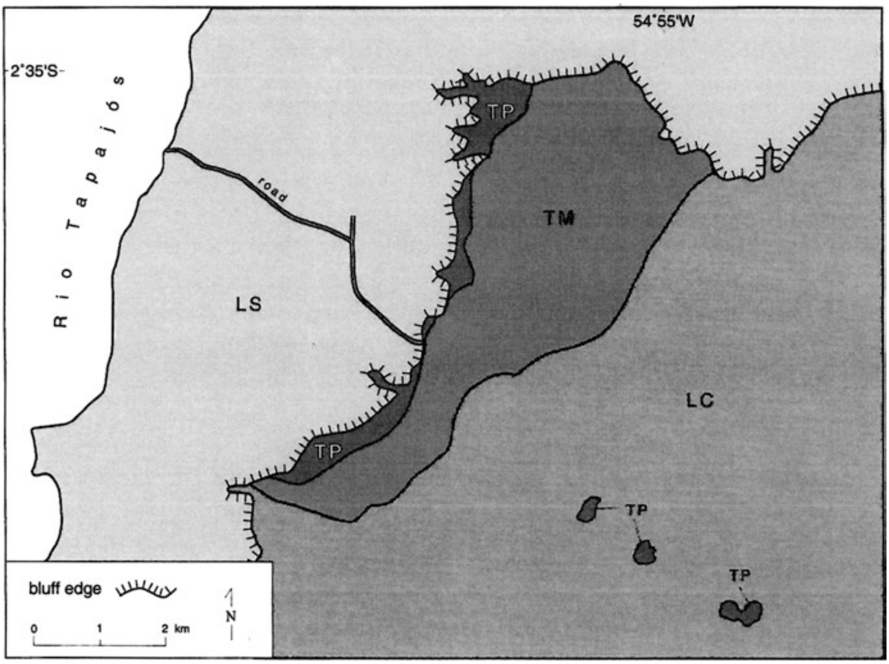

Fig. 5.1 Map showing bluff-edge *terra preta* (ca. 200 ha) along the Belterra Plateau adjacent to the Rio Tapajós, Brazilian Amazonia, with *terra mulata* (ca. 1,000 ha) inland. Note the small patches of *terra preta* beyond the bluff zones. *TP terra preta*; TM *terra mulata*; LS latosolic sand (sloping, eroded, plateau front); LC latosolic clay (interior plateau). (Adapted from Sombroek 1966, 175; Denevan 2001, 106)

(Denevan 1998; Woods and McCann 1999) to be the product of intensive cultivation practices (Fig. 5.1). Others, however, such as Smith (1980) and Eden et al. (1984), rejected an agricultural origin because of the depth of dark earth soils. Smith argued for midden origins, and he saw soil color and depth as being functions of length of village-site duration. In 1980, he was apparently unaware of the extent of *terra mulata* that contains little or no midden material.

Dark earth soils in Amazonia are considerably higher in fertility (pH, P, Mg, Ca, organic matter) than are surrounding soils. Color and chemical characteristics have been attributed by Glaser et al. (2001a, b, 2002) to a high content of black carbon particles that are residues of incomplete combustion from frequent burning by pre-European people.[7] Incomplete combustion, producing charcoal instead of ash, can be accomplished by burning when the cut woody vegetation is still moist rather than

[7] Natural fires in rain forest in the northern Amazon in the mid-Holocene only occurred at a site frequency of 389–1540 years (Kauffman and Uhl 1990).

well into the dry season, a "cool" burn in contrast to a "hot" burn.[8] The necessary burning frequency potentially could have been provided in zones of semi-intensive cultivation[9] surrounding permanent villages. What were the cultivation practices that could have produced frequent burning and carbon-rich and organic-matter-rich dark earth soils? The focus here is on the *terra mulata* soils.

Potential Origin of Terra mulata

We know that *terra mulata* generally is not formed by long-fallow shifting cultivation today (Pabst 1993). A lengthy period of time apparently is required, not a few years but rather decades or even centuries. If, indeed, this soil is related to cultivation activity, then some form of semi-intensive cropping involving frequent burning must have been involved.

Frequent Burning of Cleared Short Fallow

A short cropping/fallow cycle of cultivation probably was critical. In the lower Rio Negro region of Brazil today, a single year of cropping of dark earth soil on *terra firme* is followed by 4–5 years of fallowing (German 2003a, b), what can be considered a semi-intensive system with frequent reclearing and burning. Because of the high fertility of dark earths in Amazonia, weed invasion is very aggressive immediately after clearing, burning, and planting.[10] However, this is countered by only a few years of fallowing. Weeding requirements are greater after short fallows than after long fallows but are still less than after only one or two years of cultivation, and they are progressively greater as cultivation continues. On fertile dark earth soils, fallowing is probably more important for weed management than for restoration of fertility, but this is also often true on other soils (Chagnon 1992).

I have argued (Denevan 1992) that long-fallow shifting cultivation, so common today by both Indians and non-Indians, was infrequent in pre-European times because of the inefficiency of stone axes for clearing forest. The introduction of iron axes and, later, steel axes by Europeans induced a major, but negative, agricultural revolution by making possible labor-efficient, but land extensive, long-fallow shifting cultivation with frequent clearing for new fields. Previously, once a clearing for crops was available, it could have been farmed frequently in a short cycle similar to

[8] Average carbon recovery from charred woody biomass ("slash-and-char") is 50 percent compared to only three percent with common slash-and-burn practices (Glaser et al. 2002).

[9] In semi-intensive (or semi-permanent) cultivation, both the cultivation and fallow periods are brief (from one to several years).

[10] In the Rio Arapiuns region west of Santarém, on dark earths there may be three weedings per crop compared to one or none on adjacent oxisols (McCann 2004).

that reported by German (2003a). Fertility can be maintained by mulching, composting, intercropping, in-field burning, and other traditional techniques still practiced by some Amazonian Indians (Hecht 2003). Natural clearings are created by tree falls, wild fires, and tree blowdowns during violent storms (Denevan 2001, 120, 125). Artificial clearings could have been made with stone and wood tools where vegetation was easy to clear. Once established, a clearing could have been gradually enlarged.

Short-lived cultivation plots likely were located within a mosaic of young, managed second growth which included fruit trees, dispersed or in small orchards. This growth probably was similar to the five and six year-old managed orchard fallows that we mapped and described for the Bora in northern Peruvian Amazonia (Denevan and Treacy 1988). The importance of fruit and nut trees on dark earths in pre-European food systems is known from archaeology at Araracuara on the Río Caquetá in Colombian Amazonia (Herrera et al. 1992) and historically from Spanish chroniclers in the mid-sixteenth century (Denevan 1996). Thus, the burning of young fallows for new clearings could have occurred with a frequency of only a few years.

In-Field Burning

"In-field burning" refers to burning within a crop field or agroforestry zone, subsequent to field establishment. Particularly common today are secondary burns in which charred trunks and branches, not initially completely burned, are piled and reburned both to clear space and to create ash piles in which crops may be concentrated. Other burn piles consist of weeds, crop residues, and transported forest litter. In addition to concentrating nutrients and reducing soil acidity, burning reduces insects and disease pathogens.

The farming practices of the Kayapó of central Brazil are an example of carefully controlled in-field burning, as is described by Hecht and Posey (1989, 180; also see Hecht 2003). For the first three years of cultivation, the Kayapó manage their small burns for frequency, location, and extent, and for "volume of biomass, seasonal timing, diurnal timing, and [thus] the temperature of the burn," and these are related to specific crops and crop clustering. Burning patterns are important for the structure or architecture of Kayapó fields. The species burned can also "affect the fertility characteristics of the burn." The Kayapó have many descriptive terms for types of ash, as well as songs and rituals about burning, and their shamans specialize in burning techniques.

Some Amazonian people spend only a total of one or a few hours on burning and reburning a clearing (Barí, Kuikuru, Mirití, Shipibo, Amahuaca), whereas the Machiguenga, who are very careful burners, average 80 hours (Beckerman 1987). Other people besides the Kayapó, such as the Machiguenga, believe that ash is a good fertilizer for crops (Johnson 1983). Generally, however, a layer of ash is only a temporary soil enrichment and may not be so recognized at all by Indian farmers (Descola 1994).

Another possibility for regular in-field burning is the cutting of tree branches, piling, drying, and then burning them to create ash-charcoal piles. These could be branches from managed trees, secondary growth, or primary forest, with the trees generally recovering from such branch removal. There is little evidence for this either historically or at present in Amazonia, but it is a possibility, and it could have been done with stone axes and *macanas* (hard chonta-palm machetes). As a model for such branch lopping, we can look at the *citemene* system in central Africa (Allan 1965; Stromgaard 1985, 1988; Oyama 1996; Quinby 2001).

The Bemba and other people in Zambia lop off trunks and branches of woodland trees and stack and burn them in either small circles 20–30 ft. in diameter or in large circles of as much as an acre.[11] In these otherwise poor soil regions, crops are planted for up to 5 years, followed by 20 years or so of tree recovery before they are lopped again. The regional *citemene* landscapes are covered by these circles in various stages. Phosphate, potassium, calcium, and pH levels are raised by the ash residues. Apparently, the "hot" fires in this process, producing mainly ash, do not result in a permanent dark, fertile, anthropogenic soil. "Cool" burning leaving charcoal potentially could do so. Population densities of 4 to 16 per square mile (Quinby 2001) have been supported by this system for long periods, which is far larger than known Indian densities of less than one per square mile in upland Amazonia.

The Achuar in Ecuador (Descola 1994), shortly after the initial burn, gather incompletely consumed branches, and in the center of the clearing, they make a large stack that is then burned. Apparently, however, branches are not brought in from outside the clearing. Smaller piles of slash are arranged around stumps and burned, with the resulting ash concentrations being favored for planting yams that do well in potassium-rich soils.

Organic Amendments

Woods and McCann (1999) believe that a combination of frequent burning and organic inputs from mulching and composting resulted in heightened nutrient-retaining capacity, enhanced fertility, and self-perpetuating soil biotic activity, leading to the formation and persistence of *terra mulata*. Also, unburned slash can be left to decompose relatively rapidly from humidity and high temperatures, slowly releasing nutrients.

Kayapó mulching and composting are very important for soil management and include crop residues, chopped weeds, banana leaves, and also palm fronds that in this case may be brought from the adjacent forest (Hecht and Posey 1989; Hecht 2003). In addition, there may be direct applications of organic material mixed into

[11] For the large circles, the ratio of size of the cropped area to the cut area is 1:6.6 (Oyama 1996).

the soil around tree crops (ash, ant and termite nests, bones, shredded leaves). With these practices and in-field burning, the Kayapó are able to crop for about 5 years, followed by only 10–11 years of fallow. Fertility indicators (pH, N, P, K, Ca, Mg) not only actually increased (over forest levels) for the first year following initial burning but also were generally sustained in the fifth year of cropping and even in the tenth year under early fallow (Hecht 1989). Abandonment after the fifth year was due to intensified weed invasion, not fertility decline. There are similar descriptions for other Indians in Amazonia. The Waika (Yanomami group) in Venezuela generally cultivate polycultural swiddens for 5 or 6 years (Harris 1971). At Araracuara, there is archaeological evidence that fertility was improved by the addition of "organic material, such as domestic waste, dead leaves, wood, and weeds" to the soil (Mora et al. 1991, 79).

Where dry periods are too short for burning, all cut vegetation may be left on the ground to decompose without burning. Such "slash/mulch" systems have been described by Thurston (1997). There are several examples today in western Amazonia (Achuar, Canelos Quichua, Napo Quichua), and the technique may have been more common in prehistory. The Kayabí Indians in Brazil both burn slash and allow unburned material to decompose (Rodrigues 1993).

In the lower Rio Negro region, *caboclo* farmers today counter fertility loss by adding nutrients to both dark earth and common soils by burning weed and leaf residues, by leaving residues in place to decompose, and by making large compost piles (German 2003a, b).

Permanent Settlement and Dark Earths

The evidence for semi-intensive pre-European cultivation is in part inferential, and the dark earths themselves are such evidence. Furthermore, we now know that villages were not necessarily small, a few hundred people or less, but rather some numbered in the thousands, and these villages were not necessarily shifted but were in some places permanent. There is ethnohistorical evidence for both such size and permanence (Denevan 2003). Furthermore, there is archaeological evidence for such villages at Araracuara (Mora et al. 1991; Herrera et al. 1992), in the lower Rio Negro region (Petersen et al. 2001), and in the upper Xingu region (Heckenberger 1998; Heckenberger et al. 1999). Heckenberger mapped two Xingu sites that were 40 and 50 ha in size, most of this containing dark earth soils. In comparison, nearby present-day Kuikuru villages are 5 ha or less in area. The former were occupied continuously from at least AD 1000–1500 and contained permanent earthworks including multiple circular moats, roads, and embankments, with estimated populations of 1500 or more. Many more such large pre-European village sites undoubtedly will he found under upland forest.[12]

[12] See Pärssinen and Korpisaari (2003) on discoveries of extensive pre-European earthworks in recently deforested areas of Acre, Brazilian Amazonia.

There is even greater evidence for large sites on *terra firme* river bluffs along the Amazon River and its major tributaries (Denevan 1996). Permanent villages had to have been sustained mainly by frequent cultivation, and many of those studied by archaeologists are associated with anthropogenic dark earth soils.

I should emphasize that archaeological village sites were not necessarily fully occupied at one point in time. Houses were likely shifted around within a site because of decaying structures, buildup of vermin and garbage, or a death. Even today, there are instances of Yanomami villages staying in one area for 60–80 years with micromovement within the area (Chagnon 1992). Some Kuikuru have lived in the same area for 90 years, moving their villages four times but only a few hundred yards apart (Carneiro 1961). Thus, locational advantages were retained, including access to water, to improved soils, and to managed or manipulated plants. Meggers (2001), however, argues that the entire pre-European sites were not occupied continuously but rather were repeatedly abandoned and reoccupied by small groups, thereby accounting for large occupation areas and midden depths. DeBoer et al. (2001), Heckenberger et al. (2001), and other archaeologists disagree.

Conclusions

The scenario I suggest here is that:

1. Given the inefficiency of stone axes for clearing mature forest, pre-European farmers often relied on short-cropping/short-fallow systems, a cycle of one or two years of cultivation and four to five years of fallow, or by four to five years of cropping and ten years or so of fallow, or by other similar variations, with the fallows being managed agroforestry systems dominated by fruit trees and other useful species. The short fallows would have been insufficient to rejuvenate soil fertility which instead was sustained by ash and charcoal from frequent burning of fallow and in-field burning and by composting and mulching. These were patches of various sizes that were fairly permanent, but in which there was rotation of cultivated fields with managed fallows and fruit orchards.
2. Subsequently, over considerable time, given concentrated, semi-continuous cultivation activity, dark-brown *terra mulata* soils were formed in the persistent, frequently burned cultivation-agroforestry zones. Once established, these superior dark earth soils were particularly attractive to farmers because nutrient-demanding crops such as maize could be grown successfully on them. Thus, these soils acted as further stimulus for both maintaining settlement and cultivation in these sites and for returning to them when/if there were periods of abandonment, continuing to the present. Continuity over hundreds of years is indicated for some interior upland sites and riverine bluff sites. The preceding portrayal, however, is an oversimplification and is not intended to be universal. Undoubtedly, there has been considerable variation in Amazonia in the forms of dark earths, the specific processes and histories responsible, forms of land use, and associated settlement patterns.

In conclusion, I argue, with others, that it is possible that many dark earth soils could have been the product of semi-intensive agricultural practices. This is conjecture, but I have tried to support it with examples of current Indian practices in Amazonia. Unfortunately, the process of dark earth soil formation will be difficult to replicate given the long time spans apparently involved. However, analyses of soil chemistry and micromorphology and plant microfossils should be instructive as to soil genesis and cultivation activity.

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6.0 Tropical Agriculture

Introducer: Christine Padoch

Abstract This essay introduces two of Denevan's articles that contributed profoundly to a reevaluation of Amazonian agriculture both in the present and in the past. Denevan changed the way swidden farming is perceived, especially the ways in which this common form of agriculture relates to forests. He showed swidden to be extremely complex and highly flexible, multistaged, largely invisible, and "hiding" within what appear to be natural forests. Perhaps even more significantly, Denevan directed Amazonian researchers to look beyond swidden and in doing so changed fundamental ideas about early Amazon agriculture, Amazon forests and forest management, and the nature of traditional and smallholder resource management throughout the tropics.

Keywords Bora · Fallow management · Lowland agriculture · Shifting cultivation · Swidden

Introduction

Swidden agriculture seems among the most visible of farming methods as its more common name, slash-and-burn, graphically describes. Tree felling is easy to detect; falling trees can be seen, even heard, from far away. The forest gaps that swiddening creates stand out dramatically and remain distinct in satellite images for many years. Moreover, the fires that characterize swidden in most places are not only conspicuous, but especially in recent years, dramatic images of fires have served as the "proof" that swiddening is "primitive," "wasteful," and essentially criminal.

In the course of some groundbreaking research and with several provocative articles, William Denevan, however, convinced those who observe and study shifting cultivation that what we thought we saw, i.e., the "visible swidden," is only a small part of what swiddening actually is and that we were overlooking most of what swidden systems contribute to Amazonian livelihoods and landscapes.

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Denevan's Bora project profoundly changed the way swiddening is perceived and understood and how it relates to forests, especially in Amazonia (Denevan and Padoch 1988). He showed shifting cultivation to be far more complex than previously assumed and often "hidden" within what appears to be natural forests. And then, once we had learned all of that, and thought we understood the importance of managed swidden fallows, Denevan went on to convince us that even these "invisible" phases of the swidden cycle were historically both more and less important than we had believed. He revealed how the visibility and drama of slash and burn continued to blind us to the larger story of Amazonian forest management and of the management of Amazonia's rivers and soils, animals, and plants.

This essay introduces two of Denevan's articles (1984, 1992) that contributed profoundly to these reevaluations of how Amazonians produce food, fiber, medicines, and many other goods both in the present and in the past. Based on these and similar writings, new discoveries about present-day tropical forests and peoples, waters and soils, and new insights into the processes of change that produced them continue to be made in both Amazonia and elsewhere in the tropics. Denevan's research truly transformed how we look at and think about – or should look at and think about – tropical resource management around the globe and throughout history.

Saving Swidden

In 1957 FAO Staff, in its forestry journal *Unasylva*, published an "appeal…to governments, research centers, associations and private persons who are in a position to help" to join a coordinated research effort on shifting cultivation or rather to help with "analyses of the effects of shifting cultivation on soils and forests, as well as with proposals for improving the situation. This entails the study of all facets of the problem. FAO has, therefore, the intention of mobilizing the contributions of as many scientists as possible to help solve this problem in all interested countries" (FAO Staff 1957, 9).

The *Unasylva* article did not mince words; the "problem" it asserted was that "shifting cultivation, in the humid tropical countries, is the greatest obstacle not only to the immediate increase of agricultural production but also to the conservation of the production potential for the future, in the form of soils and forests" (FAO Staff 1957).

Furthermore, according to the article, shifting cultivation or swidden was "not only a backward type of agricultural practice…[but] also a backward stage of culture in general. In all respects it corresponds to the Neolithic period through which humanity passed between the years 13,000 and 3,000 BC, considering that the substitution of iron tools for polished stone has made no substantial difference in the way of life" (FAO Staff 1957). Moreover, FAO proposed a number of "points of attack."

Uninformed and wholesale condemnation of swidden agriculture and of its cultural concomitants was of course not surprising in 1957, nor is it now a thing of the past. What is remarkable, however, is that in the very same year as its call to eliminate shifting cultivation, i.e., 1957, the FAO published *Hanunoo agriculture*, anthropologist Harold Conklin's path-breaking study of shifting cultivation in the Philippines (Conklin 1957). Conklin's work was not the first piece of research to take a careful look at what swidden farmers actually did and what values they produced not only for themselves but for broader landscapes and societies. *Hanunoo Agriculture* did, however, profoundly affect the discussion of swiddening. It stands to this day as a pioneering work. To those willing to read it and learn, *Hanunoo Agriculture* demonstrated that shifting cultivation is in many instances complex, diverse, and developed and an environmentally and economically rational way to make a living in the humid tropical uplands. Conklin's work spurred much further research on shifting cultivation in Southeast Asia and elsewhere.

As in Southeast Asia, studies of swiddening in Amazonia have a long history. Much of what has been said about Amazon shifting cultivators is equally marred by misunderstanding, misjudgment, and condemnation. Amazonian swidden studies, however, often have had a different focus and emphasis than their Southeast Asian counterparts. They have emphasized issues such as carrying capacity, labor input, and energy efficiency and human dietary issues. Indeed, all shifting cultivation systems may have more in common with each other than not. However, the *chacras*, *roças*, or swidden fields of the lowland Amazon, dominated by tall, vegetatively reproduced cassava (manioc) plants that grow in the shade of even loftier banana plants and papayas and are underlain by a great diversity of herbs and tubers, indeed look very different from Hanunoo upland rice fields that climb the steep hills of Mindoro Island in the Philippines. Furthermore, these different visible structural features also signal different labor demands, management strategies, crop patterns and yields, and often very different long-term trajectories.

Perhaps, the most important issue that Conklin and many later studies on shifting cultivation addressed is the supposed wastefulness and destructiveness of shifting cultivation. Long before "deforestation" was swiddeners' principal purported crime, accusations that shifting cultivators were "nomadic" and that their farming wasted the soil resources that were needed to avert future famines (FAO Staff 1957, 2) were made. The "shifts" of annual crops from one wooded site to another that characterize swiddening were simply attributed to heedless overuse of soils that made shifting imperative. The most important and memorable change in these views that followed *Hanunoo Agriculture* and other important works was probably the argument that the shifting of plots was an orderly and planned process, based upon a cycle of "natural fallow" and recovery, punctuated with periods of agricultural production, rather than the misplaced assumption that shifting sites entailed a sequence of soil exhaustion, forced abandonment, and endless migration to new forests. Conklin suggested that the Hanunoo were not unlike other more "advanced" farmers who rotated crops or fallowed fields, but swiddeners rotated fields rather than crops, and their fallows were woody and their cycles were protracted.

Thus, swidden cultivation was saved – at least among those willing to take a careful look – from the ignominy of condemnation as wasteful and its practitioners as ignorant nomads. Shifting cultivators indeed kept cutting forests, but when they shifted, those very same forests would regenerate and the soils be restored in a potentially endless cycle of clearing and regrowth. Many of us, who studied shifting cultivation, represented those shifts in neat diagrams of rectangles and arrows that recreated swidden cycles. The arrows brought the farmed and fallowed plots back to a point zero where the cycle of swidden and fallow would begin yet again. That drawing was later repeated, graphically illustrated, and interpreted by others – including many who had never actually been in a shifting cultivation field. But perhaps, that picture was just a little too neat, too regular, and too rotational. The impressive complexity, flexibility, and diversity of swidden systems was lost; many of us, even against our better judgment, represented swiddening as altogether too predictable and equilibrated, too certain and unchanging, and too simple.

Revealing the Invisible

Twenty-five years after Conklin's and other studies helped us to understand why swiddeners abandoned their fields after a year or two of cropping, Denevan and the Bora project, represented here by "Indigenous Agroforestry in the Peruvian Amazon" (Denevan et al. 1984; also see Denevan and Padoch 1988), convinced us that swidden fallows were not really abandoned at all, or at least that "abandonment," if it actually occurred, was not an event but a complex process. The paper showed that what observers of shifting cultivation had long called "abandonment" was rather a change of management: extended, complex, contingent upon a plethora of observed conditions, and largely invisible to the eyes of outsiders. It added yet another layer of intricacy to a system that we had just recently learned was not at all primitive. Around the time that the Bora project took place, agroforestry had become an area of scientific focus and promise since it was deemed to be, among other things, an "alternative to slash and burn." The characterization of Bora swidden-fallow management as a form of agroforestry raised it to the level of an advanced, sophisticated, and up-to-date, improved way to farm and manage forests rather than an anachronism.

The Bora project grew out of previous research done by Peruvian agronomist Salvador Flores Paitán in the Bora community of Brillo Nuevo along a tributary of the Amazon River in Peru. Flores Paitán had been investigating Bora patterns of managing young secondary forests by enriching them with important economic plants such as coca and a broad array of fruits. The project headed by Denevan, however, helped put the forest management into a swidden context and then placed the swiddens into a broader forest landscape. No longer was a swidden field seen as abandoned when annual crop harvests ceased; nor did the fields then rotate through a sequence of natural succession that restored them to their initial conditions. Swiddens were now places where farmers made a complex series of decisions through the years and throughout the plot. Just as there was no distinct break between the farm phase and the forest phase, there was also a complex blurring of where management began and ended spatially, with edges gradually being left to something close to "natural" regrowth, while other parts of the plots were manipulated for even longer periods. The individual swidden plot therefore did not have a history distinct from the broader landscape matrix within which it was found.

Swiddening became the management of a complex, anthropogenic, uneven-aged landscape with, at best, indistinct edges. It was a place where a great variety of plant and animal species were managed, none of them quite detached from the forest, and none of them quite "natural." There was an identifiable sequence from an "original" vegetation with some economic plants present, to a swidden with many food and other annual and perennial plants, and on to an orchard fallow agroforestry phase combining managed economic plants and natural vegetation. Finally, what developed was a forest fallow in which economic plants were fewer than in the swidden, but still present in greater numbers than in an original forest.

The Bora project did not emphasize the "visible swidden" of annual crops and burning slash and the essential regularity of the swidden cycle. Bora swiddens and swidden-fallows were messy, visually, ecologically, and conceptually. Some areas were managed far longer than were others. All areas had somewhat distinct trajectories, and the effects of active and more passive management, and of what was "natural" in the forest, were blurred, confusing, and essentially invisible.

The Bora of Brillo Nuevo, while doubtless distinct in some of their resource management practices, is not the only Amazonians – be they officially regarded as indigenous or not – to manage forests for food and other goods in such complex ways. *Ribereño* and *caboclo* peasant farmers up and down Amazonia's rivers have adapted swidden-fallow management systems to a variety of particular spatial, ecological, and economic conditions. Beyond Amazonia and going back to the classics of shifting cultivation and related land use systems in Central America, Southeast Asia, and Africa, references have long been made to managed swidden fallows (including in *Hanunoo Agriculture* itself), but these were little emphasized and under-appreciated until quite recently. "Indigenous Agroforestry in the Peruvian Amazon," published in the international journal *Interciencia*, helped to launch a great many studies around the tropics that took a closer look at what really happens in swidden fallows. Although the exact influences may be difficult to trace and intellectual debts often are unacknowledged in writing, those intercontinental and intergenerational transfers of insights, perspective, and focus have been important for many scientists researching both Asian and Amazonian systems.

Discovering the Vanished

Swiddens and swidden-fallows not only still provide the products with which smallholders around Amazonia feed their families and supply urban markets, but they also hold important clues to the contours of far earlier resource use systems in the South American tropical lowlands. These systems are now concealed by the upheavals of a particularly violent history of invasion, destruction, and erasure and the transformations wrought by time and rivers. They are also obscured by layers of false assumptions, conceptual blindspots, and prejudices. Today's practices of plant, animal, land, and water management and manipulation doubtless are legacies of the past, albeit transformed, and the configurations of present-day forests are archives of the resource management technologies used by earlier Amazonians.

Combining the ethnographic and ecological clues and insights with a broad knowledge of what historical data exists, as well as some detective work and deductive reasoning, in “Stone vs metal axes,” Denevan arrived at an even more transformative idea than in the Bora studies. In the paper, he made a shocking statement: “shifting cultivation, as an ancient practice in Amazonia, seems to be a myth” (Denevan 1992, 161).

This sentence delivered a jolt to all of us who had long accepted and frequently repeated that shifting cultivation is the basic way in which tropical forests around the globe have been managed for food and other human needs by agriculturists. Despite evidence to the contrary, we have believed and even insisted that all other production systems we observed – the house garden, the managed fruit grove, and the intensively farmed plot on the riverbank – were important to village economies and ecologies but were fundamentally just add-ons, late stages, or variants of the swidden. Making gaps by cutting trees, and then (usually) using fire to clear the spot for planting crops, seemed primal, reasonable, and unquestioned – until Denevan called it a myth. The essential production system, he argued in the article that follows, was the carefully selected site, and the managed, transformed, transplanted, weeded, and manipulated garden and agroforest. The gap that was made using slash-and-burn techniques may have been the supplement, the “early stage” of the manipulated secondary forest, and the rare add-on.

Several recent archeological discoveries, including Heckenberger’s “garden cities” on the upper Xingu (Heckenberger et al. 2008), appear to corroborate Denevan’s provocative idea of extensively managed woodlands and some more intensively managed permanent plots, rather than distinct shifting farms and forests. Amazon forest vegetation gives up its secrets only with a good deal of work and even more good luck. However, the vision of Amazonia’s past suggested in “Stone vs metal axes” seems more plausible with each new discovery. Every added piece of evidence returns some lost history to Amazonia’s forests, lands, rivers, and the people and societies that have seen their histories destroyed and denied. In other regions where local communities and histories have fared somewhat better, there are other examples that suggest that technologies other than swiddening prevailed in times and places where metal tools were scarce. For instance, in interior Borneo, many complex resource management technologies of a variety of cultural groups are also commonly reduced to and decried as “slash and burn.” However, histories of earlier times and agriculture done with virtually no metal tools and largely without resorting to swiddening are also found. The Lun Dayeh of far interior Kalimantan and Sarawak, for instance, recounts that just a few generations ago, stone and wood implements were widely used instead of metal, and trees were rarely cut to make fields (Padoch 1985). In that case, farming was largely done not on the wooded hills but on broad, flat, and well-watered sites that made farming of rice in permanent pond fields the principal mode of food production. These specifically selected production areas were managed by digging, flooding, and weeding and other technologies that required little use of metal tools.

We may never find conclusive proof that would convince everyone that the Amazonians once supported substantial populations without cutting large swathes of forests, but rather by a broad repertoire of skills, technologies, and organization that created not only a wooded but a “humanized landscape” that also produced a great volume of necessary and desired foods, fibers, and other goods. The proofs lie in little researched local resource use technologies that still exist but are largely disappearing and in forests that are being replaced by large monocultures before they have been well understood. However, we do have some powerful clues and insights that have pointed us in the right direction, and new discoveries are doubtless still to be made.

Those studying resource management in Amazonia as in other tropical forest areas owe a debt of gratitude to the scholars who were and continue to be careful, perceptive, and patient observers of production systems – such as shifting cultivation – that are challenging and confusing at best and oftentimes the objects of condemnation. Denevan’s writings including the two articles featured here show unambiguously that he is a masterful observer of what is before him, a critical reader of what has been written earlier, and a perceptive listener to what there is to hear. However, these two articles also show that Bill Denevan’s contributions go well beyond these extraordinary gifts of observation and analysis. Denevan has seen well beyond what is visible, what is immediately perceptible, and what is clearly written on the page.

His major contribution, as the two articles that follow clearly show, is that he has perceived what was hidden, revealed what was invisible, and retrieved what was lost. And then through his research, teaching, and these articles, he has taught all those of us who take the time to read them and to learn from them to also look for and at last to see what is hidden and invisible and lost.

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6.1 Stone Versus Metal Axes: The Ambiguity of Shifting Cultivation in Prehistoric Amazonia

Original: Denevan, W.M. 1992. Stone vs metal axes: The ambiguity of shifting cultivation in prehistoric Amazonia. *Journal of the Steward Anthropological Society* 20: 153–165. Reprinted by permission of the Department of Anthropology at the University of Illinois, Champaign-Urbana.

Abstract Clearing forest with stone axes is a very laborious procedure, even with the assistance of girdling, tree falls, and fire. Experimental research in Amazonia and elsewhere confirms this. Iron or steel axes are many more times as efficient. Consequently, Denevan argues, shifting cultivation, which requires frequent clearing of forest, was uncommon in pre-metal axe times. Instead, cultivation was permanent or semi-permanent, maintained on even poor soils by the incorporation of ash, charcoal, and organic material. When metal axes were introduced by Europeans, forest clearing became much faster and less labor demanding, and shifting cultivation became more feasible than intensive cultivation as long as mature forest was available. Thus, the use of metal axes resulted in significant reduction of cropping frequency and greater deforestation.

Keywords Amazon · Ax use · Forest clearing · Pre-European agriculture · Shifting cultivation

Introduction

> The mastery of the forest by man requires no axe. –Sauer 1958, 108

Donald Lathrap had remarkable insights into Amazonian prehistory, the interaction between subsistence and settlement, and the position of the Amazon as center stage rather than backwater of South America. One of his major interests was the distinction between the cultures and economies of the densely populated floodplain (*várzea*) and those of the sparsely populated uplands (*terra firme*) (Lathrap 1970, 1977). A key component of this story is the stone axe and its efficiency for clearing forest. The presence of stone axes in archaeological sites is probably indicative of agriculture (Lathrap 1970, 62–63), but what kind of agriculture? Don would have appreciated what I have to say here, but I am sure he would have had some caustic but constructive comments.

Ethnologists and ethnohistorians have generally portrayed surviving indigenous hunters and gatherers (foragers), shifting cultivators, and other traditional economies as representative of prehistoric food production systems. Even where such groups have clearly undergone considerable acculturation, it has been suggested that their food-getting ecologies are essentially intact and of long standing, despite changes in crops and tools.

This perspective, however, is coming under increasing attack, as witness the debate over the authenticity of the Tasaday in the Philippines (Headland 1992), and the revisionist view of the !Kung of the Kalahari by Wilmsen (1989), who argues that the !Kung are far from living relics of stone age hunters and gatherers. Few groups anywhere have been isolated from the world economy, directly or indirectly.

In lowland South America, we find tribes such as the Nambiquari, Ache, Héta, Sirionó, Yuquí, and Yora which until recently subsisted from hunting and gathering with little or no cultivation. However, rather than being remnants of pre-agricultural societies, it now seems that most are refugee agriculturalists who have fled from more powerful tribes, from Europeans, or from demographic pressure in the floodplains, an argument made in 1968 by Lathrap and even earlier by Claude Lévi-Strauss (1963); and also see Bailey et al. (1989, 65–66).

More recently, archaeologist Anna Roosevelt stated that in Amazonia "theories about pre-Conquest subsistence cannot be tested with ethnographic data" and that "present-day Indians' resource management modes may not be representative of prehistoric ones" (Roosevelt 1989, 31). Colchester (1984, 311) provides further emphasis: "it is time that we started examining Amazonian societies in terms of the recent radical transformations that have occurred and that are occurring in their technological, demographic, and economic bases…" Furthermore, most surviving Indians are located in the *terra firme* high forests of the interfluves, where resource conditions (soils, game, and fish) are relatively poor, whereas most prehistoric Indians were located in the resource-rich floodplains on adjacent bluffs.

Our knowledge of indigenous adaptations to the *terra firme*, adaptations that are now being heralded as instructive for successful rainforest utilization, is based primarily on present-day observations. This model is characterized by short cropping/long fallow shifting cultivation; low-protein crop staples (manioc, sweet potato, plantain); small, temporary settlements (ca. 10–100 people); and very low population density (below 0.5 per km^2). Productive agroecological techniques are common, such as soil improvement, fallow management, and polycultural plantings. Considerable debate exists over the reasons for this pattern which applies to most tribes,[1] particularly over whether game (protein) scarcity is the key limiting factor (Denevan 1971, 1992, 208–209; Hames and Vickers 1983).

[1] For example: Campa, Bora, Yanomami, Kuikuru, Amuesha, Machiguenga, Kayapó, Runa, Ka'apor, Amahuaca, and Siona-Secoya; see Posey and Balée (1989); Hames and Vickers (1983).

I believe that the short cropping, long-fallow shifting cultivation pattern is primarily a post-conquest development, reflecting the shift from forest clearing with stone axes to the much more efficient iron or steel axes, and that in aboriginal times forest clearing was too labor intensive to be a common or frequent agricultural strategy. I will first compare tree cutting with stone and metal axes. Then, I will discuss the significance of this for prehistoric Indian agriculture in Amazonia.

Tree Cutting

There is some data available on the technology and the differential labor involved in clearing forest with stone and metal axes in Amazonia, in particular, from Robert Carneiro (1974, 1979a, b), who conducted experimental research with the Amahuaca in eastern Peru, the Kuikuru in the Brazilian Amazon, and the Yanomami in southern Venezuela. Felling trees with stone axes was clearly time-consuming, difficult work.

Carneiro (1979b, 69–70), for example, calculates that a 24-inch (61 cm) diameter tree of moderate hardness could take from 11.7 to 14.4 hours to fell with a stone axe versus 0.52 hours with a steel axe. Felling times of course vary with the type of axe, arm strength, cutting technique,[2] trunk thickness, and hardness of wood. The ratio of felling time, stone to steel, increases progressively with trunk size. The ratio is only 10 to 1 for a 6-inch (15 cm) diameter tree; it is 23 to 1 for a 24-inch (61 cm) diameter tree; and it is 32 to 1 for a 48-inch (122 cm) diameter tree, or 115 hours vs 2.4 hours. Differences in felling times between stone and metal axes are much greater for hardwoods than for softwoods: "holding diameter constant, a tree twice as hard as another [density or specific gravity] will take twice as long to fell." This is for a steel axe. The difference for a stone axe would be even greater (Carneiro 1979b, 62).

Hill and Kaplan (1989; Kaplan 1985), working with Ache Indian in Paraguay and the Yora in Peru, confirmed Carneiro's hypothesis that the rates of clearing times increase disproportionately with increasing tree diameter and tree hardness, with a significantly greater rate of increase for stone compared with steel axes. The hardness, however, had a much greater effect on stone axe felling times than did tree size: "For hardwoods, the time cost for stone-axe clearance can be 60 times greater than for metal tools" (Hill and Kaplan 1989, 331). Overall, the average efficiency ratio was about 10 to 1, stone to steel.

For a family plot of 1.7 acres (0.7 ha) of trees of mixed size and hardness, Carneiro (1979b, 71) calculated a total of 1,229 hours for clearing with a stone axe versus only 64 hours with a steel axe, a ratio of 19 to 1. The former equals 246 five-hour work days, which is simply not tolerable. He asks how swidden clearing could be done then? His answer is that labor time was reduced with the assistance of trunk burning, girdling or cutting a ring through the cambium layer, tree falls to

[2] Hodder (1983, 79–80) questions the validity of Carneiro's (1979a, b) results for Yanomami tree cutting efficiency because the practitioner had no prior experience using a stone axe.

knockdown additional trees, and leaving the largest trees standing. He calculates an average efficiency ratio of only 7 or 8 to 1, stone to steel, using auxiliary techniques, and 10 to 1 if all trees over 2 ft. (61 cm) in diameter are left standing.[3] Kaplan (1985), however, found that the multiple tree-fall technique did not reduce clearance time significantly. Killing and deleafing trees by girdling and burning the base of the trunk leave the trees standing, but will bring in sunlight to some of the adjacent ground surface.

Another consideration is the availability of stone for axe heads. It can take several days to make a stone axe and hours to sharpen one (Kozák et al. 1979); proper stone sources may be far away requiring long treks or trade (Denevan 1966, 47–48). Axe heads dull or break and shafts break.[4] Axes are lost or stolen. The rapidity by which tribes shifted to metal axes when available, their struggle to obtain them, and the major role of metal axes in trade are well known, reflecting their great saving of labor, as is related in "The revolution of the ax" by Alfred Métraux (1959).[5]

Agricultural Implications

The inefficiency of the stone axe has dramatic implications for prehistoric agriculture in Amazonia. Several anthropologists have suggested this without pursuing it (Colchester 1984; Hill and Kaplan 1989). Kaplan (1985), in an unpublished paper, presented the hypothesis that "aboriginal farmers, particularly in interfluvial regions, were highly selective regarding their choice of potential gardening sites and that as a result the distribution of forest types placed important constraints on settlement pattern and subsistence practices throughout the Amazon basin."

Sites for fields would have been sought where the vegetation lacked large hardwood trees and was dominated by small softwood trees, essentially secondary vegetation such as fallow-field regrowth, or along streams, or sites disturbed by tree falls and landslides. The Machiguenga (Peru) say that when they had stone tools, settlement was concentrated along small streams[6] where clearing for gardens was easiest (Hill and Kaplan 1989, 332). Even today, they often clear fields from thickets of giant bamboo (Baksh and Johnson 1990, 205). The Yora's (Peru) reliance on foraging

[3] Other studies have obtained lower stone axe to steel axe efficiency ratios (ca. 3:1–6:1), but they generally do not take into consideration variability in tree diameter and hardness; i.e. Salisbury (1962, 220); Saraydar and Shimada (1971, 1973); Steensberg (1980, 38–39); Townsend (1969, 203–204).

[4] See Carneiro (1979a, 41); Lewenstein (1987, 35–43); Townsend (1969, 201). Lewenstein compares the times to make and sharpen, efficiency, and durability of ground stone versus chipped stone axes for the Maya. Carneiro (1974, 115) quotes an Amahuaca man on former use of stone axes: "They say it was always breaking. They say it was always getting dull. That stone axe is no good!".

[5] There are many examples of the Indian obsession for stone axes in Amazonia, i.e., Deboer (1981); Denevan (1966, 97); Golob (1982, 115, 126–127, 153–154, 201–202); Isaac (1977, 141).

[6] Wilk (1985, 55) believes that in the Maya lowlands riverbank recessional farming preceded long-fallow swidden farming in the uplands, in part because of inadequate land- clearing axes.

apparently reflected limited availability of metal axes (Hill and Kaplan 1989). Allan Holmberg (1969, 272) reported that the Sirionó gave much more attention to gardening and became more sedentary as soon as he provided them with steel hatchets.

The Yanomami use of stone axes for clearing and the impact of the introduction of steel axes in the twentieth century are described by Colchester (1984). Secondary vegetation and stands of soft-stemmed musaceous species were sought for swiddens because of greater ease of clearing with stone axes than was mature forest, even though more labor is required for weeding in secondary vegetation. Plots were small, the larger trees were not felled, and treking for game and wild plant foods was of major importance for subsistence: "The Yanomami of the seventeenth century were interfluve foragers, who supplemented their subsistence with the cultivation of small plots, widely dispersed about their foraging territory" (Colchester 1984, 308). Early explorer accounts support this pattern. With the introduction of metal axes, the Yanoama (Yanomami) changed from a foraging economy supplemented by agriculture to an agricultural economy supplemented by foraging, with larger fields and villages and less mobility (Colchester 1984, 310). Likewise, Machiguenga Indians said that in the past when it was difficult to obtain steel axes their gardens were much smaller, and they relied more on forest products for food (Johnson 1977, 164).

I do not argue that clearing interfluve forest was rare in pre-metal axe times, but it was probably more restricted and much less frequent than with tropical forest tribes today, most of whom clear new fields every two or three years. Sites were undoubtedly more selective, based on ease of clearing, and once a field was established, it was probably maintained in cultivation as long as possible. It was likely that less labor was required to combat weeds (probably suppressed by controlled shade) and other pests and to use soil maintenance techniques than was required to establish new clearings. However, data is needed to confirm this.

The argument here is hypothetical, as there is little archaeological or early historical evidence on the nature of prehistoric *terra firme* agriculture, and there is no physical evidence for any form of prehistoric shifting cultivation. There were probably pockets of fairly intensive farmers, mainly along small streams. Overall populations were probably low, but possibly larger than scholars have believed, including myself (Denevan 1992, xxv–xxvii; Meggers 1992). In contrast, on the floodplain and adjacent levees, there was no vegetation to clear or only easily cleared vegetation, and the stone axe was less a liability. Soils were fertile and wildlife resources rich. Fields did not "shift," and populations were dense. Also, those savannas where the soil fertility and drainage were either not severe or could be managed could have been attractive to permanent farmers given that there were few or no trees to clear (e.g., Denevan 1966, 94–95; Posey 1985, 140–144).

We do know that hardwood forests were cleared for agriculture elsewhere by relatively dense populations using stone tools, as in Yucatán, in western Central America, and in Europe. However, these were areas with soils much superior to those of Amazonia, so that fields could be cropped for numerous years and did not have to be cleared frequently. The considerable labor involved in clearing with stone tools thus could be tolerated. A recent study by Doolittle (1992) argues that prehistoric shifting cultivation in eastern North America was less common than permanent fields.

Conclusions

I am suggesting that shifting cultivation in prehistoric Amazonia was uncommon because of the inefficiency of the stone axe, especially in the mature, high, hardwood forests of the *terra firme*. Indian shifting cultivation today has a short cropping period, reflecting poor soil, weed and pest invasion, game depletion, and social friction, but it is made possible by the steel axe which makes clearing new plots a relatively easy process – a matter of a few weeks to create a field large enough (0.5–2.0 ha) to feed a family.

Indian shifting cultivation as we know it is the product of the steel axe and also the machete. What then was the nature of prehistoric high forest agriculture? We do not know and may never know. However, there are several possibilities:

1. *House Gardens*: permanent plots of mixed annuals and perennials around the house, with careful weed control and soil management using household refuse for fertilizer. Lathrap (1977), in his classic article "Our father the cayman, our mother the gourd," maintained that the earliest agriculture in Amazonia was carried out in such house gardens.
2. *Intensive Swiddens*: located on sites where tree clearing was relatively easy, such as naturally disturbed or old field plots with young secondary growth of softwoods. A present-day example of such fields would be the highly diverse or polycultural swiddens described by Harris (1971) for the Waika (Yanomami) of the Upper Orinoco, which are cultivated for up to six years. Such fields contrast with the monocultural swidden dominated by a single species, usually manioc, which is the common form of tropical-forest Indian field today (Beckerman 1983), even for the Yanomami (Hames 1983, 18–19). Most current monocultural fields are only used for 1–3 years. Beckerman (1983, 4–6) gives several reasons for the monocultural field, but he does not consider the role of the steel axe in making short-lived swiddens feasible.
3. *Agroforestry*: forest manipulation via intentional and unintentional planting and management of crops along trails, campsites, fallow swiddens, and other activity areas ("forest fields") (Denevan and Padoch 1988; Posey 1985).

These three models of *terra firme* agriculture with a stone axe technology in reality were likely manifested by numerous transitional forms, varying with habitat, mobility, time, and demography. These activities, combined with foraging, contributed to the creation of anthropogenic forests, or semi-managed forests, with a larger than natural number of useful plants present – wild, semi-domesticates, and domesticates. The Amazon forest was not pristine in 1492, nor is it today. Probably, all of these forms of agriculture and agroforestry were present in the *terra firme*, in a mosaic of variable population densities that may have included sectors of sparse hunters and gatherers in the more difficult forests and larger semi-permanent populations where vegetation was easily cleared.

We do know from archaeological and other evidence that there were some substantial populations in the *terra firme* forest. Large sites have been seen but not studied. Reports of the Jívaro uprising in 1599 mention mobilization of over 20,000 warriors (Harner 1972, 21). Ethnohistorical accounts indicate Kayapó settlements periodically numbering over 1,000 (Posey 1987, 139, 147). The subsistence base for such numbers is unknown.

The difficulty of clearing mature tropical hardwood forest with stone axes at least partially explains the dramatic differentiation between usually scattered *terra firme* settlement and dense riverine settlement with large, permanent villages. This is not to say, however, that poor soils and limited animal protein are not contributing factors. Involved here is a major debate in Amazonian cultural ecology, and it has yet to be resolved.

The adoption of metal axes and machetes in the New World was generally very rapid where Europeans were present. According to Hans Staden (1928, 74, 90), who lived with the Tupinambá, iron tools were an important trade item on the Brazilian coast as early as 1554.[7] More remote regions obtained metal tools indirectly through trade and raiding, probably on an irregular basis. Isolated tribes continued the use of stone axes well into the twentieth century, although few still do so. This raises a question: how was agriculture and associated foraging and settlement affected when people at times had metal axes and at other times still had to depend on stone axes, or when some farmers in a village had metal axes while others did not? There seems to have been little reporting on this. An "overnight" conversion from stone to steel, as reported by Holmberg (1969, 268) and others, was probably unusual.

There are other questions that need to be pursued. How effective were the iron axes that were introduced in Colonial times?[8] Iron axes must have been less effective than steel axes, but most of the experimental data from Carneiro and others is for steel axes. Also, to what extent were metal axes present in Upper Amazonia in prehistoric times? Lathrap (1970, 178) found bronze axes on the Río Pachitea and Río Pisqui in eastern Peru, clearly traded from the Andes. These were small, however, and probably were not used to cut down large trees. Finally, what was the significance of the stone axe for agriculture elsewhere in tropical America? Gordon (1982, 57–61) discusses the use of stone and metal clearing tools in Central America, including the impact of the machete on forest species manipulation. He notes that: "clearing wet evergreen forest without metal cutting tools would clearly have been a slow and laborious process." He also believes that polycultural *milpas* were an integrated component of anthropogenic forests.

To conclude, shifting cultivation, as an ancient practice in Amazonia, seems to be a myth. There is no evidence for it. At best, it was rare, at least in short-cropping cycle form. It is not logical, given the stone axe. It is therefore a relatively modern adaptation resulting from the introduction of the metal axe. What is the significance of this, if valid? Certainly, it tells us something about pre- and post-Columbian

[7] Jean de Léry (1990, 101), who was on the Brazilian coast at the same time as Hans Staden (1556–58), said that "goods" (including iron axes) from the Europeans let the Indians "have big gardens."

[8] The development of the iron axe was instrumental in the clearing of the forests of northwest Europe in the Middle Ages. Some iron axes had strips of steel welded to the head ("steeling"), but single piece, high-carbon steel axes did not become common until the twentieth century.

adaptation and the impact of European technology: "Production and social organization were altered as each settlement chose a particular method for acquiring manufactured goods, particularly iron tools" (Golob 1982, 269).[9] However, stone axe technology also tells us that the Amazon forest can be farmed successfully and sustainably with minimal destruction by means other than shifting cultivation, which is one of the main instruments of forest destruction today.

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[9] For a treatment of the social and economic impact of the transition from stone to steel in highland New Guinea (the Siane), see Salisbury (1962), and for Cape York, Australia (the Yir Yoront), see Sharp (1952).

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6.2 Indigenous Agroforestry in the Peruvian Amazon: Bora Indian Management of Swidden Fallows.

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Abstract A distinction has long been made between swiddens (shifting or slash and burn cultivation) and fallows in which crops are abandoned to forest and soil recovery. However, our interdisciplinary team found that for the Bora in Peruvian Amazonia there is a continuity from field to fallow to managed forest to mature forest. The early fallows contain a large number of useful species mixed with forest species. The "enrichment" can be both intentional and accidental results of human activity as well as spontaneous. Other research has indicated that such fallow agroforestry is not unique but occurs elsewhere in Amazonia as well as in tropical Africa and Asia. For a more detailed study of Bora agroforestry, see Denevan and Padoch 1988.

Keywords Agroforestry · Amazon · Bora · Fallow management

Introduction

In recent years, students of Amazonia have emphasized that some of the most successful food-producing adaptations to the rainforest habitat have been those of the indigenous tribes and that consequently we have much to learn from these "ecosystem" people. "Refined over millennia, Amazon Indian agriculture preserves the soils and the ecosystem … If the knowledge of indigenous peoples can be integrated with modern technological know-how, then a new path for ecologically sound development of the Amazon will have been found" (Posey 1982, 18; 1983, 225). For similar statements for other tropical regions, see, for example, Nigh and Nations (1980), Clarke (1977), Eckholm (1982, 34–35), and Klee (1980). In particular, Indian cultivation is characterized by multiple cropping [intraspecific and interspecific] and interaction with natural vegetation.

Attention has been directed to several forms of traditional management of tropical forest resources: (1) the diverse, multi-storeyed swidden (shifting cultivation field) which protects the soil and allows for habitat recovery under long fallow (e.g., Conklin 1957; Harris 1971); (2) the house garden, or dooryard garden, was also diverse and multi-storeyed, but with a large complement of tree crops and with soil additives from household garbage, ash, and manure (e.g., Covich and Nickerson 1966); and (3) the planting, protection, and harvesting of trail side and campsite vegetation ("nomadic agriculture" or "forest fields"), involving wild, semi-domesticated, and domesticated plants (e.g., Posey 1982, 1983, 241–243). A related type of plant management is the manipulation and utilization of swidden fallows, a form of agroforestry involving a combination of annual crops, perennial tree crops, and natural forest regrowth.

Swidden-fallow management is apparently widespread among Amazon tribes and some mestizo farmers and rarely among *colono* (colonist) farmers. However, it has received little attention; brief mentions include Denevan (1971, 508–509) for the Campa in eastern Peru, Posey (1982, 1983, 244–245) for the Kayapó in central Brazil, Basso (1973, 34–35) for the Kalapalo in central Brazil, Eden (1980) for the Andoke and Witoto in the Colombian Amazon, Smole (1976, 152–56) for the Yanoama (Yanomami) of southern Venezuela, Harris (1971, 487, 489) for the Waika in southern Venezuela, and Torres Espinoza (1980) for the Shuar in eastern Ecuador. Some observers have assumed that all that is involved is a return to abandoned swiddens to search for residual crops left from the former cultivation, but indications are that actual management occurs, including planting and protection as well as utilization of certain useful wild plants that appear at various stages of fallow succession.

The purpose of this paper is to examine the swidden fallows of an Amazon native group, the Bora of eastern Peru, with the objective of demonstrating how fields are gradually abandoned. This contrasts with most studies of shifting cultivation which focus on why fields are abandoned and which present a sharp distinction between the field (swidden) and the abandoned field (fallow). For the Bora, there is no clear transition between swidden and fallow, but rather a continuum from a swidden dominated by cultivated plants to an old fallow composed entirely of natural vegetation Fig. 6.1. Thirty-five years or more may be required before the latter condition prevails. Abandonment is not a moment in time but rather a process over time.

Agroforestry is currently receiving considerable attention as a potentially stable and ecologically viable form of tropical forest land use (King and Chandler 1978; Hecht 1982; Budowski 1981; Salas 1979; Hart 1980; Spurgeon 1980). One of the major recommendations of the recent US National Research Council (1982, 4, 5, 146) report on tropical development is that the agroforestry systems of indigenous people should be studied and recorded before such knowledge is lost. We believe that certain features of Bora swidden-fallow management can be incorporated into systematic models of tropical agroforestry systems. Indeed, an examination of Bora land use indicates that "agroforestry" is new in name only to native groups in the Amazon. Under denser populations in the past (Denevan 1976), large areas of Amazon forest may actually have been stages of productive swidden fallows. Whole

Fig. 6.1 Bora man carrying peach palm fruits and chambira fronds harvested from a 10–15-year-old fallow (Photo by John Treacy)

biotic components were largely selected and managed, a condition Nigh and Nations (1980) call "intermediate disturbance" and which Gordon (1969, 69; 1982, 73–78) calls an "orchard-garden-thicket" or "tree garden."

The Research Area

Field work was undertaken from July to December 1981 in the Bora settlement of Brillo Nuevo on the Yaguasyacu river, an affluent of the Ampiyacu river (between the Napo and Putumayo) which joins the Amazon at Pebas 120 km northeast of Iquitos. The climax vegetation of the area is humid tropical forest. The closest meteorological station to Pebas is Francisco de Orellana, 75 km distant, where an annual average of 2,757 mm of precipitation was recorded (1964–72). There is a distinct seasonal distribution, with rains peaking from December to May and abating from June through November, but with the driest month (August) still having 133 mm. Temperatures average around 26 °C throughout the year (ONERN 1976, 37). Brillo Nuevo is situated beside an oxbow lake formed by the Yaguasyacu. The area is a hilly, dissected fluvial terrace interlaced with numerous seasonal streams. The soils are primarily deep Ultisols. They include red and yellow clay soils, red and brown sandy soils, and a gray soil in depressions. The Bora prefers to farm the clay soils and red sandy soils (Gasché 1979).

There are 43 families living in the settlement. All are descendants of tribal groups brought to the Ampiyacu from the Igaraparaná-Caquetá region of Colombia following Peru's loss of a border war with that country in 1934. They were resettled on land eventually granted to them by the Peruvian government and to which they retain community title. (The study was undertaken at Brillo Nuevo, rather than with a community long established in its habitat, because of previous unpublished agroforestry research there by project member Salvador Flores Paitán.) The Bora are gradually being assimilated into Peruvian society through missionaries, commerce, and access to Pebas, Iquitos, and Pucallpa. Bora villagers speak Spanish, wear manufactured clothing, and market handicraft items and lumber. Bora subsistence, however, retains many of its traditional elements, with a reliance on swidden agriculture, house gardens, fallow management, high forest collecting, hunting, and fishing. Previous accounts of the Peruvian and Colombian Bora include Whiffen (1915), Jiménez Seminario (1933), Forde (1934), Girard (1958), Gasché (1980), Guyot (1971, 1974, 1975a, b), and Paredes (1979).

Background: Bora Shifting Cultivation

A brief survey of Bora agriculture was conducted to grasp the fundamental dynamics of the system and to understand how cultivation techniques might influence fallow-field character and management. Various aspects of cropping, spacing, and zonation within fields (*chacras* in Peru) and the schedules of planting, harvesting, and weeding are examined below. Together, these affect the eventual structure and composition of the fallows (*purmas* in Peru). Almost the entire area of village land is in some stage of secondary forest due to shifting cultivation since the Bora arrived here 50 years ago. However, stands of old, mature forest are within 20 minutes walking distance from the village and extend northward across the Colombian border.

Family fields are dispersed throughout the forest surrounding the communal *maloca* (residence of the village *curaca* or ceremonial head). Fields are often closely clustered because farmers find it convenient to visit several on one trip. Most plots are accessible within 15 minutes on foot from the *maloca*; others are across the Yaguasyacu and are reached by dugout canoe. Both primary and secondary forest are cleared for gardens. Primary forest sites are recognized as more fertile, while secondary forest (fallow) is closer at hand and more easily felled. The oldest clearly identified fallow is about 35 years of age. There is botanical evidence, however, of secondary forest over 40 years of age. (The presence of buried and surface potshards indicates previous occupation of the area at unknown times, by unknown Indian farmers.)

The Bora say that a minimum of ten years of fallowing is needed before a plot can be cut and planted anew. Most swiddens, however, appear to be prepared from fallows 20 years of age or older. For the Bora, one indicator of a fallow ready to be felled and cropped is a lack of shrubby growth near ground level.

Most fields are cut and burned during the months of least rain; however, a field can be prepared any time the weather permits. Field sizes range from a fourth of a hectare to one hectare. Axes and machetes are the only tools used for felling the forest. Cutting is often accomplished within hours by community work teams, but individual families can cut a field over a period of several days. Often small hills are chosen as field sites, the highest part of the hill becoming the center of the field. Fallen vegetation is allowed to dry for two or three weeks before burning. Selective cutting, a common management technique of swidden farmers, is practiced by the Bora. Valuable timber species, such as tropical cedar, are routinely spared during clearing, and various palms and other useful trees are commonly left in or at the edges of newly cleared fields; others may coppice and be protected.

The Bora plant a wide variety of crops (Table 6.1); however, the main staple is manioc. Some 22 varieties of sweet and bitter manioc are known by the Bora, and a newly planted field bristles with manioc cuttings spaced 50–80 cm apart. The Bora intersperse pineapples, fruit trees, and minor annual crops among the manioc. Both seeds and seedlings of trees are planted. Minimum spacing for fruit trees is said to be between 1 and 2 m. However, as the planting period may extend over several weeks, farmers forget from day to day where tree seeds are already planted and often place seeds closer together inadvertently. Consequently, a few planted trees end up growing virtually side by side.

Some crops are aggregated within the field. Fruit trees are commonly clustered on high land, topography permitting. Areas away from field boundaries, or near trails, also appear to be preferred sites for these trees. Patches from 1 to 2 m^2 are made into planting beds for tubers on sites selected according to ash distribution or local soil variations. The Bora recognize various soil types, based upon texture and color. Coca is almost always planted in well-tended rows near trails and field entryways.

Peanuts, grown in the second or third-year fields, are planted using a special management technique. In a small area from which manioc has recently been harvested, soil (previously loosened by manioc growth and root decay) is gathered and packed into several dozen mounds measuring from about half to 1 $m.^2$ Ashes brought to the fields from home cooking fires are mixed in with the soil as fertilizer. Between 6 and 12 shelled peanuts, previously soaked overnight in a solution of crushed basil leaves to prevent ant predation, are planted in the mounds. From two to four cuttings of sweet manioc are placed laterally into the sides of the mounds.

Bora names for swidden stages are based upon a field's capacity to produce manioc. A field containing the first, most productive planting of manioc is called an *úmihe*. As an *úmihe* is gradually harvested and replanted, it becomes a *kapúuwa*, the term for a field yielding less productive secondary replantings of manioc. The Bora consider two replantings of manioc the maximum possible. When manioc is no longer replanted, the field is termed a *jia*, which is roughly equivalent to a fallow field or *purma*.

Initial crop zonation influences subsequent management options and the pattern of forest regeneration. First, clustering fruit trees in the field center or in areas of access allows them to be easily harvested and weeded as the field matures. Second, heavily

Table 6.1 Common Bora Cultivated and Protected Economic Plants

Common name (English; Peruvian)	Bora name	Scientific name	Use (see code)
*Annatto; Achiote	–	*Bixa orellana*	H, U
*Annona; Anona	Tacááhe	*Annona* sp.	F
*Assai; Huasaí, Chonta	Tóólliuji	*Euterpe oleracea*	F, C
*Avocado; Palta	–	*Persea americana*	F
*Balsa; Topa	Hiiñujuícyo	*Ochroma lagopus*	U
*Banana; Manzana; Guineo	Ujúoh	*Musa paradisiaca*	F, U
*Barbasco	Mujcúrriwa	*Lonchocarpus nicou*	U
Basil; Albahaca	–	*Ocimum micranthum*	F, U
*Breadfruit, Pandilla, Arbol de Pan	Nájhe	*Brosimum alicastrum*	F
*Cashapona	Iíwajyu	*Iriartea* sp.	C
*Cashew; Cashu, Marañón	Anáájihe	*Anacardium occidentale*	F
*Cedar (tropical); Cedro	–	*Cedrela odorata*	C
*Chambira	Niijihe	*Astrocaryum chambira*	C, H
Chili Pepper; Ají	–	*Capsicum* sp.	F
Citron; Citrón	–	*Citrus medica*	F
*Coca	Iípi	*Erythroxylon coca*	M
*Cocona	Roolahé	*Solanum sessiliflorum*	F
Cocoyam; Huitina	Hóónawa	*Xanthosoma* sp.	F
*Copal; Copalhuallo	Mííjíllehe	*Dacryodes sclerophylla*	F, C
Cotton; Algodón	–	*Gossypium barbadense*	M
Cowpea; Chiclayo	–	*Vigna unguiculata*	F
*Cumala	Allíunéhe, Cúúruco	*Virola pavonis*	C
Dale-dale	Cúúnijcye	*Calathea allouia*	F
*Genipa, Huito	–	*Genipa americana*	H
*Guava; Pacay	Tútsehe, Ajivahe	*Inga edulis*	F
*Guayaba	–	*Psidium guajava*	F
Huaca, Barbasco	Hawuámihe	*Clibadium asperum*	U
*Huacra pona	Aallááhe	*Iriartea* sp.	C
*Huamansamana	Méneko	*Jacaranda copaia*	C
*Huicungo	Tsitsábah	*Astrocaryum huicungo*	C
Lemon; Limón	–	*Citrus aurantifolia*	F
*Llanchama	Páácámico	?	H
*Macambo	Aáhe	*Theobroma bicolor*	F, U
*Maize; Maiz	–	*Zea mays*	F
Mamey	–	*Mammea americana*	F
Manioc; Yuca	Aániwa	*Manihot esculenta*	F
*Mauritia, Moriche; Aguaje	Iñéjhe	*Mauritia flexuosa*	F, U
Orange; Naranja	–	*Citrus sinensis*	F
*Papaya	–	*Carica papaya*	F

(continued)

Table 6.1 (continued)

Common name (English; Peruvian)	Bora name	Scientific name	Use (see code)
*Peach palm, Pejibaye; Pifuayo	Méme	*Bactris gasipaes*	F, U
Peanut; Maní	Mátsájca	*Arachis hypogaea*	F
Pineapple; Piña	Cúdsiha	*Ananas comosus*	F
*Plantain; Platano	Ujúoh	*Musa paradisiaca*	F
Rice; Arroz	–	*Oryza sativa*	F
Shapaja	–	*Scheelea* sp.	C
*Shimbillo	Wacháábowa	*Inga* sp.	F
Soursop; Guanábana	–	*Annona muricata*	F
*Star Apple; Caimito	Mutsitsihe	*Pouteria caimito*	F
Sugar Cane; Caña	–	*Saccharum officinarum*	F
Sweet Potato; Camote	Cáátuu	*Ipomoea batatas*	F
Tangarine; Tangerina	–	*Citrus reticulata*	F
Tobacco; Tabaco	–	*Nicotiana tabacum*	M
Tree Gourd; Pati; Calabash	–	*Cresentia cujete*	U
*Umari	Nímuhe	*Poraqueiba sericea*	F
Ungurabe	Ungurahui	*Jessenia bataua*	F
*Uvilla	Báácohe	*Pourouma cecropiifolia*	F
Yam; Sacha-papa	–	*Dioscorea trifida*	F
*Yarina	Tókehííbah	*Phytelephas* sp.	C

*Appears in fallows
Use code
F: Food and beverage
C: Construction or thatching
A: Handicrafts and dyes
U: Utilitarian
M: Medicinal and drugs

disturbed or weeded areas, particularly the coca and peanut zones, will frequently only support sparse, grassy secondary growth. This may be due to local soil exhaustion or compaction, plant allelopathic effects, removal of seedlings of secondary species during intense cultivation, or some combination of these. (See Uhl et al. 1981, for a discussion of microhabitat preferences of secondary seedlings in Amazonia.)

The crop composition of Bora fields can vary widely. Some fields have an apparent low diversity index, planted only with manioc, pineapple, and maize (mainly for poultry) and perhaps a few scattered plantains and bananas. Others are rich in species and numbers and feature complex zonation. While a range of options is to be expected in any swidden system (Denevan 1971), the two extremes seem to be common in Bora swiddens. A similar duality is noted by Harris (1971) for tribes in the Orinoco region of Venezuela, where fields seem to be either primarily monocropped with staples or polycropped with abundant subsidiary plants. In many of these cases, the crop composition in any one field may in part be determined by what a farmer has available from other fields in various stages of development. Since a Bora family may have six or more fields of different ages and crop mixtures, diversity between fields fulfills the same function of assuring a supply of varied crops as does diversity within a single field. Another significant point regarding crops is that simplified fields receive few visits after two or three years of harvests, while diversified fields have longer-lasting utility in the fallow stage.

Bora Swidden Fallows

A series of fields was selected to examine vegetation structures and the process of abandonment. This paper examines plots of three, five, six, nine, and 19 years of age from date of cutting. Each field was measured to determine its approximate size, and the owners were interviewed to record cropping histories and to help inventory plants found within the fields. The vegetation was sampled using the line-intercept method. In each field, zones of vegetation were identified. These included plant communities in areas occasionally weeded and areas of unweeded secondary vegetation. Each zone was sampled by extending two ten m long intercepts into the zone from randomly determined points. Plants along the lines were collected and identified by their Bora names. In addition, Bora informants identified useful plants.

The plots are not strictly comparable in terms of relief or soil type, or planting histories. However, finding a series of fields with identical histories and characteristics is impossible in practice. Nonetheless, a dynamic model of abandonment is revealed by comparing vegetation patterns in the different aged plots.

The swidden fallows described below reflect a strategy of managed succession designed to solve a shifting cultivator's dilemma of how to maintain field production in the twilight of the cropping cycle, while at the same time permitting forest regeneration. Abandonment is similar to Manners's (1981, 360) evaluation of the swidden cropping cycle, which he describes as a "successional series partly regulated by human populations on the one hand, and ecological processes on the other." A *kapúuwa* is chosen to head the sequence here because it represents a stage when human management is still relatively intense and forest regeneration is only just beginning.

Transitional Field: Three Years Old

Figure 6.2 is a representation of an enriched Bora swidden, cut from 30-year-old fallow, located not far from the settlement center. The field has developed multiple canopies, features complex zonation, and contains at least 20 cultigens. Guava, uvilla, macambo, and peach palm are the dominant tree species, all measuring between three and four m in height. The trees provide a 30% field cover but have not reached their peak yielding periods. Fruit-tree density in general is greatest near the

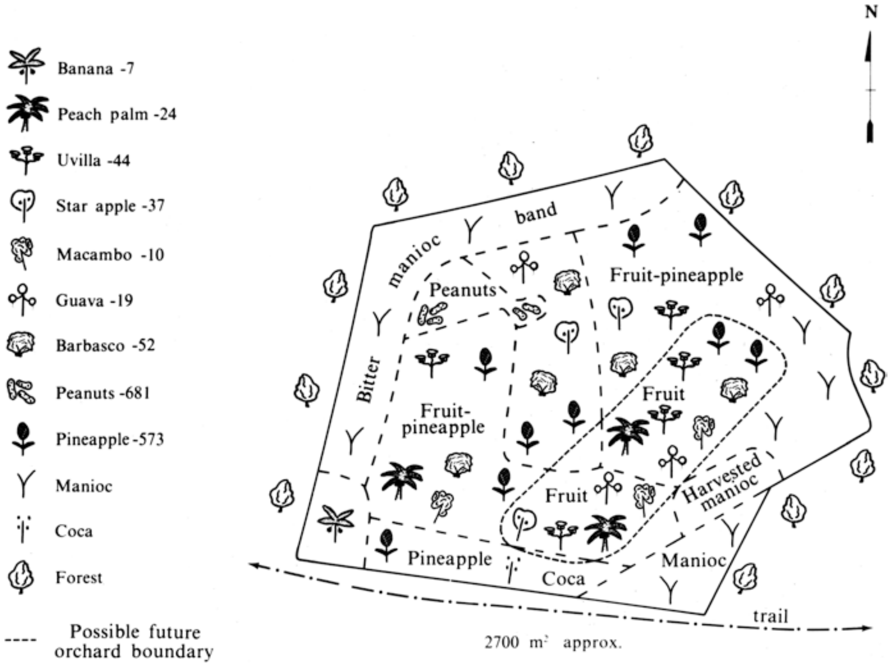

Fig. 6.2 Map of three-year-old transitional field (*kapúuwa*)

southern end along the trail. The understory of manioc is sparse because tree roots and shade prevent replanted manioc from fully developing. A small peanut patch, also containing chili pepper and other minor crop plants, is located near the northwest corner. Bananas are more or less clustered on the southwest downslope corner. The field is surrounded on three sides by 30-year-old forest and on the south by newer fields less than one year old.

The *kapúuwa*, or transitional field, is a rapidly changing mosaic of vegetation reflecting Bora management techniques. Weeding, harvesting, and replanting manioc are performed in one small area at a time, producing a pattern of different aged stands of both manioc and associated secondary growth within the field. Weeds are often pulled out by the roots. In Fig. 6.2**,** the pineapple zone on the left are weeded, and the one on the right is unweeded. Selective weeding, another widespread swidden technique, is practiced by the Bora. Seedlings of useful tree species are often spared; however, it is not axiomatic that all are left untouched.

In fields at this stage, tree coppicing is readily observed. Some of the fruit trees in the field may be coppicing trunks of trees planted in the field when it was a *chacra* 30 years before. *Inga* species, useful as soil nitrogen fixers, are persistent coppicers, so abundant that they are nevertheless dispatched with machetes as annoyances. Others, such as copal, resprout and are protected. This tree grows slowly, reaching harvestable age (for edible fruits) within some 20 years. A bonus of useful coppicers appears to be one advantage in clearing *purma* for new fields.

Because the field is periodically weeded, secondary growth makes little headway except for invasion at the edges where fallen trees were not burned during field preparation. Some two or three m of unweeded field perimeter has been ceded to the encroaching forest. The growth primarily consists of fast-spreading vines and thin saplings.

Transitional/Orchard Fallow: Five Years Old

Some of the processes outlined above were noted in this field, also cut from a 30-year-old *purma* (Fig. 6.3), but at a later stage of development. The field contains a manioc *kapúuwa* zone; however, the unharvested manioc plants were small. As cuttings are routinely thrust into the earth after harvesting the roots, manioc can continue to grow without forming much below-ground material. Manioc is also a persistent plant; cuttings merely thrown aside will occasionally take root. Twelve other cultigens were originally planted, of which six were still clearly harvestable: coca, star apple, peach palm, uvilla, avocado, and barbasco.

Zonation resulting from management is evident. The large coca patch is well weeded and maintained. A small coca patch is abandoned and empty, as is a peanut patch. Secondary growth in both these abandoned areas is limited to short grasses, low herbs, and occasional seedlings of pioneer forest trees. A fruit zone extends the length of the field along the trail. The understory consists of a viney thicket mixed with low herbs growing among old pineapples and stray spindly manioc stems. This thicket forms an intermittent subcanopy 1.5 m in height. The overstorey is primarily comprised of equal numbers of well-spaced, productive star apples (three to five m in height) and peach palm (8–10 m in height).

Secondary vegetation has swallowed about a third of the original plot. The regrowth zone contains trees 10–15 m in height and measuring from 8 to 15 cm in diameter. *Cecropia*, *Jacaranda*, and *Inga* are common. The trees and abundant upper-storey vines form a 100% canopy. The forest floor is a dense tangle of herbs, including abundant *Melastomataceae*, *Piperceae*, and *Araceae*; palms are few.

In this transitional field, pineapples, fruit trees, and minor plants deemed useful are maintained. The pineapples may be harvested for up to five years; thereafter, the fruits produced are small and bitter tasting. Visits to the field follow the ripening schedules of the fruits, although visits for hunting also occur periodically. The main activity besides harvesting fruit is weeding. Coca is weeded every three months; the fruit trees and pineapples receive a slash weeding by machete every three to four months.

The farmer identified many useful plants, both in the weedy orchard and *kapúuwa* zones and in the reforested perimeter. The most immediately harvestable species are vines and low herbs. These include utilitarian vines and ceremonial plants not now used by the Bora, including reeds, once used to make decorative noseplugs and flutes, and plants yielding body paints. Other useful, but not yet harvestable species were construction and other woods in the seedling stage.

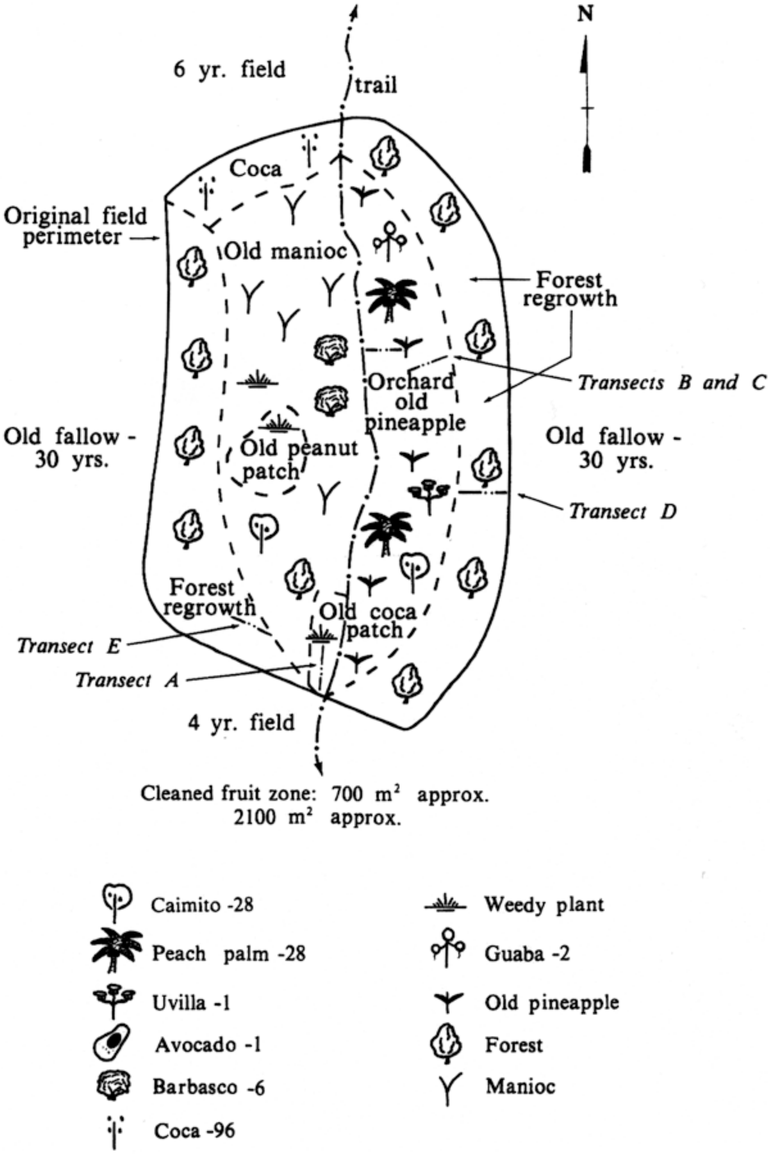

Fig. 6.3 Map of five-year-old transitional/fruit orchard

The reforested zone contained a great number of species. Thirty-four plants appeared on two 10 m transects, 13 of which were considered useful. Six were construction woods; four provided materials for weaving and dyeing baskets; and three were firewoods. Most of the useful forest species in this fallow will not be harvestable for 10–30 years. Rapidly growing construction woods are harvestable, but they are so plentiful around Brillo Nuevo that they receive no special care. The Bora casually harvest useful herbaceous plants as needed.

Orchard Fallow: Six Years Old

This orchard fallow is mapped in Fig. 6.4. Cut from primary forest, it is astride a sloping hill surrounded by newer fields on three sides.

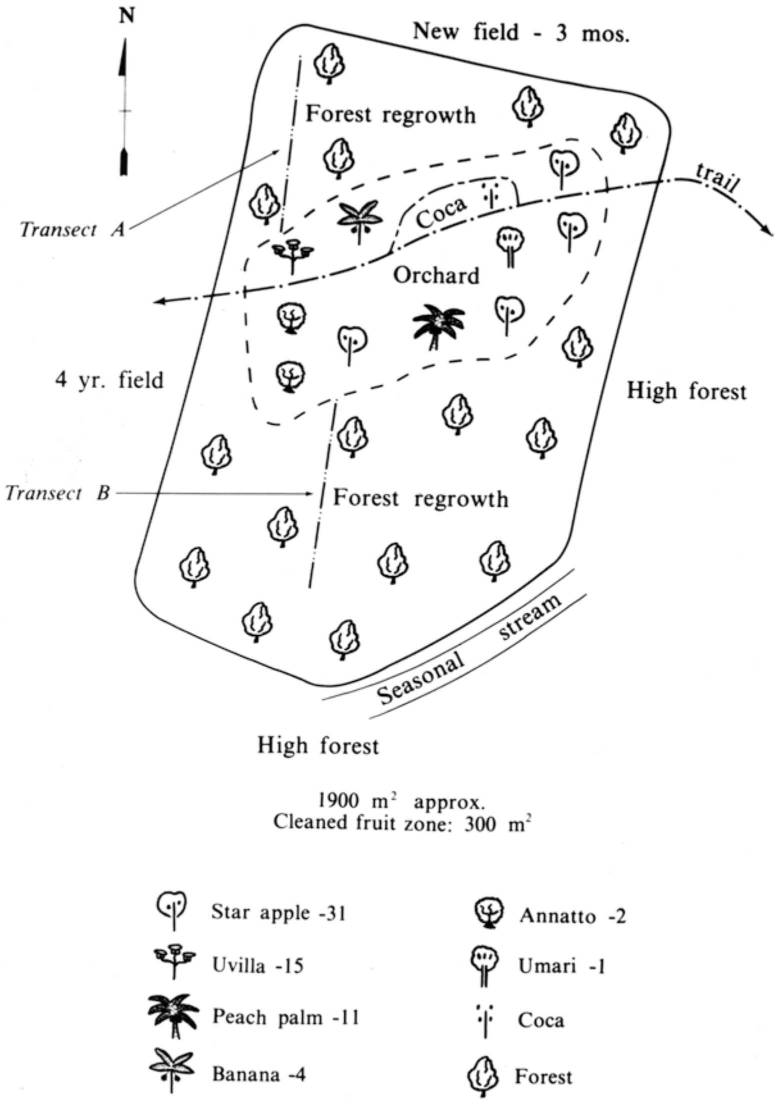

Fig. 6.4 Map of six-year-old orchard fallow

The plot consists of two general vegetation communities: a residual fruit orchard occupying about one sixth of the original cleared area and abundant secondary growth surrounding the orchard. The original field was planted with over 26 crops, some of which are tree species now surviving within the orchard. Star apple is the most numerous planted species, and these trees measure 3–5 m in height. A canopy is formed by uvilla (5–8 m in height) and peach palm (10–13 m in height). Several 18 m tall *Cecropia* trees dominate the orchard area. The orchard has a 70% canopy and is well lit by sun splash. Regular weeding has resulted in an open floor of grassy vegetation covered with slashed mulch. Harvesting fruit in such *purma* orchards is a casual pastime. The Bora use poles equipped with vine loops on the ends to ensnare and pluck fruit-laden racemes from high branches. Coca, however, has suffered from shading, and harvesting is reduced. Cuttings are removed for replanting in nearby new fields. There is little evidence of manioc besides occasional stubble debris.

Growth surrounding the orchard is topped by 25 m tall *Cecropia* and *Rubiaceae* trees towering over dense stands of 10–15 m high trees and old plantain saplings. A thick, shrub understory mixed with abundant short saplings and palm sprouts occupies the entire subcanopy regrowth zone. The forest floor has accumulated a thin layer of leaf litter, and no grasses are present. An array of useful spontaneously appearing species similar to those in the five-year-old field were present in the regrowth zone.

Orchard Fallow: Nine Years Old

This fallow (Fig. 6.5), cut from high forest, demonstrates how long a managed orchard-fallow succession can be maintained. The orchard zone is small, and cultivated trees are few; however, a vigorously growing unshaded coca patch remains. The patch contains 82 evenly spaced, well-tended bushes. Coca is clearly the most valuable crop available here. The owner visits the field on a regular basis to harvest the leaves and on those occasions may refresh himself with uvilla, guava, and star apple foraged from the residual orchard.

The secondary regrowth is a woody thicket, 10–15 m in height, with many vines and sub-storey shrubs. Several useful trees including cedar were on the field's perimeter. Because this field is a downriver site, soils and topography differ from the upland sites nearer the settlement zone. The downriver sites are less well drained, and thus the secondary communities differ from the other fallows studied.

Forest Fallow: 19 Years Old

This older forest fallow (Fig. 6.6) was surveyed for useful tree species. The original *chacra* was cut from mature forest and, according to the owner, planted with at least 11 species, including several varieties of fruit trees.

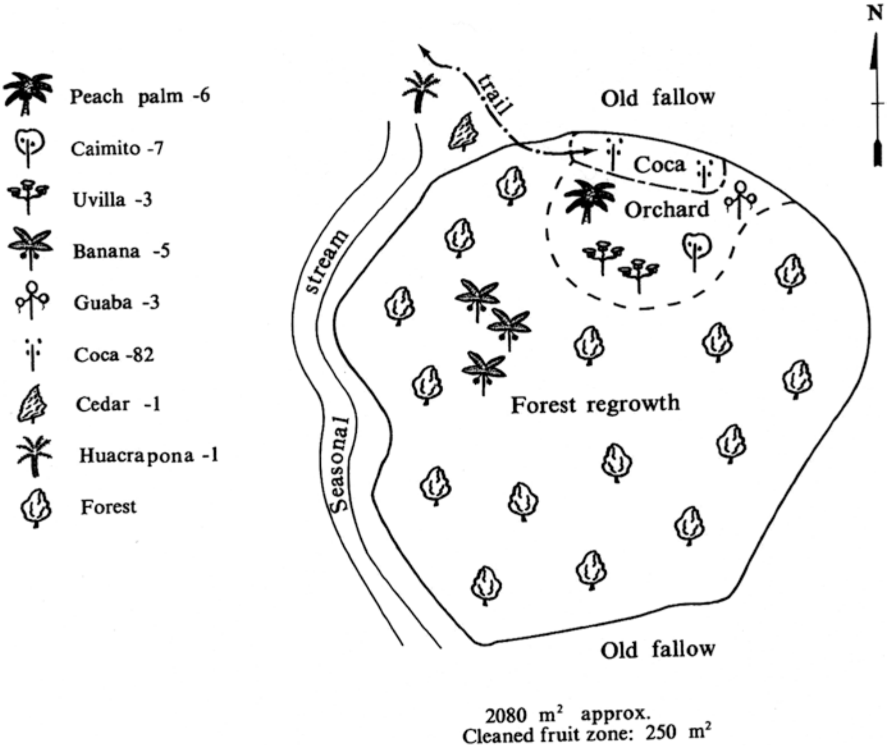

Fig. 6.5 Map of nine-year-old orchard fallow

The forest displayed clear stratification. Low vegetation consisted of herbaceous plants, including ferns, measuring 30 cm to 1 m in height. Above is a second stratum of thin, straight saplings, 5–6 m high, including many palms. Seventy-five percent of the canopy was provided by trees 15–18 m in height, while emergent *Cecropia* and *Jacaranda*, both 25 m tall, filled out the canopy. The forest floor was 40% covered in leaf litter, and walking was unhindered, except in small thicket-filled gaps caused by falling trees. All individual trees measuring 15 cm in circumference within a transect 10 m wide and 102 m long (length of the field) were tallied. Some 233 trees belonging to 82 species were counted. Over half the trees were single occurrences. Our informants identified 22 useful trees in the transect, fitting into the following categories:

(a) Construction materials: 11 species, 25 individuals, including two varieties (three individuals) of highly valued cumala; 13 huicungo palms, used for general thatching, were also present.
(b) Medicinals: four species, four individuals.
(c) Food: two species, 11 individuals, consisting of eight macambos and three assai palms bearing edible fruits.

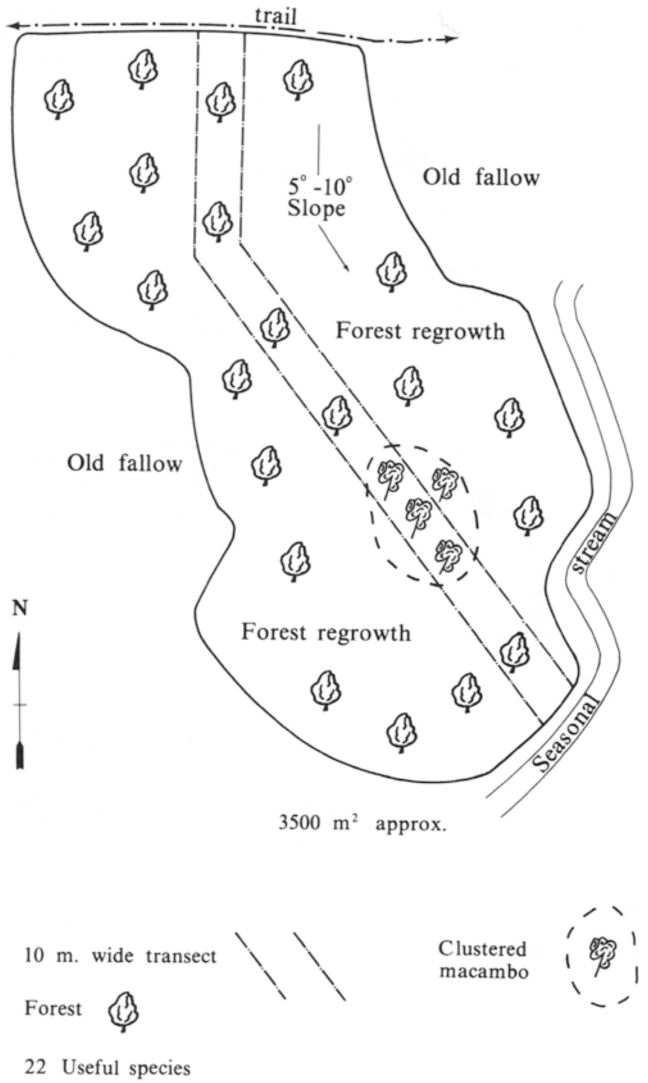

Fig. 6.6 Map of 19-year-old forest fallow

(d) Artisan material: one individual, a dye-bearing tree.
(e) Utilitarian: four individuals, four species. These included three palms from which salt is distilled, and one tree from which pitch is extracted and used to seal canoe hulls.

In addition, there were at least two other species of trees from which edible grubs are harvested. The only apparent survivors of the prior swidden were the macambos, which were clustered within the transect 60 m downslope. These are harvested sporadically.

None of the above trees, all of which are common, appeared to receive individual attention. The cumalas are not yet harvestable, nor will they be for about a decade. This old fallow apparently receives few visits for collection purposes, but hunting trips and grub foraging are frequent.

The Process of Abandonment: Analysis

The Bora recognize that two ecological processes, soil depletion and secondary succession, must be confronted. They acknowledge that manioc is not sufficiently productive to merit harvest after 3 or 4 years, mainly because of soil depletion but also because of weed invasion. Abandonment of fields planted almost entirely in manioc occurs within the space of a year. However, if fields are polycropped with trees, weeds may be the major obstacle to extended field use. Management shifts from replanting manioc to dealing with encroaching secondary vegetation threatening tree crops. With periodic weeding, trees can remain productive for several years before disappearing into the secondary forest, often succumbing to the effects of shading and competition for nutrients.

Our observations indicate that the most productive fallow stage is between about four and 12 years. Before 4 years, fruit trees are not yet producing or have limited production. After 12 years, management is minimal, and many of the smaller useful plants are shaded out. Harvesting of some species continues, however, for up to 20–30 or more years. Another important characteristic is seasonality. The various Bora fruit tree species yield sequentially allowing a spread of produce throughout the year.

A number of tree species planted in Bora fields are, however, adapted to growing in dense secondary forests. Umarí and macambo are common cultivated trees found in old fallows, either growing alone or in groups. These survivors of swidden orchards are valued components of Bora fallows. At 20 or 30 years of age, most fruit trees cannot be easily harvested; however, the Bora occasionally gather the fallen fruits. A valuable function of fallen fruit is that they attract game animals. It is common to find an umarí fruit on the forest floor with tooth marks of a majás (*Cuniculus paca*) or other browser. For this reason, older *purmas* are good hunting grounds.

The process of abandonment and forest regeneration clearly has a spatial aspect. While successionary processes are complex (Uhl et al. 1981), there is a tendency toward a pattern of centripetal forest regrowth which might be explained largely as a result of the history of weeding. Harvesting and weeding of manioc holds regrowth at bay. Once a manioc zone is abandoned, terrain is gradually surrendered to the forest, and the field shrinks in size.

Abandonment is also related to how harvesting proceeds sequentially from grain-producing annuals (rice and maize) to root crops and pineapples to fruit trees and spontaneously appearing utilitarian trees and vines.

Table 6.2 shows the succession of harvestable plants in Bora fields and fallows. While the Bora recognize many useful fallow plants, many go unharvested and are essentially neglected. The main reason for this is that high forest, from where sturdy

Table 6.2 Succession of harvestable plants in Bora fields and fallows

Stage	Planted harvestable	Spontaneous harvestable
High forest	None	Numerous high-forest construction, medicinal, utilitarian, handicraft, and food plants available
Newly planted field (*úmihe*); 0–3 months	All species developing	Dry firewood from unburnt trees for hot fires
New field (*úmihe*); 3–9 months	Corn, rice, cowpeas	Various useful early successional species
Mature field (*úmihe*); 9 months to 2 years	Manioc, some tubers, bananas, cocona, and other quick maturing crops	Abandoned edge zone has some useful vines, herbs
Transitional field (*kapúuwa*); 1–4 or 5 years	Replanted manioc, pineapples, peanuts, coca, guava, star apple, uvilla, avocado, cashew, barbasco, peppers, tubers. Trapped game	Useful medicinals, utilitarian plants within field and on edges. Seedlings of useful trees appear. Abandoned edges yield straight, tall softwoods. Balsa common
Transitional fruit field (*kapúuwa*); 4–6 years	Peach palm, banana, uvilla, star apple, guava, annatto, coca, some tubers. Propagules of pineapple and other crops. Hunted and trapped game	Abundant regrowth in field. Many useful soft construction woods and firewoods. Palms appear, including chambira. Many vines, useful understory aroids
	Planted harvestable	Spontaneous harvestable
Orchard fallow (*Jia*); 6–12 years	Peach palm, some uvilla, macambo. Propagules; hunted game	Useful plants as above, self-seeding *Inga*. Probably most productive fallow stage
Forest fallow (*Jia*); 12–30 years	Macambo, umarí, breadfruit, copal	Self-seeding macambo, umari. High forest successional species appearing. Early successional species in gaps. Some useful hardwoods becoming harvestable, e.g., cumala. Many large palms: huicungo, chambira, assai, ungurabe
Old fallow; high forest	Umarí, macambo	Same as high forest above. Full maturity not reached until 50 years or more

construction woods and vines are harvested, is still a short walk away. At present, most plants used for handicrafts, for example, are taken from high forest. Nevertheless, as the high forest frontier becomes more depleted and distant, secondary growth species become more important. There is evidence that this is occurring. The Bora have recently become interested in planting hardwoods and useful palms in swiddens and in fallowed fields.

Phased Abandonment: Implications for Agroforestry

There are similarities between complex swidden systems and agroforestry systems (Hecht 1982). Agroforestry combines the production of trees and other crops on the same unit of land (King and Chandler 1978), a strategy essentially identical to swidden-fallow management. Both systems rely on the succession of tree crops following the harvests of short-term cultigens.

Viewed in this fashion, Bora agriculture converts to an agroforestry system during the early stages of forest fallow. The enriched swidden to fallow sequence closely resembles the natural succession analogue approach to tropical agroforestry outlined by Hart (1980; also Uhl 1983, 78–79). Hart suggests that select cultigens be placed in the niches normally occupied by common early successional species. The analogue plants would have growth structures and resource requirements similar to those of their weedy counterparts. Thus, rice or maize replaces early annual species, bananas replace wide-leafed *Heliconia*, and late-appearing tree crops mimic early successional tree species. Whether by accident or design, the Bora seem to follow this approach. Bananas do well in low shady areas, where *Heliconia* plants are also common. The most obvious example is uvilla which matches its ubiquitous cousin, the *Cecropia*. Guava is also in the same genus as its semi-domesticated analogue, the shimbillo (*Inga* sp.). Further research may reveal other similarities between naturally appearing species and cultigens which could be incorporated into swidden agroforestry-type models.

Another feature of Bora swiddens that could be useful in agroforestry design is the use of space. Bora tree clustering according to local topographical conditions suggests that slope and terrain should be considered when planning agroforestry plots. More important, slowly abandoning ground to secondary forest may be a sound strategy for tropical farming. There is no reason to think that agroforestry plots should have 100% planted standing biomass. Managed forest regrowth could provide useful products, as well as canopy cover for the soil and a source of stored nutrients for when the forest is cleared to begin the swidden and agroforestry cycle anew.

Swidden-fallow agroforestry, enriched with tree crops planted in areas of forest regrowth, could approximate the "tree-garden" model of silviculture which may have been a pre-European agricultural adaptation in the Caribbean lowlands of Colombia, Central America, and the Maya region (Gordon 1982). This "thicket" model involves a combination of over-storey fruit trees and subcanopy woody shrubs, interspersed with areas of maize, bananas, manioc, and other crops. Systematic swidden-fallow agroforestry would have a fruit orchard core, or series of cores, but these would be embraced by areas of regenerated forest. The forest, in turn, could be enriched by a variety of useful analogue species able to compete in the viny subcanopy, or later on as canopy species (fruit, timber) in high-forest fallow. Timber species would be appropriate late-fallow enrichment trees. Over a large area, swidden-fallow agroforestry would resemble Gordon's image. It would be more a thicket and less a field. Furthermore, the growth rate of managed successions may be as fast or faster than natural successions (Uhl 1983, 79).

Swidden-Fallow Products

The cumulative dietary contribution of fruits and nuts, even when harvested casually, may be significant. Certainly, they provide a continuing (seasonal) variety of minerals, fats, and vitamins to tropical diets dominated by roots and tubers rich in carbohydrates. Some trees, moreover, can provide major staples. The peach palm, very important to the Bora for its fruit and heart, can compete with maize as a nutritious food (Hunter 1969; Johannessen 1966). In addition, plant products useful for beverages, condiments, construction, tools, drugs, and medicines are of more than minor importance to village societies and economies.

As with major and minor natural forest products, those in swidden fallows frequently reach markets beyond the village, at regional, national, and international levels. Even remote traditional cultivators are willing and able to respond to market opportunities for forest products and manage those products accordingly. Pelzer (1978, 286) argues that a large percentage of the rubber, black pepper, copra, coffee, and benzoin harvested for cash in southeast Asia comes from smallholder swiddens through intercropping “in what is ordinarily thought of as the ‘fallow’ period of the swiddens.” The ultimate success of agroforestry systems will depend on such cash cropping.

For isolated communities such as Brillo Nuevo, cash cropping of forest products is problematic. Tropical cedar and other timber trees can be floated downriver to market. One can only be impressed by the Bora planting or protecting tropical cedar seedlings in their swiddens and fallows, anticipating a substantial cash return for their children 30 years later. The use of swidden-fallow products, such as palm and liana fibers, tree bark, and dyes for the manufacture of handicraft items, can bring an income to Bora households. The considerable tourist and export trade in the Iquitos area provides an outlet for traditional items such as hammocks, bags, baskets, bowls, and ornaments. On the other hand, the marketing of perishable food items does constitute a difficult problem for remote villages such as Brillo Nuevo, especially in view of the poorly developed processing and marketing facilities in the region. Toasted macambo nuts, a Bora delicacy, could have market potential. Palms such as *Jessenia* and *Mauritia*, potential sources of edible oils (Balick 1982), are common in Amazon forests and could be integrated into agroforestry models.

The history of the Amazon has been one of commercial harvesting of forest products (quinine, copal, sarsaparilla, barbasco, palm heart, Brazil nuts, rubber, timber). Much of that history involved the destruction of important resources by unwise harvesting practices and the economic and social exploitation of indigenous peoples. Sustainable and equitable procedures are possible, and trade in forest products can be enhanced by incorporating forest species of commercial value into agroforestry systems. Such commercial orientation would, of course, necessitate not only the development of specific agroforestry designs and techniques but also appropriate processing, transportation, credit, and marketing facilities. The economic possibilities for Amazonian plants are vast (Myers 1983). An argument might well be made that the potential value of marketable production from sustained-yield agroforestry plots, including swidden fallows, can be significantly greater per year per hectare than that from cattle ranching or shifting cultivation.

Conclusions

The Bora process of swidden abandonment is in reality a conversion of a short-term cropping system into a longer-term agroforestry system. The main conclusions regarding abandonment and fallow management are summarized as follows:

1. Fallowing is multipurpose. The secondary forest is not only nutrient storage for future cropping but an important niche for secondary crops and useful spontaneously appearing plants. We identified 131 different useful species in Bora fallows. We propose that an appropriate designation be established to account for enriched fallows, a characteristic which may be common in tropical swidden systems. The term "orchard fallow" could be used to describe the structural and functional aspects of traditional agroforestry. In a subsequent "forest-fallow" stage, economic plants are still present but are more dispersed, fewer in number, and less managed.
2. Viewed properly, a swidden site is never completely abandoned as a resource zone. Secondary harvests of fruits, spontaneously appearing species, and even animals continue until the forest is removed for further cropping.
3. There exists an identifiable sequence from original forest with some economic plants present, to a swidden with numerous individual economic plants present, to an orchard fallow or agroforestry phase combining managed economic plants and natural vegetation, to a forest fallow in which economic plants are fewer but still present in greater numbers than in the original forest. Likewise, there is a corresponding sequence in the proportions of biomass which are cultivated or managed, spontaneous economic, and spontaneous non-economic.
4. Research is needed on analogue species with growth architectures and nutrient requirements adapted to secondary forest environments.
5. Swidden-fallow management is not unique to the Bora, (but also appears with other Amazonian groups). It is widespread in Africa (De Schlippe 1956, 215–216; Dubois 1979) and in the Pacific, including the Philippines (Conklin 1957, 125–126; Oración 1963), New Guinea (Clarke 1971, 82–84, 138–139; Hyndman 1982), and Micronesia (Yen 1974). It may once have been common in Middle America (Gordon 1982). These systems need to be studied; it is still practiced by the Huastec in Mexico (Alcorn 1984).
6. Agroforestry drawing on traditional management methods and combining planted species and natural secondary vegetation could be an ecologically appropriate and economically viable alternative to destructive short-fallow shifting cultivation in tropical areas. The ideal model would provide food crops during the swidden stage and cash crops and other crops during the fallow stage. The cash crop perennials should be relatively fast maturing species which can be harvested by around ten years so that the cycle can be renewed as soon as possible. Such a model would help fulfill the need for sustained production of food and other needed products and simultaneously do minimal damage to a fragile environment.

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7.0 Livestock and Landscape

Introducers: Richard Hunter and William E. Doolittle

Abstract Livestock have been portrayed as a bane on the landscape of Latin America, often without fair and unbiased assessment. Here, two articles by William M. Denevan, one on cattle in Bolivia and one on sheep in New Mexico, are discussed. The findings demonstrate how objective scholarship provides more light than heat on a subject that is often contentious because it is misunderstood.

Keywords Bolivia · Cattle · Grazing · Landscape · Livestock · Historical geography · Sheep · Southwest

Introduction

William M. Denevan's greatest influence within geography and cognate disciplines has arisen from his investigations into native demographics and related environmental changes in the New World. His historical geographical research has done much to dispel "the pristine myth," a term Denevan (1992) coined to describe the belief that the New World was lightly populated and a little modified by indigenous peoples. However, he has studied more than this (as this volume attests). Furthermore, his approach has never been dogmatic in the sense of maintaining a position and gathering data in its support. The line of thought often associated with Denevan and his students is one that insists asking questions is more important than proffering answers a priori. Perhaps, this is no better seen than in his writings on livestock, particularly cattle and sheep, introduced into the Americas by Spaniards in the 1500s. Denevan wrote these articles about very different environments on two different continents, work that testifies to his breadth as well as depth of scholarship. Their early publication dates and repeated citations stand as evidence that they were both pioneering and seminal articles.

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"Cattle Ranching in the Mojos Savannas of Northeastern Bolivia" (Denevan 1963a) focuses on herding in a seasonally inundated environment. Although dealing with events of the late twentieth century, it is framed in a historical context, dating back to 1682. The article traces fluctuations in herd sizes over time, revealing the importance of political stability, markets, other products, and technology. Of particular note in this article is that we can observe Denevan's early interest in the history of population sizes, although in this case he was writing about cattle rather than people. In Mojos, he recognizes that the annual flooding is the limiting factor on cattle numbers rather than drought, disease, or any other variable. High floods kill or submerge edible grasses and sedges, with the only viable forage growing on *islas* that become trampled or cattle drown trying to reach. The piece argues – successfully – that cattle ranching, then a topic of neglect and disinterest, is worthy of geographic investigation. This article is also noteworthy in that philosophically it can be considered political ecology (e.g., Robbins 2004), albeit *sans* the dogmatic rhetoric that often accompany such studies.

"Livestock Numbers in Nineteenth-Century New Mexico, and the Problem of Gullying in the American Southwest" (Denevan 1967) focuses on sheep and environmental degradation. Arroyo-cutting was doubtless occurring throughout the Southwest for millennia, but it came to the fore in the 1880s due to the proclivity of ranchers to view environments as stable and to build their houses in valley bottoms, two very big mistakes. Ranchers typically built slightly upstream of knickpoints. With a few heavy rains and runoff events, all were lost. Word of damage caused by headward cutting spreads quickly, thanks to the nascent newspaper business (Aschmann 1982). This, in turn, leads scholars to begin exploring human impacts on the environment – specifically overgrazing. Lines were quickly drawn; some scholars argued that livestock caused gullying, while others claimed there were changes in rainfall patterns. Denevan took an open-minded and even-handed approach to the issue. He explains, "The lack of widespread gullying prior to 1870 has previously been assumed to be explainable by the absence of overgrazing and consequently a denser, protective vegetation cover, or by climatic conditions favoring alluviation rather than erosion. The historical evidence of livestock and vegetation conditions during the period 1788–1848 weakens the overgrazing argument" (Denevan 1967, 693). The evidence to which Denevan refers derives from his research into livestock numbers in the early nineteenth century that revealed how the late Spanish and early Mexican ranchers stocked the ranges of northern New Mexico with nearly as many sheep as in the late nineteenth century when widespread arroyo-cutting ensued. He notes that grazing pressure was also probably higher in the later period because ranchers often kept their sheep in enclosures to protect them from "hostile Indians" rather than allow them to graze unrestricted as was common practice in the earlier period. What Denevan uncovered was that sheep numbers in and of themselves – and overstocking, however that phenomenon is measured – are insufficient to induce arroyo-cutting in this environment. Arroyo-cutting occurred in the late nineteenth century rather than the early nineteenth century not because there were so few sheep in the earlier period but rather because arroyo-cutting is triggered by a combination of "drought, followed by several years of heavier than average summer storms, high livestock numbers, and a probably weakened vegetation cover" (Denevan 1967, 702). Subsequent investigations into historical arroyo-cutting in New Mexico as well as global locations have reached similar conclusions (see, e.g., Jones 2015; Butzer and Helgren 2005, which also appeared in the *Annals* nearly four decades later).

This pair of articles marks Denevan as a pioneer in the field. Historical ranching and the environmental effects of such have now become a fertile research area within Latin American historical geography. Topics of subsequent studies are numerous and diverse. Some examples include livestock introductions (Doolittle 1987), the development of a Mediterranean-like agro-pastoral complex replete with transhumance (Butzer 1995), the formation of haciendas and latifundios (Aguilar-Robledo 2003; Edelman 1992), the establishment of distinctive ranching cultures (Jordan 1989; Bell 1998), the contribution of Africans (Sluyter 2012), the historical productivity gains of Colombian ranchers (Van Ausdal 2012), the expansion of cattle ranching in Brazil's Mato Grosso (Wilcox 1999), how a cultivation-to-pasture land use change may have contributed to the Little Ice Age climate anomaly (Hunter and Sluyter 2015), and the use of new technologies for understanding pastoral landscapes of the past (Hunter 2014). An interesting connection within, while illustrating the diversity of, the so-called Berkeley School of Geography (Spencer 1976) is that Denevan's study area of northern New Mexico was the same as that of Yi-Fu Tuan, a former fellow graduate student who was trained in geomorphology (a student of John Kesseli), went on to become the New Mexico state climatologist, and later gained fame as the discipline's foremost humanist geographer (e.g., Tuan 1974). New Mexico's livestock heritage has also been the focus of at least one historian who received geographic training at Berkeley (Dunmire 2013).

These two early articles connect to other research Denevan was conducting at the time as well as foreshadow his later career trajectory. In addition to his piece on cattle ranching in the Mojos savannas, Denevan also published in that same year a report on the quantity and distribution of earthworks in the Mojos that he made from low-flying aircraft (1963b). His report describes extensive landscape modifications including raised fields, causeways, mounds, and circular ditches. To explain this once highly managed landscape, he suggests "there were large populations of well-organized people" before the Jesuits arrived in the late seventeenth century (1963b, 543). Interestingly, in his article on the history of cattle in the Mojos, Denevan describes another, much more recent episode of landscape abandonment. He describes that by 1950 meat companies had acquired cheap World War II surplus aircraft, which raised the value of Mojos cattle because beef could now be transported to urban centers such as La Paz at a much lower cost than the traditional overland cattle drives. With their cattle suddenly much more valuable, many ranchers chose to round up and slaughter every animal they could find. "Because of the decrease [in cattle]," Denevan (1963a, 43) writes, "many ranches have been abandoned, thus creating a rural depopulation as both ranchers and their workers have left for the highland cities or the Santa Cruz region."

In his 1967 piece, he identifies and dispels a false belief that the Spaniards, Mexicans, and indigenous people of early nineteenth century northwestern New Mexico must have had relatively few sheep because there was little environmental degradation dating to that period. In a way, this was another kind of "pristine myth" mentality perpetuated by Anglo latecomers. Indeed, relying on documentary numerical data and chroniclers' accounts, to meticulously reconstruct livestock population sizes, is much the same approach that Denevan subsequently used to conclude that the entire New World had a far greater pre-European human population than previously accepted. As he writes succinctly elsewhere on this topic, "The evidence is convincing" (Denevan 1992, 370).

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7.1 Cattle Ranching in the Mojos Savannas of Northeastern Bolivia

Original: Denevan, W.M. 1963. Cattle ranching in the Mojos savannas of Northeastern Bolivia. *Yearbook of the Association of Pacific Coast Geographers* 25:37–44. Reprinted by permission of the Association of Pacific Coast Geographers.

Abstract In the Mojos (Beni) savannas of Bolivian Amazonia cattle production has been the primary economy since the Jesuit missions in the late seventeenth century. For 250 years this involved the rounding up of wild cattle. Because of seasonal flooding alternating with seasonal drought the raising of cattle was difficult resulting in high mortality every year. And because of isolation marketing was limited, requiring long drives to Santa Cruz. After World War II beef companies used cheap surplus planes for flying raw meat to La Paz and Cochabamba. Most of the wild cattle were soon rounded up. Formal ranches began to be established on high ground, including houses, fencing, corals, and air strips, along with controlled breeding and conversion of vintage criollo to Cebu stock. High prices and good transport resulted in over exploitation, and cattle numbers declined from over one million in 1952 to about 350,000 in 1962. Nevertheless, Mojos continued to be the main source of beef for the capital city of La Paz.

Keywords Cattle ranching · Bolivia · Mojos

Introduction

The Llanos de Mojos is a seasonally inundated tropical savanna occupying about 90,000 square km in the Beni Department of northeastern Bolivia. Today, the region is undeveloped, has a population of only 120,000, and remains isolated from the rest of Bolivia. Cattle ranching has dominated life in Mojos for over two centuries, but cattle have nevertheless been almost worthless until recently. However, since 1950, as a result of the use of meat cargo planes, the Mojos savannas have become the major source of beef for La Paz, and cattle are now the most valuable product of northeastern Bolivia.

Both physically and economically, the Mojos savannas have much in common with other large grassy savannas in South America: the lower Orinoco *llanos*, the Amazon *campos*, and the vast Pantanal de Mato Grosso. All of these savannas are seasonally flooded, and all are important livestock raising areas today. Mojos, however, is somewhat unique for the severity of its annual floods, for the past neglect of ranching, and for the partly negative effects of the recent and sudden availability of major markets for beef. An examination of some of the past and present aspects of cattle ranching in the Llanos de Mojos therefore seems desirable, with an emphasis on the striking changes which have taken place since ranching was described in the late 1940s and early 1950s (Weeks 1946; Comisíon Ganadera al Beni 1953; Osborne 1956, 88–91).

Background

The Mojos savannas occupy most of the Beni basin, which is located between the Andes and the western hills of the Brazilian Highlands and is filled with thousands of feet of young sediments washed down from the Andes (Fig. 7.1). This unusually level plain ranges in elevation from roughly 1000 ft. in the south to 600 ft. in the north, and the gradient is about one foot per mile. The basin is drained by the Río Beni, Río Mamoré, and Rio Guaporé (Itenez) which along with the Río Madre de Dios join in an apex near the northern border of Bolivia to form the Rio Madeira, a major tributary of the Amazon. During high water, these rivers and their tributaries overflow, and this flooding, combined with standing rain water, results in as much as 80 percent of the savannas being under several inches to several feet of water between December and June. Rainfall averages 60–75 in. a year and is concentrated between October and May. Vegetation is directly related to relief, and the associated amount of flooding, but is also strongly influenced by burning. The lowest ground has sedges (*Cyperaceae*) where permanently wet and grasses (*Graminae*) where seasonally flooded. Higher ground subject to only brief flooding has open scrub savanna dominated by palms (*Copermicia*, *Acrocomia*) and species of *Tabebuia* and *Curatella*. Sites seldom or never flooded (*islas*), such as natural levees, low divides, and low mounds, are usually forested. Rain forest or semi-deciduous tropical forest surrounds the Llanos de Mojos on all sides. The savanna soils consist mainly of acid clay loams with hard pans and low organic content. Because of the low fertility and poor drainage of the grassy savannas, all agriculture today is confined to the better soils of the forested *islas*.

Mojos was one of the legendary El Dorados of South America but remained unexplored until an expedition reached it from Santa Cruz de la Sierra [to the south] in 1580. The Indian tribes encountered had large, palisaded villages, elaborate crafts, and a total savanna population of probably several hundred thousand. Some of the tribes adapted to the inconveniences of flooding by constructing causeways, mounds, and raised and ditched crop rows. The remnants of these pre-Spanish

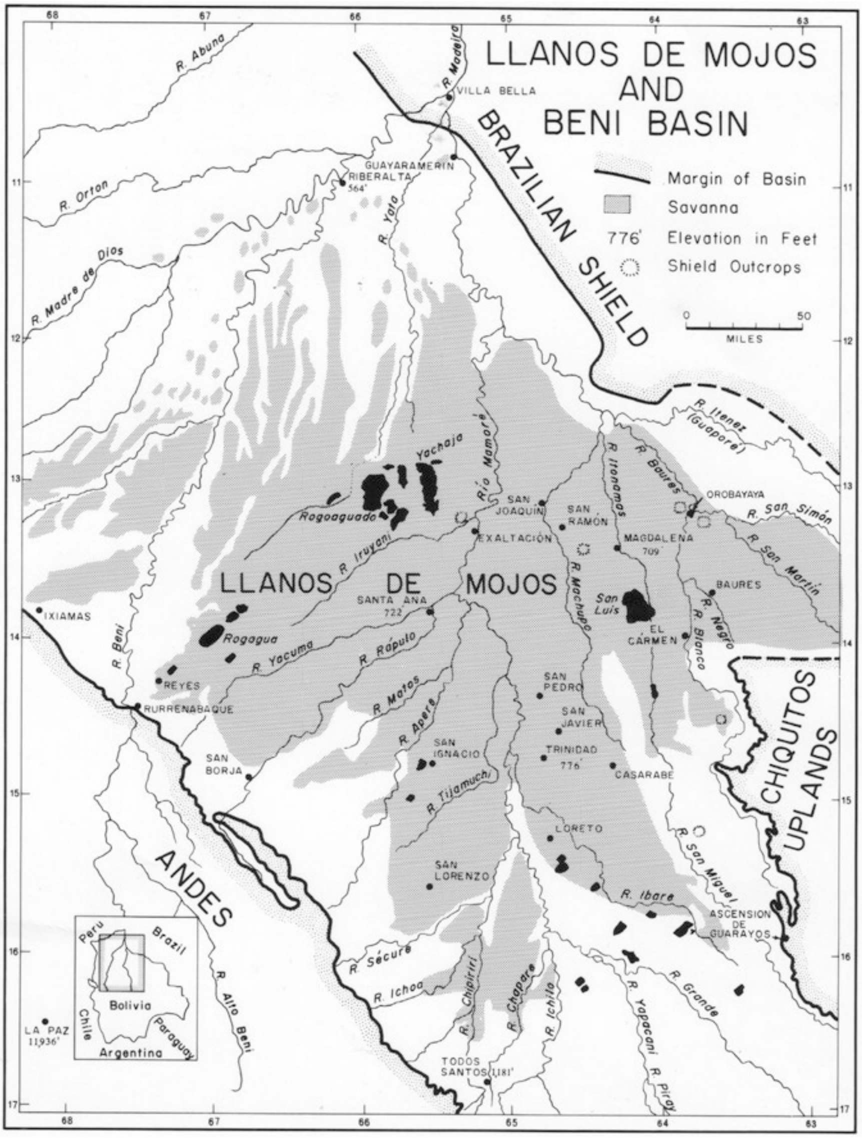

Fig. 7.1 Map of northeastern Bolivia and the Llanos de Mojos

earthworks, numbering in the tens of thousands, can still be seen but are little known (Denevan 1963). The first European settlements were Jesuit missions founded in the late seventeenth century by which time the Indians had been decimated by disease and slave raids. The Mojos missions prospered with a cotton textile industry and with large cacao and cotton plantations in the forests, but both the settlements and the economy deteriorated after the Jesuits were expelled in 1767.

History of Ranching

Cattle were introduced to Mojos by the Jesuits in 1682, when a herd of about 200 head survived a 54-day overland trip to the Loreto mission from Santa Cruz (Anonymous 1743, 456). All the missions established ranches and raised cattle for meat, milk for the Indians, and tallow for export. By 1767, there were about 55,000 cattle in 15 missions (René-Moreno 1888, 46). The Indians became excellent horsemen and *vaqueros* but seldom raised their own animals, preferring to hunt wild cattle.

Following the expulsion of the Jesuits, virtual wars of extinction were waged against livestock in Mojos. Cattle were considered part of the public domain and were treated as an unlimited natural resource to be exploited without restrictions. The present prevalence of cattle thieves in the Beni reflects the survival of this tradition. The mission cattle declined under the new curate administrators who killed them mainly for tallow, and some missions were abandoned because of the scarcity of meat and the necessity for the Indians to return to hunting wild game. Administrative reform brought protection, however, and cattle numbers increased.[1] After Bolivian independence in 1825, increasing numbers of Spaniards entered Mojos from Santa Cruz, received grants of land from the government, and established large private cattle ranches. Also, the government often paid debts and salaries by issuing letters of credit or bonds for so many thousands of head of [wild] cattle in Mojos.

The only major market for Mojos beef was in Santa Cruz, and there was a small but regular dry season movement of cattle thereto through the Chiquitos uplands. Losses from thirst, hunger, and exhaustion were very high, but the cattle initially cost little or nothing, so there was some profit. Many of the cattle were slaughtered for the preparation of *charque* (dried strips of beef) for both local consumption and export, but the majority were killed only for their tallow and hides, which were sent in long caravans of oxcarts to Santa Cruz and from there to the mining towns of the Altiplano. In the Mojos villages and ranches, the abundant tallow was used for fuel, while rawhide strips and hides found many uses.

The descriptions of the cattle industry of the nineteenth century (Keller 1875, 182–185) are reminiscent of the slaughter of the bison in the United States. Large numbers of cattle were killed in single roundups, including calves and pregnant cows, and beef loss from spoilage was high.

> The thousands of cattle which grazed on the vast plains were decimated, driven in herds to Santa Cruz, or slaughtered in the open for the sake of their fat. The carcasses were abandoned in the pampa, and the people did not even bother to select the cattle to be slaughtered (Saucedo 1942, 31).

Decrees passed to prohibit such abuses were ineffective. The herds recovered once more, however, in the years from 1870 to 1920 when many of the ranchers and their Indian *vaqueros* left the *llanos* for the booming rubber regions to the north.

[1] The *Archivo de Mojos*, Vol. 9, No. 43 (Biblioteca Nacional, Sucre) contains a 1786 circular prohibiting the killing of cattle or sale of horses “in order that they might multiply for the benefit of the province.”

Cattle numbers increased to over one million by 1900, and from then until 1952 estimates vary from one to one and one-half million (Bayo 1911, 396; Suárez 1930; Chirveches 1952). Most of these cattle were feral or nearly so, ranged unfenced and uncared for, and fended for themselves against jaguars, floods, and drought; breeding was uncontrolled and pastures unimproved. Travelers often found foot movement through the *llanos* dangerous because of herds of wild bulls.[2] The American Navy Lieutenant, Lardner Gibbon, reported 60,000 wild cattle in the Loreto *pampas* in the 1850s (Gibbon 1854, 251), and 100 years later an estimated 500,000 wild cattle were reported in just the Lago Rogoaguado area of northwestern Mojos (Osborne 1956, 88). After 1950, when beef prices began rising considerably, wild cattle were rapidly rounded up. Bush pilots who had helped ranchers spot and shoot or capture wild cattle reported that few were left in 1962.

Until about 1950, there continued to be large cattle drives to Santa Cruz and from there to northern Argentina, where the stock was rested and fattened up and re-exported by railroad to La Paz. Cattle are still sold in Brazil, mainly in Guajará Mirim at the southern end of the [former] Madeira-Mamoré railroad. In 1949 Pôrto Velho at the northern end of the railroad was receiving 6000 head of cattle a year from the Beni (Osborne 1956, 90), but this number is less now that small ranches have been established along the railroad. Some Beni cattle, however, still reach the Amazon via this railroad and the Rio Madeira.

The *Estancias*

The largest ranches in the Llanos de Mojos, such as those of the firm of Suárez Hermanos (once a rubber empire), were confiscated by the government in the 1950s as part of Bolivia's land reform program. Today, the ranches average about 5000 ha and less than 3000 head of cattle; few ranches, if any, exceed 40,000 ha and 15,000 head of cattle.

Ranches have changed little from pre-aviation years, mainly because few ranchers care to invest their profits in their ranches. Surface travel is mainly by oxcart, horseback, and canoe, and only a few ranchers have a truck or jeep. Most ranches have a *trapiche* for grinding sugar cane and possibly a small lumber mill. Crops include several hectares of rice, *yuca* [manioc], plantains, and sugar cane. Ranch buildings and corrals are of palm wood and are built on *islas* of high ground not normally subject to flooding, which average 5–10 ha in size. At most, a ranch may have 200 or 300 ha of barbed-wire-fenced pasture. Only a few ranches have a *gran casa* with running water, electricity, and some degree of comfort. Usually, the owner's house has daub-and-wattle walls, a thatched roof, dirt floors, and one screened room. Most ranches have a small landing strip, and the owners live in town [or La Paz] and commute via bush plane.

[2] Col. P.H. Fawcett (1952, 235), the British explorer, commented in 1913 that "We were warned of the wild bulls, which had killed many foot travelers."

In addition to a *mayordomo*, the average ranch has half a dozen of permanent Indian *vaqueros*. Some owners will give men (*partidarios*) 20 or 30 head of cattle, and after 5–10 years, they receive a certain percentage of all the young steers. The *partidario* receives free grazing land, medical care, tools, and immunization service in addition to the initial cattle. Most of the farming on ranch property is done by *inquilinos* (tenants) who are obligated to sell produce to the rancher at the price given in the nearest town.

The typical ranch has mostly criollo cattle, whose ancestry dates back to the early colonial period. These cattle take over four years to mature, often produce calves only once every two years, and provide low quality, tough, and tasteless beef. A few ranchers have some improved breeding stock which are full or part Cebu. Only the towns and largest ranches have air fields of sufficient length to handle meat planes. The cattle are driven to the nearest airfield where a *frigorífico* (meat company) maintains corrals and a slaughter house, but no refrigeration. The cattle are butchered overnight, and the meat is flown out the next morning.

In 1962, the *frigoríficos* paid about 28 cents per kilogram for beef, carcass weight. The round-trip cost of transporting the beef to La Paz was about 9 cents per kilogram, with each plane carrying a load of supplies from the highlands and then returning with about seven and a half tons of beef (USDA 1962, 55–60). The time from slaughter to arrival in La Paz is only seven to eight hours; consequently, there is little loss from spoilage, but neither is the meat allowed to age before sale. If a plane loaded with meat is delayed in takeoff, the beef is dried. Butter, cheese, tallow, and *charque* may be shipped by boat or trail to river towns or to Brazil, and increasing numbers of cattle reach Cochabamba by river boat and then truck via the Chapare region (Todos Santos area).

The Mojos savannas have an average yearly grazing capacity of about 10 ha per head (Braun 1961). Most of the range grasses are perennial, native tussock types, including species of *Panicum*, *Paspalum*, *Sporobolus*, *Leersia*, *Axonopus*, *Tripsacum*, and *Andropogon*. Introduced grasses being grown on a few ranches include jaragua (*Hyparrhenia rufa*) and molasses grass (*Melinis minutiflora*) on high ground and *pará* grass (*Panicum purpurascens*) and Guinea grass (*Panicum maximum*) on low ground. The best pastures are found in May and June after the floods have largely receded. By August, the mature grasses are dry and fibrous and are rejected by cattle. The grasses are burned, and new shoots appear promptly without benefit of rain. These grasses must last until the November rains; however, if the rains start late, there may be a second burning, but this invariably results in a poor wet-season growth. The critical periods for cattle are (1) the end of the dry season in September and October when forage is poor and water is scarce and (2) the periods of maximum flooding between January and March, which last from a few weeks to 2 or 3 months.

The annual floods limit the potential maximum number of cattle in the Llanos de Mojos rather than drought, poor forage, or disease (aftosa, rabies, brucellosis). Edible grasses and sedges are killed or submerged during periods of high flooding, and with the main exception of the water hyacinth (*Eichomia*), forage is only available on the scattered *islas*. If forested the *islas* may have very little forage and what there is may be trampled and destroyed by herds of cattle and also by wild animals.

During flooding, thousands of cattle die from starvation, drowning, or exhaustion while attempting to swim long distances between *islas*. Near the town of Santa Ana, some of the local ranches lost 80 percent of their stock in an unusually big flood in 1959. Even during the years with low floods, such as 1962, there is a high mortality of calves, and the calf loss from all causes exceeds 50 percent annually.

Little effort is made to drive cattle out of the low savannas to areas of high ground before the start of flooding. This is partly because of long distances to sizeable areas of high ground as well as the presence of dense forests in such areas. A few ranchers do maintain barges or boats for rescuing stranded cattle during flooding and for moving as many as possible to safe areas. Ranchers talk of using bulldozers to build artificial *islas* for cattle refuges although few have done so. Nor is anything done to provide supplemental feeding during flooding. Ranchers do their best to save their cattle after a big flood strikes, but the general attitude is one of lazy optimism that each year's flood will not be bad, and there is little preparation for potential disaster.

The Impact of Aviation

Beef has been flown directly out of the Mojos savannas to the cities of the Altiplano since the end of World War II when Bolivian meat companies began acquiring cheap war planes (B-17s and C-47s).[3] By 1952 meat cargos amounted to several million kilograms a year, and today beef from Mojos is not only the main source of meat for La Paz but is also important for Cochabamba and some mining centers. In 1960, 5200 of the 7500 tons of beef marketed in La Paz came from the Mojos savannas (La Patria 1961). Suddenly, nearly worthless cattle were valuable, and it was El Dorado all over again. Cattle were slaughtered regardless of age, sex, or quality as many ranchers rounded up all the cattle they could find, drove them to the nearest suitable landing field, sold everything, and left the Beni. Town officials in Loreto reported that 15 ranches in that area alone were abandoned in the 1950s. The sell-out-and-get-rich attitude of the ranchers is partly a lack of faith in the future of Bolivia's economy and in the security of land holdings, but it also reflects the old attitude toward cattle as a resource to be exploited for its immediate worth rather than as an industry based on sustained yields and improvement of stock.

The Beni is a unique example of an area where the economic benefits of a revolutionary improvement of transportation have thus far been largely outweighed by unfavorable side effects. Today most movement to, from, and within the Beni is by plane. This movement includes not only people and beef but also rubber, Brazil nuts, vegetables, hardwood lumber, and heavy machinery. As a result of the use of meat planes, cattle declined from over one million in 1952 to about 350,000 in 1962 (Victor Vargas Monasterio, Inspector Forestal del Beni, Trinidad, September 16, 1961, pers. comm.). This decrease partly represents losses from floods and increased disease, but mainly it results from the slaughter of cattle at a faster rate than new

[3] In 1946 the Bolivian Development Corporation began flying small amounts of beef from their ranch at Reyes to La Paz (Macaulay 1946).

cattle are being raised. Because of the decrease, many ranches have been abandoned, thus creating a rural depopulation as both ranchers and their workers have left for the highland cities or the Santa Cruz region.

In the Mojos savannas, meat is now too expensive for most local people, whereas formerly the poorest Indian family had several or could shoot cattle without fear of recrimination. Absentee ownership has increased as most owners now commute between town and ranch by bush plane. The large towns of the Beni, especially the capital, Trinidad, have increased in size and prosperity, but numerous small towns have deteriorated. Also, there has been a decline in both overland and water transport; oxcart and cattle trails are less used, and the large paddle-wheel steamers that once plied the Beni, Mamoré, and Guaporé rivers are now hulks rusting on the banks. The dominance of air transport can continue, despite high gasoline costs, as long as there are cheap airplanes and government subsidies available. Once these benefits come to an end, river transport and connecting roads will once more become important. Possibly by that time navigable waters of the Beni rivers will be connected by good roads to the highland cities.[4]

Conclusion

The history of cattle in northeastern Bolivia has been one of neglect, disinterest, and exploitation. These attitudes reflect the relatively minor economic importance that cattle long had due to the lack of a readily accessible market. However, cattle ranching has continued to be neglected and, in some respects, has declined since the booming of beef prices with the use of planes to fly meat to the highlands. Prosperity did not immediately result in major changes in traditional methods of raising cattle. The situation contrasts significantly with ranching in other New World savannas, most of which have not experienced extreme isolation. Ranching in these areas has been more progressive than in Mojos, and where some beef is now flown to market as in the Orinoco llanos, the overall effects have been favorable.

Recently, the Bolivian government, United States, and United Nations technicians and individual ranchers have been endeavoring to stabilize and modernize ranching in the Mojos savannas by introducing foreign breeding stock, artificial insemination, disease control, fencing, new pasture grasses, and experimental stations. By 1963, cattle ranching seemed to be recovering, or rather, true cattle ranching seemed to be developing and replacing what can be realistically described as the hunting of semi-wild cattle.

[4] Roads are planned which will eventually connect (1) Cochabamba with Puerto Villarroel on the Río Ichilo, the most navigable of the upper tributaries of the Río Mamoré; and (2) either Reyes or San Borja in the southwestern *llanos* with La Paz via the Alto Beni region.

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7.2 Livestock Numbers in Nineteenth-Century New Mexico, and the Problem of Gullying in the Southwest

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Abstract In the 1880s, intensive accelerated erosion began producing large gullies throughout the Southwestern United States. This modern arroyo cutting was originally attributed to deterioration of the protective vegetation cover because of below normal rainfall and overgrazing by excessive numbers of livestock (4,000,000 sheep in New Mexico in 1880). However, recent studies have stressed the greater importance of increased high intensity rainfall. Additional perspective is provided by an examination of livestock numbers in the upper Rio Grande region of New Mexico during the 19th century, particularly during the Mexican period when the ranges were heavily stocked with sheep (possibly 3,000,000 head in the 1820s), but with little or no gullying. The incomplete record of livestock numbers in relation to climate and gullying backs up the climatic argument but also gives some new support to the older view that overgrazing was a major contributive factor causing severe modern gullying.

Keywords Ranching · Rio Grande · Erosion · Overgrazing

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Introduction

The subject of arroyo cutting or gullying in the American Southwest, on which there is considerable literature, has recently received an excellent review by Tuan (1966). Modern (post mid-nineteenth century) gullies were initially attributed to accelerated erosion caused by overgrazing and consequent impairment of the vegetation cover. However, the discovery of prehistoric gullies has led to the formation of two different and conflicting theories: the Bryan-Antevs model associates arroyo cutting with drought and poor vegetation cover and arroyo filling with higher rainfall and an improved vegetation cover. The more recent Martin-Schoenwetter model, on the other hand, associates gullying with increased summer high-intensity rainfall when there may actually be a greater annual rainfall and an increased vegetation cover.[6]

On the basis of field work and historical studies of the Chaco, Puerco, and Tesuque canyons in New Mexico, Tuan presents evidence to support the Martin-Schoenwetter theory. In addition, Tuan points out that there is not as close a relation between modern arroyo cutting and overgrazing as had previously been thought. In some areas of New Mexico, gullying began before the rapid increase in sheep numbers in the 1870s (from 619,000 in 1870 to nearly 4000,000 by 1880).[7] Some gullies reached a state of equilibrium before conservation measures begun, and in other areas, gullying continued after conservation measures had been initiated. Also, modern gullies have been reported in areas where there apparently has been little or no grazing.[8]

An examination of livestock numbers, especially of sheep, in relation to vegetation, climate, and gullying in New Mexico prior to 1870 throws additional light on the suggested secondary role of overgrazing in causing accelerated gullying. The main purpose of this paper is to present evidence that the ranges of the upper Rio Grande region of New Mexico (Fig. 7.2) may have been nearly as heavily stocked, but without serious erosion, in the late Spanish-early Mexican period, as during the late nineteenth century when intensive and widespread gullying occurred.[9]

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[6] For a discussion of these two models and for references, see Tuan (1966, 583–584, footnote 2).

[7] Gordon (1883, 994) For 1880, 3,938,831 sheep and only 347,936 cattle were reported (includes entire Navaho Reservation stock); horses and mules were not reported.

[8] Dellenbaugh (1912), Gregory (1917, 132), and Peterson (1950, 421). Of interest would be a comparison of the history of erosion on the overgrazed and seriously eroded Navajo Reservation with that of the Apache Reservations where sheep and goat raising have not been important but climate generally has been comparable.

[9] The United States Department of Agriculture in 1937 reported that 75 percent of the drainage basin of the upper Rio Grande in New Mexico had experienced moderate to advanced accelerated erosion, and that "every large and practically every small valley of the watershed has been channeled from 50 to 100 percent of its length" by vertical-walled arroyos as much as 300 ft. wide and 30 or more ft. deep. See Cooperrider and Hendricks (1937, 2, 11–12).

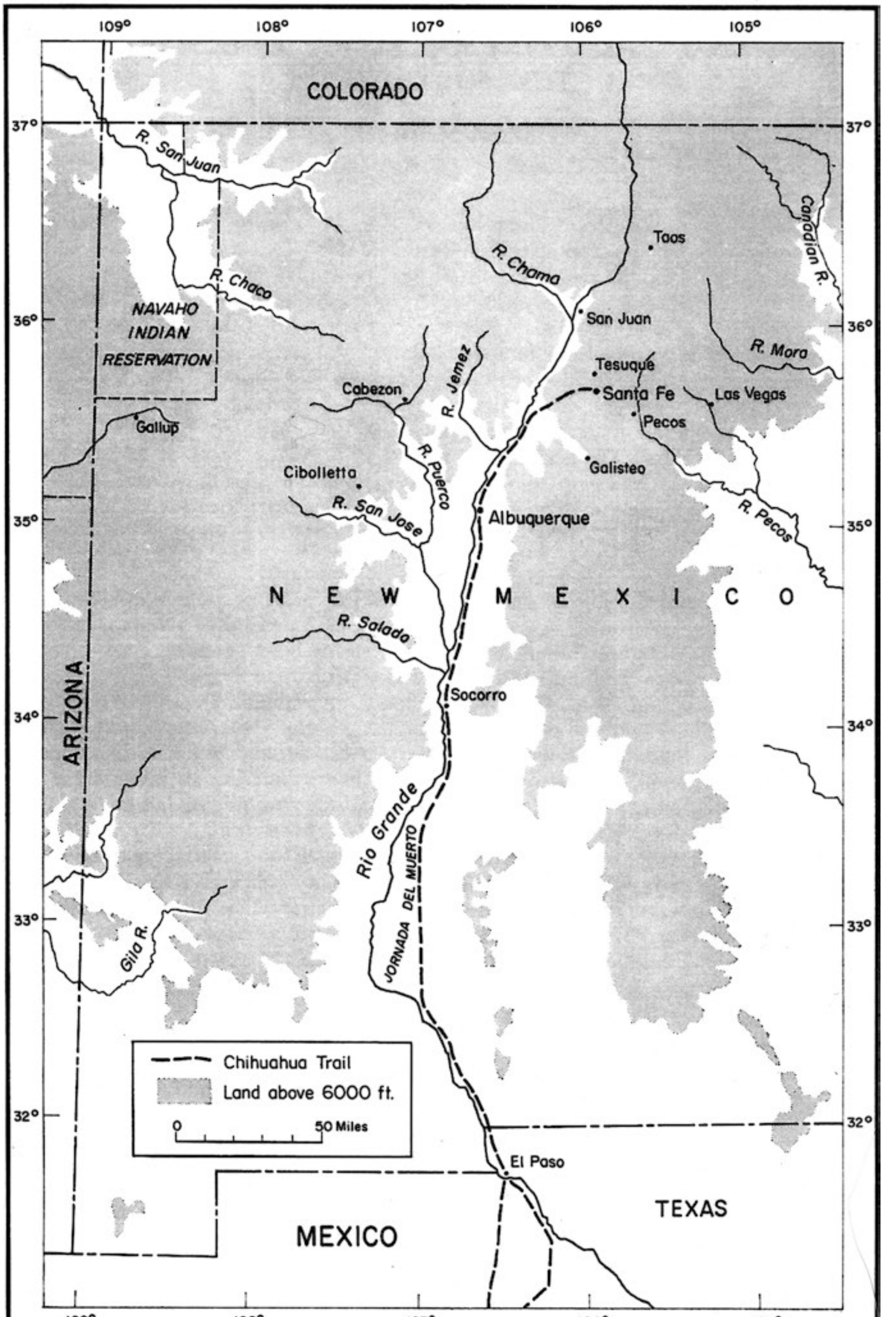

Fig. 7.2 Location map of the upper Rio Grande region of New Mexico

Modern Gullying[10]

Before proceeding to the history of sheep in New Mexico during the early to middle nineteenth century, it must be emphasized that there is evidence for only very localized gullying during the same period. Bryan dated arroyo cutting throughout the Southwest by examining early travelers' accounts and talking with old-time ranchers, and he was able to document a post-1880 initiation of severe erosion in most areas. A good example of the historical record of trenching of a stream channel is that for the Rio Chaco in northwestern New Mexico. Lt. J.H. Simpson on a military reconnaissance in 1849 wrote that "The Rio Chaco, near our camp, has a width of eight ft. and a depth of one and a half" (Simpson and McNitt 1964, 42). The channel in this area in 1925 was 20–30 ft. deep and 150–450 ft. wide. Channel trenching of the Tesuque Valley probably began about 1880, which is when accelerated erosion began in the nearby Santa Fe area (Miller and Wendorf 1958). Many other examples could be given (Bryan 1925; Thornthwaite et al. 1942, 102–104).

In all the Southwest, the only stream for which pre-1850 arroyo trenching is well documented is the Rio Puerco in New Mexico.[11] In crossing the Rio Puerco above Cabezon in 1849, Simpson reported the channel to be 30 ft. wide with vertical banks 20–30 ft. high, which had to be cut down so the Army could get its artillery across (Simpson and McNitt 1964, 29). This and other mentions of arroyos by Simpson and other travelers led Bryan, Leopold, and Tuan to conclude that discontinuous trenches existed before 1850 in many parts of the Rio Puerco Valley and its tributaries, but that they did not become continuous until after 1880.

Early American travelers reported that the plant cover in the Rio Puerco Valley in the 1840s was mostly poor (Leopold 1951a, 301–305). For example, Lt. J.W. Abert, who was with General Kearny in 1846, wrote that for the Rio Puerco 48 miles above its junction with the Rio Grande "... the valley, deep with sand, only nourishes artemisias, yucca, and cacti" (Abert 1962, 95). This was supposedly an excellent grazing area in the late eighteenth century, with 10,000 head of cattle and sheep in the 1760s and was the location of a much-disputed land grant (Twitchell 1914, 41–42). However, the Mexicans abandoned the upper Puerco in 1823 because of pressure from Navajo raiders and apparently no longer took their herds to the area (Bryan 1928). Abert wrote:

> At Albuquerque we were cautioned by people against the dangers we would run before reaching Cibolletta, as the war trail of the Navajos runs through the valley of the Puerco;

[10] Following the usage by Antevs, the words "arroyo" and "gully" are used here as geomorphic terms for wide, flat-floored and deep, vertical-walled channels, old as well as modern, in the southwestern United States. Desert washes, which are also flat floored and steep walled, differ from arroyos by being eroded mainly in gravel and independently of vegetation condition (Antevs 1952, 375).

[11] Leopold cited early travelers who reported gullies elsewhere in New Mexico and Arizona, but the descriptions are vague as to the size and nature of the gullies. Many, especially in New Mexico, apparently were desert washes rather than true arroyos (Leopold 1951a). For a description and history of the Rio Puerco Valley see Widdison (1959).

> and the Mexicans advised us to travel with great circumspection, and not to make any fires at night (1962, 73).

On the other hand, the Navajo themselves may have grazed their own large flocks in this area after 1823 and thereby maintained pressure on the plant cover. Quite possibly, however, the discontinuous gullying of the Puerco before 1850 was primarily associated with localized climatic conditions which favored trenching.

To what degree did extensive irrigation works retard accelerated runoff and erosion? Reagan suggested that many small irrigation diversion works on the tributary streams in the old Hopi Pueblo areas retarded for centuries what would otherwise have been rapid runoff (Reagan 1924, 283–285). The density of some of the areas of Hopi settlement is evidenced, for example, along Laguna Creek in Arizona where there are ruins of 202 villages. The Spaniards and Mexicans apparently irrigated much more than did the Indians, and their dams and diversions on the tributaries of the upper Rio Grande may have offset increased runoff caused by overgrazing. An estimated 30,000 acres of land were irrigated in the Rio Grande Valley prior to 1846 (Thomas 1948, 37). There are numerous references to the large irrigation systems around individual Mexican settlements; Abert, for example, related how he and his group got entangled in a labyrinth of water-filled irrigation canals (*acequias*) at the mouth of the Rio Jemez and required help to get out (Abert 1962, 71.[12] Whether either Indian or Spanish-Mexican diversion works alone, retarded arroyo cutting is questionable, but they may have had some effect and might be further studied with this in mind. Actually, it is more likely that abandonment of irrigation works did not precede and cause gully erosion but followed erosion. Cooperrider and Hendricks felt this to be true in the upper Rio Grande watershed where many irrigation works were abandoned in the 1890s (Cooperrider and Hendricks 1937, 14–18; Widdison 1959, 273–276). Bryan even concluded that Pueblo migrations in the fourteenth century were a result of the natural arroyo cutting of the late thirteenth century which destroyed the Indian methods of floodwater farming (Bryan 1941).

The lack of widespread gullying prior to 1870 has previously been assumed to be explainable by the absence of overgrazing and consequently a denser, protective vegetation cover, or by climatic conditions favoring alluviation rather than erosion. The historical evidence of livestock and vegetation conditions during the period 1788–1848 weakens the overgrazing argument. Nineteenth-century chroniclers and travelers in New Mexico reported individual herds and annual exports of sheep numbering in the hundreds of thousands, even millions, during the early part of the century when arroyo cutting was either absent or very localized. Of the many studies made of accelerated erosion and overgrazing in the Southwest, apparently none consider the possibility that the ranges may have been heavily stocked well before the period of severe modern gully erosion.

[12] For a good description of the Mexican irrigation systems in New Mexico see Davis (1857, 195–200).

The remainder of this paper will examine the evidence for large numbers of livestock in New Mexico before the American take over and then relates this evidence to the problem of gullying in the Southwest.

Sheep in Early New Mexico

Prior to the Civil War, the upper Rio Grande region of New Mexico contained far greater numbers of livestock, predominantly sheep, than any other part of southwestern North America, and the region is the major exception to the generalization that the southwestern ranges were not heavily stocked until about 1880. However, there were also some large ranches in parts of southern Arizona in the early nineteenth century, and the Navajos in Arizona and New Mexico had very large herds of sheep even before the tribe was defeated and placed in a reservation in 1868.[13]

The upper Rio Grande Valley lies mainly between 4000- and 7000-ft. elevation and is fringed on the north, west, and east by mountains rising to between 8000 and 14,000 ft. On the margins of the valley, however, there are wide plains, tablelands, and tributary valleys. The climate is semi-arid with generally between 10 and 20 in. of rainfall, but there is considerable variation from year to year and from area to area. The vegetation consists mainly of grasses and xerophytic shrubs.

Almost all the Spanish and Mexican settlements in New Mexico were confined to the Rio Grande basin, mainly between Taos in the north and Socorro in the south. Most of the towns were near the Rio Grande and were linked together by the Camino Real or Chihuahua Trail leading into Mexico. Hostile Indians hampered occupation of the upland valleys to the east and west and to the south lay desert – the Jornada del Muerto. Numerous towns or inhabited places were founded before New Mexico became a part of the United States in 1848, the first being San Gabriel (later San Juan) which was established by Juan de Oñate in 1598. Many of the early settlements, which included missions and large haciendas, were short-lived, but some still exist.

The years 1788–1848, which include the closing years of Spanish rule and the period of Mexican control of New Mexico, saw a great flourishing of the sheep industry in New Mexico. Sheep were brought into the region in 1598 by Oñate and thereafter multiplied in the missions and then dominated the *ranchero* economy after the missions declined at the end of the eighteenth century.[14] However, almost continual warfare with the Pueblo, Navajo, Apache, and Comanche Indians kept

[13] Large sheep haciendas around Tucson and Tubac, broken up by Apaches in the 1830s, were mentioned by Haskett (1936, 9). The Navahos were reported to have had 500,000 head of sheep in 1846, mainly in large herds, according to Luomala (1938, 56); based on an Indian Bureau Report. There were 30,000 sheep in the Hopi pueblos as early as 1779, according to Father Escalante as reported by Towne and Wentworth (1945, 163).

[14] At least three-fourths of the New Mexicans derived part or all of their income directly or indirectly from sheep in the view of historian Hallenbeck (1950, 299).

New Mexico in constant turmoil that prevented large-scale ranching, except locally, for much of the time until after the Civil War. A brief golden age of relative peace and prosperity followed the governorship of Juan Bautista de Anza, 1778–1788, when the main hostile Indian groups were subdued. Anza's successor, Fernando de la Concha, was very interested in promoting the raising of sheep as evidenced by correspondence in 1789 requesting that the exporting and slaughtering of female sheep be prohibited in order to increase the size of the small New Mexican herds (Bloom 1927).

With substantial military protection, there was less danger from Indians on the open range, and sheep numbers greatly increased from 1788 until Mexican independence in 1821. The Mexican government, however, was not able to give adequate military support to the New Mexican settlements; the Indians again became uncontrollable, and partly as a result, sheep raising began to decline by the 1830s.[15] A low point was reached after the United States took over New Mexico in 1848.[16]Apaches and Navajos dominated the ranges and stole tens of thousands of sheep until defeated by the US Army after the Civil War. Then, following the entry of railroads into the West, sheep numbers in New Mexico boomed from several hundred thousand to a peak of between four and five million in the 1880s.

The soil erosion survey by Cooperrider and Hendricks mentioned only one figure for livestock numbers in the vicinity of the upper Rio Grande settlements during the Spanish and Mexican periods. This is 240,000 sheep and goats in 1827. The following comment was made:

> such large numbers of animals, if the ranges adequately supported them, indicate a higher grazing capacity and a vastly better condition of the ranges close to these settlements during the early years of the nineteenth century than now exist (Cooperrider and Hendricks 1937, 28).

There is evidence, however, that there were far more than sheep in New Mexico at times during the early nineteenth century. The figure of 240,00 sheep in New Mexico comes from a livestock census included in a report in 1849 by Don José Escudero, a lawyer and statistician. But Escudero made the further comment that:

> …we can assert without doubt that even this document has not set forth the real rural wealth of New Mexico. It would be curious and extremely interesting to have before us a report of the numerous herds of sheep which annually have gone out of New Mexico for cities as far distant as Mexico City, and which still continue to go out in spite of the small price they bring (Carroll and Haggard 1942, 40).

At the other extreme, the historian Hallenbeck recently estimated that:

> … the average sheep population of New Mexico during the period 1790 to 1820 was about 3,000,000 head, with an annual increase of some 800,000 to be slaughtered or sold outside the province (Hallenbeck 1950, 299).

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[15] After 1830, "the herds declined eighty percent," according to historian Fergusson (1936, 104). Also, Gregg (1954, 134–135) and Abert (1962, 52).

[16] There were 380,000 sheep in New Mexico in 1850 and 830,000 in 1860, on the basis of official counts and estimates according to Coan (1925, 365).

My efforts to verify such a large figure, or any other figure, have only been partly successful. In original sources, there are just a few mentions of livestock numbers, and these vary greatly and are generally incomplete. Many secondary sources fail to give adequate references. For example, several writers on both the history of New Mexico and the history of sheep in America refer to the statement by Charles Lummis of Governor Bartolomé Baca owning 2,000,000 sheep and Governor Francisco Chávez owning 1,000,000 sheep during the early Mexican period (Lummis 1893, 19). Lummis, however, writing in the 1880s, gave no sources and was probably passing on local, possibly exaggerated tradition. The evidence for large numbers of sheep in New Mexico in the first half of the nineteenth century is mainly in the form of contemporary estimates. The strength of this evidence lies in the relatively large number of known estimates rather than in the accuracy of any particular one or in the reliability of any particular source.

During the Spanish and Mexican periods, there was, surprisingly, little mention of stock numbers in New Mexico by local writers. There were, however, many general references to the importance of sheep raising. For example, in 1832 the Mexican official Don Antonio Barreiro wrote:

> The thousands of sheep raised in this territory have no parallel anywhere else in the republic. This livestock increases from day to day in an incredible manner (Carroll and Haggard 1942, 103).

Most of the early livestock estimates available are from accounts by American travelers and are for the Mexican period from 1821 to 1848. The estimates can be grouped into three categories: (1) holdings by single owners, generally wealthy families referred to as *ricos*; (2) exports to Mexico and California; and (3) Indian thefts.

The first detailed description of New Mexico by an American, that of Z.M. Pike in 1807, tells us little about the Spanish economy but does report a single herd of 20,000 sheep (Pike 1889, 263). Herds of 80,000 in 1825 were mentioned in an account of the travels of a Dr. Willard, and herds of 40,000 were reported by Abert.[17] Of possibly greater reliability are the comments by J. Gregg who spent considerable time in New Mexico between 1831 and 1839. He noted that: [18]

> Nothing, perhaps, has been more systematically attended to in New Mexico than the raising of sheep. When the territory was at the zenith of its prosperity, *ranchos* were to be met with upon the borders of every stream, and in the vicinity of every mountain where water was to be had … in former times there were extensive proprietors who had their *ranchos* scattered

[17] Anonymous (1962, 242): "where farmers have six or eight thousand horses or mules, forty thousand cattle, and twice as many sheep." This is the only mention encountered of sizable numbers of other livestock besides sheep. For New Mexico as a whole, for most of the nineteenth century, sheep probably comprised 80–90 percent of the livestock total, in contrast to less than 50 percent in 1965; Abert (1962, 52).

[18] Gregg (1954, 133–334). A single proprietor owning "as many as three hundred thousand sheep" in the 1830s was mentioned by Davis (1857, 204); the source probably was Gregg, although Davis did live in New Mexico himself for several years.

> over half the province, in some cases amounting to from three to five hundred thousand head of sheep (Gregg 1954, 133–134).

Sheep on the hoof and wool products were the principal items of trade with Mexico. For over 200 years, commerce over the Chihuahua Trail was considerable and much more important for New Mexico than was the much-publicized activity on the Santa Fe Trail. Each fall, and sometimes also in the spring, large caravans or *conductus* left Albuquerque and Santa Fe for Chihuahua and Sonora. Often over 1000 people were involved as well as an armed escort and large numbers of loaded wagons and livestock. For the New Mexicans, this was the big event of the year, the only social and commercial contact with the outside world. Commercial records for these *conductas* should give an indication of sheep numbers in early New Mexico, but apparently few such records still exist. Moorhead, who has made a study of the Chihuahua Trail, recognized that the chief trade items were wool blankets and sheep, but the only official figure presented from the Spanish and Mexican archives is a report by Governor Chacón of a drive of 18,784 sheep from Santa Fe to Chihuahua in 1800 (Moorhead 1958, 45). In 1803, Governor Chacón reported that from 25,000 to 26,000 sheep were driven annually to (Nueva) Vizcaya (Bloom 1927, footnote 33). For the same period, Pike mentioned a single drive of 15,000 sheep to Mexico (1807) and an annual total of 30,000 to Mexico City, Biscay (Nueva Vizcaya), Sonora, and Sinaloa (Pike 1895, 305). Much larger totals, however, were reported in later years.

Gregg wrote in 1844 that:

> Between ten and twenty years ago, about 200,000 head were annually driven to the southern markets; indeed, it is that, during the most flourishing times, as many 500,000 were exported in one year (Gregg 1954, 133–134).[19]

Support is given to such large exports in the 1880 census where there are references to the son of Colonel Manuel Chávez saying that his father in 1839 took 75,000 sheep to Mexico and other ranchers a total of 225,000 and to people who "popularly reported" that in 1840 500,000 sheep were taken to Mexico, including 300,000 by one owner (Gordon 1883, 989). Such large numbers, if correct, probably could not have been taken south in the one or two annual *conductas*. Hallenbeck maintained that only a few sheep were ever taken on the annual *conductus* since the sheep traveled too slowly. He said that the total of 300,000 in 1839 was broken down into numerous herds of about 15,000 each (Hallenbeck 1950, 313).[20] This seems reasonable, and Indians seldom attacked movements of such size.

Further evidence of large numbers of sheep in early New Mexico comes from estimates in the 1880 census of 551,000 head of sheep being exported to California between 1852 and 1857, including 200,000 in 1856 (Gordon 1883, 992). Another

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[19] Also, see Davis (1857, 204); again, his source was probably Gregg.

[20] Hallenbeck (1950, 313); no sources given. A contemporary report (1832) of the size of sheep drives into Mexico was provided by Don Antonio Barreiro: "There are some men who have contracts in Durango to deliver annually fifteen thousand sheep …"; Carroll and Haggard (1942, 109).

estimate gave a total of 376,000 in 1858.[21] In 1854, the US Boundary Commissioner John Bartlett, on the basis of reports by Assistant US Marshals, stated that Indians had stolen 450,000 head of sheep from the Rio Grande settlements between 1846 and 1850 (Bartlett 1854, 385–386). All of these figures are for a period in New Mexico when livestock raising was supposedly at its lowest ebb.

The Navajos and Apaches stole a great number of sheep from the Mexican and New Mexican ranchers. A typical example of their impact was given by Abert in 1846:

> This morning we received notice of an incursion of the Navajos, a few miles below us [south of Albuquerque]. The *pastores* left their flocks and fled, while a large body of Indians, rushing down from the mountains, where they had secreted themselves during the night, devastated the whole valley, killing all the human kind they met, and sweeping off the flocks and herds of the Mexicans. No less than 5,000 sheep were carried off within twenty miles of the great city of Albuquerque (Abert 1962, 96).

Gregg also had an interesting comment on the sheep raiding activities of the Indians:

> Indeed, the Indians have been heard to observe, that they would long before this have destroyed every sheep in the country, but that they prefer leaving a few behind for breeding purposes, in order that their Mexican shepherds may raise them new supplies (Gregg 1954, 135)!

The well-documented hostility and sheep raiding activities of the Indians have led several recent writers to assume that livestock in New Mexico was kept close to the settlements, and therefore, that livestock numbers were never large in early New Mexico. Leopold in his vegetation study of Southwestern watersheds in the nineteenth century said that "--- until well past the middle of the nineteenth century extensive grazing had been prevented by frequent raids of hostile Indians" (Leopold 1951a, 295). Cooperrider and Hendricks (1937, 29) wrote that "frequent raids of marauding plains lndians restricted the areas on which the flocks could graze with safety".

If such restrictions were really severe, then there could not have been very large numbers of sheep in New Mexico. However, stock numbers undoubtedly fluctuated greatly from time to time, increasing when more peaceful Indian relations allowed movement onto the interior ranges. The Indians were much less of a problem in the late Spanish and early Mexican periods than they were after about 1830; nevertheless, there are numerous counts of herds seen far from settlements by American travelers even in the 1830s and 1840s when the Navajos were particularly troublesome. Abert in 1846 reported large flocks of sheep, cattle, horses, and goats between the Moro (Mora) and Pecos rivers well to the east of Santa Fe (Abert 1962, 37–41). Gregg, for the 1830s, wrote:

> Even upon the arid and desert plains, and many miles away from brook or pond, immense flocks were driven out to pasture and only taken to water once in two or three days (Gregg 1954, 133).

[21] Richardson and Rister (1934, 371); no source given; Gordon (1883, 992) however, said that the export of sheep from New Mexico to the Pacific Coast was terminated in 1858.

Furthermore, the Indians seldom dared to attack the main settlements and large ranches, so the large numbers of stock they stole must have been obtained mainly from Mexican herds on the open range.

How many sheep, then, were there in New Mexico at their most flourishing times between 1788 and 1848? Figures range from 240,000 in 1827, the only official total, but a discredited one, to the undocumented 3,000,000 estimated by Hallenback as the annual average from 1790 to 1820. Reported sizes of individual holdings range up to 2,000,000 given by Lummis, and reports of annual exports to Mexico vary from 30,000 in 1803 according to Pike to 200,000 to 5,00,000 given by Gregg and others for the Mexican period. In this writer's opinion, the evidence for peak numbers approaching 3,000,000 in the 1820s seems to be good. Additional support comes from the tenth US Census in 1880, in which there is an excellent report on livestock by Gordon, who made a detailed survey of the range in New Mexico in order to describe forage and water and stock numbers by county. Many people who were alive or whose parents were alive during the Mexican period were interviewed, and on this basis Gordon stated that:

> … the tradition is that importations with the different settlers and the natural increase had fully stocked the present New Mexico with sheep long before 1800, and that stock numbered as many from 1825 to 1835 as in 1880 [nearly 4,000,000] (Gordon 1883, 986).

The available evidence, admittedly based mainly on estimates and tradition prior to 1850, suggests that for New Mexico there was a general increase of sheep to between two and three million between 1820 and 1835, a general decrease associated with greater Indian hostility from about 1835–1850 (380,000), then a slight increase until 1870 619,000, and then a very rapid increase until 1880 (3,938,831). The peak number of sheep in New Mexico reached possibly 5,000,000 in the late 1880s, but the total has subsequently been well under 4,000,000 partly because of extensive range deterioration and partly because of much larger numbers of cattle.[22]

Most of the New Mexican sheep were owned by a few *ricos* who divided their herds into flocks of several thousand and ranged them over large areas. The main pre-1848 concentrations seemed to have been in the area Of the Rio San Jose and lower Rio Puerco, around Albuquerque, and the area between Santa Fe and Calisteo. These were still important ranges in 1880; however, with the Indians under military control, many herds were taken into the more remote plateaus and mountain valleys in the northeast and northwest (Gordon 1883, 986–992). The New Mexican ranges were clearly heavily stocked in the 1880s, and livestock numbers at that time were significantly greater than the estimated 2,000,000–3,000,000 sheep during the early Mexican period. In the 1880s, however, sheep were grazed over a much larger area than they generally were prior to the Civil War, and consequently, the grazing pressure in terms of sheep per unit of land probably was as great, although less extensive, in the earlier period as it was during the period of accelerated erosion in the 1880s and after.

[22] In 1965 there were 969,000 sheep, 1,106,000 cattle, and 43,000 horses and mules in New Mexico according to the U.S. Department of Commerce (1965, 674).

Were there really several million head of sheep in New Mexico for much of the period between 1788 and 1848? The variety of evidence presented here suggests that there were, but the evidence is mainly in the form of unreliable estimates and is not conclusive. Probably, the only means of further verification would be from a thorough examination of the Spanish and Mexican Archives of New Mexico and northern Mexico, which heretofore have revealed very little data on sheep numbers.[23]

Grazing Pressure

The years of high livestock numbers during the early nineteenth century were not necessarily years of overgrazing and vegetation deterioration which might have contributed to heavy runoff and gullying. Range condition, or carrying capacity, at a given time varies with rainfall, history of previous use, kind of management, and type of animals. In general, a higher range carrying capacity could be expected in the 1820s and 1830s when rainfall was above normal than during the 1870s and 1880s when rainfall was below normal. Locally, of course, there may have been greater or lesser grazing pressure during any given period. Hence, the desirability of examining the history of specific valleys. Unfortunately, localized rainfall, livestock, and vegetation data are lacking for individual valleys for most of the nineteenth century.

How much were livestock restricted in early New Mexico? Unrestricted grazing on the open range is generally much less detrimental to the vegetation cover than is grazing of stock in fenced-in or otherwise limited areas. A good example of this is the situation on the Navajo lands in northwest New Mexico and northeast Arizona. In 1846, the Navajos were reported to have had about 500,000 head of sheep, mainly in large herds ranging over vast areas. After the Navajos were defeated and placed on a reservation in 1868, each individual was given two sheep, for a total of about 15,000. The new pattern was one of thousands of small family herds, all with restricted ranges. Sheep and goats increased to 700,000 by 1880 (Underhill 1956, 163),[24] which was not too many more than in 1846, but the range damage that occurred after 1880 was tremendous and was probably more a result of many small restricted herds than the total number of stock on the reservation ranges. In the Rio Grande Valley, however, there was no such restriction in 1880,

[23] The periodic reports of the different Spanish governors contain some data on sheep numbers and sheep exports to Mexico, as in the previously mentioned reports of Governor Chacón for 1800 and 1803. Twitchell (1914, Vols. 1, 2) lists and describes documents in the Spanish Archives of New Mexico, but there is no indication of significant data on sheep numbers. The possibility of finding official data on sheep during the critical Mexican period seems unpromising. Also, unfortunately, the United States commercial agents in Santa Fe apparently never reported on such matters as livestock (M.L. Moorhead, pers. comm.).

[24] Underhill (1956, 163). Navaho sheep numbers reached a peak of 1,370,554 in 1931 according to Luomala (1938, 57).

except for some fencing by new American cattlemen in Colfax County in the northeast.

The 1880 census reports that most of the sheep herds were still in the hands of a few Mexican families who ranged their stock widely over the state (Gordon 1883, 992). Actually, there seems to have been much more restriction and concentration of livestock in New Mexico before 1848, because of hostile Indians, than there was during the first decades after the Civil War.

Was the grazing pressure exerted by sheep and cattle in the Southwest in the nineteenth century actually significantly greater than the pressure exerted earlier by native grazing fauna? There were vast numbers of pronghorn antelope and also buffalo, deer, elk, and other herbivores which may well have equaled or surpassed the peak domesticated stock numerically.[25] On the other hand, grazing pressure by sheep, in particular, is much greater than that of wild game. Furthermore, people interfere with the natural safety valves of migration and large-scale die-off by limiting movement, by supplementary feeding, and by destruction of predators. However, until it is demonstrated otherwise, we must grant the possibility that long before European livestock appeared in the Southwest there was periodic overgrazing by wild animals which may have contributed to excessive runoff and, as result, gullying.

Vegetation Cover and Climate during the Nineteenth Century

If, as the above evidence suggests, much of New Mexico was nearly as heavily stocked in the 1820s as in the 1880s, with little arroyo cutting in the earlier period, then strong support would be given to arguments that rainfall conditions were very important factors in causing the arroyo cutting that began in the 1880s. The vegetation and climatic conditions that preceded and accompanied the recent accelerated erosion therefore need to be placed in contrast with climate and associated vegetation cover and arroyo conditions earlier in the nineteenth century.

In view of the intense overgrazing and range deterioration in the latter part of the nineteenth century throughout the Southwest, many people have assumed that vegetation conditions were uniformly better in the middle of the century and before. L.B. Leopold, however, maintained that this is an unwarranted assumption. On the basis of examinations of a large number of diaries and field notes of members of early American exploring parties, he concluded that in Arizona and New Mexico a good vegetation cover "was originally attained only in selected localities," particularly southeastern and southcentral Arizona and southwestern New Mexico (Leopold 1951a, 295). For the upper Rio Grande basin, poor grazing was reported for the most part, but some sections of the Rio Puerco Valley were good. Leopold suggested that "even before 1850, climatic factors had already initiated a tendency toward decreased vegetation" (Leopold 1951a, 305). During the 1840s, when most of the reports were made, rainfall was subnormal; furthermore, whereas sheep numbers declined considerably in the 1840s and 1850s, the poverty of the vegetation

[25] For a discussion of this theme for the Great Plains and for pertinent references, see Clark (1956).

might be partly explained by failure to recover from overgrazing in the 1820s and 1830s.[26] Vegetation descriptions before 1840 are generally much more enthusiastic than later ones. For example, Antonio Barreiro in 1832 said that "for the most part [the country] is composed of immense plains and delightful valleys covered with extremely abundant pasture" (Carroll and Haggard 1942, 21). However, some consideration must be given to the tendency of Spaniards and Mexicans from semi-arid environments to be more favorably impressed by the vegetation of New Mexico than were Americans from the humid East.[27]

Although the character of the vegetation cover of New Mexico at different periods of the nineteenth century is not completely clear, the general climatic sequence is fairly distinct. For the upper Rio Grande region, there are incomplete climatic records back to 1850 for several towns, including Albuquerque, Las Vegas, Santa Fe, and Socorro.[28] Prior to 1850, the best evidence is from dendrochronology studies which provide sequences of relative precipitation for a large number of sites throughout the Southwest.[29] For the Southwest as a whole, rainfall was subnormal for most of the second half of the nineteenth century. The New Mexican stations recorded well below average rainfall for 14 out of the 17 years between 1857 and 1873, followed by short periods of high intensity rainfall (especially 1876 and 1878), as well as several dry years (especially 1879, 1880, 1882, and 1889).[30]

There was also a marked drought in New Mexico from the mid-l840s to the early 1850s, followed by a large number of heavy rain storms between 1859 and 1856,[31] but with little or no accelerated erosion reported; sheep numbers were very low at this time. From the mid-1820s to the early 1840s, rainfall was above normal, and vegetation conditions were reported good. Livestock numbers were high, but there was little or no arroyo cutting.

Moving further back into the early nineteenth century, the relationships in New Mexico are less clear. Tree ring growth indicates near normal rainfall from 1790 to 1817; however, there was below normal rainfall from 1818 to 1823 (except for 1821), and apparently there were large numbers of sheep by the end of this period, On the other hand, this drought does not seem to have been prolonged or intense,

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[26] The causes of reported poor stands of grass between 1843 and 1881 were poor soils, locally dry climates, and overgrazing, in the opinion of Antevs (1952, 378). In areas heavily grazed in the 1880s and after, recovery of the vegetation has often been quite slow, even where there has been protection and adequate rainfall.

[27] For a discussion of this theme, see Tuan and Everard (1964, 271–274).

[28] U.S. Weather Bureau, USDA (1932–1933, sections 27, 28, 29). Also, see Thornthwaite et al. (1942, footnote 10, figures 14 and 15).

[29] See Schulman (1956, especially figure 34) and Fritts et al. (1964, especially figures 8 and 9).

[30] For long-term stations in New Mexico, Leopold pointed out a significantly greater number of heavy rains in the second half of the nineteenth century than during the first half of the twentieth century (Leopold 1951b, 350). Following Leopold, high intensity rains are those with 1.00 inch or more of rain in 24 hours, and moderate intensity rains are those with 0.50–0.99 inch in 24 hours.

[31] Of the ninety maximum 24-hour storms at Santa Fe from 1849 to 1938, seventeen occurred from 1853 to 1856, sixteen in the 1770s, and only five in the 1880s (Thornthwaite et al. 1942, footnote 10, figure 14).

and Mexican livestock numbers probably did not reach their maximum until the following 10 years when rainfall was well above normal. Actually, judging from tree ring growth, none of the droughts of the nineteenth century appear to have been severe compared with those of earlier centuries such as the "great drought" of the thirteenth century, which some authorities have associated with major arroyo cutting.

The nineteenth century pattern that emerges, then, is one of:

1. Higher than average rainfall and high livestock numbers, with little or no arroyo cutting
2. Drought and low livestock numbers, with little or no arroyo cutting
3. High intensity rainfall, low livestock numbers, and little or no erosion
4. Drought followed by several years of heavier than average summer storms, high livestock numbers, a probably weakened vegetation cover, and intense arroyo cutting

Obviously, with adequate rainfall, the vegetation cover in a semi-arid region may be sufficiently improved that it can support considerable grazing pressure without being so damaged that severe sheet erosion occurs. However, the same vegetation cover may be of little value in preventing gullying during periods of high intensity rains with large and rapid runoff. But with vegetation cover impaired by drought or overgrazing, even greater runoff and gullying could be expected from heavy rains or only moderate rains.

The lack of widespread gullying in the mid-1850s, in spite of preceding drought and in spite of even more frequent high intensity rains than in the 1880s, might be explained by the lack of grazing pressure on the vegetation in the 1850s. If so, then the gullying of the 1880s could be as caused by a combination of overgrazing and climatic conditions. Furthermore, in the 1820s, moderate and high intensity rains must have been infrequent. Therefore, if prehistoric gullying is to be explained by an increase in high intensity summer rains, as suggested by the Martin-Schoenwetter model, then such rains were probably more intense and more frequent than the heavier rains of the second half of the nineteenth century. However, these are very tentative conclusions in view of the availability of quantitative rainfall data for only a few stations prior to 1880 and for none before 1850. Also, no consideration has been given to the little studied possibility of a lag between the time of occurrence of gully initiating events and the actual time of rapid enlargement of gullies.

Conclusion

Formerly, it was generally agreed that neither the drought nor the heavy storms of the post-Civil war period were intense enough alone to have caused the severe arroyo cutting which occurred (Thornthwaite et al. 1942, 46; Antevs 1952, 384). However, recent studies, especially those stressing the importance of high intensity rainfall, seem to minimize the role of overgrazing. The livestock record presented here backs up the climatic argument but also gives some new support to the impoverished vegetation theory.

In spite of the apparent large numbers of sheep in New Mexico before 1850, arroyo cutting did not widely occur, in contrast to the situation in the 1880s. Additional backing is thus given to arguments that climatic conditions were responsible for modern gullying. There is no conflict with either of the climatic theories previously mentioned. The lack of gullying in the 1820s and 1830s when sheep numbers were relatively high can be attributed to higher than average annual rainfall and a vegetation cover that was in substantially better overall condition than it was in the 1880s when overstocking followed a period of subnormal rainfall. In other words, modern gullying may have been the result of more frequent high intensity summer rains, as suggested by the Martin-Schoenwetter model and supported by Tuan's evidence. Actually, all three factors of overgrazing, drought, and high intensity rainfall were to some extent operative and influential in the intensive gullying that took place in the latter part of the nineteenth century. If volume of runoff is the most critical factor, then certainly the state of the vegetation cover, which may be strongly influenced by both grazing pressure and rainfall, is important for its ability to retard or accelerate runoff. The lack of major gullying in the 1850s, when there were both below average rainfall and numerous high intensity storms, but when livestock numbers were very low, would suggest that the similar climatic conditions in the 1870s–1880s would not have caused gullying or at least severe gullying, without the aid of an impoverished vegetation cover caused by overgrazing by large numbers of livestock. Storms of only moderate intensity were probably much more damaging than they would have been otherwise.

As is so often true, modifications of the physical environment involve a number of complex natural and human factors. However, besides recognizing and describing these factors, every effort should be made to place them in proper perspective as to the major or minor roles they play, and geographers should be particularly concerned with sorting out man's role.

Examinations of the historical evidence of climate, vegetation, and gullying by Bryan, Thornthwaite, Antevs, Martin, Schoenwetter, Leopold, Tuan, and others have led to changing theories emphasizing overgrazing, or drought, or high intensity rains. The added perspective of livestock population history, uncertain as it is, suggests that probably neither one factor alone nor another but rather the combination of certain climatic events and overgrazing by livestock brought about severe modern gullying in the American southwest.

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8.0 Synthetic Contributions

Introducer: Billie Lee Turner II

Abstract Insights about the intellectual origins, base arguments, and impacts of three of William Denevan's most innovative works are addressed: The Pristine Myth, A Bluff Model, and Adaptation. Grounded in the "Berkeley School" traditions of research questions as they morphed into cultural ecology, The Pristine Myth challenged various claims bubbling up during the quincentenary of the Columbian encounter that marked the environments of the Americas as relatively untouched by Native Americans. Despite various challenges to Denevan's "unpristine" claims, The Pristine Myth has stood the test of time and the advancement of the evidence relevant to the argument. A Bluff Model sought to rectify various evidence and arguments, including those previously proposed by Denevan, about pre-European populations in the Amazon. In this reconfiguration, the relatively large size of the population is recognized, while the location of its settlements and lands used for cultivation is changed in an attempt to reconcile various debates among archaeologists. Finally, Adaptation, perhaps Denevan's most conceptual work, responds to the challenges that the Berkeley traditions were sparse in terms of the explanations of the built environments addressed. Adaptation borrowed concepts embedded with the Chicago School of risk-hazards, attempting the merge the two major North American schools—Berkeley and Chicago—focused on human-environment relationships and sought to move cultural ecological research into a broader, theoretical framing.

Keywords Amazonia · Adaptation · Columbian encounter · Pre-European landscapes and population

Introduction

"Adaptation" (1983), "The Pristine Myth" (1992), and "A Bluff Model" (1996), the concluding William M. Denevan papers in this volume, tackle concepts and issues-topics that permeated and went beyond the human-environment traditions for the mid-twentieth-century "Berkeley School" in which Bill was trained. The Pristine

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Myth and A Bluff Model are quintessential Berkeley School in subject and approach. They address major controversies regarding the conditions and character of landscapes and populations in the preconquest Americas, paying careful attention to the range of empirical evidence. Both works provide evidence and insights about controversial paleo-archaeo-historical problems implicitly embedded in theses and theories about human-environment relationships, foremost those addressing society and landscapes. Adaptation, in contrast, challenges the cultural-historical landscape tradition associated with the Berkeley School, which had been critiqued for providing insufficient attention to the societal and behavioral rationales and mechanisms (i.e., explanation) that generated the landscapes examined. Interestingly, Bill's challenge is the oldest of the set of three papers, indicating that he was influenced and responded to human-environmental thought that emerged beyond the traditions in which he was schooled, although he never abandoned the topical interests of those traditions, as attested by the other two works addressed in this set. Taken together, these works are, perhaps, Bill's most conceptually innovative, tackling issues and ideas that wedded the Berkeley School's interests with other research communities loosely captured by the label, cultural ecology, with special linkages to archaeology and anthropology. Significantly, these works have proven to hold important implications for emergent, contemporary research themes that transcend human-environment research beyond geography.

Perhaps myopically portrayed in graduate seminars and written works on geographical history, the Berkeley School and Bill's research interests centered on the human imprint on the land or landscape (i.e., environment) (Sauer 1925), with special attention to the premodern Americas (Mathewson 1987). The Pristine Myth and A Bluff Model reside at this core. Of the two, The Pristine Myth had the larger public and professional impact owing to the moment in which it appeared and the significance it held for a large range of research. A Bluff Model did not share the same moment, but its implications are important for a similar range of research.

The Pristine Myth

The year 1992 was the quincentenary of the first voyage of Columbus and the effective opening of the Americas to European conquest. Commemorative events—expositions to movies—were challenged, however, on the grounds that the history in question must give equal treatment—and rightly so—to a telling from those who were conquered. These "tellings" went beyond the devastation of Native American populations and cultures to include assessments of the human-environment conditions of the Americas under Native American dominion. Various accounts unfolded that described the pre-European Americas as largely a pristine wilderness (e.g., Sale 1990; Shetler 1991), consistent with the myth that Bill ascribed to nineteenth century romanticists vision of North America (Haila 1997). This pristineness implied some combination of a sparse pre-European occupation and native occupants operating as custodians of the land—living in harmony with nature (Whitmore and

Turner 1992). Viewed in this context, it is understandable why Bill's telling captured attention from varied research communities and, subsequently, the public readership of the distillation of that research.

The Pristine Myth was neither the first work to reveal the landscape modifications and transformation made by Native Americas across the Western Hemisphere nor the only one to have appeared in 1992, as Bill would subsequently document (Denevan 2011). It was, however, the most direct and succinct case made up to its time, revealing the kinds, scale, and locations of landscape changes previous to European interventions. These landscape alternations, as rendered by the paleoenvironmental, archaeological, and historical evidence, pointed to an alternative interpretation to that of the excesses of the pristine one—the environments of the New World were not always pristine and, in some cases, were significantly transformed. Bill's interpretation was a bold one for its time, given the prevalent sociopolitical controversies surrounding the quincentenary, and it was amplified by his use of the "myth" label.

The Pristine Myth was challenged as the invention of a new myth, that of the "humanized" Americas in which wilderness or largely native ecosystems and landscapes were diminished in scope (e.g., Vale 2002). Bill, however, made no claim that all landscapes in the Americas were "humanized" (Denevan 2011). Rather, gradients of mostly wild to transformed landscapes were present, depending on the level of land pressures applied, although, from the ecologists' eyes, even low levels of land pressures often lead to significant ecosystem and landscape alterations. Indeed, some sparsely occupied, old-growth forests encountered by European explorers and subsequent colonists were altered ecologies (i.e., species composition and populations) resulting from highly disturbed landscapes in the distant past of the Americas, such as the forests encountered in the Maya lowlands (Turner and Sabloff 2012) and parts of Amazonia (below; Heckenberger et al. 2008).

Symanski (2007 and in subsequent email-essays) intensified the critique by arguing that the pristine myth landscapes of the Americas never existed. Rather, Bill invented the myth by his misinterpretation of the historical texts. Symanski argued that these texts likely inferred environments with some human alteration, but much less relative to postconquest conditions, at least in North America (but see Doolittle 2000). Without textual analysis of the historical record, as Symanski notes, it is difficult to infer the precise meaning of the past usage of the pristine term. Even the quincentenary-linked claim of the "environmental transparency of Native Americans" was never rigorously defined, although the intent of the authors appears to me to side strongly toward Bill's interpretation. The critical point is that large variances in preconquest landscape management and impacts existed throughout of Americas.

Interestingly, claims about environmental transparency imply socio-behavioral implications that are mythological, if I may be so bold, reinforcing Bill's case. Somewhat surprisingly, Bill did not directly tackle these implications, which Tom Whitmore and Turner (2001) later labeled the "green legend." This legend holds that Native Americans were unique among peoples in the world in that they could long occupy landscapes, in many cases with extremely large populations, and yet neither significantly change nor degrade environments, leaving a transparent footprint on the landscape. This inference takes us into the chasms of societal exceptionalism about nature-society relationships because long-term occupancy by substantial populations worldwide invariably involves environmental changes. Native American exceptionalism appears indefensible in the face of the evidence, be it the role of first inhabitants of the Americas in eradicating megafauna (Gill et al. 2009) or the environmental consequences of the fossil to antique systems of cultivation found throughout the Western Hemisphere (Doolittle 2000; Turner and Butzer 1992). Bill did not delve into the exceptionalism theme per se, but I suspect a similar understanding was foundational to his case.

The Pristine Myth has worn well, as documented by Bill (Denevan 2011), and found its way into in recent popular volumes (e.g., Mann 2005). Interpreted correctly—the existence of gradients of landscape changes across the pre-European Americas commensurate with land pressures—his base argument has had research impacts which, I believe, Bill did not envision. Foremost among these are the requirements in climate change research to reconstruct past environmental conditions for a variety of purposes, such as calculations of atmospheric carbon emissions from land use, as well as sustainability (Clement and Junqueira 2010). The Americas can no longer be treated as largely untouched until the conquest and colonial era, including the vast expanses of the Amazon (Brugger et al. 2011). To do so leads to spurious modeling results; to adhere to the principal tenant residing in The Pristine Myth provides more robust outcomes (Ellis et al. 2013).

A Bluff Model

A Bluff Model readily complements The Pristine Myth but reverses the entry point from landscapes to population and narrows the application to the Amazon Basin. The entry point is the capacity of pre-European Amazonians to maintain substantial populations. Such occupation, in turn, holds significance for the scale of the landscape footprint and the relative pristineness of the Amazon previous to European arrival.

The size of pre-European populations in the Americas was a long-held research interest of the Berkeley School, and none of its prodigy has undertaken a more exhaustive and systematic assessment of those populations than has Bill. The extent and density of the pre-European occupation of the Amazon region was, and to some degree remains, a controversial topic. Historical accounts, detailed by Bill in A Bluff Model, report extensive occupation by way of variously sized villages (or towns) throughout much of riverine system, and by the 1980s, the archaeological record was providing evidence that prehistoric populations were large as well in some interior parts of Amazonia (e.g., Heckenberger et al. 2007, 2008; Roosevelt 1989). A Bluff Model provides a rationale for the environmental settings in which large settlements could be supported. Bill estimated that the number of these settings in the Amazon catchment area approaching 6 million km^2 could have held a bit more than 3 million people.

This number and the argument behind it must be set in context, which takes us to the works of Betty Meggers. A Smithsonian Institution anthropologist and Amazon specialist, Meggers (1954) argued that cultural development in the Amazon was constrained by its tropical location, an argument that invoked the geographical factor (environmental determinism), at least as interpreted by geographers and many other anthropologists. Constrained cultural development, in turn, diminished the size of populations that could be supported. Dismissing the veracity of historical Spanish accounts noting numerous large settlements encountered in the exploration of the Amazon basin, and consistent with her views on cultural development within it, Meggers (1971, 1992) pegged the population of the catchment area to be about one-half the amount Bill calculated (see Table 8.1 in section "A Bluff Model of Riverine Settlement in Prehistoric Amazonia"). She made a strong case that the vagaries of flooding made occupation of the fertile levees (*várzea*) of the floodplain highly problematic. Settlements, for the most part, were forced to locate on adjacent uplands, which were much less productive for cultivation, reducing the capacity to support large numbers of people.

In A Bluff Model, Bill changed his previous arguments about settlement locations, concurring with Meggers and her natural-hazards rationale for the paucity of floodplain settlements per se. He then addressed the evidence for bluff cultivation connected to floodplain uses that permitted more than 3 million occupants subsequent to the arrival of Europeans. The bluff location, which Bill identified in the literature as early as 1957, drew on the variety of emerging work at that time indicating extensive and perhaps significant pre-European occupation in Amazonia adjacent to and inland from its many waterways (e.g., Roosevelt 1989), including that associated with *terra preta* or black earth soils found throughout the Amazon uplands (e.g., Smith 1980). Impregnated with charcoal, *terra preta* is an anthropogenic soil, likely the product of lengthy semipermanent cultivation by pre-European occupants (Glaser and Birk 2012; Woods et al. 2009). While some controversy remains over the amount of occupation on the floodplain proper (e.g., Roosevelt 1999), *terra preta* and other evidence, such as ethnographic analogy (WinklerPrins 2002), pointed to an occupational model akin to that proposed by Bill (Bush et al. 2007; Heckenberger et al. 2008).

Subsequent evidence indicates that large regional variations existed across the Amazon Basin, with its western half perhaps less occupied (McMichael et al. 2012), and more densely settled locations surrounded by less disturbed landscapes (Barlow et al. 2012). Recognizing this variation, Bill's population estimates are not excessive for land management practices on the floodplains and uplands, consistent with the bluff model. This evidence, in turn, holds major implications for current environmental concerns, be it the past human imprint on forest species or long-term carbon emissions from the basin (e.g., Heckenberger et al. 2007). Like The Pristine Myth, A Bluff Model holds consequences for research communities in which Bill did not directly engage, such as global environmental change and climate modeling (but see Dull et al. 2010).

Adaptation

In contrast to the first two articles, Adaptation takes on an orientation to problem-solving, one of the few such themes that Bill offered professionally. For this reason, it can be viewed as a stand-alone product among his considerable library of published materials. Its publication took me by surprise, appearing as it did some 6 years after I departed Bill's mentorship. Why? Throughout my graduate training, I detected that Bill was uncomfortable providing public expressions about concepts, frameworks, or theory independent of an empirical-based problem to which the abstraction might be applied (as in the two works above). Adaptation was also surprising because it took aim at the dominating approach of his Berkeley experience, the cultural historical landscape (Sauer 1925). This approach paid minimal overt attention to societal and individual decision-making related to the environment and generating the landscape or landscape feature under investigation. The apparent rationale for this lacuna resided in the landscape itself, a phenomenon that required explanation at the human-environment interface (Turner 2003), an intellectual location that held, at least in principle, its own concepts, themes, theses, and perhaps theory (e.g., Wagner 1960). To delve too deeply into either subsystem or the explanatory constructs within them was to depart from the geographical and enter the environmental or social sciences.

Bill's adaptation drew on various critiques of cultural approaches and the lacuna in question, influenced by Harold C. Brookfield (1964) and others loosely aligned in anthropology and geography under the label of cultural ecology (Turner 2003). By the late 1960s, self-labeled cultural ecologists (e.g., Netting 1971) were increasingly disenchanted with system descriptions of energy or caloric flows and sought alternative means of gaining insights about the operations and causes of the landscapes under study. Bill suggested that functional understanding of the landscape as a human-environment system could be improved by a focus on cultural adaptation (or adaptation): how and why choices are selected in the face of changes in the system.

Bill's views were informed by the concepts of resilience and instability arising in ecology, which explicitly linked to adaptation approaches at large. This direction was entirely consistent with the research interests of risk-hazards research emanating from the "Chicago School" of human-environment geography (Turner 2003), which Bill recognized. This link caught the attention of at least one luminary of that school, Robert Kates, who approached me, a new Clark arrival at the time, to discern if adaptation and resilience were employed similarly in cultural ecological and risk-hazards research. I recall my cavalier response, triggered by my concern about the paucity of attention to maladaptation and my impression that proponents of adaptation as an explanatory concept did not demonstrate what it was not. In my mind, adaptation could not be found false and thus stood outside the logic of science. My interpretation was ill conceived. Bill was joining the growing chorus of those seeking a means of understanding why landscape changes were or were not made, given both exogenous and endogenous forces acting on the human-environment system, a thematic chorus that led to not only adaptation research across multiple disciplines but within postpositive and social-critical perspectives (Turner and Robbins 2008).

Bill's version of adaptation was, in some sense, ahead of its time, given the attention to it in contemporary social-ecological systems research beyond geography, operating under such research labels as global environmental and climate change, vulnerability, and resilience. Adaptation of social-ecological (or environmental systems) has become a mainstay of activities addressed by the Intergovernmental Panel on Climate Change, International Council of Science, and US National Academy of Sciences and sustainability science (Kates et al. 2001). The various activities seek to determine the characteristics (i.e., adaptability) of sustainable social-ecological systems, especially in regard to the design of effective institutions or governance structures that adapt well to the changing forces acting on the system. Bill did not operate in these research domains. Indeed, he chastised me several times for entering them. His thoughts on adaptation, however, are part of the foundational literature (cited several hundred times).

There have been in the past and surely will be in the future many interpretations of human-environment geography. Bill's interpretations, as registered in The Pristine Myth, A Bluff Model, and Adaptation, were and remain fundamentally grounded in the topics that marked the Berkeley School human-environment tradition as noted above. These works also demonstrate, however, the degree to which Bill pushed the orientation to these topics toward what was then labeled cultural ecology but now operates under multiple labels. If my recollections of Bill hold true, I suspect that he would find it difficult to self-label under the new monikers of research orientations and likely describe himself as an empirical-based geographer interested in human-environment relationships in Latin America. The works addressed here not only support this description, but they demonstrate that he held a keen eye for important, pan-disciplinary research problems and entered them with authority.

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8.1 Adaptation, Variation, and Cultural Geography

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Abstract Explanation in much of cultural geography has been historical, change being attributed to the availability of new ideas through innovation and diffusion. This approach, however, does not tell us why an idea is adopted. A potentially useful concept for cultural geography is that of adaptation, or strategy for survival, with attention to variation and its origins, the process of selection from variation, and either change or resilience in the face of environmental change.

Keywords Adaptive variability · Cultural adaptation · Cultural geography

Introduction

The point of view, "paradigm" if you will, of traditional cultural geography in the USA seems to be one in which human behavior vis a vis the physical world is explained in terms of learned group experience, i.e., culture. Wagner and Mikesell (1962, 1) explicitly said that "cultural geography is the application of the idea of culture to geographic problems." A given culture utilizes and modifies natural resources and landscape in an area (geographical cultural ecology) in a particular way with a particular technology for particular purposes because of a particular cultural heritage. We do something in such and such a way because this is the way our people do it. In turn the various ways of doing things are borrowed, or invented, or modified, and are passed on. Cultural geography looks "into culture history for origins" (Wagner and Mikesell 1962, 1). It has taken me quite a few years as a practicing cultural geographer to come to the simple realization that this is the essence of explanation in cultural geography.

Fine, but clearly we need more than this if we are really seeking causal explanation. I am not saying that a concern with origins and diffusion is unimportant but rather that this is not enough. The genetic approach in cultural geography (as distinct from a functional approach) really only tells why a tool, idea, or behavioral pattern is available, not why it is applied in a particular ecological situation, place, or point in time. One of the problems with cultural geography is that, while it is often historically oriented and very much recognizes long-term change, it at times looks at cultures as uniform and relatively stable, a result in part of a superorganic bias (Duncan 1980), as well as a macroscale and systemic approach. In actuality, a group always has a range of options to choose from, and new options can be acquired in adapting to its environment or multiple environments, or a changing environment, or a new environment. In fact, a reservoir of alternatives may be essential to survival. Decision-making is involved. Much of human geography is now concerned with decision-making, a microlevel process, and cultural geography undoubtedly will increasingly do likewise; there will be a greater concern with the study of the selective processes involved in adaptive change. If cultural geography seeks to explain decision-making involving adaptation, or people-land relations in a broader sense, or spatial patterns, then it really differs little from the rest of human geography. The main distinction becomes one of a preference for problems associated with traditional, preindustrial, or ethnic societies.

Three major critiques of cultural geography are relevant here. The first was by Brookfield (1964), who took cultural geographers to task for not seeking explanation through the examination of the "inner workings of society"; he argued for more comparative study and more micro-regional or even village-level study in order to analyze process. Second, Duncan (1980) criticizes the whole "superorganic" underpinning of cultural geography, pointing out that it is an inadequate mode of explanation, that it has for the most part been abandoned by anthropologists; he calls for a focus on individuals and groups of individuals. A third influential position is that of Wagner (1975), who "renounces" his earlier conception of cultural geography and argues that social institutions should be the focus of cultural geography, rather than either whole cultures or the individual. The statements by Brookfield and Duncan are not entirely fair as generalizations about cultural geographers, but this need not be discussed here. In any event, since Brookfield's article appeared, a somewhat new and certainly vigorous cultural geography, cultural ecology has developed, with significant contributions that are respected outside geography. These new directions are more in terms of emphases than of innovation. They include micro-regional study, careful measurement (inputs, outputs, time, energy, production, distance, environment), and such theoretical concepts as subsistence risk, carrying capacity, agricultural intensification, ecological zonation, and efficiency (e.g. Waddell 1972; Kirkby 1973; Nietschmann 1973; Bergman 1980). Much of the work in cultural ecology in both geography and anthropology, however, has not handled change very well.

There clearly are two levels of cultural-ecological behavior, a cultural (or institutional) level, which is shared, and a level of individual strategies, which may or may not be widely shared. Variability is largely at the individual level, and change occurs first at this level, drawing upon innovation or diffusion. Broader cultural adoption may never occur or may occur after a considerable time lag. I suggest that geographers could become more sensitive to small-scale variation as a key to understanding changing cultural adaptation in response to a changing human or physical environment.

Adaptation

This brings us to the concept of "cultural adaptation" as a potentially useful focus for cultural geography. It is now well established in ecological anthropology, including archaeology, with controversial ties to cultural materialism (Harris 1979), and has produced a considerable "adaptational rhetoric" (Kirch 1980, 101); see recent statements by Moran (1982), Jochim (1981), Winterhalder (1980), Kirch (1980), Orlove (1980), and Ellen (1982), as well as earlier discussions by Alland (1970, 1975), Cohen (1974), Bennett (1976), and Hardesty (1977). One of the few geographers to make explicit use of an adaptational methodology is Butzer (1980a, b, 1982); others, to a lesser extent, include Brookfield (1973), Trimbur and Watts (1976), Denevan and Schwerin (1978), and Porter (1965). Cultural adaptation is not mentioned in Mikesell's (1978) recent presidential address on cultural geography (although Bennett's 1976 book is cited) but is considered briefly in a recent statement on cultural evolution by Wagner (1977). Porter (1965, 22) indicates that a major question in human ecology for geographers is "How do cultures and individuals within a culture adapt and adjust to change?" Grossman (1977) considers the adaptive strategies approach to be "one of the most fruitful prospects in man-environment studies." He suggests that heretofore the main difference between the anthropological and geographical approaches to cultural ecology and to adaptation is that the former is concerned with "adaptation to the environment" and the latter with "adaptation of the environment" (Grossman 1977, 132). This differentiation seems less distinct to me now than it once did (Denevan 1966, 349).

There is confusion as to the meaning of the term "adaptation" and over what the adaptive unit is—the individual, the community, the culture, or a system. Kirch (1980, 108–110) says it should be the ecological population; I would agree. He defines "adaptation" as "the process of becoming adapted, that is, of being viable and able to reproduce in a specific environment." "Cultural adaptation," then, is the *process of change* in response to a change in the physical environment or a change in internal stimuli, such as demography, economics, and organization. "Adaptability" is the *capacity to adapt*. Complete "adaptedness" is probably rare and not of long duration. Cultural adaptations may not be the best solutions or entirely rational solutions as they reflect not only present conditions but those in the past as well as future contingencies; preadaptation involves prediction and even chance.

We ask, how does a given culture or a population, at a given time, utilize available, perceived natural resources in order to provide food and other necessities and why? Clearly much of cultural geography has been concerned with this, although the term "adaptation" is seldom used. Butzer (1980b) has suggested that adaptation be made a dominant or the dominant research focus of cultural geography and cultural ecology in general.

Variation, Selection, and Resilience

The mechanisms of adaptation are variation and selection in specific environments. We need to account for both the sources of variation and the processes of selection. Variation provides the options for changes in adaptation. A basic problem with applying the concept of cultural adaptation is that while both temporal and spatial variability are recognized at the macroscale, they tend to be overlooked at the microscale. Variation in adaptation may seem obvious, but it is not reflected in many of the studies of traditional food production by cultural geographers, ethnographers, and ecologists. Environments are overgeneralized, temporal variability is reduced to ranges or averages, and technology as described is uniform. The adaptive system, as a result, is often presented in publication as homogeneous, stable, or conservative. Reality, I would suggest, is invariably more dynamic, involving diversity in land-use practices and diversity in the land itself as the environment changes spatially and from year to year as a result of *both* the forces of nature and human impact. Examples of geographical studies of variable adaptation to microenvironmental gradients include Porter (1965) on East Africa, Nietschmann (1973) on east coast Nicaragua, and Bergman (1980) on the upper Amazon floodplain.

Cultures are not uniform but consist of subgroups and individuals, both of which are aware of a variety of technological options for dealing with the environment. Many of these options may be used seldom or not at all until there is a change in the ecological situation; they are dormant. There is a tendency, moreover, to attribute a new mode of adaptation to an external origin (diffusion) or to a technological innovation (invention) with adoption being rapid and more or less automatic. I disagree. I suspect that most techniques of environmental manipulation are long present within a culture before wholesale adoption occurs. They are known to and used by individuals or subgroups as secondary techniques or for unique situations. When conditions and needs change sufficiently, such latent techniques, as well as new ones, may become dominant. The knowledge of or availability of a potentially useful technique does not necessarily lead to adoption. Rather, perceived need leads to wide use of techniques (or concepts) already available.

One of the best examples of this situation is the adoption of full-time agriculture as the major means of food production in central Mexico. In the Tehuacán Valley, the first domesticated crops appear about 7000 BC; by 3400 BC only 25% of the valley's food came from agriculture, and not until after 850 BC, over 6000 years after initial domestication, was more than 50% of the diet provided for by crops (MacNeish 1971). The origin of and knowledge of agriculture did not cause a rapid shift from hunting and gathering to cultivation, nor to the development of pottery, nor to sedentary villages, nor to the development of cities and civilization. Clearly something else was involved, a perceived need, one probably associated with population growth and inadequate other options. But the *availability* of agriculture did make population growth and urbanization possible.

Traditional farmers, or herders, in contrast to the visiting observer, tend to be sensitive to the dynamic character of both nature and society. From their heritage, they are aware of short-term fluctuations and longer-term extremes as well as their own diversity of options for coping or adapting. The traditional farmer is concerned with minimization of risk given an uncertain environment. The physical world is one of constant potential hazard, great or small, and failure to adequately adapt (maladaptation) may mean death.

That the environment is uncertain and that its perturbations are of great importance in understanding human behavior and cultural history are now broadly recognized; the literature on the relationship of hazards and environmental change to human society is proliferating. University courses are given on natural hazard perception, and books are written on peasant decision-making. Accordingly, the unidimensional depictions of traditional man-land systems by cultural geographers and anthropologists should be replaced by more flexible descriptions reflecting diversity and change. This will be difficult but will at least attempt to better represent reality. The next step is to attempt to explain the selection from variation that results in significant change. This will require rigorous description, measurement, and analysis of environment, perception, demography, economy, and the social realm and may seldom be completely satisfactory.

In biological thinking, the concept of resilience is more and more replacing that of equilibrium or stability, and the same can be anticipated for cultural ecology (Vayda and McCay 1975). Most adaptation involves resiliency in that adjustments are made to either gradual change in the environment or to sudden but short-lived change, with these adjustments minimally disrupting either the ecological or social systems. Sudden catastrophic environmental change may lead to destruction or great modification of the cultural system; such change is beginning to receive more attention.

In traditional subsistence systems not dependent on external resources, the key to resiliency seems to be diversification, or multiple strategies, both actual (at a moment in time) and potential (knowledge of options). Successful adaptation, I think, requires both, and the second may not be apparent to the field observer. For instance, from studying shifting cultivation I am aware that, while one cultivated species may dominate a field, most swidden farmers have access to 50 or more different species and are capable of making greater or less use of them as conditions warrant. The same applies to the techniques of cultivation and resource management. In addition, resiliency based on cultural adaptability may be related to diversity in environment.

It should be emphasized that for a situation where the environment and population have been very stable for a long time (which may be unlikely), knowledge of adaptive options may significantly decrease making a society more vulnerable (less resilient) when environmental change does occur.

Examples

From my own field work with Indians in South America, I can provide two examples of some of the points made here regarding variation, selection, and resilience. They illustrate adaptation at the individual and subgroup level in contrast to a generalized cultural level of adaptation. One group is the Campa, located in the eastern foothills of the Peruvian Andes (Denevan 1971), and the other is the Karinya, located in the Orinoco Llanos of Venezuela (Denevan and Schwerin 1978). The literature describes each as typical Amazonian swidden or shifting cultivators, an oversimplification.

For the Campa, different swidden methods were being practiced not only by different villages and by different farmers in the same village but also by individual farmers simultaneously in different but ecologically similar fields. The ideal seasonal pattern of swidden activities seldom existed. The optimum sequence is one in which vegetation is cleared early in the dry season, dried during the dry season, and burned at the end of the dry season; crops are planted immediately after burning, grown during the wet season, and harvested when mature; the field is then fallowed when yields decline or weed invasion becomes too severe. Campa farmers, however, were encountered who had several fields in which all these activities could be observed at the same time. Some swidden phases were clearly out of sequence or not in the optimum season, for example burning during the wet season and planting at the start of the dry season.

The Campa are not poor farmers, but unpredictable social and environmental events occur resulting in deviation from the ideal strategy. The overall system is sufficiently resilient so that farmers are able to adapt to these changes without disaster. Yields may be lower but are compensated for by planting larger or more fields. There may be a period without production, but this is compensated for by relying on wild products or on neighbors. The Campa are constantly trying out new techniques, crops, and varieties. Some of these are discarded, some are used to a minor degree, but many are added to the cultural inventory of options that might become important under particular conditions.

The Karinya Indians provide another example of the variability of cultural adaptation over time and space that is in contrast to the static view we are given of most food production systems. Research in two villages showed that shifting cultivation was only one of numerous forms of agriculture. The Karinya have adapted to at least nine distinct biotopes, including savanna, swamp, gallery forest, natural levees, and seasonally flooded *playas* and islands. The main types of farming are extensive long-fallow and short-fallow shifting cultivation, intensive manual swamp drainage, mechanized savanna cultivation, seasonal *playa* cultivation, and house gardening.

Individual farmers may or may not practice more than one method, but all methods were present for the two villages combined. The presence and relative importance of these have changed over time and continue to change, depending on village location, population pressure, changing technology, transportation, and market opportunity; the growth of large nearby industrial cities such as Ciudad Guiana, Ciudad Bolivar, and El Tigre; availability of wage labor; and oil, gas, and mineral development. Once the backwater of Venezuela, Karinya lands are now crisscrossed by oil and gas exploration activities. Field studies between 1961 and 1980 and historical evidence since the sixteenth century indicate that agricultural systems have reflected the needs, capabilities, and opportunities of the moment in relation to the local environment. The Karinya have not so much changed their agricultural systems by gradually improving them as they have adopted completely new systems when it has been advantageous to do so. They have accomplished this without abandoning previous systems that still proved useful, although the proportional importance of the different systems has changed.

The Karinya demonstrate that adaptation need not proceed in a straight line from simpler (or less intensive, or less efficient, or less productive) to more complex. Instead adaptation may take any one of a number of directions over time and space, what might be termed adaptive radiation. Adaptation may sometimes involve the return to, or change of emphasis to, a simpler or less intensive system (Brookfield 1964). This may seem obvious, but it is a point missed in much of the literature that gives inadequate attention to process.

Many other examples illustrate the variability for and the flexibility of cultural adaptation. Both are obviously essential for survival if we grant that environmental and social conditions are not stable but undergo change which may be gradual or rapid, oscillating or irregular, cumulative or not, or of low intensity or catastrophic.

Part of the problem in the failure to recognize the presence of internal adaptive flexibility through variation, including innovation, lies in the ways in which field research is conducted. At one extreme at the macro-level (more likely involving the geographer), general patterns are sought and the details of variation are overlooked; at the other extreme at the village or farm level (more likely involving the anthropologist), observed patterns may be assumed to be general patterns. In one situation, the field worker virtually does not get out of the car, but in the other, he or she virtually does not get out of the village.

Conclusion

The adaptive behavior or strategies approach (Bennett's "adaptive dynamics") corresponds to what Orlove (1980) calls "processual ecological anthropology." This is a theoretical shift from "cultural ecosystemicism" with an emphasis on energy flow, homeostatic equilibrium, a short timescale, and no need to assign causes (Bennett 1976, 166). A processual approach instead is diachronic and examines mechanisms of change, including the relation of production systems to demographic variables and to environmental stress. As in biology, the move is toward the recognition and analysis of instability and resilience, including the role of individual events or actions. Criteria influencing adaptive strategy include efficiency, security, and satisfaction of needs and wants. In cultural geography, this can mean a return to the particularistic, i.e., the description and analysis of specific people/land processes involving crops, tools, and techniques of resource management or exploitation. Sauer (1941, 9), of course, viewed cultural-historical geography as the study not only of origins but of process; Wagner and Mikesell (1978, 1) defined geographical cultural ecology as the study of “specific processes.” Also, the adaptation concept can incorporate current geographical themes of resource perception, risk avoidance, and response to environmental hazards, at both individual and collective levels. Explanation of past adaptive behavior will never be complete as cognitive processes can only be inferred, but a degree of understanding is possible with good environmental data and reasonable assumptions about society, as recently demonstrated by Knapp (1983).

I would suggest that if cultural adaptation is examined diachronically and spatially, adaptation frequently will be found to be highly variable and flexible over short periods of time, reflecting both environmental and socioeconomic changes. Traditional cultures are seldom homogeneous masses of non-innovating, custom-bound conservatives. They are open to change and they are usually acquainted with options because those options have already been tried, if only by individuals. One might say that these options are within the cultural “gene pool,” or, to make another biological analogy, they are like mutations which have survived, if only in memory, and which can become dominant when conditions become appropriate and are so recognized by groups or institutions. Retention and change then both occur at the individual level and may or may not apply to the cultural norm.

When adaptive change does occur, we need to consider not just where the innovation came from (genetic explanation) but why one of several options was chosen (causal explanation). Failure to thoroughly focus on causes, I believe, has led some cultural ecologists into neo-environmental-deterministic explanations for adaptive change (e.g., Chappell 1981 in geography and Meggers 1979 in anthropology). A simplistic example of such reasoning is as follows: the climate became drier; migration occurred to a wetter climate; ergo, the drought caused the migration. The reasoning instead should be as follows: the climate became drier; a new adaptation became necessary; the option chosen was migration to a wetter climate. The question that needs to be asked then is why this particular mode of adaptation was chosen, instead of changing crops, reverting to a nonagricultural subsistence, switching to irrigation, or controlling population. Furthermore, these options may all have been known and did not require a great inventor or a lecture by an AID agronomist. The answer may not be fully determinable, but hypotheses can be formulated if the available options for adaptation are known.

Finally, let us ask what is happening to people’s capacity for adaptive resilience today. It is easy to assume that modern science gives us tremendous variability for dealing with the environment. But I wonder. Is what we are given really a capacity for greater production and labor efficiency with the problems arising from environmental perturbations and uncertainty mainly being handled via socioeconomic safeguards? Or can we also count on a reservoir of ecologically viable options for either long range or temporary adaptation?

I think that most traditional farmers have more available variability for adaptation than does either an Iowa farmer or a corn conglomerate. I know of agricultural strategies in Latin America that once supported millions of people but which professors in the University of Wisconsin School of Agriculture never heard of. Returning to our earlier levels of adaptation, given recurrent ecological stress, does adaptive variability at the individual level make possible greater resilience and hence survival at the cultural (or institutional) level?

Paradigms of (a) culture as an adaptive system and (b) cultural adaptation as an explanatory model for the mutual relations of people and environment can present problems. With our current interest in environmental change and natural hazards, we may need to be reminded by Sauer that "culture is the agent" (1925, 46) and is not passive; "that the habitat is revalued or reinterpreted with every change in habit" (1941, 7); that there is no "necessary adaptation" (1925b, 51); and that environment itself is often a "cultural artifact".

I have tried to make a few basic points about adaptation, briefly and simply, without developing a full methodological statement. The concept of adaptation is being used as a major theoretical framework by anthropologists; much of the content is geographical and geographers should be involved in the dialogue.

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8.2 The Pristine Myth: The Landscape of the Americas in 1492

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Abstract The myth persists that in 1492 the Americas were a sparsely populated wilderness, "a world of barely perceptible human disturbance." There is substantial evidence, however, that the Native American landscape of the early sixteenth century was a humanized landscape almost everywhere. Populations were large. Forest composition had been modified, grasslands had been created, wildlife disrupted, and erosion was severe in places. Earthworks, roads, fields, and settlements were ubiquitous. With Indian depopulation in the wake of Old World disease, the environment recovered in many areas. A good argument can be made that the human presence was less visible in 1750 than it was in 1492.

Keywords Demography · Earthworks · Native American settlement · Pre-European New World · Vegetation change · 1492

Introduction

This is the forest primeval… –Evangeline: A Tale of Acadie (Longfellow 1847)

What was the New World like at the time of Columbus? "Geography as it was," in the words of Carl Sauer (1971, x).[1] The Admiral himself spoke of a "paradise on earth…" "very beautiful and green and fertile," teeming with birds, with naked people living there whom he called "Indians" (Sauer 1966, 29, 31; 1981, 280). But was

[1] Sauer had a lifelong interest in this topic (1963, 1966, 1971, 1980).

the landscape encountered in the sixteenth century primarily pristine, virgin, a wilderness, and nearly empty of people, or was it a humanized landscape, with the imprint of native Americans being dramatic and persistent? The former still seems to be the more common view, but the latter may be more accurate.

The pristine view is to a large extent an invention of nineteenth-century romanticist and primitivist writers such as Hudson, Cooper, Thoreau, Longfellow, and Parkman and painters such as Catlin and Church.[2] The wilderness image has since become part of the American heritage, associated "with a heroic pioneer past in need of preservation" (Pyne 1982, 17; also see Bowden 1992, 22). The pristine view was restated clearly in 1950 by John Bakeless in his book *The Eyes of Discovery*:

> There were not really very many of these redmen…the land seemed empty to invaders who came from settled Europe…that ancient, primeval, undisturbed wilderness…the streams simply boiled with fish…so much game…that one hunter counted a thousand animals near a single salt lick…the virgin wilderness of Kentucky…the forested glory of primitive "America" (Bakeless 1950, 13, 201, 223, 314, 407).

But then he mentions that Indian "prairie fires … cause the often-mentioned oak openings…Great fields of corn spread in all directions … the Barrens … without forest" and that "Early Ohio settlers found that they could drive about through the forests with sleds and horses" (Bakeless 1950, 31, 304, 308, 314). A contradiction?

In the ensuing 40 years, scholarship has shown that Indian populations in the Americas were substantial, that the forests had indeed been altered, and that landscape change was commonplace. This message, however, seems not to have reached the public through texts, essays, or talks by both academics and popularizers who have a responsibility to know better.[3] Kirpatrick Sale in 1990, in his widely reported *Conquest of Paradise,* maintains that it was the Europeans who transformed nature, following a pattern set by Columbus. Although Sale's book has some merit and he is aware of large Indian numbers and their impacts, he nonetheless champions the widely held dichotomy of the benign Indian landscape and the devastated colonial landscape. He overstates both.

Similarly, *Seeds of Change: Christopher Columbus and the Columbian legacy*, the popular book published by the Smithsonian Institution, continues the litany of Native American passivity:

> Pre-Columbian America was still the First Eden, a pristine natural kingdom. The native people were transparent in the landscape, living as natural elements of the ecosphere. Their world, the New World of Columbus, was a world of barely perceptible human disturbance. (Shetler 1991, 226)

To the contrary, the Indian impact was neither benign nor localized and ephemeral, nor were resources always used in a sound ecological way. The concern here is

[2] See Nash (1967) on the "romantic wilderness" of America, Bowden (1992, 9–12) on the "invented tradition" of the "primeval forest" of New England, and Manthorne (1989, 10–21) on artists' images of the tropical "Eden" of South America. Day (1953, 329) provides numerous quotations from Parkman on "wilderness" and "vast," "virgin," and "continuous" forest.

[3] For example, a 1991 advertisement for a *Time-Life* video refers to "the unspoiled beaches, forests, and mountains of an earlier America" and "the pristine shores of Chesapeake Bay in 1607."

with the form and magnitude of environmental modification rather than with whether or not Indians lived in harmony with nature with sustainable systems of resource management. Sometimes they did; sometimes they did not. What they did was to change their landscape nearly everywhere [they lived], not to the extent of postcolonial Europeans but in important ways that merit attention.

The evidence is convincing. By 1492, Indian activity throughout the Americas had modified forest extent and composition, created and expanded grasslands, and rearranged microrelief via countless artificial earthworks. Agricultural fields were common, as were houses and towns and roads and trails. All of these had local impacts on soil, microclimate, hydrology, and wildlife. This is a large topic, for which this essay offers but an introduction to the issues, misconceptions, and residual problems. The evidence, pieced together from vague ethnohistorical accounts, field surveys, and archaeology, supports the hypothesis that the Indian landscape of 1492 had largely vanished by the mid-eighteenth century, not through a European superimposition but because of the demise of the native population. The landscape of 1750 was more "pristine" (less humanized) than that of 1492.

Indian Numbers

The size of the native population at contact is critical to our argument. The prevailing position, a recent one, is that the Americas were well-populated rather than relatively empty lands in 1492. In the words of the sixteenth-century Spanish priest, Bartolomé de las Casas, who knew the Indies well:

> All that has been discovered up to the year forty-nine [1549] is full of people, like a hive of bees, so that it seems as though God had placed all, or the greater part of the entire human race in these countries. (Las Casas, in MacNutt 1909, 314)

Las Casas believed that more than 40 million Indians had died by the year 1560. Did he exaggerate? In the 1930s and 1940s, Alfred Kroeber, Ángel Rosenblat, and Julian Steward believed that he had. The best counts then available indicated a population of between 8 and 15 million Indians in the Americas. Subsequently, Carl Sauer, Woodrow Borah, Sherburne F. Cook, Henry Dobyns, Goerge Lovell, N. David Cook, myself, and others have argued for larger estimates. Many scholars now believe that there were between 40 and 100 million Indians in the hemisphere (Denevan 1992a). This conclusion is primarily based on evidence of rapid early declines from epidemic disease prior to the first population counts (Lovell 1992).

I have recently suggested a New World total of 53.9 million (Denevan 1992a, xxvii). This divides into 3.8 million for North America, 17.2 million for Mexico, 5.6 million for Central America, 3.0 million for the Caribbean, 15.7 million for the Andes, and 8.6 million for lowland South America. These figures are based on my judgment as to the most reasonable recent tribal and regional estimates. Accepting a margin of error of about 20%, the New World population would lie between 43 and 65 million. Future regional revisions are likely to maintain the hemispheric total within this range. Other recent estimates, none based on totaling regional figures, include 43 million by Whitmore (1991, 483), 40 million by Lord and Burke (1991), 40–50 million by Cowley (1991), and 80 million for just Latin America by Schwerin (1991, 40). In any event, a population between 40 and 80 million is sufficient to dispel any notion of "empty lands." Moreover, the native impact on the landscape of 1492 reflected not only the population then but the cumulative effects of a growing population over the previous 15,000 years or more.

European entry into the New World abruptly reversed this trend. The decline of Native American populations was rapid and severe, probably the greatest demographic disaster ever (Lovell 1992). Old World diseases were the primary killer. In many regions, particularly the tropical lowlands, populations fell by 90% or more in the first century after contact. Indian populations (estimated) declined in Hispaniola from 1 million in 1492 to a few thousand 50 years later or by more than 99%; in Peru from 9 million in 1520 to 670,000 in 1620 (92%); in the Basin of Mexico from 1.6 million in 1519 to 180,000 in 1607 (89%); and in North America from 3.8 million in 1492 to 1 million in 1800 (74%). An overall drop from 53.9 million in 1492 to 5.6 million in 1650 amounts to an 89% reduction (Denevan 1992a, xvii–xxix). The human landscape was affected accordingly, although there is not always a direct relationship between population density and human impact (Whitmore et al. 1990, 37).

The replacement of Indians by Europeans and Africans was initially a slow process. By 1638, there were only about 30,000 English in North America (Sale 1990, 388), and by 1750 there were only 1.3 million Europeans and slaves (Meinig 1986, 247). For Latin America in 1750, Sánchez-Albornoz (1974, 7) gives a total (including Indians) of 12 million. For the hemisphere in 1750, the Atlas of World Population History reports 16 million (McEvedy and Jones 1978, 270). Thus, the overall hemispheric population in 1750 was about 30% of what it may have been in 1492. The 1750 population, however, was very unevenly distributed, mainly concentrated in certain coastal and highland areas with little Europeanization elsewhere. In North America in 1750 there were only small pockets of settlement beyond the coastal belt stretching from New England to northern Florida (see maps in Meinig 1986, 209, 245). Elsewhere, combined Indian and European populations were sparse, and environmental impact was relatively minor.

Indigenous imprints on landscapes at the time of initial European contact varied regionally in form and intensity. Following are examples for vegetation and wildlife, agriculture, and the built landscape.

Vegetation

The Eastern Forests

The forests of New England, the Midwest, and the Southeast had been disturbed to varying degrees by Indian activity prior to European occupation. Agricultural clearing and burning had converted much of the forest into successional (fallow) growth and into semipermanent grassy openings (meadows, barrens, plains, glades, savannas, prairies), often of considerable size.[4] Much of the mature forest was characterized by an open, herbaceous understory, reflecting frequent ground fires. "The de Soto expedition, consisting of many people, a large horse herd, and many swine, passed through ten states without difficulty of movement" (Sauer 1971, 283). This situation has been described in detail by Michael Williams in his recent history of American forests: "Much of the 'natural' forest remained, but the forest was not the vast, silent, unbroken, impenetrable and dense tangle of trees beloved by many writers in their romantic accounts of the forest wilderness" (1989, 33).[5] "The result was a forest of large, widely spaced trees, few shrubs, and much grass and herbage … Selective Indian burning thus promoted the mosaic quality of New England ecosystems, creating forests in many different states of ecological succession" (Cronon 1983, 49–51).

The extent, frequency, and impact of Indian burning are not without controversy. Raup (1937) argued that climatic change rather than Indian burning could account for certain vegetation changes. Emily Russell (1983, 86), assessing pre-1700 information for the Northeast, concluded that: "There is no strong evidence that Indians purposely burned large areas," but Indians did "increase the frequency of fires above the low numbers caused by lightning," creating an open forest. But then Russell adds: "In most areas climate and soil probably played the major role in determining the precolonial forests." She regards Indian fires as mainly accidental and "merely" augmental to natural fires, and she discounts the reliability of many early accounts of burning.

Forman and Russell (1983, 5) expand the argument to North America in general: "regular and widespread Indian burning (Day 1953) [is] an unlikely hypothesis that regretfully has been accepted in the popular literature and consciousness." This conclusion, I believe, is unwarranted given reports of the extent of prehistoric human burning in North America and Australia (Lewis 1982), and Europe (Patterson and Sassaman 1988, 130), and by my own and other observations on current Indian and peasant burning in Central America and South America; when unrestrained, people burn frequently and for many reasons. For the Northeast, Patterson and Sassaman (1988, 129) found that sedimentary charcoal accumulations were great where Indian populations were greatest.

Elsewhere in North America, the Southeast is much more fire prone than is the Northeast, with human ignitions being especially important in winter (Taylor 1981). The Berkeley geographer and Indianist Erhard Rostlund (1957, 1960) argued that Indian clearing and burning created many grasslands within mostly open forest in the so-called "prairie belt" of Alabama. As improbable as it may seem, Lewis (1982)

[4] On the other hand, the ability of Indians to clear large trees with inefficient stone axes, assisted by girdling and deadening by fire, may have been overestimated (Denevan 1992). Silver (1990, 51) notes that the upland forests of Carolina were largely uninhabited for this reason.

[5] Similar conclusions were reached by foresters Maxwell (1910) and Day (1953); by geographers Sauer (1963), Brown (1948, 11–19), Rostlund (1957), and Bowden (1992); and by environmental historians Pyne (1982, 45–51), Cronon (1983, 49–51), and Silver (1990, 59–66).

found Indian burning in the subarctic and Dobyns (1981) in the Sonoran Desert. The characteristics and impacts of fires set by Indians varied regionally and locally with demography, resource management techniques, and environment, but such fires clearly had different vegetation impacts than did natural fires owing to differences in frequency, regularity, and seasonality.

Forest Composition

In North America burning not only maintained open forest and small meadows but also encouraged fire-tolerant and sun-loving species. "Fire created conditions favorable to strawberries, blackberries, raspberries, and other gatherable foods" (Cronon 1983, 51). Other useful plants were saved, protected, and transplanted, such as American chestnut, Canada plum, Kentucky coffee tree, groundnut, and leek (Day 1953, 339–340). Gilmore (1931) described the dispersal of several native plants by Indians. Mixed stands were converted to single species dominants, including various pines and oaks, sequoia, Douglas fir, spruce, and aspen (M. Williams 1989, 47–48). The longleaf, slash pine, and scrub oak forests of the Southeast are almost certainly an anthropogenic subclimax created originally by Indian burning, replaced in early Colonial times by mixed hardwoods, and maintained in part by fires set by subsequent farmers and woodlot owners (Garren 1943). Lightning fires can account for some fire-climax vegetation, but Indian burning would have extended and maintained such vegetation (Silver 1990, 17–19, 59–64).

Even in the humid tropics, where natural fires are rare, human fires can dramatically influence forest composition. A good example is the pine forests of Nicaragua (Denevan 1961). Open pine stands occur both in the northern highlands (below 5,000 ft.) and in the eastern (Miskito) lowlands, where warm temperatures and heavy rainfall generally favor mixed tropical montane forest or rain forest. The extensive pine forests of Guatemala and Mexico primarily grow in cooler and drier, higher elevations, where they are in large part natural and prehuman (Watts and Bradbury 1982, 59). Pine forests were definitely present in Nicaragua when Europeans arrived. They were found in areas where Indian settlement was substantial, but not in the eastern mountains where Indian densities were sparse. The eastern boundary of the highland pines seems to have moved with an eastern settlement frontier that has fluctuated back and forth since prehistory. The pines occur today where there has been clearing followed by regular burning, and the same was likely in the past. The Nicaraguan pines are fire tolerant once mature, and large numbers of seedlings survive to maturity if they can escape fire during their first three to seven years (Denevan 1961, 280). Where settlement has been abandoned and fire ceases, mixed hardwoods gradually replace pines. This succession is likely similar where pines occur elsewhere at low elevations in tropical Central America, the Caribbean, and Mexico.

Midwest Prairies and Tropical Savannas

Sauer (1950, 1958, 1975) argued early and often that the great grasslands and savannas of the New World were of anthropogenic rather than climatic origin and that rainfall was generally sufficient to support trees. Even nonagricultural Indians expanded what may have been pockets of natural, edaphic grasslands, at the expense of forest. A fire burning to the edge of a grass/forest boundary will penetrate the drier forest margin and push back the edge, even if the forest itself is not consumed (Mueller-Dombois 1981, 164). Grassland can therefore advance significantly in the wake of hundreds of years or more of annual fires. Lightning-set fires can have a similar impact, but more slowly if less frequent than human fires, as in the wet tropics.

The thesis of prairies as fire induced, primarily by Indians, has its critics (Borchert 1950; Wedel 1957), but the recent review of the topic by Anderson (1990, 14), a biologist, concludes that most ecologists now believe that the eastern prairies "would have mostly disappeared if it had not been for the nearly annual burning of these grasslands by the North American Indians," during the last 5000 years. A case in point is the nineteenth-century invasion of many grasslands by forests after fire had been suppressed in Wisconsin, Illinois, Kansas, Nebraska, and elsewhere (M. Williams 1989, 46).

The large savannas of South America are also controversial as to origin. Much, if not most, of the open vegetation of the Orinoco Llanos, the Llanos de Mojos of Bolivia, the Pantanal of Mato Grosso, the Bolívar savannas of Colombia, the Guayas savannas of coastal Ecuador, the *campo cerrado* of central Brazil, and the coastal savannas north of the Amazon is of natural origin. The vast *campo cerrados* occupy extremely senile, often toxic oxisols. The seasonally inundated savannas of Bolivia, Brazil, Guayas, and the Orinoco owe their existence to the intolerance of woody species to the extreme alternation of lengthy flooding or waterlogging and severe desiccation during a long dry season. These savannas, however, were and are burned by Indians and ranchers, and such fires have expanded the savannas into the forests to an unknown extent. It is now very difficult to determine where a natural forest/savanna boundary once was located (Hills and Randall 1968; Medina 1980).

Other small savannas have been cut out of the rain forest by Indian farmers and then maintained by burning. An example is the Gran Pajonal in the Andean foothills in east-central Peru, where dozens of small grasslands (*pajonales*) have been created by Campa Indians—a process clearly documented by air photos (Scott 1978). *Pajonales* were in existence when the region was first penetrated by Franciscan missionary explorers in 1733.

The impact of human activity is nicely illustrated by vegetational changes in the basins of the San Jorge, Cauca, and Sinú rivers of northern Colombia. The southern sector, which was mainly savanna when first observed in the sixteenth century, had reverted to rain forest by about 1750 following Indian decline and had been reconverted to savanna for pasture by 1950 (Gordon 1957, map p. 69). Sauer (1966, 285–288; 1975, 8) and Bennett (1968, 53–55) cite early descriptions of numerous savannas in Panama in the sixteenth century. Balboa's first view of the Pacific was from a “treeless ridge,” now probably forested. Indian settlement and agricultural fields were common at the time, and with their decline the rain forest returned.

Anthropogenic Tropical Rain Forest

The tropical rain forest has long had a reputation for being pristine, whether in 1492 or 1992. There is, however, increasing evidence that the forests of Amazonia and elsewhere are to varying degree anthropogenic in form and composition. Sauer (1958, 105) said as much at the Ninth Pacific Science Congress in 1957 when he challenged the statement of tropical botanist Paul Richards that, until recently, the tropical forests have been largely uninhabited and that prehistoric people had “no more influence on the vegetation than any of the other animal inhabitants.” Sauer countered that Indian burning, swiddens, and manipulation of composition had extensively modified the tropical forest.

“Indeed, in much of Amazonia, it is difficult to find soils that are not studded with charcoal” (Uhl et al. 1990, 30). The question is, to what extent does this evidence reflect Indian burning in contrast to natural (lightning) fires, and when did these fires occur? The role of fire in tropical forest ecosystems has received considerable attention in recent years, partly as result of major wildfires in East Kalimantan in 1982–1983 and small forest fires in the Venezuelan Amazon in 1980–1984 (Goldammer 1990). Lightning fires, through rare in moist tropical forest, do occur in drier tropical woodlands (Mueller-Dombois 1981, 149). Thunderstorms with lightning are much more common in the Amazon, compared to North America, but in the tropics, lightning is usually associated with heavy rain and noncombustible, verdant vegetation. Hence Indian fires undoubtedly account for most fires in prehistory, with their impact varying with the degree of aridity.

In the Río Negro region of the Colombian-Venezuelan Amazon, soil charcoal is very common in upland forests. C-14 dates range from 6260 to 250 BP, well within human times (Saldarriaga and West 1986). Most of the charcoal probably reflects local swidden burns; however, there are some indications of forest fires at intervals of several hundred years, most likely ignited by swidden fires. Recent wildfires in the upper Río Negro region were in a normally moist tropical forest (3530 mm annual rainfall) that had experienced several years of severe drought. Such infrequent wildfires in prehistory, along with the more frequent ground fires, could have had significant impacts on forest succession, structure, and composition. Examples are the pine forests of Nicaragua, mentioned above, the oak forests of Central America, and the babassu palm forests of eastern Brazil. Widespread and frequent burning may have brought about the extinction of some endemic species.

The Amazon forest is a mosaic of different ages, structure, and composition resulting from local habitat conditions and disturbance dynamics (Haffer 1991). Natural disturbances (tree falls, landslides, river activity) have been considerably augmented by human activity, particularly by shifting cultivation. Even a small number of swidden farmers can have a widespread impact in a relatively short period of time. In the upper Río Negro region, species diversity recovery takes

60–80 years and biomass recovery 140–200 years (Saldarriaga and Uhl 1991, 312). Brown and Lugo (1990, 4) estimate that today about 40% of the tropical forest in Latin America is secondary as a result of human clearing and that most of the remainder has had some modification despite current low population densities. The species composition of early stages of swidden fallows differs from that of natural gaps and may "alter the species composition of the mature forest on a long-term scale" (Walschburger and Von Hildebrand 1991, 262). While human environmental destruction in Amazonia currently is concentrated along roads, in prehistoric times, Indian activity in the upland (interfluve) forests was much less intense but more widespread (Denevan 1992b).

Indian modification of tropical forests is not limited to clearing and burning. Large expanses of Latin American forests are humanized forests in which the kinds, numbers, and distributions of useful species are managed by human populations. Doubtless, this applies to the past as well. One important mechanism in forest management is manipulation of swidden fallows (sequential agroforestry) to increase useful species. The planting, transplanting, sparing, and protection of useful wild fallow plants eliminate clear distinctions between field and fallow (Denevan and Padoch 1988). Abandonment is a slow process, not an event. Gordon (1982, 79–98) describes managed regrowth vegetation in eastern Panama, which he believes extended from Yucatán to northern Colombia in pre-European times. The Huastec of eastern Mexico and the Yucatec Maya have similar forms of forest gardens or forest management (Alcorn 1981; Gómez-Pompa 1987). The Kayapó of the Brazilian Amazon introduce and/or protect useful plants in activity areas ("nomadic agriculture") adjacent to villages or camp sites, in foraging areas, along trails, near fields, and in artificial forest-mounds in savanna (Posey 1985). In managed forests, both annuals and perennials are planted or transplanted, while wild fruit trees are particularly common in early successional growth. Weeding by hand was potentially more selective than indiscriminate weeding by machete (Gordon 1982, 57–61). Much dispersal of edible plant seeds is unintentional via defecation and spitting out.

The economic botanist-anthropologist William Balée (1987, 1989) speaks of "cultural" or "anthropogenic" forests in Amazonia in which species have been manipulated, often without a reduction in natural diversity. These include specialized forests (babassu, Brazil nuts, lianas, palms, bamboo), which currently make up at least 11.8% (measured) of the total upland forest in the Brazilian Amazon (Balée 1989, 14). Clear indications of past disturbance are the extensive zones of *terra preta* (black earth), which occur along the bluffs of the large floodplains as well as in the uplands (Balée 1989, 10–12; Smith 1980). These soils, with depths to 50 cm or more, contain charcoal and cultural waste from prehistoric burning and settlement. Given high carbon, nitrogen, calcium, and phosphorus content, *terra preta* soils have a distinctive vegetation and are attractive to farmers. Balée (1989, 14) concludes that "large portions of Amazonian forests appear to exhibit the continuing effects of past human interference." The same argument has been made for the Maya lowlands (Gómez-Pompa et al. 1987) and Panama (Gordon 1982). There seem to be no virgin tropical forests today, nor were there in 1492.

Wildlife

The indigenous impact on wildlife is equivocal. The thesis that "overkill" hunting caused the extinction of some large mammals in North America during the late Pleistocene, as well as subsequent local and regional depletions (Martin 1978, 167–72), remains controversial. By the time of the arrival of Cortéz in 1519, the dense populations of central Mexico apparently had greatly reduced the number of large game, given reports that "they eat any living thing" (Cook and Borah 1971–1979, 3:135, 140). In Amazonia, local game depletion increases with village size and duration (Good 1987). Hunting procedures in many regions seem, however, to have allowed for recovery because of the "resting" of hunting zones intentionally or as a result of shifting of village sites.

On the other hand, forest disturbance increased herbaceous forage and edge effect and hence the numbers of some animals (Thompson and Smith 1970, 261–264). "Indians created ideal habitats for a host of wildlife species…exactly those species whose abundance so impressed English colonists: elk, deer, beaver, hare, porcupine, turkey, quail, ruffed grouse, and so on" (Cronon 1983, 51). White-tailed deer, peccary, birds, and other game increase in swiddens and fallows in Yucatán and Panama (Greenberg 1991; Gordon 1982, 96–112; Bennett 1968). Rostlund (1960, 407) believed that the creation of grassy openings east of the Mississippi extended the range of the bison, whose numbers increased with Indian depopulation and reduced hunting pressure between 1540 and 1700 and subsequently declined under White pressure.

Agriculture

Fields and Associated Features

To observers in the sixteenth century, the most visible manifestation of the Native American landscapes must have been the cultivated fields, which were concentrated around villages and houses. Most fields are ephemeral, their presence quickly erased when farmers migrate or die, but there are many eyewitness accounts of the great extent of Indian fields. On Hispaniola, Las Casas and Oviedo reported individual fields with thousands of *montones* (Sturtevant 1961, 73). These were manioc and sweet potato mounds 3–4 m in circumference, of which apparently none have survived. In the Llanos de Mojos in Bolivia, the first explorers mentioned *percheles* or corn cribs on pilings, numbering up to 700 in a single field, each holding 30–45 bushels of food (Denevan 1966, 98). In northern Florida in 1539, Hernando de Soto's army passed through numerous fields of maize, beans, and squash, their main source of provisions; in one sector, "great fields…were spread out as far as the eye could see across two leagues of the plain" (de la Vega 1980, 2:182; also see Dobyns 1983, 135–146).

It is difficult to obtain a reliable overview from such descriptions. Aside from possible exaggeration, Europeans tended not to write about field size, production, or technology. More useful are various forms of relict fields and field features that persist for centuries and can still be recognized, measured, and excavated today. These extant features, including terraces, irrigation works, raised fields, sunken fields, drainage ditches, dams, reservoirs, diversion walls, and field borders, numbering in millions are distributed throughout the Americas (Denevan 1980; also see Doolittle 1992 and Whitmore and Turner 1992). For example, about 50,000 ha of abandoned raised fields survive in the San Jorge Basin of northern Colombia (Plazas and Falchetti 1987, 485), and at least 600,000 ha of terracing, mostly of prehistoric origin, occur in the Peruvian Andes (Denevan 1988, 20). There are 19,000 ha of visible raised fields in just the sustaining area of Tiwanaku at Lake Titicaca (Kolata 1991, 109), and there were about 12,000 ha of *chinampas* (raised fields) around the Aztec capital of Tenochtitlán (Sanders et al. 1979, 390). Complex canal systems on the north coast of Peru and in the Salt River Valley in Arizona irrigated more land in prehistory than is cultivated today. About 175 sites of Indian garden beds, up to several hundred acres each, have been reported in Wisconsin (Gartner 1992). These various remnant fields probably represent less than 25% of what once existed, most being buried under sediment or destroyed by erosion, urbanization, plowing, and bulldozing. On the other hand, an inadequate effort has been made to search for ancient fields.

Erosion

The size of native populations, associated deforestation, and prolonged intensive agriculture led to severe land degradation in some regions. Such a landscape was that of central Mexico, where by 1519 food production pressures may have brought the Aztec civilization to the verge of collapse even without Spanish intervention (Cook and Borah 1971–1979, 3:129–176).[6] There is good evidence that severe soil erosion was already widespread, rather than just the result of subsequent European plowing, livestock, and deforestation. Cook examined the association between erosional severity (gullies, *barrancas*, sand and silt deposits, and sheet erosion) and pre-Spanish population density or proximity to prehistoric Indian towns. He concluded that "an important cycle of erosion and deposition therefore accompanied intensive land use by huge primitive populations in Central Mexico, and had gone far toward the devastation of the country before the white man arrived" (Cook 1949, 86).

Barbara Williams (1972, 618) describes widespread *tepetate*, an indurated substrate formation exposed by sheet erosion resulting from prehistoric agriculture, as "one of the dominant surface materials in the Valley of Mexico." On the other hand,

[6] Barbara Williams (1989, 730) finds strong evidence of rural overpopulation (66% in poor crop years, 11% in average years) in the Basin of Mexico village of Asunción, ca. AD 1540, which was probably "not unique but a widespread phenomenon." For a contrary conclusion, that the Aztecs did not exceed carrying capacity, see Ortiz de Montellano (1990, 119).

anthropologist Melville (1990, 27) argues that soil erosion in the Valle de Mezquital, just north of the Valley of Mexico, was the result of overgrazing by Spanish livestock starting before 1660: "there is an almost total lack of evidence of environmental degradation before the last three decades of the sixteenth century." The Butzers, however, in an examination of Spanish land grants, grazing patterns, and soil vegetation ecology, found that there was only light intrusion of Spanish livestock (sheep and cattle were moved frequently) into the southeastern Bajío near Mezquital until after 1590 and that any degradation in 1590 was "as much a matter of long-term Indian land use as it was of Spanish intrusion" (Butzer and Butzer 1993). The relative roles of Indian and early Spanish impacts in Mexico still need resolution; both were clearly significant but varied in time and place. Under the Spaniards, however, even with a greatly reduced Indian population, the landscape in Mexico generally did not recover due to accelerating impacts from introduced sheep and cattle.[7]

The Built Landscape

Settlement

The Spaniards and other Europeans were impressed by large flourishing Indian cities such as Tenochtitlán, Quito, and Cuzco, and they took note of the extensive ruins of older, abandoned cities such as Cahokia, Teotihuacán, Tikal, Chan Chan, and Tiwanaku (Hardoy 1968). Most of these cities contained more than 50,000 people. Less notable, or possibly more taken for granted, was rural settlement—small villages of a few thousand or a few hundred people, hamlets of a few families, and dispersed farmsteads. The numbers and locations of much of this settlement will never be known. With the rapid decline of native populations, the abandonment of houses and entire villages and the decay of perishable materials quickly obscured sites, especially in the tropical lowlands.

We do have some early listings of villages, especially for Mexico and Peru. Elsewhere, archaeology is telling us more than ethnohistory. After initially focusing on large temple and administrative centers, archaeologists are now examining rural sustaining areas, with remarkable results. See, for example, Sanders et al. (1979) on the Basin of Mexico, Culbert and Rice (1990) on the Maya lowlands, and Fowler (1989) on Cahokia in Illinois. Evidence of human occupation for the artistic Santarém Culture phase (Tapajós chiefdom) on the lower Amazon extends over thousands of square kilometers, with large nucleated settlements (Roosevelt 1991, 101–102).

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[7] Highland Guatemala provides another prehistoric example of "severe human disturbance" involving deforestation and "massive" soil erosion (slopes) and deposition (valleys) (Murdy 1990, 186). For the central Andes, there is some evidence that much of the *puna* zone (3200–4500 m), now grass and scrub, was deforested in prehistoric times (White 1985).

Much of the rural precontact settlement was semi-dispersed, particularly in densely populated regions of Mexico and the Andes, probably reflecting poor food transport efficiency. Houses were both single-family and communal (*pueblos*, Huron long houses, Amazon *malocas*). Construction was of stone, earth, adobe, daub and wattle, grass, hides, brush, and bark. Much of the dispersed settlement not destroyed by depopulation was concentrated by the Spaniards into compact grid/plaza-style new towns (*congregaciones*, *reducciones*) for administrative purposes.

Mounds

James Parsons (1985, 161) has suggested that: "An apparent mania for earth moving, landscape engineering on a grand scale, runs as a thread through much of New World prehistory." Large quantities of both earth and stone were transferred to create various raised and sunken features, such as agricultural landforms, settlement and ritual mounds, and causeways.

Mounds of different shapes and sizes were constructed throughout the Americas for temples, burials, and settlement and as effigies. The stone pyramids of Mexico and the Andes are well known, but equal monuments of earth were built in the Amazon, the Midwest USA, and elsewhere. The Mississippian period complex of 104 mounds at Cahokia near East St. Louis supported 30,000 people; the largest, Monk's Mound, is currently 30.5 m high and covers 6.9 ha (Fowler 1989, 90, 192). Cahokia was the largest settlement north of the Río Grande until surpassed by New York City in 1775. An early survey estimated "at least 20,000 conical, linear, and effigy mounds" in Wisconsin (Stout 1911, 24). Overall, there must have been several hundred thousand artificial mounds in the Midwest and South. De Soto described such features still in use in 1539 (Silverberg 1968, 7). Thousands of settlement and other mounds dot the savanna landscape of Mojos in Bolivia (Denevan 1966). At the mouth of the Amazon on Marajó Island, one complex of 40 habitation mounds contained more than 10,000 people; one of these mounds is 20 m high while another is 90 ha in area (Roosevelt 1991, 31, 38).

Not all of the various earthworks scattered over the Americas were in use in 1492. Many had been long abandoned, but they constituted a conspicuous element of the landscape of 1492 and some are still prominent. Doubtless, many remain to be discovered, and others remain unrecognized as human or prehistoric features.

Roads, Causeways, and Trails

Large numbers of people and settlements necessitated extensive systems of overland travel routes to facilitate administration, trade, warfare, and social interaction (Hyslop 1984; Trombold 1991). Only hints of their former prominence survive. Many were simple traces across deserts or narrow paths cut into forests. A suggestion as to the importance of Amazon forest trails is the existence of more than

500 km of trail maintained by a single Kayapó Village today (Posey 1985, 149). Some prehistoric footpaths were so intensively used for so long that they were incised into the ground and are still detectable, as has recently been described in Costa Rica (Sheets and Sever 1991).

Improved roads, at times stone-lined and drained, were constructed over great distances in the realms of the high civilizations. The Inca road network is estimated to have measured about 40,000 km, extending from southern Colombia to central Chile (Hyslop 1984, 224). Prehistoric causeways (raised roads) were built in the tropical lowlands (Denevan 1991). One Maya causeway is 100 km long, and there are more than 1600 km of causeways in the Llanos de Mojos. Humboldt reported large prehistoric causeways in the Orinoco Llanos. Ferdinand Columbus described roads on Puerto Rico in 1493. Gaspar de Carvajal, traveling down the Amazon with Orellana in 1541, reported "highways" penetrating the forest from riverbank villages. José de Acosta (1880, 1:171) in 1590 said that between Peru and Brazil, there were "waies as much beaten as those betwixt Salamanca and Valladolid." Prehistoric roads in Chaco Canyon, New Mexico, are described in Trombold (1991). Some routes were so well established and located that they have remained roads to this day.

Recovery

A strong case can be made for significant environmental recovery and reduction of cultural features by the late eighteenth century as a result of Indian population decline. Henry Thoreau (1949, 132–137) believed, based on his reading of William Wood, that the New England forests of 1633 were more open, more parklike, with more berries and more wildlife, than Thoreau observed in 1855. Cronon (1983, 108), Pyne (1982, 51), Silver (1990, 104), Martin (1978, 181–182), and Michael Williams (1989, 49) all maintain that the eastern forests recovered and filled in as a result of Indian depopulation, field abandonment, and reduction in burning. While probably correct, these writers give few specific examples, so further research is needed. The sixteenth-century fields and savannas of Colombia and Central America also had reverted to forest within 150 years after abandonment (Parsons 1975, 30–31; Bennett 1968, 54). On his fourth voyage in 1502–1503, Columbus sailed along the north coast of Panama (Veragua). His son Ferdinand described lands which were well-peopled, full of houses, with many fields, and open with few trees. In contrast, in 1681 Lionel Wafer found most of the Caribbean coast of Panama forest covered and unpopulated. On the Pacific side in the eighteenth century, savannas were seldom mentioned; the main economic activity was the logging of tropical cedar, a tree that grows on the sites of abandoned fields and other disturbances (Sauer 1966, 132–133, 287–288). An earlier oscillation from forest destruction to recovery in the Yucatán is instructive. Whitmore et al. (1990, 35) estimate that the Maya had modified 75% of the environment by AD 800 and that following the Mayan collapse, forest recovery in the central lowlands was nearly complete when the Spaniards arrived.

The pace of forest regeneration, however, varied across the New World. Much of the Southeastern USA remained treeless in the 1750s according to Rostlund (1957, 408, 409). He notes that the tangled brush that ensnarled the "Wilderness Campaign of 1864 in Virginia occupied the same land as did Captain John Smith's 'open groves with much good ground between without any shrubs'" in 1624; vegetation had only partially recovered after 240 years. The Kentucky barrens in contrast were largely reforested by the early nineteenth century (Sauer 1963, 30). The Alabama Black Belt vegetation was described by William Bartram in the 1770s as a mixture of forest and grassy plains, but by the nineteenth century, there was only 10% prairie and even less in some counties (Rostlund 1957, 393, 401–403). Sections of coastal forest never recovered, given colonist pressures, but Sale's (1990, 291) claim that "the English were well along in the process of eliminating the ancient Eastern woodlands from Maine to the Mississippi" in the first 100 years is an exaggeration.

Wildlife also partially recovered in eastern North America with reduced hunting pressure from Indians; however, this is also a story yet to be worked out. The white-tailed deer apparently declined in numbers, probably reflecting reforestation plus competition from livestock. Commercial hunting was a factor on the coast, with 80,000 deer skins being shipped out yearly from Charleston by 1730 (Silver 1990, 92). Massachusetts enacted a closed season on deer as early as 1694, and in 1718 there was a 3-year moratorium on deer hunting (Cronon 1983, 100). Sale (1990, 290) believes that beaver were depleted in the Northeast by 1640. Other furbearers, game birds, elk, buffalo, and carnivores were also targeted by white hunters, but much game probably was in the process of recovery in many eastern areas until a general reversal after 1700–1950.

As agricultural fields changed to scrub and forest, earthworks were grown over. All the raised fields in Yucatán and South America were abandoned. A large portion of the agricultural terraces in the Americas were abandoned in the early colonial period (Donkin 1979, 35–38). In the Colca Valley of Peru, measurement on air photos indicates 61% terrace abandonment (Denevan 1988, 28). Societies vanished or declined everywhere and whole villages with them. The degree to which settlement features were swallowed up by vegetation, sediment, and erosion is indicated by the difficulty of finding them today. Machu Picchu, a late prehistoric site, was not rediscovered until 1911.

The renewal of human impact also varied regionally, coming with the Revolutionary War in North America, with the rubber boom in Amazonia, and with the expansion of coffee in southern Brazil (1840–1930). The swamp lands of the Gulf Coast of Mexico and the Guayas Basin of Ecuador remained hostile environments to Europeans until well into the nineteenth century or later (Mathewson 1987; Siemens 1990). On the other hand, highland Mexico-Guatemala and the Andes, with greater Indian survival and with the establishment of haciendas and intensive mining, show less evidence of environmental recovery. Similarly, Indian fields in the Caribbean were rapidly replaced by European livestock and sugar plantation systems, inhibiting any significant recovery. The same is true of the sugar zone of coastal Brazil.

Conclusions

By 1492 Indian activity had modified vegetation and wildlife, caused erosion, and created earthworks, roads, and settlements throughout the occupied Americas. This may be obvious, but the human imprint was much more ubiquitous and enduring than is usually realized. The historical evidence is ample, as are data from surviving earthworks and archaeology. And much can be inferred from present human impacts. The weight of evidence suggests that Indian populations were large, not only in Mexico and the Andes but also in seemingly unattractive habitats such as the rain forests of Amazonia, the swamps of Mojos, and the deserts of Arizona.

Clearly, the most humanized landscapes of the Americas existed in those highland regions where people were the most numerous. Here were the large states, characterized by urban centers, road systems, intensive agriculture, a dispersed but relatively dense rural settlement pattern of hamlets and farmsteads, and widespread vegetation and soil modification and wildlife depletion. There were other smaller regions that shared some of these characteristics, such as the Pueblo lands in the Southwestern USA, the Sabana de Bogotá in highland Colombia, and the central Amazon floodplain, where built landscapes were locally dramatic and are still observable. Finally, there were the immense grasslands, deserts, mountains, and forests elsewhere, with populations that were sparse, or moderate, or absent, with landscape impacts that mostly were ephemeral or not obvious but nevertheless significant, particularly for vegetation and wildlife, as in Amazonia and Northeast USA. In addition, landscapes from the more distant past survived to 1492 and even to 1992, such as those of the irrigation states of north coast Peru, the classic Maya, the Mississippian mound builders, and the Tiwanaku Empire of Lake Titicaca.

This essay has ranged over the hemisphere, an enormous area, making generalizations about and providing examples of Indian landscape transformation as of 1492. Examples of some of the surviving cultural features are shown in Fig. 8.1. Ideally, a series of hemispheric maps should be provided to portray the spatial patterns of the different types of impacts and cultural features, but such maps are not feasible nor would they be accurate given present knowledge. There are a few relevant regional maps, however, that can be referred to. For example, see Butzer (1990, 33, 45) for Indian settlement structures/mounds and subsistence patterns in the USA, Donkin (1979, 23) for agricultural terracing, Doolittle (1990, 109) for canal irrigation in Mexico, Parsons and Denevan (1967) for raised fields in South America, Trombold (1991) for various road networks, Hyslop (1984, 4) for the Inca roads, Hardoy (1968, 49) for the most intensive urbanization in Latin America, and Gordon (1957, 69) for anthropogenic savannas in northern Colombia.

The pristine myth cannot be laid at the feet of Columbus. While he spoke of "Paradise," his was clearly a humanized paradise. He described Hispaniola and Tortuga as densely populated and "completely cultivated like the countryside around Cordoba" (Colón 1976, 165). He also noted that "the islands are not so thickly wooded as to be impassable," suggesting openings from clearing and burning (Columbus 1961, 5).

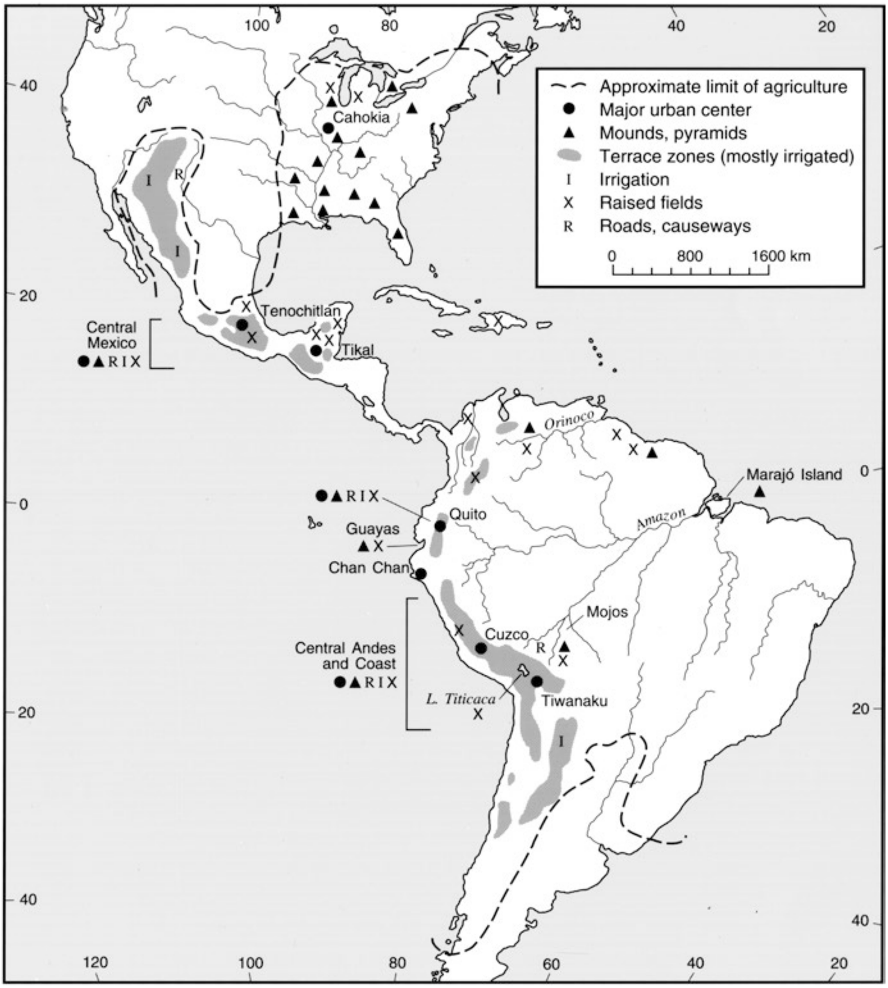

Fig. 8.1 Selected features of the prehistoric cultural landscape. Some cities and agricultural works had been abandoned by 1492. The approximate limit of agriculture and the distribution of terraces is based on Donkin (1979, 23); other features were mapped by the author

The roots of the pristine myth lie in part with early observers unaware of human impacts that may be obvious to scholars today, particularly for vegetation and wildlife.[8] But even many earthworks such as raised fields have only recently been discovered (Denevan 1966, 1980). Equally important, most of our eyewitness descriptions of wilderness and empty lands come from a later time, particularly

[8] The English colonists in part justified their occupation of Indian land on the basis that such land had not been "subdued" and therefore was "land free to be taken" (Wilson 1992, 16).

1750–1850 when interior lands began to be explored and occupied by Europeans. By 1650, Indian populations in the hemisphere had been reduced by about 90%, while by 1750 European numbers were not yet substantial and settlement had only begun to be expanded. As a result, fields had been abandoned, settlements vanished, forests recovered, and savannas retreated. The landscape did appear to be a sparsely populated wilderness. This is the image conveyed by Parkman in the nineteenth century, Bakeless in 1950, and Shetler as recently as 1991. There was some European impact, of course, but it was localized. After 1750 and especially after 1850, populations greatly expanded, resources were more intensively exploited, and European modification of the environment accelerated, continuing to the present.

It is possible to conclude not only that "the virgin forest was not encountered in the sixteenth and seventeenth centuries; [but that] it was invented in the late eighteenth and early nineteenth centuries" (Pyne 1982, 46). However, "paradoxical as it may seem, there was undoubtedly much more 'forest primeval' in 1850 than in 1650" (Rostlund 1957, 409). Thus the "invention" of an earlier wilderness is in part understandable and is not simply a deliberate creation which ennobled the American enterprise (Bowden 1992, 20–23). In any event, while pre-European landscape alteration has been demonstrated previously, including by several geographers, the case has mainly been made for vegetation and mainly for Eastern North America. As shown here, the argument is also applicable to most of the rest of the New World, including the humid tropics, and involves much more than vegetation.

The human impact on environment is not simply a process of increasing change or degradation in response to linear population growth and economic expansion. It is instead interrupted by periods of reversal and ecological rehabilitation as cultures collapse, populations decline, wars occur, and habitats are abandoned. Impacts may be constructive, benign, or degenerative (all subjective concepts), but change is continual at variable rates and in different directions. Even mild impacts and slow changes are cumulative, and the long-term effects can be dramatic. Is it possible that the thousands of years of human activity before Columbus created more change in the visible landscape than has occurred subsequently with European settlement and resource exploitation? The answer is probably yes for most regions for the next 250 years or so and for some regions right up to the present time. American flora, fauna, and landscape were slowly Europeanized after 1492, but before that, they had already been Indianized. "It is upon this imprint that the more familiar Euro-American landscape was grafted, rather than created anew" (Butzer 1990, 28). What does all this mean for protectionist tendencies today? Much of what is protected or proposed to be protected from human disturbance had native people present, and environmental modification occurred accordingly and in part is still detectable.

The pristine image of 1492 seems to be a myth, then, an image more applicable to 1750, following Indian decline, although recovery had only been partial by that date. There is some substance to this argument, and it should hold up under the scrutiny of further investigation of the considerable evidence available, both written and in the ground.[9]

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[9] Also see the later related articles by Denevan (2011, 2016).

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8.3 A Bluff Model of Riverine Settlement in Prehistoric Amazonia

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Abstract In Amazonia, prehistoric settlement was especially concentrated along the major rivers. This has been explained by the superior soil and wildlife resources of the floodplain (*várzea*) compared to the interfluve uplands (*terra firme*). However, the floodplain is a high-risk habitat because of regular and periodic extreme flooding of even the highest terrain. A bluff model is proposed arguing that most settlement was not in the floodplain but rather on the valley-side bluff tops adjacent to active river channels. Subsistence was a multistrategy utilizing floodplain *playa* (beach) and levee soils and aquatic wildlife periodically in combination with more stable bluff-edge gardens, agroforestry, and hunting. That permanent and semipermanent cultivation systems were established on the poor bluff soils is evidenced by archaeology, ethnohistory, paleoecology, and zones of anthropogenic soils (*terra preta*). However, bluff occupation was sporadic rather than continuous, with large settlements mostly located where main river channels impinged against bluffs. This pattern persisted with colonial missions, and it continues today.

Keywords Amazonia · Bluff settlement · Demography · Intensive agriculture · Prehistoric · Riverine · *terra preta*

Introduction

> The population density and level of cultural complexity achieved by protohistoric Amazonians is one of the most controversial topics in American archaeology. (Meggers 1993–1995, 91)

Fig. 8.2 Amazon Basin, locating rivers, places, and tribes mentioned

In Amazonia (Fig. 8.2), it is established that late prehistoric settlement and agriculture were concentrated along the major rivers. In the *Handbook of South American Indians*, Julian Steward (1948, 886) stated that "the important ecological differences were those between water-front and hinterland peoples… The differences were in resources, and these partly determined population density and community size"; this is reflected in Steward's (1949, 659) map of South American population densities at European contact. Betty Meggers' (1971) *Amazonia* book is organized on the basis of *várzea* (floodplain) and *terra firme* (upland or interfluve) distinctions. The same emphasis was made by Donald Lathrap (1968a, b, 1970), Robert Carneiro (1970, 1995), Anna Roosevelt (1980), myself (Denevan 1966b, 1976), and others.

This riverine orientation is supported by the archaeological and ethnohistorical records, although both are meager; few river tribes survived the colonial period. The riverine pattern seems explicable by the good soil and rich wildlife resources of the *várzea* compared to the *terra firme*, in addition to ease of movement. The general assumption has been that settlement and resource use were focused in the floodplain. Roosevelt (1980), for example, repeatedly refers to agriculture and dense populations "in" the floodplains.[10] However, the floodplain is a high-risk habitat because of regular annual flooding plus periodic extreme flooding that overflows even the highest terrain, in addition to daily tidal flooding in the delta region. Flooding is what floodplains do, and "floodplain" is defined here accordingly.[11]

[10] The "evidence from archaeology and ethnohistory made it clear that dense populations and advanced cultures had developed in Amazonian floodplain habitats in late-prehistoric times" (Roosevelt 1980, 252). Later, however, Roosevelt (1987, 154–155) stated that late prehistoric chiefdoms were "settled along the banks, levees, and deltas of the major floodplains," which is still ambiguous as to precise location. She does not define "floodplain."

[11] "At maximum river stage, the entire floodplain is covered by a continuous sheet of water" (Sippel et al. 1994, 73). However, there are some high levees, ancient floodplain surfaces, and river terraces that never seem to flood and can thus be considered to be within the valley but not part of the active floodplain.

Meggers has retreated somewhat from her earlier assertion (1971, 122) that "population density and level of cultural development were considerably greater on the *várzea* than on the *terra firme* at the time of European contact." She now argues in an article on "Prehistoric Population Density in the Amazon Basin" that overall densities were comparable for both *várzea* and *terra firme*, 0.3/km^2 (Meggers 1992, 203), and that "[r]iverine villages are [were] not significantly larger and more permanent than hinterland ones" (Meggers 1993–1995, 97). For *terra firme*, her 0.3/km^2 is based on an average of present indigenous densities which vary from 0.04 to 1.0/km^2 with a mean of ca 0.3. No references are given. However, 0.3/km^2 is close to the 0.23/km^2 (0.6/mi^2) of Steward and Faron (1959, 52–53)[12] and my own 0.2/km^2 (Denevan 1976, 226)[13] (Table 8.1).

Meggers (1992, 203) applies the same density of 0.3/km^2 to the *várzea*. She admits that "higher concentrations could have existed on the *várzea* but that these are offset by large uninhabitable regions." She believes that flood-plain occupation and agriculture would have been limited by periodic high floods that covered the entire sectors of floodplain: "on the *várzea*, the high fertility of soils is offset by unpredictable variations in the timing and intensity of annual river fluctuations, with attendant risk of frequent crop loss" (Meggers 1994, 416). As a result, "the *terra firme* [was] an equally if not a more reliable habitat for humans" (Meggers et al. 1988, 291). Riverine settlement, being at risk from floods, depended on supplementary food production from *terra firme* (Meggers 1993–1995, 106), presumably from long-fallow shifting cultivation supporting low densities, hence the application of the 0.3 *terra firme* density to the overall *várzea*. Meggers (1984, 642; 1971, 133) believes that sites in the *várzea* were few, mainly "temporary camps … during low water" or "fishing stations" (of which few have survived).

My initial reaction to this argument was that it is contrary to all my previous thinking about the cultural ecology of prehistoric floodplain settlement (Denevan 1966a, 1970, 1976, 1984, 1992a). I suggested overall floodplain densities ranging from 5 to 15 per km^2 (Table 8.1). A reconsideration of riverine settlement and agriculture, as presented here, now convinces me that Meggers is essentially correct about the demographic significance of high floods but that she underestimates the contribution of bluff agriculture and both local and overall riverine population densities and numbers (Denevan 2003).

I propose a model that identifies valley-side bluffs as favored sites for relatively large and semipermanent settlements. I believe that most prehistoric "riverine" settlement was not located in the floodplain but rather on those fringing bluff tops that were adjacent to active river channels. Subsistence was a multiple strategy that involved the seasonal utilization of floodplain *playas*[14] and levee soils and wildlife

[12] This is a revision of Steward's (1949, 659) regional density estimates of between 0.10 and 0.40/km^2 for the non-riverine Amazon forests.

[13] Population densities listed by Beckerman (1987, 86) for 13 contemporary Amazon *terra firme* tribes average 0.39/km^2, with a range from 0.01 to 1.00.

[14] In Peru, a distinction is made between emergent sandbars (*playas*) and mudflats (*barreales*). Here, *playa* (beach) refers to both.

Table 8.1 Amazonian population density estimates, 1492 (per km^2)

Source	Riverine	Terra firme	E and SE coast	Wet savanna	Dry savanna	Lower montane	Overall density[a]	Total population, Greater Amazonia[b]	Total population, Amazon Basin[c]
Steward (1949, 659–663)	0.20–0.60	0.10–0.40	0.60	0.15–0.60	0.10	0.35	0.220	2,153,000	1,302,000
Steward and Faron (1959, 52–53)	0.39–0.78	0.23	0.78	0.43–0.78	0.12	0.39	0.278	2,720,000[d]	1,645,000
Hemming (1978, 492)							0.275[e]	2,686,000	1,627,000
Meggers (1992, 203)	0.30	0.30					0.300[f]	2,931,000	1,775,000[g]
Denevan (1970, 79–82)	5.30	0.20	9.50	1.30–2.00	0.50	1.20	0.589	5,750,000	3,485,000
Denevan (1976, 226–234)	14.60	0.20	9.50	1.30–2.00	0.50	1.20	0.696 or 0.522	6,800,000 or 5,100,000[h]	4,118,000 or 3,088,000
Denevan (1992a, xxv–xxix)	13.90						0.580	5,664,000	3,431,000
Newson (1996, 10, 11, 15)	2.95 (E Ecuador)	0.41–0.51 (E Ecuador)				1.61 (E Ecuador)	0.760–0.830[i]	7,424,000 to 8,108,000[j]	4,496,000 to 4,910,000[k]
Denevan (herein)	Bluff patches: 10.0 or more; remainder: 0.30						0.561	5,487,000[l]	3,319,000[m]
Denevan (2003, 181, 187)	10.4							5.6 million	3.4 million
Denevan (2014, 211)								8–10 million	

Table 8.1 (continued)

[a]Total population estimate for Greater Amazonia divided by the total approximate area of Greater Amazonia, 9,769,000 km^2, unless otherwise noted. Greater Amazonia, as I have defined it, includes the tropical interior of South America east of the Andes and north of the Tropic of Capricorn, including the Orinoco Basin but not the Gran Chaco (Denevan 1976, 230, 231)

[b]Totals are either for Greater Amazonia or extrapolations to Greater Amazonia in the cases of Hemming, Meggers, and Newson. Totals are based on tribal, regional, or habitat counts and estimates or density estimates, including projections backward in time

[c]Overall density multiplied by the area of the Amazon Basin (catchment area) of 5,916,000 km^2 as given by Sternberg (1975, 15)

[d]For the several categories of tropical forest, eastern Brazil and northern Venezuela, which together are approximately equal to Greater Amazonia

[e]Derived from Hemming's total of 2,184,000 for tropical Brazil (total for Brazil minus the three southern states)

[f]Meggers' average density for both *várzea* and *terra firme*

[g]Meggers' own total is "depending on the boundaries employed … 1,500,000–2,000,000 for Amazonia as a whole"

[h]The second estimate is a 25% reduction to allow for unoccupied buffer zones between antagonistic social groups

[i]Newson's density for eastern Ecuador

[j]Extrapolation of Newson's density for eastern Ecuador to Greater Amazonia

[k]Newson's own total for the Amazon Basin (size not given) is 4,860,000–5,460,000, based on her density for eastern Ecuador

[l]This is an adjustment of my 1992 total, changing only the riverine density, area, and population

[m]Newson (1996, 15) indicates that my (1996) new Amazon Basin total should be 3,640,000, but her method for deriving this figure is not indicated

in combination with more permanent bluff-edge gardens and agroforestry. However, bluff settlement was spatially sporadic rather than continuous, being dense mainly along sectors where a river channel impinged against a bluff. These sectors could continue for many kilometers, alternating with unpopulated bluffs isolated from active river channels.

The importance of bluff location of prehistoric sites in Amazonia has been noted previously by archaeologists and others, but without elaboration. Here I will review the evidence and suggest an integrated bluff/*várzea* strategy of complementary resource use and reexamine spatial patterns of settlement and population density.

The Riverine Bluffs

Amazon River bluffs are near-vertical walls rising above and confining the entrenched valley, separating the recent alluvium of the *várzea* from the impoverished soils of the Tertiary and Pleistocene uplands (Fig. 8.3). Heights are variable depending on location and seasonal water level; 10–20 m during low water is common, but some bluffs are much higher. The bluffs, as well as floodplain levees, are subject to collapse (bank caving) when the river channel is immediately adjacent and migrating (Sternberg 1960, 402–404), such collapse destroying past and present settlement sites on the bluff edges (Lathrap 1968a, 69, 76). The likelihood of site destruction from lateral erosion is considerable for the highly meandering tributaries of the Upper Amazon (Fig. 8.4), but much less so for the more stable, more linear mainstream Amazon (Fig. 8.5) and Orinoco. Many prehistoric sites have survived on the bluffs, as evidenced by dated anthropogenic soils, bones, and cultural material.

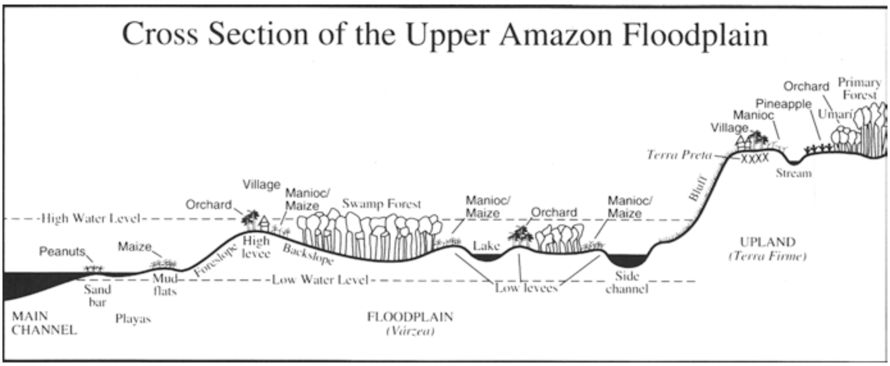

Fig. 8.3 Representative cross section of the Upper Amazon floodplains near Iquitos showing the main channel, side channel, water levels, natural levees, bluffs, villages, and a *terra preta* site. (Source: Adapted from Coomes 1992, 164)

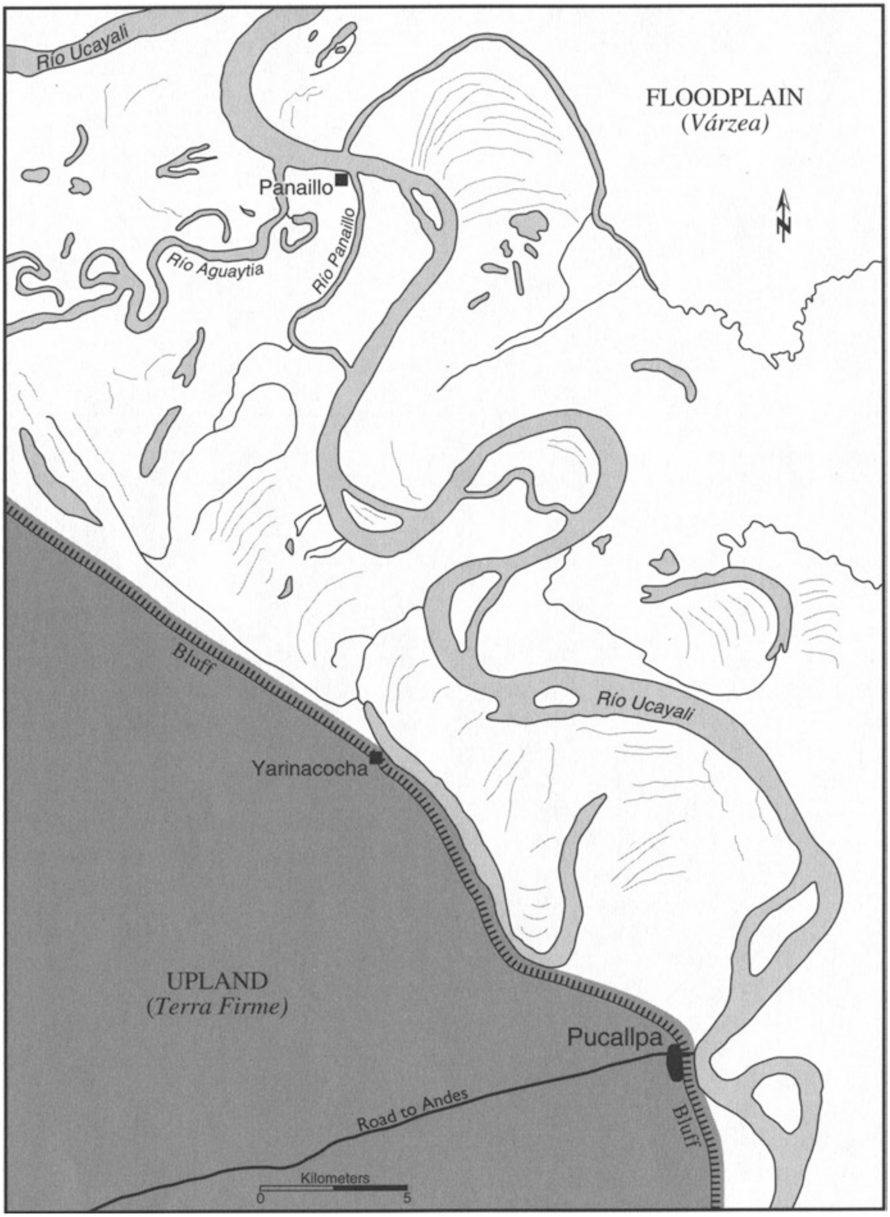

Fig. 8.4 The central Río Ucayali floodplain in Peru. Note the strongly meandering channel, numerous lakes, islands, and side channels. The floodplain is over 25 km wide here. The city of Pucallpa is on the western bluff adjacent to the main channel. The Shipibo Village and archaeological site of Yarinacocha is on the bluff overlooking a cutoff lake that was until recently part of the main channel. The lake still provides access to the Ucayali via a small channel. The Shipibo Village of Panaillo is within the floodplain on a high levee that is flooded in some years. The village does not have easy access to *terra firme* resources, so high floods are a serious crisis. (Source: Adapted from Bergman 1980, 13)

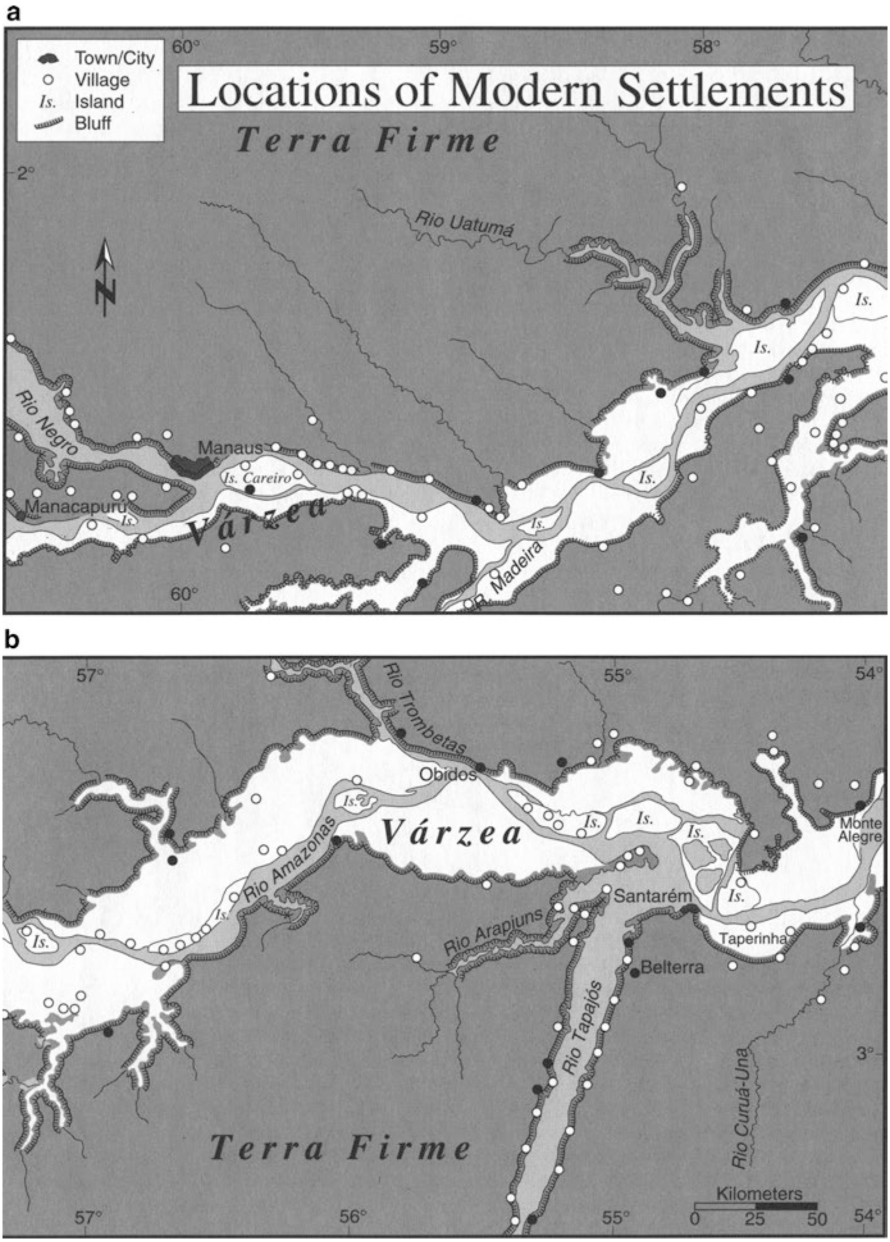

Fig. 8.5 Amazon floodplain between Manaus and Monte Alegre, Brazil, showing channels, islands, bluffs, and recent settlement locations. There are 153 villages, towns, and large cities shown. One hundred and six of these are on bluff edges; 35 are within the floodplain, most on islands, all but two being small villages; and 12 are on *terra firme* within 10 km of the floodplain (additional towns further from the bluffs are not shown; most are along roads). Of the 106 bluff-edge settlements, most are adjacent to major channels, and of the remainder, most are adjacent to large floodplain lakes. Sources: Adapted from maps in RADAM (1976, Vol. 10; 1978, Vol. 18)

In the central Amazon there are high levee sites that are over 2000 years old, suggesting long-term channel stability (Sternberg 1975, 18). The Taperinha shell midden site east of Santarém in Brazil is 7000–8000 years old (Roosevelt 1991).

A bluff site is only attractive for settlement, however, if there is navigable water in the river channel immediately below. Such a proximate channel (or lake) must be navigable year-round, not just seasonally, and if not the main channel, then it must be connected to the main channel. As a river shifts its course away from a bluff site, settlement is likely to be abandoned. In Peru, a migrating river channel is likely to return to a bluff edge in about 500 years (Lathrap 1968a, 75), at which point a former village site may be reoccupied. At San Francisco de Yarinacocha (Fig. 8.4), a bluff site on the Río Ucayali in Peru, there are 18 archaeological levels (components) with an average duration of about 100 years and a maximum of 200 years (Lathrap 1968a, 67).

Sections of bluff may extend for tens of kilometers without being fringed by a river channel, the channel or multiple channels being either at the opposite bluff or in mid-floodplain. This is illustrated in Fig. 8.5. Present-day towns, small villages, and individual dwellings are invariably located on bluffs overlooking active channels, as were colonial missions (see below). The same was apparently true of most prehistoric bluff settlements (Fig. 8.6), and this has profound significance for prehistoric demography. Dense riverine populations and associated agriculture apparently were sporadic rather than continuous.

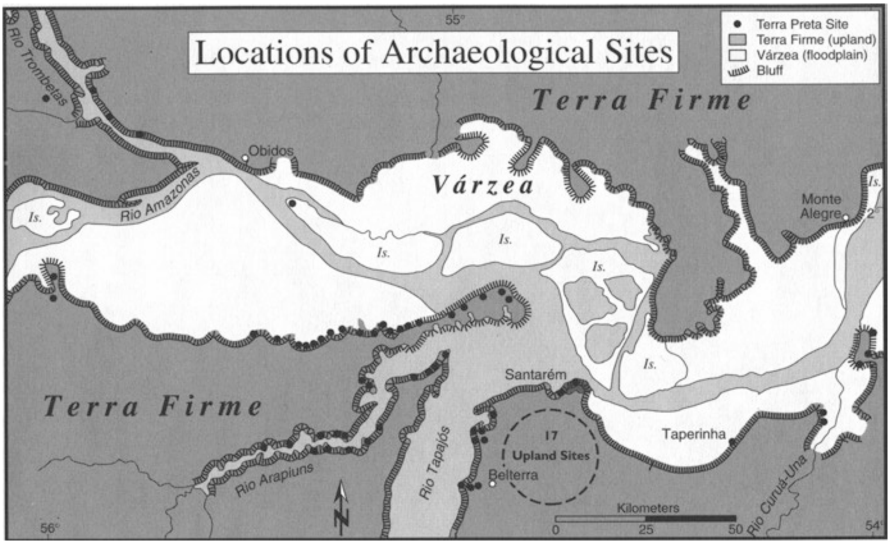

Fig. 8.6 Locations of 75 *terra preta* village sites along the Amazon in the vicinity of Santarém and the Rio Tapajós. Most sites are on bluffs, but several are on *terra firme* south of Santarém. Source: Adapted from Meggers (1971, 132; 1984, 642), which is based on Nimuendajú (n.d.)

In addition, bluff sites must have access to freshwater. Hauling water in pots or gourds up a steep bluff is possible but difficult. Many bluff sites today are located where small upland streams meet and dissect the bluff.

Ethnohistorical Evidence for Settlement Location and Size

The remarkable Spanish voyages (Fig. 8.7) on the Amazon in the mid-sixteenth century give some indication of the topographical location, size, and distribution of indigenous settlement. High ground is consistently referred to, but the terms used are not necessarily indicative of fringing bluffs in contrast to natural levees within the floodplain. In the description of the Orellana expedition down the Amazon in 1541–1542, "high banks," "hills," and "elevated spot overlooking the river" are likely bluffs, but "high land" is more ambiguous (Carvajal 1934a, 199, 201, 204,

Fig. 8.7 Upper Amazonia, locating places and archeological sites mentioned and the upper routes of the Orellana (1541–1542) and Ursúa/Aguirre (1560–1561) expeditions. (Sources for routes: Medina 1934, 48 and Minta 1994, vi–vii)

209, 217, 277; 1934b, 424, 425).[15] In the land of Chief Arripuna (Tapajó Indians), Carvajal (1934a, 227) noted that for several leagues,[16] "down close to the shore there were no settlements, for they all appeared to be in the interior of the land" and that fortresses were "about two or three leagues back from the river." In his second version of the Orellana voyage, Carvajal (1934b, 427, 430, 431, 440) mentioned villages "on a hillside," "situated on high land," "on a slope of a hill," and warriors "upon the high bank of the river." Salinas de Loyola (1965, 201–202, 207) in 1557 observed that the Río Ucayali tribes "have their settlements on top of the ravines [*barrancas*] of the river."

Villages also were observed within the floodplains, particularly on islands. Myers (1992b, 132) mentions two reports from the 1560–1561 Pedro de Ursúa (Orsúa)/Lope de Aguirre expedition of large Omagua villages on Garcia Island on the Amazon west of the mouth of the Río Napo. The issue here, however, is not whether settlements were located in the floodplain, but rather the importance of complementary settlement on the bluffs.

There are several mentions of village sizes by members of the mid-sixteenth-century Spanish expeditions. Captain Altamirano, who was with Ursúa and Aguirre, reported Omagua villages (Río Napo to Río Putumayo) numbering 2000, 8000, 6000, and 2000 Indians (Vásquez de Espinosa 1948, 383–384). Myers (1992b, 132–133) lists several early accounts of Omagua villages with 30–60 large houses each. Salinas de Loyola reported villages (Cocama) on the Ucayali numbering 200, 300, and 400 houses, with about 20 persons per house; the largest could have contained 8000 people (Myers 1974, 140–141). Carvajal (1934b, 426) described a Yurimagua village as having 500 houses. Evaluating these early reports, Myers (1992b, 138–139) estimates that in 1541–1542 there were 30 Omagua villages, averaging 20 large communal houses each, averaging 40–50 occupants in each house[17] or 800–1000 per village, for a total of 24,000–30,000 people, with a reduction to possibly 6000 by 1651. Thus, it is feasible that the largest Omagua villages held a few thousand people, but not likely 6000–8000. Smaller villages were also mentioned. In a recent examination of the early voyages, Porro (1994, 86) for the Yurimagua (Yoriman) between Tefé and Coari in 1647 estimates that villages averaged 20–24 houses, with a reported 4, 5, or more families each (Acuña 1942, 79), for an average of about 550 people in each village. This is still a significant village size, and undoubtedly there had been population reductions since the initial Spanish contacts a century before.

[15] For example, "un pueblo que estaba en una loma," and "sobre un alto una hermosa poblacion" (Carvajal 1894, 54).

[16] A sixteenth-century Spanish league (*legua*) measured about 3 2/3 mi or 5.9 km (Medina 1934, 47); however, the length of a league varied somewhat in time and place, often 5 km being given. The accuracy of measurement during early Amazon River travel is unknown; some distances reported are clearly excessive.

[17] Communal houses containing over 100 people have been observed in the twentieth century (Myers 1992b, 138).

Sixteenth-century accounts also provide an indication of the degree of continuity of settlement along the Amazon. Carvajal (1934a, 202, 203, 212; 1934b, 428) described linear[18] Omagua and Tapajó villages that were 1, 2, 2.5, 4, and 5 leagues long. An Omagua village "stretched for five leagues without there intervening any space from house to house," and for 80 leagues "there was not from village to village a crossbow shot" and at most under half a league (Carvajal 1934a, 198). One village was broken up into sectors, each with its own "landing place down on the river" (Carvajal 1934a, 203). Altamirano described a Yurimagua village three leagues long with the houses touching one another and another on a high bluff (*barrancas muy altas*) reached by a staircase with over 100 steps cut into the bluff (Vásquez de Espinosa 1948, 385). Francisco Vázquez (1981, 225), who also was with Ursúa and Aguirre, described the same village as being only two leagues long. Acuña (1942, 79) reported a Yurimagua village that was over one league long 80 years later.

As to the spacing of villages, conflicting figures are given. For an Omagua territory of over 80 leagues, Carvajal (1934a, 184, 188) indicated 26 overlords (subchiefs), probably heads of individual large villages, for an average spacing of three leagues. Participants in the Ursúa/Aguirre expedition 20 years later reported 10–15 and 15–20 Omagua villages over 150 leagues, but the historian Ortiguera, who apparently interviewed survivors, gave 25–30 villages over the same distance (Myers 1992b, 133). Ten to 30 villages over 150 leagues would give an average spacing between villages of from 5 to 15 leagues. These potentially large separations conform to the model of sporadic bluff settlement proposed here.

Initial expeditions on the Amazon above the Rio Negro reported large uninhabited sectors, apart from broad village spacing, possibly 25% of the mainstream between the Negro and the Putumayo and 50% along the lower Napo and the Ucayali (DeBoer 1981, 376). Such percentages are probably too high overall; below the Rio Negro, large gaps apparently were absent. The approximate locations of the empty sectors are mapped by DeBoer (1981, 368), based on Carvajal (1934a, 179, 200) in 1541–1542, Salinas de Loyola (1965, 207, 213) in 1557, and Acuña (1942, 72) in 1639. The reported lengths of these empty lands varied from 10 to 200 leagues. In addition, Gonzalo de Zúñiga (1981, 6), who was with Ursúa and Aguirre in 1560–1561, reported three unpopulated sectors along the upper Amazon measuring 300, 150, and 300 leagues separating occupied provinces of 150 and 200 leagues; the empty sectors only contained fishing camps of one or two houses.

DeBoer (1981) and Myers (1976) believe that the longer empty sectors were buffer zones or "no-man's lands" between hostile groups. Their argument is convincing; however, such empty lands, or some or parts of them, also could have been sectors where the river channels did not impinge on bluffs and thus were unattractive to settlement. Myers (1992a, 88) points out that "[w]hen Orellana and Orsua were out of touch with people, they were traveling through stretches where the course of the Amazon passed through the middle of the *várzea*, remote from the high banks

[18] One seventeenth-century account reported Omagua villages perpendicular to the rivers, possibly to focus on a prime river landing (Cruz 1885; in Myers 1992b, 133). However, I know of no reported archaeological sites so oriented.

which were the site of human habitation." Also, the Spaniards intentionally avoided the settled sides of the river (Carvajal 1934a, 227). Stocks (1983, 265) suggests that the empty sectors reflect the uneven distribution of fluvial lakes (prime fishing sites), but this does not seem likely (Myers 1990, 14). On the lower Napo, Carvajal (1934b, 420) reported villages "completely abandoned… because the river overflowed its course and inundated everything." These villages may have been relocated to bluff sites during seasonal high water, which would explain why river travelers did not observe settlement when they were out of view of the bluffs. Another explanation for empty lands could have been regional depopulation from early European-introduced epidemics, but this probably would not account for the spatial patterns observed.

Interaction between the floodplains and the bluff fringes and interior is indicated by early Spanish reports of roads extending inland from the rivers. Carvajal (1934a, 200, 202, 203, 209, 210) mentioned "many roads," "like royal highways and wider," "leading into the interior," "roads that came down to the river" in the Omagua region.[19] Sancho Pizarro, with Ursúa, reported following an Omagua road for 30 leagues for 30 days into the interior (Vásquez de Espinosa 1948, 385–386). Nimuendajú (1952, 11) observed prehistoric roads near the Rio Tapajós that were 1–1.5 m wide and 30 cm deep that "run almost in a straight line from one black earth [old village site] deposit to another." These roads may have been simply wide trails, but they are suggestive of resource use inland from the bluffs, as well as social interaction between *várzea* and *terra firme*.

Evaluation of the Early Accounts

Meggers (1992, 203; 1993–1995, 93–96, 102–104) considers information in Carvajal and the other early Amazon accounts to be unreliable, and she discounts the reports of large populations and villages, whereas Myers (1992b), Roosevelt (1987, 154), Porro (1994, 86), and Whitehead (1994, 35–36) disagree. Carvajal's (1934a, b) 1542 accounts, it should be noted, were written within weeks or a few months after completion of the voyage, not years later when memory had faded. Hyperbole and fantastic happenings are generally obvious. Certainly, more credence can be given to direct observations by the Spaniards in contrast to what the Indians reportedly told them. And more credence can be given to numbers of villages and houses, which are readily countable, than to estimates of numbers of warriors attacking or people in a village or clustered on a riverbank.

Carvajal's record of the Orellana voyage consists of two versions (Medina 1934, 7–163). The first written was published by the Chilean historian José Toribio Medina in Spain (Carvajal 1894) and later in English translation edited by H. C. Heaton (Carvajal 1934a). This exists as two transcripts of the lost original.

[19] "Había muchos caminos que entraban la tierra adentro muy reales" (Carvajal 1894, 42, also see pp. 45, 46, 53, 67). "Highways" were not likely, but neither is it likely that these were all simple trails, which the Spaniards were quite familiar with.

The first (Muñoz version) is incomplete. The second is that compiled by Medina. There are only a few differences, but some are relevant to our discussion. In the Muñoz version, armies of 10,000 and 8000 Indians are mentioned in the Province of Machiparo (Aisuari), whereas in the Medina version the information given is, first, "more than 400 Indians" and, second, "many Indians" and "beyond count" (Carvajal 1934a, 196–197).

The second account by Carvajal was prepared for the great historian of the Indies Gonzalo Fernandez de Oviedo y Valdés, who was in Santo Domingo when Orellana's voyage ended. Oviedo requested a report from Carvajal and then published it in his *Historia de las Indias*. Heaton published an English translation (Carvajal 1934b) in the same volume with the Medina version. Heaton (1934, 385–389) believed that the Medina version was probably written hastily by Carvajal in late 1542 on the Island of Cubagua as a report to Orellana; that the version for Oviedo was written more carefully some months later; and that, given that some passages are almost identical whereas others are quite different, it is likely that Carvajal had the earlier account in front of him and corrected or amplified some passages but not others as he prepared the new version. Changes may also have been made by Orellana in the first version and by Oviedo in the second on the basis of interviews they had with other members of the expedition; also, changes could have been made by transcribers.

The point to be made is that the Oviedo version is probably more reliable than the Medina version. However, it is the Medina version that most modern scholars cite. There is more likely to be exaggeration of armies and village sizes in the Medina version than in the Oviedo version. There is no mention of 50,000 or of 10,000 and 8,000 warriors as there is in the Medina (and Muñoz) versions (Carvajal 1934a, 190, 196, 197) nor of villages four or five leagues in length. There is one mention of 5,000 warriors in the Oviedo version (Carvajal 1934b, 440). In the latter, there are various general references to numerous villages, both large and small, and to large numbers of Indians. Thus, the Oviedo version of Carvajal provides a more moderate and believable view of Amazon demography and settlement than does the often-criticized Medina version.

Village lengths of from one to five km were reported by four different people between 1542 and 1639; some are probably exaggerations, but villages of at least one league (5.9 km) long are possible. There are archaeological sites along bluffs that are indeed several kilometers in length (see below). Meggers' (1993–1995, 98) argument that all large prehistoric sites represent multiple reoccupations, and not single villages at one point in time, is weakened by the several sixteenth- and seventeenth-century reports of large, linear, occupied villages.

As for the figures from the other mid-sixteenth-century expeditions—Ursúa/Aguirre and Salinas—there were village sizes reported as large as 6,000–10,000 people, but with houses numbering only 20–60. This again suggests an exaggeration of village populations and more reliability of house numbers. (A village of 20–60 houses at 40–50 people per house would total 2,400–3,000 Indians.) The estimates of villages with 200–500 houses for the Cocama would mean 4,000–10,000 each, using Myers' estimate of 20 persons per house; however, house sizes may have been smaller in those provinces. In general, there is a degree of consistency in the various accounts of the three voyages.

Archaeological Evidence

The concentration of prehistoric Amazonian riverine settlement sites on bluffs was observed by Nimuendajú (1952, 11) in 1949 for the Santarém region. Lathrap (1968a, 77) for the central Ucayali believed that while there was settlement in the floodplain, it was "more typically on the bluffs adjoining the flood plain." Myers (1973, 240–243) lists 40 archaeological sites for the central Amazon, Río Napo, and central Río Ucayali, of which 32 are on high banks, bluffs, or hills. Meggers' (1971, 132; 1984, 642) map of sites centered on the Rio Tapajós shows 75 by my count, based on a manuscript map by Nimuendajú (n.d.) (Fig. 8.6). Nimuendajú (1952, 9–11) said that he had located 65 sites, less than half of those in the region, almost all "found on high ground, safe from inundation." Some are on interior *terra firme* east of the Rio Tapajós. A 1924 map by Nimuendajú (1952, 9–11) of the area just west of the Tapajós shows 28 *terra preta* (black earth) sites. N. Smith (1980, 563) indicates 17 *terra preta* riverine sites between Santarém and Manacapuru. Meggers et al. (1988, 289) map 23 sites along the Río Xingú, and Meggers (1992, 199) maps 37 prehistoric sites along the Río Tocantins, most seeming to be on bluffs. Andrade (1986, 22–23) maps 51 *terra preta* sites, including those of N. Smith.

Archaeological sites have been discovered in the floodplains, usually on high levees, but they do not seem to be common. Most of the many that once existed have been destroyed by river erosion or buried under sediment. Myers (1973, 241) reports floodplain lake sites at Cushillococha on the Peru/Brazil border. Lathrap (1968a, 74) described them at Cumancaya on the Ucayali. Sternberg (1960, 417) found sites 1,000–2,000 years old on levees on Careiro Island near Manaus. Most floodplain villages, however, probably did not have sufficient duration to create *terra preta* of significant depth. Furthermore, most garbage probably went into the river, rather than accumulating. Meggers (1984, 642) points out that "temporary camps undoubtedly existed on the flood plain during low water to facilitate agricultural work and other subsistence activities, but evidence has been obliterated by shifts in the courses of the channels and deposition of sediment." Many villages consisted of houses on pilings that could survive periodic high floods; however, crops cannot, although vulnerability varies with crop and with duration and depth of flooding.

Terra Preta

Terra preta, or *terra preta de índio* (Indian black earth), is an anthropic soil of prehistoric origin, black or dark brown, rich in organic material, and laden with cultural debris (ceramics, bones, ash). Such soil has been reported along the Amazon, Orinoco, Negro, Guaporé, Tocantins, Tapajós, Xingu, Napo, Ucayali, Caquetá, Corentyne (Guyana), other rivers, and Marajó Island and also in the Colombian Llanos (Nimuendajú 1952; N. Smith 1980; Eden et al. 1984; Balée 1989, 10–14; Herrera et al. 1992; Katzer 1944; Meggers 1993–1995, 98; Andrade 1986, 22–23; and Woods 1995). Dates are as old as 100–450 BC (Eden et al. 1984, 126). Most *terra preta* sites are on bluff edges. Some are enormous, such as the one underlying much of the city of Santarém. Roosevelt (1989b, 45) believes that some of these sites were large nucleated towns of chiefdom status, with a permanence of several hundred years. "Santarém habitation sites extend almost continuously along the river for hundreds of miles" (Roosevelt 1989a, 82).

An early description of *terra preta* soils was provided by the English traveler Herbert Smith (1879, 238), who found American Confederate families farming them near Santarém: "it [tobacco] is cultivated on the rich black lands along the edge of these bluffs… All along this side of the Tapajós … [which] must have been lined with these villages, for the black land is almost continuous, and at many points pottery and stone implements cover the ground like shells on a surf-washed beach."

Terra pretas are usually former settlement sites whose dark color is mainly due to residue from fires for cooking and warmth; soil carbon is high. N. Smith (1980, 561–562) found that phosphorus levels are high, the result of ash, fish and game and human bones, feces, urine, and shells. Bones also account for a high calcium content. The pH levels are also higher than for adjacent soils, and aluminum levels are moderately low. The soil fertility of *terra preta* is significantly higher than for most *terra firme* soils.[20] Currently, both Indians and non-Indians seek out *terra preta* soil for their fields.[21]

Terra preta on bluffs occurs on a variety of *terra firme* soils, including oxisols, ultisols, eutrophic soils (*terra roxa*), and spodosols (N. Smith 1980, 557). Most of these soils are of very low natural fertility, the *terra roxa* being the main exception. Soil darkness varies considerably and there is no agreement on color criteria for a *terra preta*. The Araracuara Project researchers on the Río Caquetá in the Colombian Amazon found brown soils adjacent to or surrounding pockets of black soil. They believe that the brown soils are not settlement zones but rather permanent or semi-intensive agricultural sectors that were maintained by organic additives that produced the brownish color. The brown soils differ from the black soils in color, less depth, less phosphorous, and having fewer cultural remains (Andrade 1986, 53–54; Mora et al. 1991, 75–77). Sombroek (1966, 175) points out the occurrence of brown soil *(terra mulata)* in the Belterra area east of the Rio Tapajós. This is a soil lighter than *terra preta*, without artifacts, occurring in bands around *terra preta* on *terra firme*. He believed that "this soil has obtained its specific properties from long-lasting cultivation." Undoubtedly, different kinds of *terra preta* originated and evolved through different pathways and on different parent soils (Woods 1995).

Terra preta bluff sites are linear, paralleling the rivers. The 17 river-edge sites examined by N. Smith (1980, 563) range in size from about 1 to 90 ha and average 21.2 ha. A site on the Rio Xingu near Altamira is 1.8 km long and 500 m wide, covering 90 ha, and one at Manacapuru on the Amazon is 4 km long and extends

[20] For Araracuara, Eden et al. (1984, 134) found the *terra preta* soils to be higher in phosphorus and organic carbon than adjacent soils, but similar in acidity, calcium, and exchangeable aluminum.

[21] Farmers today even transport black earths considerable distances by truck to fertilize their fields and gardens (Smith 1980, 562).

200 m inland, totaling 80 ha according to N. Smith (1980, 560; 2 km by 400 m according to Myers 1973, 240). Roosevelt (1987, 157) says that the *terra preta* underlying Santarém covers 5 km^2 (500 ha). The site at Tefé is 6 km long (*terra preta?*) (Myers 1973, 240). Although some *terra preta* sites have been excavated, apparently none have been mapped.

Darkness and depth of *terra preta* are probably indicative of length of occupation, whether continuous or periodic. The riverine sites examined by N. Smith (1980, 563–564) had depths of up to 2 m with an average of 0.73 m. He suggests an accumulation rate of about 1 cm per 10 years of occupation; thus 2 m depth would mean 2000 years of settlement, but this seems unlikely. Ceramics, which are common in black earths, vary in style, indicating different cultural phases and hence, possibly, discontinuous settlement. Meggers (1992) demonstrates this on the basis of seriation sequence analysis of ceramics in sites along the Rio Xingu, Río Tocantins, and other tributaries. She believes that sites with large surface areas "represent multiple reoccupations rather than large single villages" (Meggers et al. 1988, 291) and that "[h]ence, the surface extent of archaeological sites cannot be used to infer village size, as has often been assumed" (Meggers 1995, 29). Roosevelt (1989b, 45–46), on the other hand, believes that many large sites were of long duration, as do Mora et al. (1991, 39, 61, 77) and Herrera et al. (1992, 110) for Araracuara. Reoccupation was probably a factor, but it remains to be demonstrated how long and how large most specific occupations were, a critical issue for estimating site populations.

There have been a few *terra preta* sites reported within floodplains on high levees or on river terraces (Sternberg 1960, 417, 419; Coomes, pers. comm. 1995). However, most are on *terra firme* bluffs. N. Smith (1980, 562) found that even the 12 interfluve sites he examined were located either along a small river or within a few hundred meters of a perennial stream, indicating the importance of navigation, potable water, and relatively easily cleared riparian forest. Bluff sites are often just above falls or rapids or where bluffs jut into rivers or at tributary junctions (N. Smith 1980, 562–563; Myers 1990, 19), suggesting strategic considerations. Sombroek (1966, 175) observed that *terra preta* sites in eastern Brazil "are especially frequent at outer bends of the rivers, where no floodplains occur between the water and the upland, and where the waterway can be scanned freely."

Certainly not all bluff sites are *terra preta*. For example, the large Finca Rivera site near Leticia (Colombia) (1.5 km long, 45–60 m wide, 16 ha) is up to 30 cm deep, with no indication of black earth, though there is a brown midden layer (Bolian 1975, 22, 27).

Numerous small *terra preta* sites have been reported in interfluve forests in Brazil, including a total of about 50,000 ha between the Rio Tapajós and Rio Curuá-Una (Katzer 1994, 35–38). Such sites are usually much smaller than most bluff sites; N. Smith (1980, 563) obtained an average of 1.4 ha for 12 interfluve sites. Some are as small as 0.3–0.5 ha. Most are shallower than bluff sites, suggesting shorter periods of occupation, but sufficient to create black earth. They tend to be circular and probably represent a few large communal houses or a circle of smaller houses. However, there are also some very large (100 ha or more) interfluve *terra* *preta* sites, such as between the lower Tapajós and Arapiuns rivers, some being distant from any perennial source of water (McCann 1994); however, Nimuendajú (1952, 11) reported prehistoric wells in this region.

Present-day *terra firme* Indian settlements seldom produce *terra preta*, undoubtedly because they are of short duration and are supported by shifting cultivation. Permanent villages would require some form of stable, sustainable agriculture. Exceptions might occur with some of the older mission villages, and these should be examined for black earth formation. Black earth is still being formed today in the backyards of Amazonian towns in Brazil visited by N. Smith (1980, 555–556). He notes that the rate of formation is probably slower now than in prehistory due to pig and chicken scavenging and the current practice of building fires on above-ground platforms.

The Terra Preta *of Araracuara*

Colombian scholars have studied the *terra preta* bluff soils at Araracuara on the Río Caquetá in the Colombian Amazon (Fig. 8.7) (Andrade 1986; Mora et al. 1991; Herrera et al. 1992; and Cavelier et al. 1990; also Eden et al. 1984). Analyses of soils, pollen, phytoliths, plant remains, and ceramics, plus radiocarbon dating, provide systematic evidence for the nature of prehistoric bluff settlement and land use.

Site 2 is on the 140 m high Araracuara sandstone plateau overlooking the Río Caquetá. The site was originally occupied about 2700 BC; anthropic soil is lacking. The second period of occupation was continuous from AD 385 to AD 1175, nearly 800 years, covering an area of 6 ha of brown anthropic soil. Open zones of savanna were created. The dominant crop was maize, with some manioc. Palms and other fruit trees included *Iriartea*, *Oenocarpus*, *Jessenia*, *Mauritia*, *Astrocaryum*, *Bactris*, *Attalea*, and *Lepidocaryum*, all important house-garden and swidden-fallow trees today. This suggests that agroforestry systems had been established. By AD 800 agriculture had become intensified (and was nearly continuous) with additions of chili peppers, caimito (*Pouteria cainito*), and varieties of manioc. The maximum percentages of palms coincide with maximums of cultivated crops. Algae and silt occur in the soil, possibly from swamps in the floodplain, suggesting the transport of alluvial silt and organic matter to the fields to improve fertility and to reduce erosion. Most of the surrounding forest was maintained even with a growing population.

By AD 1200, the settlement was abandoned, and the savannas disappeared (burning ceased?) and were replaced by forest. Cultivars decreased in variety, but some manioc remained, suggesting the continuation of small swidden plots. A variety of fruit trees persisted, such as avocado, star apple (*Chrysophyllum cainito*), guava (*Psidium guajava*), and peach palm (*Bactris gasipaes*), suggesting swidden-fallow management. The settlement itself may have been related to an existing village (Site 3) 3 km away, which expanded at about the same time. Abandonment, however, was probably not for ecological reasons, as the original site had been productive for hundreds of years.

The anthropic soils at Site 3 span from AD 0 to 1800. Initial fields were small with long fallows. After AD 800, the site experienced intensified agriculture (long cropping, short fallowing), assisted by soil additives including domestic waste, dead leaves, wood, and weeds, plus silt and algae. These additives occur in greater quantities than at Site 2. The site is larger than Site 2, with 14.5 ha of brown and black soil, extending for about 1 km. The black soil probably originated at house sites and the brown soil at intensive field and garden sites.

It took an estimated 245 tons/ha of alluvial silt annually to maintain soil fertility at Site 3 according to Herrera et al. (1992, 111). However, the present average depth of the silt layer is only 36 cm, much less than would be expected. Also, this material would have had to have been carried at least 900 m from the riverbank and up the bluff, a considerable labor requirement. Thus, a massive movement of silt can be questioned, and local organic inputs may have been more important.

The Araracuara Project seems to provide support for the bluff model presented here: lengthy permanent settlement and large populations; permanent agriculture involving artificial soil fertility maintenance, supplemented by forms of agroforestry; and interaction with an adjacent floodplain. That this land-use pattern was widespread in prehistoric Amazonia seems likely but remains to be demonstrated.

Agricultural Evidence

What was the primary habitat that sustained prehistoric riverine farmers? The archaeological and early ethnohistorical evidence is meager and inconclusive. Later ethnohistoric and recent ethnographic evidence is probably not indicative of prehistoric conditions. Thus, the model suggested here is largely inferential, but nevertheless feasible and probable.

Várzea *Cultivation*

Floodplain agriculture is described by Meggers (1971, 125–126), based on early accounts, primarily for the Omagua Indians. By 1700, most indigenous riverine farming had disappeared. One of the few surviving river tribes is the Shipibo on the Río Ucayali in Peru, whose subsistence at Panaillo is described by Bergman (1980).

The large floodplains are highly varied environments in terms of flooding regimes, soils, and microrelief (Denevan 1984). For the Amazon, *várzea* width ranges between about 10–50 km, generally being narrower below the Rio Negro. During low water, large amounts of terrain are exposed as *playas*, islands, and low levees for sufficient lengths of time to allow the cultivation today of fast-growing crops such as maize, peanuts, beans, and watermelon. The highest natural levees are above flood levels most years and can be planted with the longer maturing varieties of manioc, other annuals, plantains, and native fruit trees.[22] A gradient of plantings was thus possible in relation to water-level duration, as is illustrated for the Shipibo by Bergman (1980, 60). The margins of *várzea* lakes were also preferred sites for prehistoric settlement and cultivation because of rich, year-round fish resources (Lathrap et al. 1985, 42).

The floodplains have clear advantages for agriculture. Soils, while variable in texture and nutrients, are mostly of relatively high fertility, renewed by annual flood deposits, with adequate moisture availability which is capillary and not necessarily dependent on direct rainfall, although sandy soils may dry out (WinklerPrins 1999). Forest clearance, ordinarily difficult in upland forests using stone axes, often is not a problem here. The *playas* and low levees lack large trees and may be cleared of herbaceous vegetation by the annual floods and deep silt deposits. The high levees may be forested with early successional growth which is relatively easily cleared, but they may also have large, mature trees. The need for weeding varies with site. Cropping can be annual, although *playas* frequently shift in location. For the Río Pachitea in Peru, Campa Indian fields on levees are fallowed for only 1–3 years, and only because of weed invasion, not declining fertility (Allen and Tizón 1973, 145). However, at San Jorge near Iquitos, swiddens on high levees are cropped for 2–3 years followed by about 10 years of agroforest prior to reclearing (Hiraoka 1989, 92–93). High levee soils only infrequently receive flood silt deposits, and hence fertility may be less than for the low levees and *playas*, and this results in field fallowing according to Hiraoka (1985, 8, 15).

The disadvantage of floodplains resides in the irregularity and variability of flooding. On the tributaries, destructive floods can occur without warning, even during low-water seasons. The rise and fall of the main Amazon is more regular, but periodic extreme floods occur, filling the entire floodplain, topping the natural levees, and destroying most crops. On the Río Ucayali (Fig. 8.4), floods covering the highest levee for a month or so occur about every 10 years, and slightly lower levees may be topped twice every 10 years (Bergman 1980, 53; Chibnik 1994, 221). At Manacapuru, during the seven-year period of 1979–1985, the entire floodplain was flooded only in June 1982 (Sippel et al. 1994, 75). Hydrograph records for Manaus and Manacapuru from 1903 to 1985 show 17 discharges near the 1982 level or greater, for an average of about one year in five (Richey et al. 1989, 246). For the central Amazon, Meggers (1971, 12) points out that "[al] though the 1953 crest reached only ten ft. above average, it had a disastrous impact on crops and cattle." This was the highest river stage during the period 1903–1953

[22] Agroforestry systems with annuals and fruit trees on high ground within floodplains have been described in Peru by Padoch and de Jong (1987, 190–192), Coomes (2004), and Hiraoka (1989, 81–84). On floodplain forest management for varied products on islands in the lower Amazon, see Anderson et al. (1995).

(see Sternberg 1975, 22). Thus, floodplains are a relatively rich but very high-risk habitat.

As for crops, the accounts in the sixteenth and seventeenth centuries mention the importance of both maize and manioc in the floodplains. For the Omagua, additional crops included sweet potatoes, peanuts, beans, tobacco, achiote (*Bixa orellana*), cotton, gourds, peppers, pineapples, cacao, and avocado and other fruits (Meggers 1971, 125). Carvajal (1934a, 192, 200, 211) reported Orellana's men obtaining very large quantities of food in some villages. However, generally it is not clear as to whether the primary production was coming from *playas* and islands, levees, or *terra firme* or from all, which is likely, and which crops were grown where. There is historical and archaeological evidence that both maize (mud flats) and manioc (levees) were primary floodplain crops (Fritz [1723] 1922, 50; Acuña [1641] 1942, 35; Heriarte [1692] 1952, 17; also see Roosevelt 1980, 112–159).

Food Storage

Food was stored in order to feed people during the high-water and flood periods. The storage of manioc in pits in the floodplain, keeping for up to 2 years without rotting, is mentioned by Fritz (1922, 50) and Acuña (1942, 35–36). The Tapajó stored maize in baskets buried in ash for protection from weevils (Carvajal 1934b, 432). Maize was normally stored in rafters and in raised cribs (Carvajal 1934b, 398). Bitter manioc was made into cassava flour, or mixed maize-manioc flour (Carvajal 1934b, 398, 425), and also stored. Such *farinha* today keeps for long periods. In addition, fish were smoked and turtles were kept live in pens (Acuña 1942, 39). Orellana's men obtained 1000 turtles in a single village according to Carvajal (1934a, 193), and Ursúa's men reported a village with 4000 turtles (in *corrales*) caught in the dry season for eating in the wet season (Vásquez de Espinosa 1948, 385). The capacity for food storage alleviated the problem of flooding but may not have been sufficient to counter high floods of long duration or floods occurring several years in sequence. The loss of crops to flooding was mentioned by Fritz (1922, 50): "when the River is in high-flood, they are left without a *chagra* [field] and not seldom without anything to live upon." Likewise, Acuña (1942, 35) said that the Indians "are exposed to a great reduction and loss [of crops], because of the powerful floods." In addition, fish availability is greatly reduced in the river channels during high water.

Storage of food was mainly seasonal and at best provided for a year or so of nonproduction. More than food was involved, however. Seed and tuber cuttings must be preserved in adequate quantity for future plantings. Seed can be stored, if not eaten in emergencies, but I know of no long-term storage of manioc cuttings. If manioc plants were destroyed by flooding, new cuttings would have to have been obtained from *terra firme* fields.

Thus, non-floodplain (*terra firme*) sources of food would have been essential for the support of large numbers of people over the long run. Today, of course, floodplain farmers and fishers have access to market sources of food; however, in the Iquitos region, trading for staple food between floodplain and upland farmers is still their "safety net," not the market (Coomes, pers. comm. 1995) [while in Santarém, there are networks of circulation that connect floodplain to upland and urban areas (WinklerPrins 2002b; WinklerPrins and de Souza 2005)].

River Terrace Cultivation

The premise of "A Bluff Model" is that *várzea* agriculture, while very productive, is at risk because of periodic high floods that fill the floodplains. This is not entirely so, however. There are large river terraces within the valley, surfaces of Pleistocene and Holocene age that are higher than the highest levees and are not flooded. Ages range between about 5000 and 100,000 years. Dumont et al. (1990, 128, 131) map terraces in the Río Ucayali valley with dates from 8520 BP to 32,750 BP. Thus, river terrace soils are relatively young and less weathered than *terra firme* soils and have a relatively high nutrient content (Räsänen et al. 1993, 211). Other terrace soils, however, are poorer colluvial material washed down from uplands. Floodplain terraces cover 0.7% or about 4800 km^2 of the Peruvian Amazon, based on measurements on Landsat photography (Räsänen et al. 1993, 211). In the eastern Amazon, river-terrace formations have been explained by sea-level oscillations, whereas tectonic activity in the sub-Andean fault system is believed to be responsible in the Upper Amazon (Salo and Kalliola 1991, 248–249).

River terraces, in part at least, have a considerable potential for settlement and cultivation because they do not flood, but only if they are readily accessible. On the large rivers, they do not seem to be utilized much today, possibly because they are distant from the main channels, not easily reached by boat, and because they are fragmented. The same difficulties probably prevailed in prehistory. River terraces have seldom been examined by archaeologists, so their past agricultural importance remains unknown. Mora et al. (1991, 6) map terraces along the Río Caquetá in the Colombian Amazon and show several prehistoric sites on them. Where present and accessible, they may have provided a non-flooded alternative to bluff occupation.

Bluff Cultivation

The bluffs are part of the well-drained, low-fertility *terra firme* where prehistoric agriculture has been portrayed as long-fallow shifting cultivation comparable to that of surviving Indians today (Meggers 1971, 42, 99; Roosevelt 1980, 87). However, there are contemporary Amazonian examples of more permanent production, based on soil protection and maintenance, such as the short-fallow swiddens and house gardens of the Kayapó (Hecht and Posey 1989), Waika (Harris 1971), Siona (Siona-Secoya) (Vickers 1983, 37–38), and Amuesha (Salick 1989, 201–205). Agroforestry systems based on perennials have been examined for the Bora (Denevan and Padoch 1988) and Runa (Irvine 1989).

An important consideration for prehistoric *terra firme* agriculture in Amazonia is the use of stone axes to clear forest (Denevan 1992). Given the inefficiency of stone axes, compared to metal axes, enormous amounts of time and energy were required to clear mature forest, particularly hardwoods and trees of large diameter. Experiments indicate that steel axes are 7–60 times more efficient than are stone axes. Consequently, such clearing was probably minimized or avoided. Once a clearing was established, it was likely maintained semipermanently in production, possibly gradually enlarged, with only short periods of fallowing.[23] Small plots could be established at tree-fall openings.[24] Otherwise, fields would have been concentrated along the edges of rivers and streams where growth was largely secondary, dominated by small trees and softwoods that could be comparatively easily cleared with stone axes. Field sizes became larger with the use of metal axes (Denevan 1992a, 157, 162).

A second consideration in reconstructing prehistoric *terra firme* agriculture is that agroforestry systems were probably integrated with permanent gardens and swiddens. Such agroforestry would have included swidden-fallow management (enriched fallows, successional management) and fruit orchards (Denevan and Padoch 1988) and forms of forest manipulation (Posey 1985, 144–152). The use and cultivation of fruit trees is mentioned frequently in the early accounts, attesting to their importance.[25] Fruits were cultivated, managed in disturbed sites, and wild. Cultivated fields, in particular, suggest permanent fields and settlement, since perennials are less common in short-cropping, long-fallow systems where people move frequently. The high concentration of fruit trees at the Araracuara bluff site (see above) is indicative of agroforestry systems, such as orchards, house gardens, and managed fallows (Mora et al. 1991, 43). The location of fruit trees is generally not clear, but they were probably on both high levees and bluffs. Laureano de la Cruz (1885, 188) in 1651 reported that Omagua Indians "went into the interior forests [*monte adentro*] to search for the fruits of palms and other trees," possibly trees growing in swidden fallows, indicating forest utilization inland from the bluffs.

[23] Lathrap was convinced that "shifting slash-and-burn agriculture was a secondary, derived, and late phenomenon within the Amazon Basin" and that both floodplain and upland farmers had intensive garden systems involving "a bewildering arsenal of crops and cycles of rotation extending up to 25 or 50 years" (Lathrap et al. 1985, 54–55).

[24] Tree-fall gaps comprise about 1% of the forest area of Amazonia at any time, averaging several hundred square meters each (Ruokolainen and Tuomisto 1993, 141–142).

[25] Acuña (1942, 37); Carvajal (1934a, 210, 217; 1934b, 415, 426); Cruz (1885, 188); Heriarte (1952, 17); Ortiguera (1934, 317); Salinas (1965, 199–207); Vázquez (1981, 211); Vásquez de Espinoza (1948, 384); Zúñiga (1981, 15). In the Iquitos region today, there are at least 193 species of useful fruits, of which 74 are cultivated (domesticates and semi-domesticates); see Vazquez and Gentry (1989, 352–356).

Permanent and semipermanent production systems were probably characteristic of the bluff zones where there is archaeological and ethnohistoric evidence of relatively dense population concentrations. *Terra preta* soil is indirect evidence of intensive *terra firme* cultivation, in that permanent or semi-permanent settlement creating *terra preta* is usually associated with permanent or semi-permanent fields. The black soil itself was undoubtedly cultivated, as it is today. In addition, brown anthropic soils (*terra mulata*) adjacent to black soils probably were either agricultural soils enriched over time by organic additives (Sombroek 1966, 175; Andrade 1986, 54; Mora et al. 1991, 77) or possibly zones of sporadic settlement.

Whether agricultural pressure on bluff-zone vegetation was sufficient to create savannas and soil degradation is uncertain. Carvajal (1934a, 227) mentioned Tapajó "fortresses scattered along the tops of hills and for the most part stripped bare," but this was "two or three leagues back from the river." These could have been interior natural savannas, as occur today in the Santarém region. However, Carvajal (1934b, 435) at apparently the same site stated that "[a]ll along that side of the river … there were not only savannas, but also uplands and slopes and hills cleared of trees." Thus, intensive agriculture may have created anthropic savannas. This is also suggested for the Araracuara site in Colombia (Mora et al. 1991, 39).

Bluff Cultivation in Contemporary Peru

Analogs of prehistoric bluff agriculture and agroforestry exist today but have been little studied. One example is that of *ribereño* settlers along the Amazon in the Iquitos region (Fig. 8.7). The village of Tamshiyacu is on the east bluff of the floodplain, which is 15–25 km wide, adjacent to the main channel (Hiraoka 1986). This is Tertiary age upland (*altura*), 40 m above mean river level. A small sector of levees and sandbars is planted in rice, maize, beans, and vegetables at low water. Cultivation extends inland from the bluff edge for between one and five km, the greatest distance being from the village itself. The immediate bluff zone (1.0–1.5 km wide) is used for short-fallow swiddens. A middle zone (1.0–2.5 km wide) is used for agroforestry. The most interior zone (1.0–2.0 km wide) is used for scattered long-fallow shifting cultivation, beyond which is mature forest (Hiraoka 1986, 358). However, for villages along the nearby Río Tahuayo just south, Coomes (1992, 82–188) did not find this zonation, although the same types of fields occur. He describes a patchwork of these types with the mix related to the age of the settlement, the older villages having more managed fallow and more fields that are distant from the village (four km or more).

In the short-fallow fields at Tamshiyacu, one or two crops of manioc are obtained over two to three years, with yields of 9.0–9.5 metric tons/ha/year. No fertilizers are needed, even though the soils are very acidic and low in nutrients. Other crops do poorly, however. For manioc, several weedings are necessary, and this is the main problem rather than declining yields. The low-protein manioc staple is supplemented with fishing and hunting and floodplain seed crops. The agroforestry zone consists of a young managed fallow or transitional stage (two to six years), with pineapple dominating, plus fruit trees such as *Pourouma*, *Inga*, star apple, cashew, and peach palm. The older agroforest (over six years) is dominated by groves of umarí (*Poraqueiba sericea*) which last 20–30 years. Other older economic trees include *Mauritia flexuosa*, *Astrocaryum chambira*, Brazil nuts, peach palm, and avocado. These agroforestry systems are derived from Indian antecedents. The zone most distant from the bluff at Tamshiyacu consists of long-fallow shifting cultivation within primary forest. A year of maize, rice, and beans is followed by one to two years of manioc and plantains, followed by mostly natural growth for 20 years or more. Most families also have a home garden (1,000–2,000 m^2).

At Santa Rosa, another *ribereño* village on the lower Río Ucayali, farmers also cultivate both *várzea* and *terra firme* habitats (Padoch and de Jong 1992). The village was originally on a natural levee but was moved recently to a bluff site because of high floods. The floodplain *playas* and levees continue to be cultivated, including the old village-site levee, but most farmers also have swidden and agroforestry plots in the bluff zone. A variety of cropping combinations representing 12 different production systems are employed, but the most common component is swidden-fallow agroforestry. On the Río Tahuayo, most floodplain farmers have *terra firme* plots for security (flood insurance), whereas many *terra firme* farmers do not also have floodplain plots (Coomes 1992, 171). This is indicative of which habitat is most risky. On the other hand, floodplain farmers move to higher ground after flood “wipe outs” but still prefer to locate as close to the river as possible. The Shipibo at San Fernando de Yarinacocha and at Sarayaku plant crops on both floodplain and *terra firme* (Myers 1990, 33, 57). However, other Shipibo have all their fields in the floodplain (Bergman 1980, 90; Myers 1990, 33).

Elsewhere, Brazilian *caboclos* at Coari on the middle Amazon have some of their fields on the *várzea*, but most of their fields are on the upland as protection against loss of crops to high floods (Parker et al. 1983, 181–183). Tucuna Indians near Leticia, Colombia, live in the floodplain but may have fields on the *terra firme* as “an insurance” garden when major floods are expected, and people on the *terra firme* have fields both there and in the floodplain (Bolian 1975, 20).

Projecting to Prehistory

The long-fallow zone at Tamshiyacu probably did not exist when stone axes were used. The short-fallow zone adjacent to the bluff edges probably consisted of semi-permanent fields, orchards, house gardens, and young agroforest. This zone of intensive cultivation initially was probably not very wide, given the difficulty of clearing primary forest, except along tributary streams. However, as the population grew, this strip could have been expanded as mature trees at the edge were gradually fallen. The swidden-agroforest zone may have been as much a manipulated forest as a managed agroforest, with, however, small gardens located at tree falls, their density decreasing with distance from rivers and streams. Where shifting cultivation (long or short fallow) was practiced, the fallow vegetation was probably managed to maintain a high proportion of useful plants, as is done today by the Bora and other Amazon people (Denevan and Padoch 1988). These fallows would have been recleared while young, and tree falling was still feasible with stone axes.

Discussion

*Bluff/*Várzea *Complementarity*

I have examined historical, archaeological, and agricultural evidence to support the thesis that prehistoric riverine settlement in Amazonia was primarily located on fringing bluffs rather than in the floodplains, based on a dual strategy or complementarity of *várzea* and *terra firme* resource use. Bluff locations are less productive than the floodplain but are more reliable, and they are also more defensible. The critical archaeological evidence is *terra preta*, anthropic black soil created by prehistoric settlement mainly on the bluffs and often of considerable extent and depth. The evidence for intensive bluff agriculture is mainly inferential. Large, permanent villages require productive, stable agriculture. There is brown anthropic soil probably resulting from intensive cultivation around some village sites. Integrated bluff/floodplain production systems today are indicative of what was possible (WinklerPrins 2002a). Bluff/floodplain interaction is indicated by sixteenth-century locational information and by reports of roads leading from bluff edges into the interior, of landing places "down" on the river, of fruit trees being harvested in the interior, and of fish being traded inland. Also, there are many examples today of integrated bluff/floodplain production systems.

We do not know the nature of prehistoric socio-economic complementarity between bluff and floodplain, nor do we know much more about non-market complementarity today. People may have had fields in both habitats, including satellite villages in one or the other. There may well have been a form of transhumance or shifting of residence from temporary huts on the *playas* to the bluffs during normal high water. People living in more permanent villages on high natural levees may have had recourse to living temporarily with kin on the bluffs during exceptional floods.[26] There probably also was a seasonal shift in emphasis from *várzea* fishing during low water to bluff-zone hunting during high water, when fish availability declines. There was probably an exchange of upland manioc for floodplain fish and of upland fruit, fish poison, drugs, and medicinals for floodplain products. This exchange could have been by trade, via kinship, by control of multiple resource habitats, or all of these. In addition, there must have been exchange up and down river and from bluff edge to the interior, often involving considerable distance and time. In any event, it is not likely that a floodplain community could manage over

[26] Myers (1990, 34) suggests that multiple habitat access and use may have been associated with economic security based on extended families rather than on the immediate family or village.

long periods without some means of access to upland resources. Hence, most floodplain villages were likely located where they had access by water to the base of bluffs, a clustering or patch pattern paralleling that of bluff villages.

Cultivation in the *várzea* consisted of patches of plots in different microhabitats varying in length and duration of flooding and with soil fertility and texture, with a zonation by elevation along the sides of levees and islands. Bluff fields were a mosaic of semi-permanent plots amidst managed forest, combined with a zonation based on distance from riverbank. Villagers relied on both bluff and *várzea* for both crops and wildlife. Subsistence strategy varied depending on ecology, season, demography, and distance. Bluff cultivation and forest resources provided a safety valve when *várzea* fields were destroyed by high floods. These patterns can be observed along the Amazon today. *Várzea* and bluff-edge vegetation was in large part manageable with stone axes. Primary forests beyond the bluff edges were gradually modified and managed for several kilometers. Trekking to the interior for plant and animal resource exploitation and for trade would have contributed to this diversified subsistence strategy. Such an integrated subsistence system could conceivably have supported permanent settlements of several thousand people or more.

Decline of Bluff Settlement and Cultivation

By the mid-seventeenth century, there were reports of villages on islands and *playas*. Acuña (1942, 35) in 1641 indicated that in Omagua territory, the river was full of islands, all of which were inhabited or cultivated, but that the Indians "are exposed to great loss, on account of the powerful floods." Laureano de la Cruz (1885, 86–88) in 1647 reported a large village on the Island of Piramota below the mouth of the Napo. Fritz (1922, 50) in 1689–1691 said that Omagua houses and fields "are generally situated on islands, beaches or banks of the River; all low-lying lands liable to be flooded," rather than on high land, even though their crops might be destroyed; this was because of custom or fear of forest Indians. Heriarte (1952, 16), for the Tapajó region in 1692, believed that the riverbanks (bluffs?) were not inhabited because the Indians had fled to escape the Portuguese. Myers (1992b, 134) and Bolian (1975, 15–16) suggest that by the seventeenth century much of Omagua bluff settlement had shifted to islands for defensive reasons. Thus, bluff control by *várzea* Indians seems to have broken down early in some areas, probably because bluff settlement was unsafe given increased European river activity.

In 1616 a Jesuit mission was founded at Belém near the mouth of the Amazon, and others were soon established along the river. Hemming mapped 87 missions in 1759 along the central and lower Amazon (Fig. 8.8). Preston James in his text *Latin America* noted the bluff location of these missions: "upstream, wherever the river in its meandering course swept against the base of the valley bluff and so provided high ground next to the navigable river channel, mission stations were built" (James 1969, 840). Other missions, such as Sarayacu (Franciscan) on the Ucayali, were located

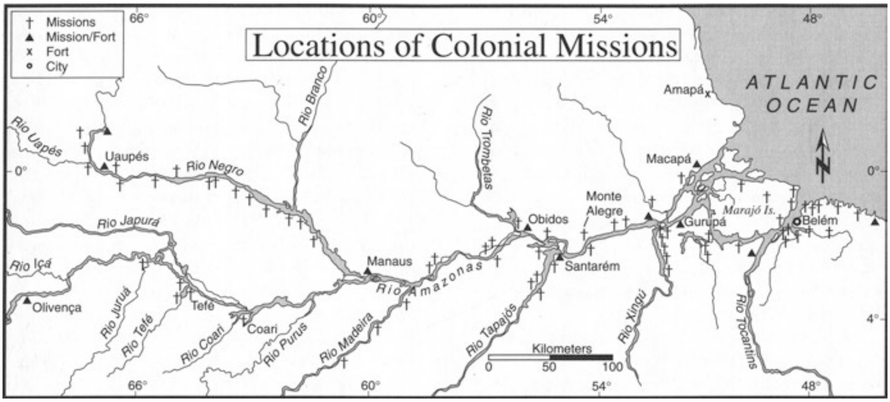

Fig. 8.8 Central and lower Amazon showing locations of 75 missions and mission/forts in 1759. All but four or five appear to be on bluffs. Source: Adapted from Hemming (1978, xxii–xxiii)

within but at the edge of the floodplain and crops were planted both in the floodplain and on the adjacent *terra firme* (Myers 1990, 57). While many of the missions were abandoned as their Indian populations declined, others became river ports and rubber collection centers—the antecedents of present river towns and cities.

By 1850, there were very few riverine Indians surviving along the Amazon and its main lower tributaries as a result of disease, slaving, fleeing to the interior forests, and detribalization. Depopulation and tribal extinction were proportionally much greater than in the interfluves and along lesser tributaries. The rubber boom brought new people into riverine Amazonia, and new bluff settlements were established, but forest extraction was more important than agriculture. With the decline of rubber and other extractive products, subsistence agriculture was slowly reestablished by the remaining people, detribalized Indians from the interior and settlers, all now referred to as *ribereños* in the Upper Amazon and as *ribeirinhos* or *caboclos* in Brazil (Parker 1985). They utilize both *várzeas* and bluffs, and unless markets are close by (a day or so by water), they are still dependent on bluff/*várzea* complementarity. Most of the larger, permanent settlements continue to be on bluffs (Fig. 8.5). "There are no cities on the Amazon floodplain proper" (Goulding et al. 1996, 14).

Demographic Implications

If the model of bluff settlement presented here is basically correct, what are the consequences for estimating late prehistoric riverine populations in Amazonia? In three earlier attempts to estimate Amazon populations near the time of initial European contact, I obtained samples of known densities for small areas of floodplain (5.3, 14.6, and 13.9/km^2) and projected these to all floodplains to get estimated total populations (Denevan 1970, 67; 1976, 218; 1992a, xxvi). The same was done for other major habitat types to obtain total populations for Greater Amazonia (Table 8.1). For the floodplains, I now reject this entire methodology, given the argument here that settlement was not evenly dispersed *in* the floodplains but rather was mainly concentrated in clusters *on* bluff segments that impinged against active channels of the Amazon and its major tributaries. We do not know how many such villages there were or how large they were, except for a few *terra preta* sites that have been measured. Hence, it is impossible to estimate prehistoric riverine population densities or total populations.

As an exercise, however, I used available population densities and sustaining areas for contemporary Indian and peasant villages to obtain a potential aboriginal population (Denevan 2003). Sustaining areas (agriculture, plant foraging, hunting, fishing) average a radius of ten km for hunting and five km for fishing (usually much less for agriculture). For a bluff village, the average sustaining area would be 196 km^2. For the Amazon bluff village of Tamshiyacu in Peru with a population of 2040 (Hiraoka 1986, 357), the population density for 196 km^2 would be 10.4/km^2. Assuming that an estimated 20% of the bluff terrain was on navigable channels (based on an examination of fluvial maps), a 15-km-wide sustaining area extending ten km inland from bluff edges and five km into the *várzeas*, each side of the river, and a density of 10 persons/km^2, the total riverine population for 20% of the Amazon River and 21 major tributaries (estimated sectors with floodplains) in Greater Amazonia would be 1,464,000. The rest of the riverine zone was not devoid of people, although settlement was mostly sparse and unstable. Average density is unknowable, but if Meggers' overall Amazon density of only 0.3/km^2 is used for the remaining 80% of the riverine sustaining area, this would add 176,000 people for a riverine total of 1,640,000.

Table 8.1 provides previous estimates of habitat densities, overall densities, and total populations for Greater Amazonia and for the Amazon Basin by myself and others. Previous riverine density estimates range from 0.3 to 14.6 persons/km^2 but are for floodplains only and do not allow for deep hunting and agroforestry sustaining areas on *terra firme*.

Using a riverine total of 1,640,000, adjusting floodplain and riverine sizes, and retaining previous habitat estimates yield a new estimate for Greater Amazonia population of 5,487,000, a reduction of 177,000 from my 1992 estimate (Table 8.1), a relatively minor amount. Although the assumptions are tenuous and the densities of 10 and 0.3 persons/km^2 are feasible but conservative, working numbers at best, the new perspective based on the bluff model raises new questions on Amazonian demography and settlement.

As I have indicated, I now have serious reservations about all earlier estimates based on habitat densities. Thus, I am reluctant to suggest a new total population. However, based on 20 years of considering the question of Amazon Indian numbers as of 1492, new historical and archaeological research, and Newson's (1996; see Table 8.1) recent estimates resulting from a careful examination of ethnohistorical information from the Ecuadorian Amazon, l believe that reasonable ranges of estimates would fall between 5.5 and 7.5 million for Greater Amazon and between 3.5 and 5.0 million for the Amazon Basin, numbers well above those of Steward, Hemming, and Meggers (Table 8.1) [More recently, I estimated eight to ten million for Greater Amazonia (Denevan 2014, 211)].

Conclusion

The concept that prehistoric riverine food production involved the integration of bluff and floodplain cultivation has been presented previously by several scholars, mainly inferred from the same forms of evidence examined here. One of the first was the archaeologist Peter Paul Hilbert (1957, 2–3). According to Lathrap (1970, 44):

> The tropical forest farmer living on the bluff of old alluvium adjacent to the active flood plain could simultaneously farm the limited but excellent recent alluvial soils in an intensive and continuous manner and the poor but essentially unlimited soils of the old alluvial deposits using slash-and-burn agriculture.

Meggers (1991, 199) comments that most known archaeological sites are "riverine in location, but the sustaining area is primarily *terra firme*" (also see Meggers 1984, 632; 1993–1995, 106). Myers (1990, 30) points out for the Shipibo that "a village accessible to both kinds of terrain would be most advantageously situated… Furthermore, a village with gardens on interfluvial land would be assured of produce throughout the flood season." And Eden (1990, 79) states that: "adjacent tracts of the *terra firme* were also used for cultivation, providing some insurance against the flood risk of the *várzea*." Carneiro (1995, 57–58) emphasizes that "*Várzea* could never have been the only land reserved for growing crops. Some reliance must also have been placed on *terra firme*… In years of excessive flooding, then, *terra firme* would have served as a kind of 'crop insurance' … unquestionably this [high bluff] must have been a choice site for an Indian village." Finally, geographer N. Smith (1995, 228) states that "[m]any villages were established along the upland bluff overlooking floodplains so that the inhabitants could take advantage of animal and plant resources from both *várzea* and upland environments" (also see Goulding et al. 1996, 24). What we do not know is the degree to which individual households utilized both habitats with multiple dwellings or relied upon specialization and exchange.

Thus, prehistoric bluff/*várzea* complementarity for riverine Amazonia was suggested earlier, but the implications were not adequately acknowledged in archaeological research on settlement patterns, demography, and subsistence. Rather than rationalizing that most riverine sites were floodplain and thus destroyed, archaeologists should focus on bluff sites, which were primary and which have survived to a considerable extent. The evidence is good that large settlements existed on the bluffs, contrary to Meggers' (1992, 203) assertion that "[T]he conclusion that early eyewitness accounts exaggerate the indigenous population density seems inescapable."[27] However, the distribution of large bluff villages was sporadic rather than continuous, which has not been pointed out previously. They were separated by smaller, unstable

[27] This is a reversal of Meggers' earlier position that: "in spite of their deficiencies, however, the early chronicles make it clear that population density and level of cultural development were considerably greater on the *várzea* than on the *terra firme* at the time of European contact" (Meggers 1971, 122).

settlements in the floodplains and by sparse settlement in the bluff zones isolated from river channels. Accordingly, population densities alternated between relatively dense (10/km^2 or more) and sparse (0.3/km^2 or less) as compared to Meggers' overall riverine density of 0.3. Permanent bluff settlement was made possible by the integration of house gardens, intensive fields, fruit orchards, and managed secondary forest in combination with seasonal cultivation of floodplain soils. Variability and flexibility prevailed. There is evidence to support this patchy bluff/*várzea* complementarity model, but further research is needed. The model also may be relevant to other prehistoric bluff/floodplain contexts, such as the Mississippi River where there are numerous settlement sites on both levees within floodplains and on adjacent upland.[28]

Amazonianist ethnohistorian Antonio Porro (1994, 86, 91) has "no doubt about the demographic density of the floodplain as a whole and the great size of many villages"; and he believes the early "sources suggest that the [fluvial] provinces were territorially defined and socially stratified," with "centralized political power" (i.e., chiefdoms). The bluff model presented here should help substantiate this riverine argument made by Porro, as well as earlier by Lathrap, Carneiro, Myers, Roosevelt, and others. The bluff model, in part at least, explains concentrated riverine settlement; otherwise, populations likely would have been thinly dispersed over thousands of kilometers of floodplain.

Finally, the bluff model undermines the conventional dichotomy between *várzea* and *terra firme* settlement and agriculture based on "soil constraints" expressed by many Amazon scholars. If bluff soils could be made to help support relatively large numbers of people, so could other *terra firme* soils. lnterfluve *terra preta* sites confirm this. That such sites are sporadic was the result of other factors, including the use of inefficient stone axes, the limited availability of animal protein, lack of demographic pressure, and village fissioning.

The bluff model, if valid, has significant implications for contemporary Amazonian development. Uniform settlement and cultivation of the major riverine zones is unlikely, given that the floodplains are subject to periodic destructive floods (today farmers may be protected by credit or insurance), given the limited extent of un-flooded bluffs that are adjacent to active year-round navigable river channels, and given distances to markets.[29] The patch pattern that existed prehistorically exists today and is likely to persist in the future. Second, *terra firme* soils can be cultivated permanently by traditional methods. To be sure, high labor costs, polycultural cropping, a diversity of food production systems, and forest management and extraction, while appropriate for subsistence, may not be feasible for a strongly market-oriented economy. On the other hand, a complementary bluff/floodplain mosaic system of land use can maintain some form of forest, ensure reliable security, and support a modest population density, such as apparently existed in the indigenous past.

[28] For the American Bottom region of the Mississippi, Woods and Holley (1991, 50) indicate that "[I]t is very likely that [prehistoric] upland settlements were exploiting both upland and bottomland environments."

[29] For a discussion of the general problems of agricultural development of the Amazon floodplain in Peru, see Chibnik (1994, 221–223). Goulding et al. (1996, 165, 166) indicate that the governments of Amazonian countries are beginning to shift development efforts from the uplands to the floodplains, where serious environmental problems are now occurring as a result.

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9.0 By Way of Background: A Biographical Sketch of William M. Denevan

Introducer: Kent Mathewson

Abstract This biographical sketch of the life and career of William M. Denevan traces his family history and his formative years and schooling in Southern California, his move to the University of California, Berkeley to complete his undergraduate training in geography, and his time spent in graduate school, punctuated by several exits for travel to South America and to Washington, D.C. for employment, only to return to complete and produce a highly regarded doctoral dissertation on ancient raised field complexes in eastern Bolivia. It follows him after graduate school to his first and only academic appointment – in the Geography Department at the University of Wisconsin, Madison, where he taught for 30 years before retiring in 1994. During these three decades, Denevan became recognized as a major figure in a several of research arenas, including New World historical demography, the landscape archaeology of pre-European forms of intensive agriculture, and New World tropical cultural and historical ecology. He also oversaw several dozen dissertations and master's theses, all but a few on related Latin American topics. His publication record was equally robust, authoring a number of books and edited collections, as well as dozens of articles and book chapters. Highlights of his retirement years witness a continued commitment to research and publication, proceeding at a pace equal to, if not surpassing his university years.

Keywords William M. Denevan · Biographical sketch · Berkeley geography University of Wisconsin · Madison

William Maxfield Denevan, like many geographers' coming of age in the early to mid-twentieth century, exhibited a strong interest in faraway places and high adventure from childhood. Unlike most of these erstwhile would-be adventurers, exploring the world through their stamp collections, cartophilia, and recreational reading habits (the stock adventure classics – Defoe, Twain, Kipling, London, and Halliburton to name a few), to his credit Denevan did not sublimate these youthful impulses once he

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started formal geographic study at the college level. If anything, he intensified them, in part propelling him through his highly productive career and laying a number of milestones and making major contributions along the way. In this biographical appreciation of Bill Denevan's life as a geographer, I focus on his contributions to geography and the related fields that he has so effectively tilled and tended (and so well illustrated by his publications in this volume, along with the accompanying commentaries). But I also recount elements and episodes from his own story, the context from which the more well-known and recognized results have been registered. I thank Bill himself for preparing and sharing a very detailed (we would expect less?) autobiographical essay, which digs deeply into his own genealogical history and then brings it up to the present. I've relied heavily on his essay in putting the biographical pieces in place and commenting on the contexts. Would that every scholar of Denevan's stature and accomplishments provide this kind of text and testimony.

Bill Denevan was born October 16, 1931, in San Diego, California, the oldest of four boys. His was a standard Depression and war years' childhood, but somewhat mitigated by the relatively benign Southern California milieu. In 1932 the family moved to Long Beach, where Bill grew up in a modest neighborhood, attending school and junior college. Bill's father Lester Denevan was born in Palouse, Washington, in 1903. He grew up in Spokane and counted Bing Crosby and Orvil Anderson (famed balloonist and WWII general) among his best childhood friends. Lester Denevan was a lifelong small "de" democrat, who dropped out of Gonzaga Jesuit High during WWI to work "in the woods." He witnessed the famous "Centralia Incident," a pitched armed battle between IWW militants and an American Legion mob under the pay of lumber barons. After marriage and moving to Southern California, he worked as an insurance salesman for various companies. Lester's parents (English, French, and Irish ancestry) were originally from Quebec and Iowa. Both families homesteaded on the prairie frontier near De Smet in the Dakota Territory in the 1880s. They were acquainted with Laura Ingalls Wilder's family, and the life they lived was well captured in Wilder's books. The spelling of the surname Denevan seems to have transmuted several times over two centuries: Dennovan (in France), DenEven (in Quebec), and later it was "Anglicized" to Denevan. Bill's mother, Wilda Alicia Maxfield, was born in 1909 in a log cabin with a sod roof on a ranch in the Colorado Rockies. Her parents and grandparents were originally from Maine, but came west for gold and silver mining. William Alfred Maxfield, Sr. (Bill's maternal great grandfather) was a mining engineer "who made and lost a fortune." When Wilda was six, the family moved to Victor, Colorado, a gold mining town that was in full throttle during the time they lived there – "numerous saloons, whorehouses, murders and suicides, bloody strikes at the mines, martial law" (Denevan nd). Wilda's maternal grandfather, William Gardner Snow, was a sailing captain who carried supplies to Union troops during the Civil War. Her father, Bill's grandfather and namesake, William Alfred Maxfield, Jr., was shot and killed during a train station robbery in Pando, Colorado, in 1912. When she was 14, the family moved to Grass Valley in the northern California Sierra Nevada, a much quieter mining area. Both of Denevan's parents instilled habits of hard work, self-reliance, the value of education, and a quiet skepticism of "the way things are." Sometimes

Lester Denevan, enjoying his daily spirits, would rail against “certain politicians, religious extremists, the John Birch Society, and others” (Denevan n.d.).

Bill’s boyhood in Long Beach during the late Depression years, the war years, and after were spent as much outside as inside, whether home or school. Inside he enjoyed reading, especially books about travel and exploration, especially Africa (Burton, Speke, Stanley, Livingstone, and Osa Johnson), and adventure stories – Kipling, Jules Verne, Jack London, Edgar Rice Burroughs. Stamp collecting was another favorite pastime – particularly the British colonial lands. Outdoors, beaches were close at hand, and he liked to go ice skating with his father on weekends or hang out at the amusement park. Once he discovered tennis, it became his main pastime. He excelled and helped his teams win championships. He made the varsity squad, lowest member, at Berkeley. They were voted the national NCAA champions his senior year. He continued to play into his 80s, winning numerous local and regional age-division tournaments. As he is quick to point out, other devoted tennis-playing geographers have included Carl Sauer, Ellen Churchill Semple, Leslie Hughes, George Lovell, and Bill Davidson. In high school he also began his Spanish classes that proved to be a key asset in his graduate studies and throughout his career. As was the custom and requirement in US doctoral education until the late 1960s, he had to pass French and German reading tests. His French proved passable (with Clarence Glacken administering the exam) but with German under John Kesseli’s watch, a native Swiss German and French speaker, he was not so lucky. He failed Kesseli’s German tests “over and over” but finally managed to pass it under Glacken when Kesseli was out sick for a spell.

In his senior year of high school, he began considering college and where to go. On the strength of his tennis, he was offered a partial scholarship at Redlands University. He interviewed, but realized that without a full scholarship, it was out of reach. Instead, he enrolled in Long Beach City College in the fall of 1949. There he thrived, a standout in tennis and his course work, especially history, anthropology, and geography. His favorite professor was Dr. Adolf Stone, a German Jewish geographer who escaped Germany in the 1930s. Stone had been a student of Karl Haushofer’s at the University of Munich and after the war helped interrogate his old professor at the Nuremberg trials. Graduating from Long Beach City College with an associate’s degree in 1951, he entered Berkeley that fall. Before leaving for Berkeley, Denevan met with Professor Stone, who advised him to take courses with Carl Sauer. Stone’s recommendation was followed, and Sauer’s lectures convinced Denevan that geography was the logical major. He also took courses from Clarence Glacken, John Leighly, and Erhard Rostlund in geography, John Rowe and Robert Lowie in anthropology, and George Stewart in English. Along with geography, Denevan was a committed sports fan, never missing a home football or basketball game. And, of course, he played tennis on the championship Berkeley team. He graduated in June 1953, with a major in geography, an “unofficial” minor in anthropology, and a varsity (“Big C”) letter in tennis.

After graduating from Long Beach City College, his father had advised him to join the Naval Reserves, thus avoiding a possible Army draft during the Korean War. Entering Berkeley he received a deferment, but upon graduating he was committed to do his 2-year service in the Navy. He went from boot camp in San Diego, to naval air ground crew training in Norman, Oklahoma, and then on to weather (aerology) school in Lakehurst, New Jersey, given his degree in geography. There he was trained in weather dynamics, mapping weather data and making and recording weather observations. He graduated first in his class of 30, which gave him a choice of 30 naval air stations in the USA, Cuba (Guantanamo), and in the Mediterranean aboard the aircraft carrier USS Lake Champlain. He chose Miramar Naval Air Station near San Diego, to be close to, as it turned out, an ephemeral girl friend.

Discharged in June 1955, Denevan took a summer field course in geography at UCLA. The course convinced him to go on to graduate school in geography, but at Berkeley, not UCLA. His initial year in the master's program did not go well. Unlike his undergraduate experience, he didn't excel, and mostly scraped by. He decided to drop out and do his geography directly – hit the road and travel. He found out that he could work on a Norwegian freighter circumnavigating South America and see the ports on both coasts from Los Angeles to Punta Arenas and back again through the Panama Canal. He jumped at the chance, but made it only to Lima, Peru, where he jumped ship. Once in Lima, he found work writing for the *Peruvian Times*, an English language newspaper. For the next 5 months, he learned how to write feature stories, as well as simple news items. His job as a roving reporter took him to many places in Peru, Bolivia, and Brazil, including Amazonia.

While still at Berkeley, Bill had applied for a Fulbright Fellowship to do master's fieldwork in the Atacama Desert. He had assumed it was a dead letter, but he received a quite alive letter in Lima offering him a Fulbright, not for Chile, but for Nicaragua. He immediately accepted and turned his sights on Nicaragua. Covering the country on trucks, buses, horses, mules, boats, rafts, and on foot between January and August 1957, he hit on a study of Nicaragua's upland pine forests for his master's research. He also spent a couple months working for the Inter-American Geodetic Survey indicating place names on air photos in the highlands, giving him valuable knowledge of vegetation distributions. Having completed master's fieldwork in advance of his return to graduate school, he was in a much better shape to resume his studies.

Earlier Bill had one of those amazing coincidences that travel sometimes precipitates. Before travelling up the length of the Amazon reporting for the *Peruvian Times*, he ran into his future master's and Ph.D. advisor, James J. Parsons, and LSU geographer Richard J. Russell in an outdoor restaurant-bar in Belém, Brazil. Denevan recognized Parsons, and over beers, Parsons (who had been on leave during Denevan's lackluster first year of graduate school) persuaded Denevan to give it another try. Thus, he was doubly primed to take another run at it. Parsons was all for the pine study, having done a pine study himself a few years earlier on Nicaragua's Miskito Coast. Parsons chaired the committee, with botanist/pine specialist Nicholas Mirov and forester Edward Stone. The thesis argued that the pine formations were the result of aboriginal burning and not strictly an adaptation to physical conditions, such as edaphic or climatic factors. Besides successfully defending his thesis (*The Upland Pine Forests of Nicaragua*) and graduating in 1958, he met Patricia Sue French, senior English major from Mill Valley. He had been working as a "hasher" (meal server and dishwasher) at her residence hall. They were married on the solstice (June 21, 1958) following graduation.

Having been awarded his master's thesis, getting married, and not getting any special encouragement from the Berkeley geography faculty to continue on for the Ph.D., Denevan began to look around for a job. He found work with the National Intelligence Survey (NIS) that produced basic reports for the State Department, embassies, the military, and the intelligence agencies. Denevan's office was charged with editing the physical geography compendia on a wide variety of topics from foreign countries, including maps on climate, terrain, soils, vegetation, and coastal waters. The office head was C.S.F. Sharpe, a geomorphologist who had worked with Carl Sauer on a survey for the Soil Conservation Service during the 1930s. Denevan surmises that this connection my have helped him in getting the job. There were downtimes, and during those slack periods, Denevan was able to read the office's back issues of the *Annals of the Association of American Geographers* and *Geographical Review,* "on government time."

By spring Bill realized that maybe a Ph.D. program would be in his future. He attended the AAG meeting in Pittsburgh and ran into James Parsons. Parsons was very encouraging, saying that they felt his thesis actually was excellent, and they wanted to publish it in the department's monograph series, *University of California Publications in Geography*. On the strength of this, Parsons also said that they could offer him an assistantship if he returned. The choice was not hard. Wife Susie was now pregnant with their son Curt, and California was beckoning. Denevan jumped into his new role as ex-intelligence analyst and future dissertator. In 1958–1959 he was teaching assistant for John Leighly's cartography class and John Kesseli's California regional course. He took seminars with Sauer and Parsons and wrote papers for Sauer on the origins of human-horse relations and the domestication of the peanut. That summer, 1960, he spent teaching geography at the University of Manitoba. This was his first teaching experience, and Denevan, who admits to being not the most extroverted of individuals, found it "very traumatic," but ultimately rewarding. Thus, another milestone passed, he returned to Berkeley with increasing confidence.

That fall he audited a class on South America given by Hubert Wilhelmy, a visiting professor from Tübingen University. In one lecture Wilhelmy talked about the little-known Llanos de Mojos region of eastern Bolivia. There were rectangular and linear miles of ancient earthworks amid the seasonally flooded savannas. Wilhelmy said that the famous geographer Carl Troll had told him that the French anthropologist Alfred Métraux had learned of these strange constructions from reports written by the Swedish ethnologist Erland Nordenskiöld who had actually seen them around 1910 (Denevan 2009; Nordenskiöld 2009). Denevan remembered seeing configurations in the landscape while flying over the region a few years before, on assignment for the *Peruvian Times*. He had initially considered a study of the Mojos' present-day inhabitants and their adaptation to the seasonal round of flooding and desiccation for his dissertation research. But in the field he turned his head toward the Mojos' pre-European inhabitants and their adaptions to this challenging environment. Once on this trail, Denevan followed all the leads that he could turn up. Oil geologists, both at Berkeley and later in Bolivia, helped by supplying aerial photos of the region. The more he looked, the more he found. He applied for and was awarded a National Research Council dissertation fellowship that allowed Bill, Susie, and Curt to set up shop in Cochabamba, Bolivia, for the year 1961–1962. From there Denevan took passenger prop planes to Trinidad, the regional capital of the Beni Department. Aerial photos, augmented by extensive survey of the region on foot, by horseback, canoe, oxcart, truck, and bush plane, established that there was a vast array of pre-European constructed features, primarily linear raised fields for cultivation, causeways for foot traffic during the flood season, canals, and mounds for habitation. One of the high points of the year was receiving a letter from Carl Sauer congratulating him on "finding pay dirt" and pursuing "a major problem."

In August 1962, after the year of fieldwork with much accomplished, Bill, Susie, and Curt returned home on an Italian freighter. One of Bill's last days in the field was spent riding in an oxcart and running out of potable water. Drinking out of a ditch, he contracted hepatitis, which he came down with on the return voyage. It took several months to fully recover, with slow to no dissertation progress. Also needing income, he was offered a 2-month job with the International Association for Economic and Social Development (Rockefeller Foundation) with a team of geographers, soil scientists, and agronomists surveying the agricultural potential of the scrub savanna landscapes of the Planalto Central of Brazil. The team concluded that the region had considerable potential if fertilized with crushed limestone. The report was ignored for several decades, but then Brazil's soybean revolution proved them right. Back in Berkeley in January 1963 (two of the previous years in Latin America and one in Washington, DC), recharged and ready to write, he pushed through and filed the dissertation in August of that year. Parsons chaired the committee, with Carl Sauer and archaeologist John Rowe the other committee members. This time the Berkeley faculty were more demonstrative in letting Denevan know that he had done an excellent job with his dissertation work, and the dissertation was selected for publication (1966a) in the University of California Press *Ibero-Americana* series.

Denevan's time in and out of graduate school spanned less than a decade – 1955–1963, which was about average for completing graduate programs in geography at that time. He was 31 years old when he received his Ph.D. degree in August, 1963. He immediately began looking for a teaching position. He visited UCLA, but with two Berkeley Ph.D's already on the faculty, there was little interest in hiring another at that point. He turned to San Fernando Valley State College (now Northridge), and they were more receptive. He received an offer and accepted, but then he received a phone call from Andrew Clark at the University of Wisconsin-Madison, inviting him for an interview. At the time, the Madison Geography Department was the National Research Council's top-ranked department. Presumably Parsons (who had been in the 1938 entering Ph.D. class at Berkeley with Clark, along with several of the other "Berkeley All Stars" including Robert West, Dan Stanislawski, and George Carter) or perhaps Sauer himself (Clark's Ph.D. advisor) had made a call to Clark. Phone call arranged hirings were, if not quite the norm, not unusual in those days. Denevan was spared giving a job talk and landed the job. In some ways his first year teaching at Madison was not unlike his first year in graduate school. He survived it, but was less than satisfied with his performance or what the departmental powers relegated and delegated to the junior faculty. He was given the conservation course to teach, and this proved something of an ordeal, requiring lots of late nights preparing lectures. For his second assignment, he was actually posted as Karl Butzer's teaching assistant for the physical geography class! Denevan was expected to take that on as one of his "bread and butter" courses, but apparently the quadrumvirate that ran the department (Andrew Clark, Richard Hartshorne, Arthur Robinson, and Glenn Trewartha) felt he needed extra preparation before being trusted as instructor of record in this class. Something similar had happened to Butzer when he was hired in 1959. Initially he was not allowed to conduct the introductory physical geography class.

After this shaky first year, Denevan applied for a post-doctoral fellowship from the Ford Foundation and was awarded one to do research in Peru for 2 years starting in January 1965. The family (now with the addition of daughter Victoria, born in 1963 in Madison) moved to an apartment in Lima's Miraflores district. Susie taught at the American School, as she had done earlier in Cochabamba. From Lima Bill ranged widely, both in the Andes and the Amazonian lowlands. In the high Andes he studied the raised fields he helped locate around Lake Titicaca, and in eastern slopes of the Andes, he studied house gardens in Moyobamba. In the Oriente he studied Campa Indian subsistence and the ecology of the Gran Pajonal and Río Heath savannas. He took advantage of hopping rides on bush planes with various missionary groups, particularly the Summer Institute of Linguistics. While in Lima, he ran into Woodrow Borah, Berkeley historical demographer. Borah invited him to attend the International Congress of Americanists in Mar del Plata, Argentina. Denevan presented a paper on the 1492 population of western Amazonia. This effectively launched his career-long interest and expertise in the aboriginal population of the Americas. This was also the precursor to his authoritative edited book, *The Native Population of the Americas in 1492* (1976, 1992).

While his time in Peru allowed him to not only help lay the foundations for his future status as one of geography's foremost Andeanist and Amazonianist scholars and regain his footing (he was much more sure-footed in the field than early on in the classroom), his absence from Madison was apparently not viewed all that favorably. The departmental powers let him know that gaining tenure was unlikely, and he should go on the job market. From Peru, during the pre-internet age, this was not all practicable, as well as premature, given that he had only been on campus three semesters, not the normal 3 years before this kind of review. At the same time, the department lost three of its most promising faculty – Karl Butzer, Jonathan Sauer, and Fred Simoons, all with cultural/historical/human-environment interests not dissimilar from Denevan's. All three were offered jobs at LSU. Sauer and Simoons accepted, and Butzer accepted a subsequent offer from the University of Chicago.

To his credit, Denevan returned to Madison and fought the edict. He was soon reviewed for tenure and promotion and was denied. Given another year, on the second try in 1968, he made it. His increasing file of significant publications paved the way. These included several articles and monograph drawn from his dissertation and his (1966a) often-cited cultural ecological explanation of "former aboriginal" settlement of the Amazon Basin. He also published on livestock in the Mojos and New Mexico. But it was his articles on ancient raised fields that attracted the most attention and helped to establish a scholarly research current that continues to the present. The highest profile publications were his cover article in *Science* (1970) and with Parsons in *Scientific American* (1967). Though occasional geographers, anthropologists, and archaeologists going back to Alexander von Humboldt had some understanding that ancient Americans had made major wetland landscape modifications, especially with the surviving example of Mexico's famed *chinampas*, it was not until Denevan put the pieces together and begin to see them in their continental dimensions and their historical ecological significance, that the phenomenon was recognized for what it is: an agro-environmental adaptation of import and complexity equal to the great pre-European works of water (irrigation) and slope (terracing) management.

By the late 1960s, Denevan had begun to attract graduate students. His first, Daniel Gade, had been working with Jonathan Sauer, but with Sauer's departure, Denevan became co-director with Henry Sterling of Gade's (1967) dissertation, a study of plant use and agriculture in the Vilcanota Valley, Peru. Marshall Chrostowski also pursued doctoral research in Peru, but never completed his dissertation – the only one of Bill's 21 Ph.D. candidates who did not finish. Chrostowski served as Denevan's research assistant in biogeographical field studies in the Gran Pajonal of eastern Peru (Chrostowski and Denevan 1970) and for raised field research in the Orinoco Llanos of Venezuela. Denevan's next Ph.D. student, Bernard Nietschmann, elected to follow Denevan's footsteps in Nicaragua and not Peru. Nietschmann (1970) studied the subsistence ecology of the Miskito Indians on Nicaragua's Caribbean coast. His dissertation was published by Seminar Press (1973), becoming something of an instant classic that helped launch his career first at the University of Michigan and then at Berkeley. Denevan oversaw the completion of the dissertation of another one of Jonathan Sauer's biogeography students, Roger Byrne, who studied vegetation change in the Bahamas (1972). Byrne, like Nietschmann, joined the Berkeley geography faculty. In 1972, 4 years after his first promotion, Denevan was promoted to full professor. Thus, in 9 years (two of those in Peru), he went from entry assistant professor to full. While not a record (Carl Sauer went from instructor to full professor in 7 years), it was a vindication of his abilities and a good deal faster than the normal span of twelve years.

By the mid-1970s the campus turmoil that had rocked Madison for the previous decade abated, and the departmental "hierarchical/authoritarian" rule (to quote one of the eminent departed former faculty members) was dissipated through retirement (Hartshorne, Robinson, and Trewartha) and premature death (Clark in 1975). In this more stable environment, Denevan was able to push on with a number of projects, attract new students, and solidify his position as one of American geography's foremost Latin Americanists. Among these new students, Roland Bergman (1974) took the thread back to Peru, studying Shipibo indigenous subsistence in the Peruvian Amazon. Billie Lee Turner II (1974) opened up a new theater in the Denevan doctoral students' foci – Mesoamerican research. Turner produced a paradigm-shifting study of ancient Maya subsistence and intensive agriculture in the Maya lowlands. With an article in *Science* before even defending his dissertation, and prior appointments at the University of Maryland-Baltimore and the University of Oklahoma, Turner's career went into high gear with an appointment at Clark University by the end of the decade. Denevan's final doctoral student in the decade of the 1970s, Mary Daum, had begun her research with Sterling, a specialist on Venezuela, but with Sterling's retirement, Denevan chaired the committee. She (1977) did a study of land amalgamation in Barinas state, Venezuela. Starting in 1969 and continuing through the 1970s, Denevan also oversaw a number of master's theses. These were as follows: Roland Bergman (1969) Chirripo shifting cultivation in Costa Rica, Thomas Magness (1969) conch harvesting in the Bahamas, Hector Rucinque (1972) agricultural colonization in eastern Colombia, Stuart White (1975) logging in eastern Peru, Paul Blank (1976) Macusi subsistence in northern Amazonia, Kent Mathewson (1976) *tablón* horticulture in highland Guatemala, Daniel Parr (1978) land use in northern Guatemala, and Gregory Knapp (1979) sunken fields in coastal Peru. These helped fill out the early map of Denevan students' research locations.

By the mid-1970s Denevan was taking on new responsibilities, advising an increasing number of students, widening his international networks, and refining some of his earlier research foci. His long-time interest in pre-European historical demography resulted in his (1976) landmark edited volume, *The Native Population of the Americas in 1492*. This book established Denevan as one of the main authorities and arbitrators on the debate over the size of the pre-European populations of the Americas. He had shifted his earlier Berkeley cultural-historical approach to human-environment relations in the American tropics, to a more theoretically rigorous cultural-ecological perspective and approach as evidenced by his (1978) co-authored monograph on *Adaptive Strategies in Karina Subsistence, Venezuelan Llanos*. He published 14 articles and book chapters, including his (1973) widely cited and circulated critique of Amazonian development – "Development and the Imminent Demise of the Amazon Rain Forest." His number of advisees in the 1970s doubled from the previous decade. During the 1970s Denevan was a regular attendee and participant in the Conference of Latin Americanist Geographers (CLAG). He edited the 1978 CLAG Proceedings and served on the CLAG board. He presented papers at six international conferences, received honors from the Sociedad Geográfica de Lima, and was awarded grants from the American Council of Learned Societies and the National Science Foundation among other sources. In 1977 he was awarded a John Simon Guggenheim Fellowship, one of US academia's most prestigious awards. He served on several editorial boards, including *Geographical Review*, *Luso-Brazilian Review*, and *Antropológica* (Caracas), and organizational boards including the Organization for Tropical Studies. He also served as director of the University of Wisconsin, Madison Ibero-American Studies Program and Latin American Center.

The decade of the 1980s saw Denevan in full stride, beginning with a 3-year term as chair of the Geography Department. In 1987 he was awarded with a named professorship – the Carl O. Sauer Professor of Geography. He was given top honors from the AAG and CLAG. He received grants from UNESCO, the US National

Science Foundation, and the National Geographic Society for research on agroforestry and on terracing in Peru, also a National Endowment for the Humanities grant for research on his (2001) book *Cultivated Landscapes of Native Amazonia and the Andes.* He edited five books including a tribute reader (1989) of his advisor James Parsons' publications and 20-some articles and book chapters. He continued service on earlier editorial boards and joined the *Professional Geographer* board. His service included committee and board memberships on a number of national and international organizations, and he was elected ntional councilor of the AAG. He presented papers at nine international conferences and symposia. He had frequent invitations to speak at various universities and serve as panelist and discussant at meetings and conferences. During the decade he directed six successful Ph.D. dissertations, all based on South American research. Stuart White (1981) went to the field in highland Peru to do a cultural ecological study, but ended up producing a "novelistic" work. Hildegardo Córdova (1982) followed this up with a more conventional study of the "negative development" impacts of road building in Frias, Peru. Gregory Knapp (1984) and Kent Mathewson (1987) did studies of pre-European intensive agriculture in Ecuador. Knapp's study was situated in the northern Andean basins and Mathewson's in the coastal Guayas Basin. John Treacy (1989) produced a study of terracing in the Colca Valley of Peru, as part of a large interdisciplinary project directed by Denevan. David "Toby" McGrath (1989) broke new ground, at least for Denevan students, studying Brazilian Amazonian river traders. Master's students included Pascal Girot (1984), coffee farming in northern Peru; Michael Johns (1985) uneven development in Nicaragua; Andre Parvenu (1986) Central American refugees; and Lisa Naughton (1987) conservation in Costa Rica.

Moving into the decade of the 1990s, Denevan increasingly looked forward to retirement, having accomplished much of what he envisioned doing in his formal career as field-oriented geographer and advisor of graduate students. With his largest and most ambitious field project behind him – the multiyear, multi-personnel, and multidisciplinary Colca Valley Terracing study (1986, 1988) – and his major book project (2001) funded and on its way, he took relatively early retirement in 1994 at age 63 and became Carl O. Sauer Professor Emeritus. Still a break with Madison, and daily interaction with colleagues and students, was a few years off. He continued to publish through the decade at the same pace as earlier – some 14 articles and book chapters. He also revised a second edition of his benchmark pre-European population book for the Columbian Quincentenary (1992). Also, as part of a quincentenary volume, the memorable 1992 special issue of the *Annals of the Association of American Geographers* on "The Americas Before and After 1492," he contributed "The Pristine Myth: The Landscape of the Americas in 1492."

The "Pristine Myth" was something of a sensation, reverberating through various halls of academia, but also ricocheting around the popular media, including favorable mention by such disparate figures as conservative radio host Rush Limbaugh, Native American historian Dan Flores, and columnists and journalists such as Charles Mann. Mann (2005, 2011) expanded Denevan's main theme – that pre-1492 the Americas were hardly a pristine wilderness as often depicted – into his excellent and best-selling book *1491: New Revelations of the Americas Before Columbus* and its sequel, *1493: Uncovering the New World Columbus Created*. Twenty-five years later, Denevan (2016) published a sequel to "The Pristine Myth," titled "After 1492: Nature Rebounds."

While Denevan may have taken formal leave of the classroom in 1994, there was still the matter of advisees in "the pipeline" that needed supervision. During this final Madison decade, he oversaw more completed theses and dissertations than any previous decade – seven Ph.D. dissertations and ten master's theses. Oliver Coomes (1992) studied peasant economy in the Peruvian Amazon; Robert Langstroth (1996) reprised aspects of Denevan's Mojos work in Bolivia; Laurie Greenberg (1996) looked at Yucatecan house lots; Michael Castellon (1996) studied deforestation in eastern Guatemala; and Sarah Brooks (1998) contributed to the Colca Valley Project with a terrace study. In addition, Denevan co-chaired three dissertations: Lourdes Giordani's (1997, in Anthropology) study of Yabarana ethnogenesis in Venezuela, William Gartner's (2003) massive survey of "raised field landscapes in North America," and Joseph McCann's (2004) study of anthropogenic soils in Amazonia. In a sense, Gartner brought it "all back home" with his Upper Midwest-based raised fieldwork, keeping Denevan on duty as a formal graduate student mentor for a decade past retirement. The master's theses included Emily Young (1990) on Mixtec migration in Mexico, Serge Dedina (1991) on Tijuana River Valley development, Mrill Ingram (1991) on Agave fiber industry in Ecuador, Michael Castellon (1992) on forest preservation in highland Guatemala, Christian Brannstrom (1992) on Nicaragua's trans-isthmian canal, Ellen Webber (1993) on Colca Valley cattle raising, Maya Kennedy (1993) on pastoralism in Sardinia, William Gartner (1993 co-directed) on soil analysis of a Wisconsin archaeological site, Joseph McCann (1993) on fruit and fiber gathering in Peruvian Amazon, and Louis Carlo (1995) on agricultural study in Puerto Rico. Denevan, over a 30-year span, oversaw more than 20 master's theses and 20 doctoral dissertations. All but one dissertation, William Gartner's, involved fieldwork in Latin America or the Caribbean, though Gartner did take Denevan's signature topic – raised field agriculture – and demonstrate that it was widespread in parts of North America, just as Denevan had done himself for Latin America. All of his master's students, save for two, also wrote theses on Latin American topics. No other North American Latin Americanist geographer has overseen as many dissertations and theses on Latin American topics as Denevan. Six of his dissertators have gone on to tenured positions in Ph.D. granting geography departments, and they in turn have overseen some dozens of dissertations, most with Latin American foci.

In addition to his formal advisory role with his own graduate students, spending untold hours, days, weeks, months, and years, guiding toward degrees, he was informal mentor to a number of graduate students in geography and in other Wisconsin departments, including anthropology, history, botany, and Latin American studies. These include the following (with their dissertation locales): Ray Henkel (Bolivia), Barbara Williams (Mexico), David Stemper (Ecuador), and Antoinette WinklerPrins (Brazil). Denevan served as an external examiner for the dissertations by anthropologists Clark Erickson (1988) (Illinois) and Pawel Gorecki (1982) (Sydney) and geographer Emily Young (1995) (Texas). In addition, he served as informal advisor or mentor to a number of other scholars, both in graduate school and out. Among those with closest ties are Bill Doolittle, Clark Erickson, Susanna Hecht, George Lovell, Christine Padoch, Darrell Posey, and the journalist Charles Mann. Many students and former students collaborated with Bill and contributed to research on topics featured in this volume. As his complete publication list (contained herein) attests, he has co-authored articles and monographs with a number of his students along with scholars in a variety of fields and institutions.

In January 1996 Bill and Susie made the final move to their home and retirement destination at Sea Ranch, in northern California. They had originally bought property for their eventual retirement home at the planned residential complex in 1970. Over the years they spent many vacations at Sea Ranch, but with most of his students defending their dissertations by 1996, he was ready to close the Madison chapter of his career. However, Denevan's retirement and residence at Sea Ranch did little to slow down his scholarly production. If anything, for the next two decades, it picked up a notch or two. He completed his (2001) magnum opus – *Cultivated Landscapes of Native Amazonia and the Andes* – and co-edited (2009) a reader of Carl Sauer's publications, *Carl Sauer on Culture and Landscape*. Over this period he published another 30 articles and book chapters, many drafted in his crow's nest study using materials from his extensive library, which was transferred from Madison to Sea Ranch. Retirement honors include being elected Fellow of the American Academy of Arts and Sciences (2001) and receiving the Arch C. Gerlach Prize (2001–2005) from the Pan American Institute of Geography and History for "Outstanding contributions to the development of Geography in the Americas." While the pleasures of a stunning view of the Pacific Ocean and all the amenities of Sea Ranch's resort-style residential development were at hand, during these two decades, Denevan continued to be an active conference goer and correspondent.

Not all retirement travel has been grist for the geographic mill. Since retirement, Bill and Susie have travelled in Europe, Canada, and Mexico. In 2001 he went with his brother David (a Vietnam War veteran) and their nephew Robin and niece Monica to Vietnam, and the next year they went to Angel Falls in southern Venezuela. Occasionally, the world (or at least members of Denevan's professional world) has come to Sea Ranch. In 1994, 2007, and 2016, the Denevans' hosted well-attended weekend get-togethers following the AAG national meetings held in San Francisco. Even more expansive have been the several special sessions held in his honor at AAG meetings over the years. The legacy of William Maxfield Denevan in American geography is clearly evident, not only in his enviable record of fieldwork and publication but also in the career accomplishments of his many students. Perhaps even more legible is his impact and influence on scholarly activity outside of geography's precincts. As this volume makes abundantly clear, he directly instigated or inspired research and publication and a half-dozen or more arenas – including raised field investigations, hemispheric study of aboriginal demography, cultural biogeographic work, and indigenous ecology, to name the most salient. Although a three-degree geographer and a lifelong devotee of the discipline and its craft, his views and purview have been registered well beyond. And given the solidity and durability of what Bill Denevan has pursued and produced, this legacy is not likely to fade or be forgotten by those that carry on what he, in many cases, started or elaborated.

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10.0 Being a Student of Bill Denevan

Introducer: Gregory W. Knapp

Abstract Bill Denevan supervised or co-supervised 20 doctoral dissertations and 22 master's theses over four decades. His students have often retained intellectual affinities and friendships long after graduate school. This is a personal appreciation of Bill's role as a mentor, based on my own years at Madison and subsequent interactions in the field, professional meetings, and at Sea Ranch. The discipline is now populated by second- and even third-generation students, whose advisors and advisor's advisors have continued to transmit the Denevanite ethos of respect for the local, integrity, attention to detail, and skepticism about dominant discourses of progress.

Keywords Intellectual affines · Graduate school · Mentor · Student-advisor relationship

Writing about Bill's role as a mentor is a daunting, and necessarily highly personal, task. Here is my take; errors of fact or interpretation are of course my own, and readers should bear in mind that my perspective is based on my own years at Madison (1976–1984) and sporadic involvement subsequent to my taking a position at Texas, including visits with Bill at Madison, professional meetings, in the field, and Sea Ranch (see also Knapp 1999).

Bill Denevan supervised or co-supervised 20 doctoral dissertations and 22 master's theses; not including duplications, these amount to 35 graduate students. Bill continued to chair and cochair students for a decade after his retirement in 1994; the span of his advising encompasses four decades. His students form a cohesive group in many ways, often retaining intellectual affinities and even friendships long after graduate school. Many pursued academic careers in colleges and universities large and small, including California-Berkeley, Vermont, Arizona State, Texas, Louisiana State, McGill, Pontifical Catholic University of Peru, and New Mexico; many went on to influence further generations of students through publications, teaching, and

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supervision. Many of Bill's master's students went on to study at other institutions for their doctorates and also pursued influential careers.

Bill's influence also extends far beyond his own directly supervised students to other students he mentored at various stages of their careers. His influence on the discipline has thus been profound, not only through his own research contributions but through facilitating a school of thought which in various ways can be labeled "Denevanite."

I arrived in Madison in 1976, after several advisees of Bill's had received their doctorates and/or moved on (Dan Gade, Bernard Nietschmann, Roger Byrne, Roland Bergman, Mary Daum, and Bill Turner II), but their influence was still very much in the air. Bill was Director of the Latin American Center (and was to be department chair from 1980 to 1983), but his administrative duties did not distract from his research and teaching.

I had come to Madison from Berkeley with an interest in studying agriculture in Mesoamerica, where I had just spent 3 months exploring indigenous landscapes in Belize, Guatemala, and Mexico, but in an early visit to Bill's office, he encouraged me to go to South America. One day in his class he showed slides of "sunken fields" in Chilca, Peru. These were peculiar agricultural landforms, apparently excavated into the waterless desert to make use of a water table. In typical Denevanite fashion, he said, "someone should go there and study them." I agreed, and that became my thesis topic. Bill provided me with readings and a list of experts to contact, in the USA and Peru. Grants were minimal for master's students so I self-funded my summer trip to coastal Peru and the small town of Chilca. Bill visited me in Lima and we shared a Pisco Sour in the Hotel Crillon. I was hooked on South Americanist research and shifted to Ecuador after my master's thesis, where I continued to work on prehistoric and traditional agriculture with Bill's expert advice. Bill also visited me in the field in Ecuador, and we walked across raised field landscapes together. For both the thesis and dissertation, for seminar papers, and for the articles and chapters I wrote, Bill was painstaking in his editorial comments and corrections. His journalistic experience was evident; he insisted on clarity of expression and good diction.

In other words, Bill in many ways was an ideal supervisor, paying close attention to all the details of his students' work. He was also an excellent teacher. Bill's undergraduate teaching style was similar to that of his Berkeley mentor, James Parsons (I had sat in on Parsons' classes at Berkeley and was able to see the similarities). In addition to the intensive use of slides (most of them taken by himself), there was an intellectual passion for the subject matter and a high respect for the importance of primary data and firsthand observations.

Bill's graduate seminars at that time were organized around the theme of his current projects. I participated in one that focused on demography, related to his book *The Native Population of the Americas in 1492*. However, the most important tool of grad student advising was the office visit. Bill had a formidable office in Science Hall. He would meet students at his desk facing the front door, but the best part of his office was upstairs where he had more books, including extensive materials on Amazonia, and a storage area where students in the field could leave their things.

It took a lot of nerve to ask Bill to be a supervisor of a thesis or dissertation. If anything, Bill tried to dissuade students from making the request and always seemed surprised by the interest. But once one had been a student for a period of time, this became a bond, with Bill and with his other students, past, present, (and future). This somewhat medieval bond of mentoring was characteristic of Madison in general at that time; there was a tight knit group of geomorphology students under Jim Knox, biogeographers under Tom Vale, and urban and historical geographers under David Ward, for example. Each group had its own code of behavior, drinks of choice, and forms of relaxation. Many activities bridged the different groups, including participation in the Teaching Assistant union, activities associated with the Élisée Reclus Geography Club, and hosting such radical geography visitors as Bill Bunge.

Receptions and parties at Bill's home (with his wife Susie) were convivial but also an important part of absorbing the Denevanite *geist*, the hidden transcripts of being part of the student community. In addition to the music, food, drink, and dancing, there were conversations and interactions with multidisciplinary guests, partners, and such visitors as the Amazonian anthropologist Donald W. Lathrap. The importance of adventurous fieldwork in remote and dangerous places was reinforced by storytelling and anecdote. Fraud, self-importance, and timidity were quickly exposed.

Bill's mentorship was also striking and influential for what it did not include. Assuming that one was grounded in fieldwork, there was little pressure toward methodological or theoretical conformism. Approaches could be positivist, humanist, libertarian, anarchist, or Marxist. Within limits, his students could blaze their own trails in search of topics, literatures, and frameworks. It was important to publish, but venues were decidedly not limited to high-impact journals; Bill himself often published in obscure journals or inaccessible reports. This also meant that Bill was constantly scouring the horizon for obscure but stimulating writings that he could cite and share with students.

We learned that it was important to be creative and original and even more important to have personal, academic, and intellectual integrity. There was not an ounce of fakery or pretension in Bill or Bill's work. This is probably the single most important factor in accounting for the longevity of his publications and the themes that he pioneered. They are timeless because they are grounded in a fearless quest for the actual (to use Alfred North Whitehead's term). The merely fashionable was despised.

There were certain authors that were touchstones, of course. Humboldt, Sauer, Parsons, and the Berkeley lineage were important, and there was a constant sense of becoming part of this extended intellectual community. Humboldt and other explorers and naturalists were valued for their first-person accounts of environments and cultures. Authors who pursued long-term empirical fieldwork were especially valued. Students were also exposed to contemporary debates in cultural geography, cultural ecology, environmental history, and ecological anthropology, but Bill resisted identification with trendy catchphrases. In part this was due to his emphasis on research conducted as an individual or with partnerships with a limited number of other scholars. “Big science” was normally not on the agenda; the closest Bill came was his project working on Colca Valley terracing. I observed Bill in the process of writing one of his rare theoretical articles, the excellent “Adaptation, Variation, and Cultural Geography” (Denevan 1983); he found the task painful, but the article has kept its importance.

There was thus a bit of hero worship, a subtle sense of being part of a counter hegemonic elite (in an academy and discipline oriented toward contemporary, policy-relevant quantitative and technical work). But in my experience, this was not invidious, and I don’t recall Bill ever directly disparaging any individual scholar or group in the discipline. Moreover, some of Bill’s students went on to work very much in the context of positivist big science with Bill’s continued moral support.

Bill promoted interdisciplinarity. Archaeology, anthropology, and history were vital subjects, and students were encouraged to take classes from Louisa Stark, Frank Salomon, Donald Thompson, and other faculty from across campus. Archival work was valued and reading of sources in original languages, Spanish, French, or German. Physical geography, botany, soil science, and ecology were also recommended for readings and course work. All were appropriate background for the kind of diligent long-term rural international fieldwork that was considered essential for both theses and dissertations.

Bill instilled the importance of making contact with other scholars working on similar topics and resolutely gathering detailed bibliographies of prior work. This meant lots of written correspondence in the days before electronic mail. Bill of course had extensive professional connections and in many cases could recommend contacts in various disciplines. It was important to make contact with local scholars as well; whether in Quito or Iquitos, one was to knock on doors and speak with researchers in universities and institutes, archives, and laboratories.

That did not extend to bureaucrats, however. Bill (and other scholars at Madison) retained skepticism toward governmental agencies and organizations, as well as toward intergovernmental organizations and initiatives. In part this was due to a Sauerian cautiousness about the language and goals of modernization and progress, including the green revolution and quantitative social science with its models and systems. Bill favored small, focused professional organizations such as the Conference of Latin Americanist Geographers (CLAG), with an orientation toward field and archival work. He also favored international organizations such as the International Congress of Americanists.

Many although not all of Bill’s students had a preference for the “old ways” due to either personal taste, a radical anarchist background, or an intellectual bent for the Counter Enlightenment (Gade 2011). Many of Bill’s students supported the TA union and strike of the late 1970s and were involved with the Socialist Geography Specialty Group meetings at Madison. Bill himself was not overtly political, but his focus on research that respected local empowerment, and combatted nature-society dualisms, was subtly radical (see Hecht elsewhere in this volume). It is not surprising that many of his students pursued careers that were innovative and unconventional. Stuart White comes to mind; submitting a dissertation that consisted mostly of a novel based on several years of work with Peruvian shepherds, he went on to purchase and run a hacienda in Ecuador, supporting local communities in the face of the creation of national preserves that might threaten their livelihoods.

I often think of Bill's advice when I am performing research on contemporary agriculture and ethnic empowerment, or when teaching, including study abroad courses. Many of his other students have also spread his practices and ideals through administration, teaching, research, writing, and action. The discipline is now populated by second- and even third-generation students, whose advisors and advisor's advisors have continued to transmit the Denevanite ethos of respect for the local, integrity, attention to detail, and skepticism about dominant discourses of progress. I know I speak for all of his students when I thank Bill for his mentorship!

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*Master's Theses Chaired (22) *Also Ph.D.*

Roland Bergman, 1969*
Thomas Magness, 1969
Norman Berday, 1971
Hector Rucinque, 1972
Stuart White, 1975*
Paul Blank, 1976
Kent Mathewson, 1976*
Daniel Parr, 1978
Gregory Knapp, 1979*
Pascal Girot, 1984
Michael Johns, 1985
Andre Parvenu, 1986
Lisa Naughton, 1987
Emily Young, 1990
Mrill Ingram, 1991
Michael Castellon, 1992
Christian Brannstrom, 1992
Ellen Webber, 1993
Maya Kennedy, 1993
William Gartner (Co-Chair), 1993*
Joseph McCann, 1993*
Louis Carlo, 1995

Doctoral Dissertations Chaired (20)

Gade, Daniel, (Co-Chair), 1967, "Plant Use and Folk Agriculture in the Vilcanota Valley of Peru" (Published 1976); Deceased (June 15, 2015), formerly Professor Emeritus, Department of Geography, University of Vermont.

Nietschmann, Bernard, 1970, "Between Land and Water: The Subsistence Ecology of the Miskito Indians, Eastern Nicaragua" (Published 1973); Deceased (January 22, 2000), formerly Professor, Department of Geography, University of California, Berkeley.

Byrne, Roger, 1972, "Man and the Variable Vulnerability of Island Life: A Study of Recent Vegetation Change in the Bahamas" (Published 1980); Deceased (March 11, 2018), formerly Emeritus Professor, Department of Geography, University of California, Berkeley.

Bergman, Roland, 1974, "Shipibo Subsistence in the Upper Amazon Rainforest" (Published 1980); Department of Sociology and Geography, Shepherd University, West Virginia, retired 2018.

Turner II, B.L., 1974, "Prehistoric Intensive Agriculture in the Mayan Lowlands: New Evidence from the Río Bec Region" (Published 1983); currently, Professor, School of Sustainability and School of Geographical Sciences and Urban Planning, Arizona State University.

Daum, Mary, 1977, "Land Amalgamation in Government Colonies in the Aroa Valley and Barinas Piedmont Regions of Venezuela;" formerly, Brookhaven National Laboratory, New York.

White, Stuart, 1981, "Moments in the Narrative Landscape of Highland Peru;" currently, Department of Geography, University of Vermont (Adjunct Assistant Professor) and General Coordinator of Fundación Cordillera Tropical, Ecuador.

Córdova, Hildegardo, 1982, "Negative Development: The Impact of a Road on the Agricultural System of Frías, Northwestern Peru;" currently, Professor, Humanities (Geography Section), Universidad Católica, Lima, Peru.

Knapp, Gregory, 1984, "Soil, Slope, and Water in the Equatorial Andes: A Study of Prehistoric Agricultural Adaptation" (Published 1991); currently, Associate Professor and Director, Sustainability Studies, Department of Geography and the Environment, University of Texas, Austin.

Mathewson, Kent, 1987, "Landscape Change and Cultural Persistence in the Guayas Wetlands, Ecuador;" currently, Professor, Department of Geography and Anthropology, Louisiana State University.

Treacy, John, 1989, "The Fields of Coporaque: Agricultural Terracing and Water Management in the Colca Valley, Arequipa, Peru" (Published 1994); Deceased (October 23, 1989), formerly George Washington University.

McGrath, David, 1989, "The Paraense Traders: Small-Scale, Long Distance Trade in the Brazilian Amazon;" currently, Deputy Director and Senior Scientist, Earth Innovation Institute, San Francisco, and Professor, Graduate Program in Society, Nature, and Development, Federal University of Western Pará, Brazil.

Coomes, Oliver, 1992, "Making a Living in the Amazon Rain Forest: Peasants, Land, and Economy in the Río Tahuayo River Basin, Peru;" currently, Professor, Department of Geography, McGill University.

Langstroth, Robert, 1996, "Forest Islands in an Amazonian Savanna of Northeastern Bolivia;" currently, Sector Senior Specialist, Environmental Safeguards Unit, Inter-American Development Bank, Washington, D.C.

Greenberg, Laurie S.Z. (Geography and Land Resources), 1996, "You Are What You Eat: Ethnicity and Change in Yucatec Immigrant House Lots, Quintana Roo, Mexico;" currently, Owner, Cultural Landscapes LLC, Madison, WI.

Castellon, Michael, 1996, "Dynamics of Deforestation: Qeqchi-Maya Colonists in Guatemala's Sierra de las Minas, 1964–1995;" Geography Faculty, Front Range Community College, Fort Collins, Colorado.

Giordani, Lourdes (Co-Chair, Anthropology), 1997, "Imagining Who We Were, Believing Who We Are: Ethnogenesis Among the Yabarana Indians of Venezuela;" formerly, teacher at a

Navajo school in New Mexico; currently, staff member, Washington State University Health Professions Student Center.

Brooks, Sarah, 1998, "Prehistoric Agricultural Terraces in the Río Japo Basin, Colca Valley, Peru," retired, formerly lecturer, Department of Geography, University of New Mexico.

Gartner, William (Co-Chair), 2003, "Raised Field Landscapes of Native North America;" currently, Senior Lecturer, Department of Geography, University of Wisconsin, Madison.

McCann, Joseph M. (Co-Chair), 2004, "Subsidy from Culture: Anthropogenic Soils and Vegetation in Tapajônia, Brazilian Amazonia," independent, New York, NY.

External Examiner, Doctoral Dissertations (3)

Gorecki, Powel, 1982. "Ethnoarchaeology at Kuk" (New Guinea); Anthropology, University of Sydney, Australia.

Erickson, Clark, 1988. "An Archaeological Investigation of Raised Field Agriculture in the Lake Titicaca Basin of Peru;" Anthropology, University of Illinois, Urbana.

Young, Emily, 1995. "Elusive Edens: Linking Local Needs to Nature Protection in the Coastal Lagoons of Baja California Sur, Mexico;" Geography, University of Texas, Austin

11.0 Bill Denevan: An Appreciation

Introducer: Daniel W. Gade and Kent Mathewson

Abstract For more than 60 years, William M. Denevan has put his mind to a range of themes and venues that together constitute a sterling record of scholarly achievement. His thinking crystallized during his student days at the University of California at Berkeley. Especially influential were courses taught by Carl Sauer, James Parsons, and Erhard Rostlund and ideas presented in their published work.

Keywords Sauerian tradition · Berkeley school · field work · historical ecology · Latin America

Bill Denevan as Scholarly Exemplar

Denevan's interests largely revolved around understanding landscape patterns and the processes of change associated with them. Fieldwork is implicit in that quest, for such information is not generated by official agencies. Far too many scholars keen on building theory depend on data generated by someone else. Avoiding bandwagons of the moment, Denevan's studies are likely to have intellectual longevity for there are no trendy theories to become passé. For Bill, geography is a field science, not a discourse on fashionable ideas of the moment. Interweaving physical and cultural processes, his work embodies the Sauerian tradition (Denevan and Mathewson 2009).

Note: This piece was originally contributed as the introductory essay to Section 3 – Cultural Plant Geography, but contained more about Bill than we had intended in the introductory essays. Unfortunately, Dan was unable to edit this contribution due to his passing in 2015. We decided to keep the essay in the volume as a tribute to Bill's first student and because it adds to the whole. The text has been shortened and edited for clarification.

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More implicit than explicit, Bill's Sauerian vision has included the refusal to use arguments borrowed from cultural materialism. Denevan also manifested a particular interest in morphology. He was the first to study ridged fields as forms and he did the same with terraces, savannas, swiddens, and human artifacts. Like Sauer, Denevan was wedded to looking back in every study that he undertook. Part of his historicist thinking involved an ability to put himself in a different time and place to reconstruct what might have happened. For example, Bill's insight that stone axes (Denevan 1992b) are incapable of felling large numbers of trees came from watching use of metal axes in the rainforest. His interest in morphology is seen in his typology of anthropogenic earthworks and a view of savannas as a particular vegetation form. Although he never articulated it in print, another aspect of Denevan's perspective is his organicism. Like it was for Parsons and Sauer, organicism has been an important facet of Bill's worldview.

Bill has also been instrumental in the continuity of the Sauerian tradition. His productive mentorship of Ph.D. students at Wisconsin made him the key individual in the second generation of the Sauerian genealogy (Brown and Mathewson 1999).[1] Several graduate students whose degrees were in anthropology but who worked primarily with Bill also benefited from his major input in their dissertations. Viewed as a whole, Denevan's contributions evoke several broad issues about scholarship not often discussed.

Knowledge from Fieldwork as an Inexorable Dilemma

Bill's mentor at Berkeley was James J. Parsons, but Carl Sauer, though recently retired, still formally gave seminars and advised several graduate students and informally advised others. When Bill went to Nicaragua in 1957 to carry out his master's thesis project, he wrote Mr. Sauer a progress report. One heartfelt sentence of that letter addresses a fundamental contradiction of being a field geographer: "I only wish that along with my youth, hardiness, and acceptance of hardship, I had the wisdom of mature men in this calling who are no longer able to ride a mule 40 mi a day or a dugout and live on beans and plátanos for days at a time!" (Denevan 1957). In that extract, Bill captured the fundamental difficulties of field research in Latin American rural settings. The novice stalwart that he was in the 1950s thrived on the rigors and daily challenges encountered, but lacked experience and judgment about which methods worked and which did not. How best to evaluate the information

[1] Of Denevan's 21 Ph.D. candidates, 20 completed their dissertations. Marshall S. Chrostowski, Bill's co-author and field assistant in the Gran Pajonal, later initiated his own biogeography project in eastern Peru. The inability of Chrostowski to complete writing his dissertation for the Ph.D. is repeated in every departmental history, but advanced student abandonment of their programs remains in the realm of anecdotes.

collected and how to fit a field study into larger concepts were challenges to be met. One can read into that communication from Denevan to Sauer that he was on his way to trusting his impulsive, knowing self. As sagaciously implied in that letter, the dilemma is reversed with advancing age. Experienced scholars in their 50s and 60s have acquired a range of competencies that make much less problematical the best way to acquire data, construct useful categories, and place one's findings in a larger context. At some point the field geographer has to deal with the aging process of his or her body's tolerance level. Flexibility and endurance, necessary for sustained fieldwork in the boondocks, decline before the brain does (Gade 2011a, 455). Few researchers past 60 undertake major new field projects in which they themselves collect all the data. As Parsons and Sauer before him, Denevan's final writings benefited from the recapitulation and reflection of his septuagenarian years.

The Virtue of Pure Inquiry

Denevan's scholarly dedication through the years came from a strong and deep motivation to formulate a range of research questions and ferret out answers to them. The fieldwork for his thesis and dissertation focused on topics that were of deep interest to him, not merely projects to be logged in order to get an M.A. or Ph.D. and then forgotten. The successful execution of this early fieldwork set the stage for a productive life of scholarship. It is worth asking if a research trajectory can be prompted by the inspiration received from teachers who were also scholars. Bill's whole path has been driven by a fundamental intellectual curiosity. He had no do-gooder compulsion that many geographers working in poor countries find necessary to kick-start their projects. Like many other Sauerians, he resisted the underlying assumption of the development paradigm. One aspect of the paradigm is that development agents, formerly USAID operatives and now NGO promoters, are the "experts" and thus should hold cognitive authority over peasants. Sauer, Parsons, Denevan, and a long line of other skeptics have held that it has been peasants who possess knowledge about agriculture that the world should know.[2] In that perspective, the illiterate peasant is the teacher and the professor from 7000 km away is the learner. Another aspect of development that cannot be ignored is its destructive character that sacrifices all at the altar of modernization. Bill (Denevan 1973) expanded that idea in his 1973 article in the *Professional Geographer*.

[2] Bill recounted the clash of perspectives on this issue at the CLAG meeting in Muncie in 1970 (Denevan 2008). Leading the charge as Bill's antagonist was Barry Lentnek, an assistant professor at SUNY Buffalo. In his published speech, Lentnek (1971, 161) wrote that "…pre-Columbian and other historical studies by geographers are a nicety well worth preserving for a handful of our colleagues and their academic audiences. They should not, however, form the bulk of our literature during the coming crucial decades if we wish our work to be of some service to the tens of millions of Latin Americans who must, perforce, live through the agonies of modernization during the next generation." However, Lentnek himself published little on Latin American economic development that might have demonstrated the kind of "useful" research he advocated.

Bill did not succumb to the idea that geographical or any other kind of research should be influenced by what others think one should do. He received criticism from those who believed their research priorities should be those of others as well. Although he reacted to it with equanimity and tolerance, he knew what a sustained research required above all: intrinsic motivation. True scholarly motivation over the long term comes not from being a state-of-the-art theoretician, moral imperativist, or pedagogue, but from a compelling interest in particular phenomenon that is accessible through determination and discernment. Denevan's output confirms that intellectual curiosity results in work of timeless value. Pursuing his interests based on intrinsic motivation has also given Bill lasting power. Retirement from teaching in 1994 did not mean Bill relinquished the life of the mind and the hard work of writing for publication. In the glorious isolation of Sea Ranch, technology and sport helped him to achieve that. The computer kept him in touch and a tennis regime kept him in form. In the 25 years since he became emeritus, Bill authored one book (Denevan 2001), edited three others, and produced (by one recent count) 29 articles or chapters. None of Bill's creative energies went into the preparation of textbooks. The lure of fat royalties held no candle to the satisfaction derived from contributing to the fund of knowledge on his own terms. Several years on the Guggenheim Foundation screening committee made Bill appreciate that geography as a discipline does not benefit in the court of academic decision-making and scholarly judgment when its practitioners spend research time working on pedagogical materials.

A Cognitive Style

A scholarly path taken reflects a certain perspective on research that the scholar may not articulate in anything written. Bill's research shows parallels with that of his mentor Jim Parsons. They shared a keen interest in Latin American landscapes, but also in inquiry based on the practice of intellectual autonomy. They did not accept requests from official agencies to investigate topics that met bureaucratic needs. Neither did they conceptualize research as a way to validate their ideological beliefs. Bill learned from Jim Parsons that Latin America offered many fascinating research possibilities connecting humans to their resource base in space and time. Though rarely so phrased, Denevan and Parsons, as well as Carl Sauer, were heirs to a sensibility that included a reach for the exotic (which explains fieldwork in distant places); an interest in landscape form, function, and origins; the importance of understanding the past to explain the phenomenon studied; and a focus on coaxing larger ideas out of the evidence rather than on an a priori agenda of some grand theory (Gade 2011b, 2012). His intellectual curiosity has been so productive because of his ability to discern the research problem and pursue it with self-discipline.

Denevan and the Written Word

In addition to an identifiable cognitive configuration, Bill Denevan's published work manifests particularities of writing style. How geographers express themselves in print is a theme given scant attention, as if the written word were a matter of simply recording facts uncovered by observation.[3] But no knowledge generated by any scholar, even by hard scientists, is independent of the language constituting his or her published corpus. The understated expressivity of Bill's books, chapters, and articles reflect his low-keyed personality. Even if one had never met Bill, one might be able to claim to know him from reading his work.

His ideas and facts were expressed in a spare, straightforward prose that avoided the overuse of adjectives and, even more so, of adverbs. His word choices rarely sent readers to a dictionary. He makes few value judgments and does not sermonize. Conclusions presented at the end of an article convey a judicious balance. Readers respond positively to his style of prose and will continue to do so in the future. Authenticity pervades all the pieces he has written. Unlike those who write semi-popular books on civilization collapse and the fate of human society, Bill never pontificated about matters he could not know about. Since he did not phrase his research problems in a quantitative frame, he never claimed the kind of truth that, for so many positivist geographers, turned to dust when withering critiques demolished their objectivist assumptions. An attentive reader gets the sense that Bill wrote no more and no less than what he meant to say.

The pristine myth piece (Denevan 1992a) captured a large readership and became Denevan's most highly cited publication (Denevan 2011). Four books reprinted the article. Its appeal fit into the postmodernist Zeitgeist of irreverence vis-à-vis old established beliefs. It was also so well crafted that no postmodernist jargon was necessary.

Denevan's publications reflect craftsmanship of the written word. While astute copyeditors have intervened, as they do with most authors, credit must go chiefly to the careful thought Bill has put into organizing and rewriting his material to achieve maximum clarity. Standard scholarly prose dominates his output, but some articles also feature the first person strategy to convey immediacy of experience.[4] Bill's account of his trip to the savannas of the Río Heath in Peru and Bolivia gets the reader into the mind of a scholar seeking to understand the truth of the world in its landscapes (Denevan 1980). The trip to that then-remote region was hard, but Bill neither hyped the vicissitudes of getting there and back nor the tentative findings

[3] Textual analyses that focus on the written word are available for some individuals of the stature of Charles Darwin, but no one, to my knowledge, yet in geography. Carl Sauer's rhetorical power as a scholar had much to do with his disarming prose style, which combined preference for short words of Anglo-Saxon origin and a succinct, almost epigrammatic, expression.

[4] In the University of Wisconsin graduate program of the early 1960s, Andrew Clark warned students not to use the first person in writing because it detracted from the objective tone of their work. By the 1980s few scholars chose to broach the notion of objectivity, even in texts backed up with numerical data.

that came out of it. Yet the article communicated the power of curiosity about places as a motive for geographical exploration. An attentive reader might derive, as I did, a sense that when all is said and done, geographical fieldwork in distant places is about answering the call to adventure that one issues to oneself. The individual adventure is a way to live the heroic life that is about overcoming difficulties and, in the end, prevailing.

Conclusion

Four dimensions stand out in Bill Denevan's scholarship over more than half a century. Though not his only or even major scholarly focus, cultural plant geography appears in one way or the other in much of his work. His discussions of plants focus more on what they represent than on the objects themselves. To geographers, plants are vital indicators of climate and to some extent of soils. As Denevan's work shows, plants are also a manifestation of human interaction in changing the face of the earth. People have greatly affected the world's vegetation formations and associations and have carried species hither and yon. Humans also manipulated plants, transforming wild taxa into new species dependent upon Homo Sapiens. A notable share of Bill's work embraces a biocentric perspective that showed him understanding the world as an organic, rather than a mechanistic, entity.

A second dimension of Bill's collective scholarly endeavor is a historicist perspective. Virtually all of his projects point to a belief that the essence of anything can be appreciated only by knowing its past. The real charm of the Mojos for Bill was less that a huge savanna occurred in the Western Amazon, but what had gone on in that grassy landscape in the past. The incredible bench terrace complex of the Colca Canyon, most of it no longer used for crops, was part of the pre-European past that he wanted to reconstruct. Bill was very much interested in origins, but he also realized that, though archaeology revealed much, those questions could often be answered only by speculative inquiry. At the same time, he was very much into the concrete, which, in a philosophical sense, is in keeping with historicism as a reaction against rationalist abstraction. Bill Denevan's investigations recall the empirical foundations of Humboldtian science that stressed the historicity of observable phenomena and the close relationship of people and natural processes.

A third dimension of Denevan's oeuvre is the importance of location and scale. Grasping the whole came from attention to the intermediate scale that brings to life concrete places in their relationship to other places. He was also intent on grasping the whole, which required paying attention to the intermediate scale, as demonstrated by his work in the Mojos, Gran Pajonal, and the Colca. By mapping the whole, it was possible to decipher what had gone on in the past and why. Documentation of the massive anthropogenic intervention in the Mojos savanna was, in retrospect, perhaps his greatest insight. Viewing the Amazon at an intermediate scale and identifying the biotopes and resources of each, Bill was also able to conceptualize the river bluff zone, not the floodplain, as the privileged settlement site of the region (Denevan 1996). This approach used the classic idealist foundation of reconstructing the past in the mind's eye and then making a series of plausible assumptions in order to achieve greater understanding.

Fourth, Denevan integrated natural and cultural processes and described his main scholarly interest as cultural ecology (but later as historical ecology). His approach was not, however, the cultural ecology of Julian Steward, whose cultural materialism opened a dubious trap door to deterministic arguments. In the 1990s, when political ecology overwhelmed cultural ecology in the American academy, Denevan did not, as so many others did, swing toward that fashionable enthusiasm of the moment. Many practitioners of political ecology, obsessed with how individuals or groups applied power, framed their problems in an ideological context and ignored the biophysical components. Bill took to heart intertwining culture history with natural history. That approach has useful application. As anthropogenic global warming becomes increasingly evident, the planetary wisdom moves in the direction of a major conceptual change in the way Western cultures construct categories. Rather than standing apart from "nature," humans will be forced to realize they are inextricably bound with the earth in a skein whose threads can no longer be unraveled. At some point, people will wonder how they could have been so naïve as to think in terms of the dualism of human and nature. Denevan's work contributes a solid piece to that gigantic realization.

Bill Denevan's trajectory cannot be appreciated without the contingency of his formative years as a student at Berkeley. Being at the right university at the right time and finding Jim Parsons as the right mentor provided an intellectual foundation that has inspired him throughout his years of teaching and into retirement. The heyday of cultural-historical scholarship from 1940 to 1980 corresponded to a blossoming national interest in foreign area study and its foundation support in the social sciences. A sharp growth of interest in Latin America occurred, which, given California's relative proximity to Mexico, made a southward orientation for field studies sensible. In that time period, academic slots opened in a cultural geography, historical geography, and regional geography and, after 1970 and for about two decades, in cultural ecology that was not to last. These specialties had lost their priority in the academic marketplace and have even disappeared from some geography departments that once had them. But many of the themes pioneered by Sauer and his descendants have persisted, even when that debt is not acknowledged in print (Bowman et al. 2011).

In the context of the late 1950s and early 1960s, Denevan's interest in traditional resource management and landscape transformation was encouraged and validated. If he had been at Berkeley half a century later, the classic notion of geography as exploration and discovery would have required recalibration toward something quite different.[5] What recently happened at Berkeley has occurred in many geography departments, without the legacy of a world-renowned school of thought.

[5] The UC Berkeley Geography Department in the 1950s was all about the pursuit of intellectual curiosity as the supreme value. Half a century later, faculty and some graduate students were vetted for ideological conformity. Karl Marx had displaced Carl Sauer as the great god behind the Sierra.

Without the anchor of a centralizing mobilizing idea, geography as a discipline lacks conceptual continuity. More than anthropology and much more than history, geography suffers from attention deficit syndrome. Yet Bill Denevan, who saw himself as an heir to a noble scholarly tradition, had no need to reinvent the discipline with some new technique or theory. Rather he pursued ideas about land and life in the past, filled them in with his empirical findings, and encouraged others to push toward their own understandings on similar topics. A multidisciplinary constituency for Denevan's published work will continue well into the future. His eminently readable corpus of writings will continue to inform and inspire.

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