Daniel Little

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Chapter 1
Introduction: History's Pathways

"History" is a deceptively simple concept. It invokes the notions of change over time, human agency, the role of material circumstances in human affairs, and the question of the putative meaning of historical events. It raises the possibility of "learning from history"—whether from the experience of the Peloponnesian War or the Korean War, the 1918 avian influenza pandemic or the Chinese Great Leap Forward famine. And it suggests the possibility of better understanding ourselves in the present, by understanding the forces and circumstances that brought us to our current situation. It is therefore unsurprising that philosophers have sometimes turned their attention to efforts to interrogate "history" and the problems that the concept raises, from a philosophical point of view. These reflections can be grouped together into a body of work called "philosophy of history." But it is a highly heterogeneous grouping, involving idealists, positivists, logicians, and theologians. Some philosophers have been primarily interested in the "metaphysics" of historical change. Others have focused on the epistemology of historical knowledge. And yet others have asked large questions about the meaning or direction of history. Given this plurality of voices within the "philosophy of history", it is impossible to give one definition of the field that suits all these approaches.[1]

This book is intended to raise fresh questions about the nature of our knowledge and representation of the past. And it proposes to begin to answer these questions—and discover new ones as well—through careful attention to some of the best and most innovative historians writing today. As critical minds cope with the intellectual challenge of offering concrete historical interpretations, they are implicitly compelled to deal with these conceptual complexities. And so we can tease out new answers to these questions by engaging carefully with the historical reasoning of talented historians. Fundamentally, it is historical *cognition* that is the central point of focus for this book: how do we conceptualize, represent, interpret, and discover the past?

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[1] Important twentieth-century contributions to the philosophy of history include Collingwood (1946), Löwith (1964), Walsh (1968), Gardiner (1952, 1974), Dray (1957), Dray (1964), Gallie (1964), Danto (1965), Hempel (1942), White (1969), and Ankersmit (2001).

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The position I will take advocates that there is indeed a rigorous interpretation to be offered for historical cognition. It is an interpretation that does without teleology; that emphasizes causal mechanisms; that emphasizes conjunction and contingency; and that offers a nuanced understanding of "large historical structures." And we will make every effort to draw lessons from best historical practice. Much of the discussion of history by philosophers in the past 20 years has taken its lead from the "linguistic turn" in philosophy. Philosophers such as Hayden White (White, 1973) and Frank Ankersmit (Ankersmit, 1995) have emphasized the literary and linguistic features of historical representations rather than their claims to truth about the past. Hans Kellner puts the point clearly in these terms: "Philosophers have shown less interest in the truth-value of the historical statement and have turned to the narrative as a whole, which will have a truth more akin to the truth of a novel or a painting than to that of a syllogism" (Ankersmit and Kellner, 1995, p. 1).

I do not doubt that there are important and interesting things to discover when we look at great historical writing from this perspective. But my concern here is different. It is to look at historical writing as an aspiration towards discovering some of the truth about the past—what happened, why it happened, and how social institutions and circumstances played a role in key turning points. Inquiry, objectivity, explanation, and interpretation are the key activities for the historian from my perspective. I look at historical inquiry as a companion to social science research, and the problems of evidence, theory development, causal reasoning, and interpretation of action are key elements of historical inquiry in common with social science research. So the philosophy of history that is advanced here falls distinctly on the side of "social science history" rather than "post-modern historical understanding."

Why do we need a better philosophy of history? Because we think we know what we mean when we talk about "knowledge of history," "explaining historical change," or "historical forces and structures." But—we do not. Our assumptions about history are often superficial and fail to hold up to scrutiny. We often assume that history is an integrated fabric or web, in which underlying causal powers lead to enduring historical patterns. Or we assume that historical processes have meaning—with the result that later events can be interpreted as flowing within a larger pattern of meaning. Or we presuppose that there are recurring historical structures and entities—"states," "cultures," and "demographic regimes" that are repeatedly instantiated in different historical circumstances.

I do not say that these assumptions are entirely wrong. I say that they are superficial, misleading, and simple in a context in which nuances matter. Take the idea of recurring historical structures. Is there some state "essence" possessed in common among the Carolingian state described by Marc Bloch, the theatre state of Bali described by Clifford Geertz, and the modern Chinese party state described by Vivienne Shue? If so, what is this set of essential properties that states have? If not, what alternative interpretation can we provide to "state talk" that makes coherent sense?

Likewise, consider the problem of providing a more adequate treatment of concepts that are often taken for granted in historical discourse: e.g. structure, mentalité, class, event, or revolution. Each of these concepts raises problems that philosophers can help to address. The twin problems of disaggregation and reification arise in each instance. What is a historical structure such as "the fiscal system of the *ancien regime"* composed of? What errors are we led to by asserting things like "the Revolution in the west was less about economic interests and more about local political competition"? What do we need to provide as foundation if we want to refer to the "artisanal mentalité of Marseille workers"? In each instance there is a productive space in which the philosopher and the working historian can arrive at a deeper and more adequate conceptual scheme in terms of which to analyze the historical reality under consideration. We need to make explicit the presuppositions that are associated with a given conceptual scheme for history. And it is worth noticing that this work is not purely analytical; it is also substantive, in the sense that it is shedding new light on real historical phenomena. We might describe this work as falling in the domain of substantive historical ontology.

Or take the idea of historical causation: "The French Revolution was caused by the fiscal crisis of the *Ancien regime*." Perhaps this is true. But what does it mean? How do fiscal crises bring about revolutions? What do we know about "causal mechanisms" in historical circumstances such that we can assign rigorous and useful meaning to the causal hypothesis?

And what about the idea that large historical configurations have "meaning"? Hegel believed something like this: "All history is the unfolding of human freedom; so specific episodes can be interpreted in terms of their contribution to the saga of freedom."[2] Other philosophers in the continental tradition of the philosophy of history are equally interested in discovering the meaning of historical events (Löwith, 1964). Meaning to whom? Inherently? Participants assign meanings to many things, as do those who follow (including historians). But is there any rigorous basis for attributing meaning to a congeries of events?

Another important aspect of a philosophical treatment of history falls on the side of epistemology and the theory of explanation. Philosophers want to know how good the claims are for "knowledge" in various fields. This is another compelling reason for pursuing a philosophy of history—to answer fundamental questions about historical knowledge. What is the status of specialists' knowledge of the past? What methods exist for arriving at knowledge about the past? How broad or narrow is the range of uncertainty about different kinds of historical claims?

In a similar vein, philosophers are interested in exploring and resolving some of the concepts and assumptions that have been invoked to describe history itself. There are many such puzzles in historical claims to knowledge: What are "contingency" and "necessity"? Are historical beliefs "objective" or "biased"? What is the relation between history and memory? Are there "periods" and "epochs" in history? Are there civilizations and peoples?

[2] Hegel's philosophy of history is most fully expressed in his *Lectures on the Philosophy of World History* (Hegel, 1975). Dennis O'Brien provides a readable interpretation of Hegel's philosophy of history (O'Brien, 1975).

So there is a reasonable subject matter for the discipline. But what is wrong with the philosophy of history we currently possess? First, writings on this subject do not really add up to a coherent and reasonably comprehensive set of ideas. Certain topics have grabbed the stage—Are there laws in history? What is a narrative? Is history teleological?—and have refused to give the spotlight to the other characters. So we might say, we need fresh thinking by talented philosophers and historians who can re-identify a leading set of topics for discussion.

But second, and more fundamentally, philosophers have usually engaged "history" at too great a distance from great historians. Read any really excellent piece of historical writing today—Jonathan Spence, Simon Schama, Robert Darnton, Albert Soboul, Michael Kammen, Peter Perdue—and you will be struck by a raft of interesting philosophical and conceptual issues. And a new philosophy of history needs to incorporate as much of this range of working historical thinkers as possible.

In fact, philosophers can learn a great deal from considering the range of ways in which talented historians frame their results: Albert Soboul's methodical exposition of the macro-level class identities and interests of the groups contending in Paris in 1789 (Soboul, 1989), Simon Schama's discontinuous exposition of elements of European history around the theme of landscapes (Schama, 1995), Philip Kuhn's exposition of the cultural-emotional climate of late Imperial China through the lens of a witchcraft scare (Kuhn, 1990). To what extent do these choices about exposition and framework inflect upon the substantive historical findings? How does Peter Perdue's shift of focus from Ming-Qing China to the shifting power relationships in central Asia between Russia, China, and the Mongol empire change our perspective on Chinese diplomacy and warcraft (Perdue, 2005)? The philosophical work to be done here is analytical; but it is also illuminating and cumulative, in the sense that it lays the basis for a better understanding of the relationship between the historian and the historical domain under scrutiny.

These examples suggest a distinctive starting point for a new philosophy of history: Begin with good examples of historical analysis and exposition, and then ask about some of the core historical assumptions that enter into these examples of historical thinking and writing. What kinds of puzzles and presuppositions do these concepts bring with them? And what can we discover about historical explanation and knowledge by carefully observing the work of talented, innovative historians? One thing seems clear: the philosopher needs to formulate topics and problems in close proximity to the historical research of talented historians. In this respect it seems right to regard the philosophy of history as an "applied" field. In this way the philosopher avoids the hazard of a uselessly a priori approach to the philosophical study of history. And it suggests yet another way of bridging the divide between philosophical and substantive knowledge-building in the philosophy of history: by establishing close and mutually insightful partnerships between philosophers and historians.

Finally, why does it matter? It matters because history matters. At any point in time we are created, influenced and formed by our histories. And philosophy reasonably should shed some light on this fact. It is time for philosophers to take a fresh look at the issues raised by history in all its facets. We need some new approaches to the philosophy of history. So it is indeed important to reinvigorate the discipline of the philosophy of history. But it cannot proceed by picking up the traces from Hempel and Gardner, or from Hegel and Gadamer. We need to pose new and more penetrating questions. We need to work in close concert with the most gifted historians and historical social scientists. And we need to arrive at a framework of discussion that invites even more innovative and illuminating historical research and explanation in the future.

1.1 The Historian's Tasks

What are the intellectual tasks that historians are attempting to perform? In a sense, this question is best answered in the chapters that follow and a careful reading of some good historians. But it will be useful to offer several obvious answers to this foundational question as a sort of conceptual map of the nature of historical knowing.

First, historians are interested in providing conceptualizations and factual descriptions of events and circumstances in the past. This effort is an answer to questions like these: "What happened? What was it like? What were some of the circumstances and happenings that took place during this period in the past?" Sometimes this means simply reconstructing a complicated story from scattered historical sources—for example, in constructing a narrative of the Spanish Civil War or attempting to sort out the series of events that culminated in the Detroit race riot/uprising of 1967. But sometimes it means engaging in substantial conceptual work in order to arrive at a vocabulary in terms of which to characterize "what happened." Concerning the disorders of 1967 in Detroit: was this a riot or an uprising? How did participants and contemporaries think about it?

Second, historians often want to answer "why" questions: "Why did this event occur? What were the conditions and forces that brought it about?" This body of questions invites the historian to provide an explanation of the event or pattern he or she describes: the rise of fascism in Spain, the collapse of the Ottoman Empire, the great global financial crisis of 2008. And providing an explanation requires, most basically, an account of the causal mechanisms, background circumstances, and human choices that brought the outcome about. We explain a historical outcome when we identify the social causes, forces, and actions that brought it about, or made it more likely.

Third, and related to the previous point, historians are sometimes interested in answering a "how" question: "How did this outcome come to pass? What were the processes through which the outcome occurred?" How did the Prussian Army succeed in defeating the superior French Army in 1870? How did Truman manage to defeat Dewey in the 1948 US election? Here the pragmatic interest of the historian's account derives from the antecedent unlikelihood of the event in question: how was this outcome possible? This too is an explanation; but it is an answer to a "how possible" question rather than a "why necessary" question.

Fourth, often historians are interested in piecing together the human meanings and intentions that underlie a given complex series of historical actions. They want to help the reader make sense of the historical events and actions, in terms of the thoughts, motives, and states of mind of the participants. For example: Why did Napoleon III carelessly provoke Prussia into war in 1870? Why has the Burmese junta dictatorship been so intransigent in its treatment of democracy activist Aung San Suu Kyi? Why did northern cities in the United States develop such profound patterns of racial segregation after World War II? Why did young men in the 1910s and 1920s prefer dangerous, noisy internal combustion automobiles to safe, quiet electric vehicles? Answers to questions like these require interpretation of actions, meanings, and intentions—of individual actors and of cultures that characterize whole populations. This aspect of historical thinking is "hermeneutic," interpretive, and ethnographic.

And, of course, the historian faces an even more basic intellectual task: that of discovering and making sense of the archival information that exists about a given event or time in the past. Historical data do not speak for themselves; archives are incomplete, ambiguous, contradictory, and confusing. The historian needs to interpret individual pieces of evidence; and he/she needs to be able to somehow fit the mass of evidence into a coherent and truthful story. Complex events like the Spanish Civil War present the historian with an ocean of historical traces in repositories and archives all over the world; these collections sometimes reflect specific efforts at concealment by the powerful (for example, Franco's efforts to conceal all evidence of mass killings of Republicans after the end of fighting); and the historian's task is to find ways of using this body of evidence to discern some of the truth about the past.

In short, historians conceptualize, describe, contextualize, explain, and interpret events and circumstances of the past. They sketch out ways of representing the complex activities and events of the past; they explain and interpret significant outcomes; and they base their findings on evidence in the present that bears upon facts about the past. Their accounts need to be grounded on the evidence of the available historical record; and their explanations and interpretations require that the historian arrive at hypotheses about social causes and cultural meanings. Historians can turn to the best available theories in the social and behavioral sciences to arrive at theories about causal mechanisms and sources of human behavior; so historical statements depend ultimately upon factual inquiry and theoretical reasoning. Ultimately, the historian's task is to shed light on the what, why, and how of the past, based on inferences from the evidence of the present.

1.2 History of the Present?

Is there a place for historical thinking in understanding one's contemporary circumstances? What might be involved in writing a history of the present? The idea is not quite the contradiction it may appear to be. It is often enough that we find ourselves in the middle of complicated, confusing, and interwoven events locally, regionally, or globally—events that require much the same sort of conceptual and integrative work that the Manchu conquest of China or the Odessa mutiny requires for the traditional historian. For example, think of the Red Shirt protests in Thailand in 2009, or the financial crisis in September 2008. And think of the intellectual challenge presented for the contemporary observer to try to arrive at a somewhat detailed interpretation of what was occurring. This is an act of "historical apperception"—taking many separate pieces of evidence and experience and forging them together into a unified representation. And it seems to have a great deal in common with more traditional historical cognition.

There seems to be one specific way in which the task cannot be done at all. When we are in the midst of something big, we may be able to recognize that it is momentous without really being able to say what "it" is. That is because we do not yet know how it is going to turn out. Is it a popular revolution of the have-nots against Thailand's elites, or a short period of unrest? Is it the beginning of another Great Depression, or just a serious episode of financial turbulence? We cannot answer these questions until the events play out.

That point is fair enough, but it does not really end the discussion. There is still the question, what can contemporary observers do to understand and document an important event as it unfolds? And here the answer is very similar to traditional historical research. Observers can collect and record documents in real time. They can interview participants. They can view and interpret the communications of the powerful and the insurgents. And, on the basis of these kinds of investigations, they can begin to arrive at interpretations of what is occurring, over what terrain, by what actors, in response to what forces and motives. In other words, they can attempt to arrive at an evidence-based integrative narrative of what the processes of the present amount to. And this is very similar to the process that historians undergo in trying to make sense of the past.

Think, for example, of western academics who found themselves in Shanghai in the late 1930s. They were in a position to talk with ordinary people, Communist activists, and Guomindang officials. They were able to collect the ephemera of the social struggle that was underway. They were able to observe at close range the Japanese assault on the city. And, perhaps, they were forced to join one of the great mass evacuations in history, with tens of thousands of ordinary Chinese people fleeing the city on foot. These observers lived a bit of China's history; but they were also in a position to write a part of its current history in 1938.

We can extend these examples indefinitely. Think of the young African-American activists who went to the American South in 1963, who lived and made this piece of American history; and think of the perspective they were able to arrive at in conceptualizing America, 1963. And for some of these men and women, the discovery and writing of the history was itself an important part of the struggle.

So several things seem true. One is that there is a form of "historical apperception" that is just as necessary for understanding the present as for understanding the past. A second point is that a given "history of the present" is doubly contestable: the contemporary's angle of view may be limited enough that future historians will conclude that the apperception was fundamentally flawed; and the processes underway may turn out so differently from what was expected, that the mid-stream apperception may be judged basically misleading when the process is complete.

But a few other things are true as well. The participant has an immediate access to documents, speeches, and events that later historians can only envy; so by recording these observations the participant can lay a good foundation for later interpretations. The participant has often had direct experiences that give him or her a specific understanding of some aspects of the events—for example, the passions and motives associated with the period. And third, the participant's historical observations may in fact be remarkably acute, taking observations of current activities and constructing them into a historical representation that holds up well. So attempting to write a history of significant events in the present is a valid intellectual goal, and one that has a great deal in common with more traditional forms of historical knowing.

1.3 Metaphors for History

What kind of thing is "history"? Think of the history of the Roman Empire, or the history of Tokugawa Japan, or the history of the American banking system. We want to be able to conceptualize these complex stories as possessing some kind of unity over centuries of time, thousands of locations, and millions of lives. We want to be able to identify common threads of development, common themes or topics that continue to recur throughout the history of the period. And yet it is plain that history consists of unmeasured diversity and heterogeneity as well—individuals, psychologies, local conditions, aberrant princes, external threats, famines, floods, and panics. So historians are led to adopt different kinds of metaphors to attempt to provide a degree of unity to their subject. There are quite a few different metaphors that have been used to characterize history: a river, a tree, a labyrinth, an ocean, a mosaic, a landscape. Several are particularly worth unpacking, but the metaphor that I prefer is "pathway."

Here is how the river metaphor might work as a way of thinking about the "course" of history. Rivers have tributaries—rivulets of water flowing down hill into the broader concourse. History has "streams" of contributing events that lead to the larger outcome—the confluence of developments in the French medieval rural economy, the development of the fiscal crisis of the *Ancien regime*, and the emergence of a town-based bourgeoisie, for example, coming together to contribute to the unfolding of the French Revolution. There is a seeming unity to a river over time, even though the constituent water simply passes through continuously; analogously, one might view history as a "stream" of events that individual humans pass through, constituting a larger and more stable historical current. (Though of course we won't forget the Heraclitus paradox.) Rivers are to some extent constrained—by existing topography, but also by human artifacts (dams, levees, flood walls). Historical developments too are constrained by circumstances such as agricultural productivity, population levels, and warfare. Rivers sometimes change their course—for example, the occasional changes of course of the Yellow River over many centuries. But more commonly they become longstanding features of the terrain over centuries or millennia. Analogously, there is at least the semblance of long, steady periods of continuity of human affairs within human history—interrupted by crises and turning points. Rivers have a direction of flow—from north to south, from high ground to lower ground. And some interpreters of history have argued analogously for a direction of change in history as well—towards "progress," "modernization," greater administrative intensity, higher standards of living for the population, or greater democracy, for example. And rivers have a powerful momentum of their own—we can be swept away in the currents of the Mississippi River, as John Reed was swept up in the events of the Russian Revolution.

The river metaphor captures some of our intuitive thinking about history—tributaries, currents, stretches of turbulence. But it also conveys a necessity or inevitability that fails to come to grips with the deep contingency of history. A river has an inexorable course of flow—from high ground to low ground. And the topography essentially determines the shape and configuration of the river bed. This metaphor suggests that history too has an inevitable course or direction—which is profoundly untrue. Historical events have a profound aspect of contingency that is inadequately captured by the river metaphor.

How about the idea of history as a tree? Here, the idea is that there are "branches" in history—points where developments could have gone "left" or "right", and the next phases of history are dependent on the specific branches that have been taken before. America could have invested in canals rather than railroads—and its transportation history and subsequent urbanization would have been significantly different. (Robert Fogel makes an argument along these lines; Fogel, 1964.) The analogy isn't exactly between "tree" and "history"; instead, it is between the branches of a tree and the space of hypothetical historical possibilities. Actual history is one specific pathway through this tree of possibilities. Finally, trees have systems of "roots"—the structures under the ground and out of sight that explain the nutrition and growth of the tree. And how often have historians turned to expressions like "the roots of the Cold War extend back to X, Y, and Z."

This metaphor does a better job of capturing the contingency of history, in that it highlights that the actual course of history is simply the aggregate result of the branches or choices taken previously—with the clear understanding that other routes through the space of possibilities were possible as well (Ferguson, 1999a, Levy and Goertz, 2007). One of the obvious difficulties with the tree metaphor, though, is the extreme uncertainty that exists about the branches, the hypothetical alternative outcomes that might have put a given society on a different trajectory. For any given major historical event we can speculate with varying degrees of rigor about how things might have come out differently; but we can't really go very far down the route of the "alternative history" that might have ensued. So the idea of a "tree diagram of alternative histories" is only a metaphor, not something that could be accomplished through historical research.

Here is a third possible metaphor for history: history is an accumulation of pathways and roadways that embed human action over time. I find that this metaphor works well as a way to characterize the course of history. Paths are created by purposive agents, going somewhere with an understanding of the topography. Pathways become roadways, and they become systems of constraint and opportunity. And they sometimes become the elements or segments of larger systems with long historical and human consequences (for example, the Roman road system). Road systems illustrate the meaning of "path-dependence"; once the pathway exists, other routes across the terrain become less likely. And the metaphor illustrates as well the perpetual interaction of agent and structure that good historians almost always emphasize; the plasticity of social entities; the contingency of their specific properties; and their constraining power influencing human choices.

The pathways metaphor incorporates both diachronic and synchronic elements into our conceptualization of history. At a given time, history presents us with a given set of embodied constraints and opportunities that represent the accretion of the past as a context for the present. The system of roads penetrating through a medieval town represents a snapshot of its history over a 1000 years—and a set of frustrating obstacles to the contemporary driver. Marx puts the weight of history's legacy in these terms in the *Eighteenth Brumaire*:

> Men make their own history, but they do not make it as they please; they do not make it under self-selected circumstances, but under circumstances existing already, given and transmitted from the past. The tradition of all dead generations weighs like a nightmare on the brains of the living. (Marx, 1974, Chapter 1)

But there is a diachronic aspect of the metaphor as well: the structures that constrain the present today are themselves involved in a temporally extended process of modification and accretion from yesterday to today to tomorrow. The road system continues to evolve in response to contemporary needs and wants, and presents a new set of constraints and opportunities to the generation to come.

On this approach, history does not have any ultimate directionality; it is simply the sum of a long series of inventions, actions, interventions, and accidents over decades or centuries. At the same time, it is subject to a degree of explicability, in that earlier moments of historical development set the stage for choices and inventions in the next phase. Outcomes are "path-dependent", in that they depend critically on the circumstances and accidents of the past. But at the same time, there is a degree of "sunk costs," social momentum, and embodied infrastructure that make some historical developments much more likely than others.

The "pathways" motif works well as metaphor in characterizing the philosophy of history that you will find here. It captures the relative "stickiness" that history presents to the actor; certain actions are substantially more difficult in one set of circumstances than another. And the metaphor illustrates as well the perpetual interaction of agent and structure that I want to emphasize as a key feature of social life and the constitution of history—the plasticity of social entities, the contingency of their specific properties, and their constraining power influencing human choices.

Chapter 2
History and Narrative

2.1 Philosophy and the Historians

The approach that I am taking in this book asks abstract questions about historical processes and historical knowledge, but it does not derive from existing research traditions of the traditional philosophy of history. Instead, it takes its inspiration from the philosophy of the special sciences. I take the view that historians are attempting to make sense of the past in ways that can be supported by the evidence of the present. They are interested in identifying "significant" historical events or outcomes (e.g. the French Revolution, the outbreak of the American Civil War, the collapse of the Qing Empire); giving realistic and factual descriptions of these events; and answering questions about the causes and effects of these events. And they are interested in examining the intentions, goals, and meanings that were involved in historical actions by the actors who performed them. The task of the philosophy of history as I will pursue it is to analyze and assess the practice of outstanding historians in order to uncover the assumptions they make about the goals of historical inquiry, examine the ways in which evidence, theory, and inference can lead to discoveries within historical disciplines, and identify some of the conceptual and methodological difficulties that arise in the practice of historical investigation.

How, then, should the philosophy of history interact with the practice of working historians? The philosophy of history is challenged to discover and explore the most fundamental questions about historical inquiry and knowledge. How should this research be conducted? And how should the philosopher's development of the subject make use of the practice of the historian?

The guiding intuition is that historians implicitly define the rationality and objectivity of the discipline of historical knowledge; and philosophers can elucidate (and criticize) that ensemble of assumptions about historical inquiry and knowledge in a way that illuminates both the nature of historical knowledge and the ways in which current approaches might be strengthened. In other words, the philosophy of history can function as a conceptual enhancement for working historians, and it can function as a source of rational criticism of specific methods or approaches within contemporary historiography.

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Look at this question from the point of view of the historian, and we will find that the separation between "doing" and "reflecting upon" history is not as sharp as it might appear. For the best historians, there is no recipe for good historical inquiry and exposition. There are methods and practices of archival research, to be sure, and there are general recommendations like "be well informed about existing knowledge about your subject matter." But the great historians take on their subjects with fresh eyes and new questions. They often arrive at novel ways of framing their historical questions; they find new ways of using available historical evidence, or finding new historical evidence; they discover new ways of drawing inferences from historical data; they arrive at new ways of presenting their knowledge and narratives; and they question existing assumptions about "causation," "agency," or "historical period." As the historian grapples with the topic of research and the evidence that pertains to the topic, he or she is forced to think creatively about issues that go to the heart of historical inquiry and reasoning. In other words, the historian is forced to think as a philosopher of history, in order to achieve new insights into the problems she considers.

There is a less creative approach to historical research, of course. One can choose a familiar topic; seek out some new sources that have not yet been fully explored; adopt some familiar theoretical motifs; and place the findings into a standard narrative for publication. This mechanical approach resembles "normal science" for historians. But the results of this type of approach are inherently disappointing; it is unlikely in the extreme that new historical insights will emerge.

So when we consider the work of really imaginative historians, we find that the historian is functioning as a philosopher of history at the same time as he or she is developing an innovative approach to the historical question under examination. And this means that the philosopher can gain great insight by working very carefully with the writings of these great historians. The philosopher can probe questions of historical inquiry, historical reasoning, historical presentation, and historical knowledge, by thinking through these questions in conversation with the working historian.[1]

Consider a few examples that illustrate this productive possibility. First, consider the evolving state of affairs in historical treatments of the French Revolution. In the past 40 years historians have taken a shifting series of perspectives on the events, social conflicts, cultural circumstances, and political realities of the Revolution. New research and new narratives have emerged on the *ancien regime*, the revolution, the Terror, and the consolidation of power by Napoleon. Fertile historians such as Soboul, Cobb, Darnton, Schama, Sewell, or Chartier have tested and explored a variety of new perspectives—from Marxism, from social history, from cultural studies. And they have provided a much more nuanced body of knowledge about

[1] There is quite a bit of reflective work underway on the scientific foundations of relevant areas of the social sciences, in which practitioners of international relations theory, comparative politics, and globalization are rethinking the nature of scientific study of these forms of social processes. Particularly valuable are Lebow and Lichbach (2007), Elman and Elman (2003), Geddes (2003), and George and Bennett (2005).

the social and cultural reality of the Revolution. This body of work provides a rich domain of conceptual and historiographical material for the philosopher of history.

A second example is the lively debate that has occurred about comparative economic history of England and China. In the past 15 years historians of Chinese economic history have challenged standard models of economic development and have argued for a more balanced comparative economic history for Eurasia. This debate has moved into great detail in the effort to answer such basic questions as whether China's agricultural economy was declining, static, or rising in productivity in the eighteenth century; or whether the standard of living was higher or lower at opposite ends of Eurasia. Once again, a philosopher of history can find great stimulation to further conceptual and philosophical research by studying this debate in detail; the debate provides a living example of how historical knowledge is born. (This debate is considered in some detail in Chapter 8.)

So my answer to the primary question here is this: that the philosophy of history needs to be fully immersed in some specific historical debates involving the most creative and imaginative historians. Careful study of these debates and sustained interaction with historians like these will lead in turn to much more developed understanding of the nature of historical reasoning.

A good example of a working historian with a sophisticated philosophy of history is Robert Darnton. And his philosophy of history emerges very clearly from his numerous reviews of books on the period of the French Revolution in the *New York Review of Books* (Darnton, 1973, 1975, 1980, 1985, 1989, 1991, 2004). (Darnton's own book, *The Great Cat Massacre: And Other Episodes in French Cultural History*, was also an innovative contribution to historians' practice, including especially his adept use of tools of ethnography to illuminate a baffling and seemingly small incident in French history (Darnton, 1984). This book is discussed in Chapter 9.)

Written over roughly a 30 years period, Darnton's intelligent reviews provide a nuanced perspective on how the historiography of the French Revolution has changed. From the structural, class-centered approach of Albert Soboul, through Richard Cobb's insistence on *mentalités* or Simon Schama's person-centered telling of the story, it is possible to see a shifting scene of historians' judgments about causes, structures, ideas, movements, and scale. All by itself this is an important insight into historical understanding. And it illustrates an important fact about historical knowledge: no event is ever known with finality.

But it is also possible to look at Darnton's reviews themselves as an extended and implicit historiographical essay. In his commentary on the writings of others Darnton also reveals many of his own historical intuitions. And of course Darnton's own ethnographic turn in *The Great Cat Massacre* (Darnton, 1984)—worked out while Darnton was teaching an interdisciplinary seminar with Clifford Geertz—is itself an important step on the historiography of French social change. So the project of trying to discover whether there is a coherent and innovative philosophy of history embodied within these reviews is a fruitful one. Several points come out of this set of reviews quite vividly: for example, the deep contingency of historical change, the importance of the particular, the importance of experience and *mentalités*, the dialectic of events and agents, and the difficulty of framing a large historical event. And this provides an interesting new avenue of approach to the problem of formulating a philosophy of history, a different insight into what we can learn from observing the practice of great historians.[2]

2.2 What Is History?

Let us consider a foundational question: what is history? Most innocently, it is the human past and our organized representations of that past. We can of course write about the chronology of non-human events—the history of the solar system, the history of the earth's environment over a billion year expanse of time. But the key issues in the philosophy of history arise in our representations of the human past—a point emphasized in Collingwood's philosophy of history (Collingwood, 1946, pp. 215–216). And history is fascinating for us, because (in Marx's words) "Men make their own history, but not in circumstances of their own choosing" (Marx, 1974). That is to say: history reflects agency—the choices by individuals and groups; and it reflects constraining structures and circumstances. So historical outcomes are neither causally determined, nor entirely plastic and unconstrained. Therefore it is open to the historian to attempt to discover the historical circumstances that induced and constrained historical agents to act in one way rather than another, thus bringing about a historical outcome of interest. So we might begin by saying that history is a temporally ordered sequence of events and processes involving human doings, within which there are interconnections of causality, structure, and action, within which there is the play of accident, contingency, and outside forces.

But we might also say: there is no such thing as "history in general." The description just provided suggests that there is a comprehensible collection of historical processes that might be characterized as a "total" human history: population growth, urbanization, technological innovation, economic differentiation, the growth of knowledge and culture, and so on. But this impression is highly misleading. It suggests a degree of order and structure that history does not possess. There are only specific histories: histories of various conditions or circumstances of interest to us. Historical space is dense: at any given time there are countless human actions and social processes underway in the world, and the "cardinality" of historical events does not diminish over time. So to single out the history of something specific—agriculture, the French Revolution, modern science, Islam—is unavoidably to select, from the full complexity of events and actions, an abstract set of characteristics that will be traced through a process of development. And this in

[2] William Sewell is another good example of an historian who makes a strong contribution to the philosophy of history. His *Logics of History* (Sewell, 2005) offers a singular contribution to historiography, with careful, analytical attention to some of the problematic constructs and frameworks that underlie the ways in which scholars attempt to characterize and explain historical change.

turn raises the point that "history" depends partly on "what occurred" and partly on "what we are interested in."

This point does not undercut the objectivity of the past. Events and actions happened in the past, separate from our interest in them. But to organize them into a narrative about "religious awakening" or "formation of the absolutist state" is to impose a structure of interpretation on them that depends inherently on the interests of the observer. There is no such thing as "perspective-free history." So there is a very clear sense in which we can assert that history is constituted by historical interpretation and traditions of historical interest—even though the events themselves are not.

What, then, is historical representation? We want to know, represent, understand, and explain the past. This perspective emphasizes our cognitive or epistemic relationship to the past. We use facts in the present—ruins, inscriptions, documents, oral histories, parish records, and the writings of previous generations of historians—to support inferences about circumstances and people in the past. Here we can single out several ideas: the idea of learning some of the facts about human circumstances in the past; the idea of providing a narrative that provides human understanding of how a sequence of historical actions and events hangs together and "makes sense" to us; and the idea of providing a causal account of the occurrence of some historical event of interest. Notice that these descriptions invoke some of the important philosophical issues that arise in the philosophy of history: the role of interpretation of meaningful human actions; the role of causal explanation; the status of empirical knowledge of facts about the past; and the status of assertions about "meaning" of large historical events. Each of these formulations raises new and difficult issues for philosophical clarification.

But the cognitive relationship to the past is not the only relationship we have to history. We also possess an expressive or performative relationship to the past. We also create, interpret, fictionalize, mythologize, and valorize the past. And we use some of our stories about the past—our "histories"—to represent the right way of acting, good and bad political behavior, the character of one nationality as opposed to another, and to justify our conduct in the future. This feature of historical representation too raises philosophical problems. Do these stories have epistemic standing? Are some of these value-laden interpretations more justified than others? And can we sharply distinguish between the two kinds of representation of the past? (This aspect of history plays a key role in the formation of ethnic and national identities (Anderson, 1983; Kammen, 1991).)

2.2.1 Micro, Meso, Macro

Doing history forces us to make choices about the scale of the history with which we are concerned. Are we concerned with the whole of the Chinese Revolution, the base area of Yenan, or the specific experience of a handful of villages in Shandong during the 1940s? And given the fundamental heterogeneity of social life, the choice of scale makes a big difference to the findings.

Historians differ fundamentally around the decisions they make about scale. William Hinton provides what is almost a month-to-month description of the Chinese Revolution in Fanshen village—a collection of a few hundred families (Hinton, 1966). The book covers a few years and the events of a few hundred people. Likewise, Emmanuel Le Roy Ladurie offers a deep treatment of the villagers of Montaillou; once again, a single village and a limited time (Le Roy Ladurie, 1979b). Diane Vaughan offers a full study of the fateful decision to launch the Challenger space shuttle (Vaughn, 1996). She hopes to shed light on high-risk technology decision-making through careful study of a single incident. These histories are limited in time and space, and they can appropriately be called "micro-history."

At the other end of the scale spectrum, William McNeil provides a history of the world (McNeill, 1967) and a history of the world's diseases (McNeill, 1976); Massimo Livi-Bacci offers a history of the world's population (Livi-Bacci, 2007); Jared Diamond offers a history of the interrelationships between the Old World and the New World through the medium of weapons and disease (Diamond, 1997); and Goudsblom and De Vries provide an environmental history of the world (De Vries and Goudsblom, 2002). In each of these cases, the historian has chosen a scale that encompasses virtually the whole of the globe, over millennia of time. These histories can certainly be called "macro-history."

Both micro- and macro-history have their shortcomings. Micro-history leaves us with the question, "how does this particular village shed light on anything larger?". And macro-history leaves us with the question, "how do these grand assertions about causality really work out in the context of Canada or Sichuan?". The first threatens to be so particular as to lose all interest, whereas the second threatens to be so general as to lose all empirical relevance to real historical processes.

There is a third choice available to the historian, however, that addresses both points. This is to choose a scale that encompasses enough time and space to be genuinely interesting and important, but not so much as to defy valid analysis. This level of scale might be regional—for example, G. William Skinner's analysis of the macro-regions of China. It might be national—for example, a social history of Indonesia. And it might be supra-national—for example, an economic history of Western Europe. The key point is that historians in this middle range are free to choose the scale of analysis that seems to permit the best level of conceptualization of history, given the evidence that is available and the social processes that appear to be at work. And this mid-level scale permits the historian to make substantive judgments about the "reach" of social processes that are likely to play a causal role in the story that needs telling. This level of analysis can be referred to as "meso-history," and it appears to offer an ideal mix of specificity and generality.[3]

Here are a few works that represent good examples of meso-history: R. Bin Wong (1997), Kenneth Pomeranz (2000), and Charles Tilly (1990). Wong and Tilly define their scope in terms of supra-national regions. Pomeranz argues for

[3] The issue of causal analysis across levels of social and historical organization has received attention in recent years. Goertz and Mahoney focus attention on the importance of identifying the levels of analysis and discovering the causal relations that exist within and across levels (Goertz and Mahoney, 2005).

a sub-national scale: comparison of England's agricultural midland with the Yangzi region in China. Each pays close attention to the problem of defining the level of scale that works best for the particular task. And each does a stellar job of identifying the concrete social processes and relationships that hold this regional social system together.

Both macro- and meso-history fall in the general category of "large-scale" history. So let's analyze this conception of history. Large-scale history can be defined in these terms.

- The inquiry defines its scope over a long time period and/or a large geographical range;
- the inquiry undertakes to account for large structural characteristics, processes, and conditions as historical outcomes;
- the inquiry singles out large structural characteristics within the social order as central causes leading to the observed historical outcomes;
- the inquiry aspires to some form of comparative generality across historical contexts, both in its diagnosis of causes and its attribution of patterns of stability and development.

Large-scale history falls in several categories.

- History of the "long durée"—accounts of the development of the large-scale features of a particular region, nation, or civilization, including population history, economic history, political history, war and peace, cultural formations, and religion
- Comparative history—a comparative account, grounded in a particular set of questions, of the similarities and contrasts of related institutions or circumstances in separated contexts. For example, states, economic institutions, patterns of agriculture, property systems, bureaucracies. The objective is to discover causal regularities, test existing social theories, and formulate new social theories
- World history—accounts of the major civilizations of the world and their histories of internal development and inter-related contact and development

The choice of scale is always pertinent in historical analysis. And in many instances, I believe that the most interesting analysis takes place at the meso-level. At this level we get explanations that have a great deal of power and breadth, and yet that are also closely tied to the concrete historical experience of the subject matter.

2.2.2 Longue Durée

Let us turn briefly to a different kind of question of scale: the structure of historical time. Many historical changes take place on a human scale—the Great Depression came and went within the lived experience of many millions of people, and they were able to tell comprehensible narratives of the beginning, middle, and end. Likewise with periods of political transition and upheaval—the Vietnam War protests, the Reagan revolution, the Cold War. So these events can be scaled within the historical sensibilities of individuals who experienced them. But what about changes that are so extended and so gradual that they are all but imperceptible? How is history of the *longue durée* to be understood?

Think of some of the gradual processes of change that have important effects on human society: for example, soil erosion, water pollution, loss of jobs, inflation, diffusion of innovation, a firm's decline in market share, and a nation's decline of naval power, to name a heterogeneous list. And think about the very different time scales associated with large processes of change, from days to months to years to decades and centuries. Does the scale over which a change unfolds make a difference in the ability of an organization to respond? It does, at both ends of the spectrum. Is there a special problem for historical cognition posed by long, slow processes?

Paul Pierson's *Politics in Time: History, Institutions, and Social Analysis* (Pierson, 2004) raises some of the challenging research questions that are raised by the time scale of a historical process. He provides a very useful taxonomy of events in terms of "time horizon of cause" and "time horizon of outcome". This creates four categories of events around "long" and "short"; illustrations of each category include tornado (short-short), earthquake (long-short), meteorite (short-long), and global warming (long-long). And he points out that much research in the social sciences focuses on examples from the "short-short" category—events with discrete causes and time-limited effects. The issue of time scale is also invoked in the history of the *longue durée*, including particularly writings by Fernand Braudel and Emmanuel Le Roy Ladurie, where the historians of the *Annales* school paid particular attention to the long, slow changes in structures that influenced European history. We might say that these are examples of historical processes working "behind the backs" of the participants.

The sorts of changes I have in mind here run along these lines: a long, slow increase of population density relative to available resources; a gradual shift in the gender ratio or age structure of a population; the gradual silting of a river system and estuary; a slow erosion of a traditional system of values; and an extended process of increasing or decreasing tolerance between intermixed religious groups. In each case it is possible for the changes to be slow enough to defy recognition by historical participants; and yet each of these slow processes may have very important historical consequences.

The question here is a simple one: what are the methods of observation and inference through which historians can identify and investigate these sorts of long, slow processes? And what is the standing of such processes insofar as they stand outside the scope of events of ordinary historical experience? Given that participants have no basis for identifying the long, slow processes within which they swim, what is the status of the historian's hypotheses about such processes?

As for the question of how historians can identify these kinds of century-long processes: this task is really no more challenging than the problem of arriving at hypotheses about unseen processes in other areas of science. It takes ingenuity and imagination to hypothesize how a gradual increase in local violence might relate to slow demographic trends; but once the historical demographer turns her eye in this direction, it is no great leap to hypothesize that a rising male-to-female ratio may be a part of the cause (as Valerie Hudson and Andrea Den Boer argue in *Bare Branches: The Security Implications of Asia's Surplus Male Population* (Hudson and Den Boer, 2005)). Jack Goldstone's efforts to link the occurrence of revolution to slow demographic processes falls in this category as well (Goldstone, 1991). Demography and the natural environment offer many examples of long, slow processes that are relevant to human history. What is necessary, though, is a fairly rigorous ability to measure variables of interest at different points in time and to discover trends among these observations. In other words, the turn to cliometrics—quantitative observation of historical trends—is more or less essential to the history of the *longue durée*. And it is not surprising that the *Annales* historians were deeply interested in demographic history, price series, and historical measurements of economic activity.

So this answers part of the question: a history of long processes requires careful observations of quantities over time, and it requires the formulation of causal hypotheses about how these trends influence other historical circumstances of interest.

And what about the other question—the status of historical conceptions of these long, slow processes? They are not abstractions from the historical self-understandings of participants. By hypothesis, participants cannot perceive these sorts of processes. Instead, they constitute a more hypothetical historical structure that may nonetheless play a future role in the narratives participants tell about themselves. A slow process of climate change may be imperceptible at a given point in time. But once it is identified and articulated by the analytical historian the construct may come into popular consciousness; what was previously invisible may become part of the furniture of the popular narrative.

So if we conceptualized historical episodes along the lines of life events, then the *longue durée* would be forever outside of history. If, on the other hand, we include in our definition of history all the structures and trends that can be identified by analytical history, then the history of the *longue durée* is entirely comprehensible. Moreover, it is apparent that ordinary historical apperception can itself incorporate the theories of historians. And in this sense, the *longue durée* can enter back into ordinary historical experience.

2.2.3 Marc Bloch's History

Marc Bloch was one of France's most important medieval historians in the first half of the twentieth century, and he died at the hands of the Gestapo while serving in the Resistance in Paris in 1944. Bloch's historical imagination and his innovative research strategies qualify Bloch as one of the truly great historians of the twentieth century.[4]

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[4] Carole Fink's biography is an outstanding treatment of his thought and life (Fink, 1989); also important is *Marc Bloch, l'historien et la cite* (Deyon et al., 1997). Susan Friedman (Friedman, 1996) provides an excellent intellectual history of Bloch's development.

Here I am primarily interested in the substantive contributions Bloch brought to the writing of history. Bloch was one of the founders of the *Annales* school of history, along with Lucien Febvre, and he left a deep impression on subsequent historical imagination later in the twentieth century. In particular, he gave a strong impetus to social and sociological history, and he brought a non-Marxist materialism into the writing of history that represented a very important angle of view. The largest impact of the *Annales* school, through the writings of such historians as Febvre, Bloch, Ladurie, Braudel, and Le Goff, is the set of perspectives it forged for the understanding of social and cultural history. This group of historians emphasized the value of looking closely at the structures and experiences of ordinary people as one foundation for the formation of history. This required the invention of new historical vocabulary and new sources of data. And Bloch was central in each area.

Bloch and the other scholars of the *Annales* school of French history characteristically placed their analysis of historical change within the context of the compelling structures—economic, social, or demographic—within which ordinary people live out their lives. They postulate that the broad and enduring social relations that exist in a society—for example, property relations, administrative and political relations, or the legal system—constitute a stable structure within which agents act, and they determine the distribution of crucial social resources that become the raw materials on the basis of which agents exercise power over other individuals and groups. So the particular details of a social structure create the conditions that set the stage for historical change in the society. (André Burguière provides an excellent discussion of the *Annales* school; Burguière, 2009.)

The *Annales* school also put forward a concept that applies to the temporal structure of historical change: the idea that some historical changes unfold over very long periods of time and are all but invisible to participants—the history of the *longue durée*. So large enduring structures, applying their effects over very long periods of historical time, provided a crucial part of the historical imagination of the *Annales* school. Bloch's treatment of French feudalism illustrates a sustained analysis of a group of great structures enduring centuries over much of the territory of France (Bloch, 1964), as do Le Roy Ladurie's treatment of the causes of change and stasis in Languedoc in *The Peasants of Languedoc* (Le Roy Ladurie, 1974) and Fernand Braudel's historical formulation of the Mediterranean world (Braudel, 1995).

Several of Bloch's books are most significant. *Feudal Society* (Bloch, 1964) is a foundational contribution to our understanding of the institutions and social relations of French feudalism—the manorial system, vassalage, and kingship. And his writings about French agricultural history are of special interest (Bloch, 1966, 1967). These books document many important aspects of French rural social life, both high and low. But even more importantly, Bloch brought several distinctive ideas into historical writing that continue to serve as illuminating models about how to understand the past. One is a version of materialist historical investigation. Bloch provides great insight into the forces and relations of production in rural medieval France and the material culture of the Middle Ages. A second is an adept ability to single out and scrutinize some of the forms of political structure and power that defined French feudal society. And a third is a subtle way of characterizing the social whole of medieval society and mentalité that owed much to Durkheim. In a curious way, then, Bloch's work picked up some of the themes that constituted modern social theory in Marx, Weber, and Durkheim.

Bloch's materialism is most evident in *French Rural History* (Bloch, 1966). Here Bloch gives a detailed and scholarly treatment of the social and community consequences of the diffusion of the heavy wheeled plough. He provides a careful technical analysis of the advantages and exigencies of the heavy plough, which was most suited to the heavy soil of northern France. And he works out the social prerequisites of this technology—fundamentally, a degree of community organization that could successfully coordinate land use consistent with ownership and the turning radius of the heavy implement and its team of horses. The technical requirements of the plough required certain social arrangements. The social structure of the northern French village satisfied these conditions—in striking contrast to the looser coordination found in southern French villages.

This is materialism; but it is not especially Marxist materialism. It does not give primacy to class relations. And it does not support any kind of teleology in historical development. But the central point was clear. Bloch paid close attention to the concrete social relations that obtained in rural France, and he attempted to discern the complex system of social life and agricultural technology that constituted peasant agrarian life in certain regions of France. In particular, Bloch sought to demonstrate that a major technology—cultivation with the heavy plough—incorporates and implicates a whole complex social and cultural system. And a major part of social history is to discover the sequence of adjustments through which the technology system is incorporated.

The Durkheim part of the story is also an important one. Durkheim was a major influence on French social thought in the first decades of the twentieth century, and the vector to Bloch was particularly direct. Bloch and his generation were greatly influenced by Durkheim's journal, *L'année sociologique* (Burguière, 2009; Rhodes, 1978). Bloch brought into his historical writing a deep sensitivity to the social reality of communities, moralities, and social collectivities. Susan Friedman (Friedman, 1996) argues that Bloch's historical sensibilities and methods were deeply influenced by the debates among the historians, sociologists, and geographers that set the terms of Bloch's development; but that ultimately his thinking remains "historical."

> Even in his later years when he came closest to Durkheimian sociology, Marc Bloch remained essentially an historian. He was an historian in the sense that his primary interests lay in change and differences rather than laws and theory and that the problems which he chose to address were human ones rather than those of the physical environment. (Friedman, 1996, Chapter 10)

The final feature of Bloch's thought to highlight is his vocabulary of structure and power in his treatment of French feudalism. There is a parallel with Weber in this body of thinking. Bloch spent a year studying in Germany and was presumably aware of Weber's thought, although there is no clear evidence of direct influence. But there are several ways in which some of Bloch's thought parallels Weber's. One is in his use of ideas about historical concepts that are similar to Weber's concept of ideal types. And the other is his careful analysis of the historical realities of relations of power and social structures that embody power.

Let us examine more closely Bloch's treatment of the nature and development of French medieval agriculture. His treatment brings together the history of technology, the social relations of rural France, and the material culture that bound social life and work together in early medieval France. Here I will draw some important lines of argument in *French Rural History* (Bloch, 1966). The heart of what I want to emphasize in Bloch's treatment of French agriculture is the notion that there are distinctive and enduring practices that embodied this agricultural system; that these practices can be identified through various markers (place names, agricultural implements, and field shape, for example); and that they are distinctive of this region in this *longue durée*. Agricultural practice is thus an important example of a dispersed set of knowledge and techniques within a population, transmitted by social mechanisms that can be studied, with long-standing implications for such things as commercial development, transportation, movements of peoples, and the transmission of ideas. "An agrarian regime is not characterized solely by its crop rotation. Each regime is an intricate complex of techniques and social relations" (Bloch, 1966, p. 35).

Techniques of cultivation represent a fairly visible illustration: the practical knowledge, tools, and techniques associated with the growing of crops and the preparation of soil represent a specialized knowledge that diffused perceptibly through France in the Middle Ages. Field shape is one of the compelling examples that Bloch analyzes—the long rectangular fields of northern France, in contrast to the patchwork of irregular geometries of southern France. Crop selection and cultivation varied across regions—"the rules governing cultivation varied considerably according to the region" (p. 26). It is possible to discern different systems of crop rotation across the map of France—all embodying attempts to allow the soil to recover its natural fertility, but implemented in regionally and culturally specific ways. This body of activity and practice reflects a form of "local knowledge", embodied in the practices, tools, and folk beliefs conveyed through concrete local mechanisms of influence and education.

Bloch emphasizes throughout the importance of regional variation of agricultural practices—another marker of socially transmitted forms of local knowledge. He writes, "When one considers all the patient observation, practical intuition and willing co-operation, unsupported by any proper scientific knowledge, which from the dawn of our rural history must have gone into the cultivation of the soil, one is filled with feelings of admiration akin to those which inspired Vidal de la Blache" (p. 26).

> The exact geographical distribution of these two rotations [biennial and triennial] has not so far been established. It would probably not be difficult to reconstruct the pattern as it was in the late eighteenth century, before the more flexible rotation introduced by the agricultural revolution put an end to fallowing; but for this we should need detailed studies which are at present lacking. What is certain is that the two systems occupied distinct blocks of territory, and had done so since the Middle Ages. (p. 31)

Consider the main forms of evidence that Bloch uses in establishing the nature, distribution, and evolution of social practices in medieval agriculture: place names, estate surveys, edicts, rustic calendars, village groundplans, census records, and seigneurial archives. One of Bloch's recurring sources of evidence for varying social practices is linguistic; thus, in describing systems of triennial rotation he writes that "the names for these divisions vary with the region and include *soles*, *saisons*, *cours*, *cotaisons*, *royes* or *coutoures*, and in Burgundy, *fins*, *épis* or *fins de pie"* (p. 30). Likewise, he offers inventory of a variety of words used to describe bounded parcels: "quartiers, climates, cantons, contrées, bènes, triages, delles" (p. 38). These forms of specialized vocabulary found in historical records permit Bloch to arrive at rigorous and data-grounded conclusions about changes in the agrarian regime of France over a very long time.

It is worth noting the play of contingency and opportunism in Bloch's historical vision. He describes, for example, the gradual increase in field size as the plough is driven a little beyond its legal limit, year after year (p. 37). Here is an instance of the opportunism of the medieval actor leaving a permanent imprint upon the land. On the other hand, Bloch identifies the role of compulsion as an ineffable mark on the face of the agrarian community: "Only a society of great compactness, composed of men who thought instinctively in terms of community, could have created such a regime" (p. 45). Another telling observation: "How true it is that all rural customs take their origin from an attitude of mind! In 1750, when there was a proposal to introduce into Brittany a modified form of the common herd, under which the arable would still be protected, the representatives of the Breton Estates rejected as unpracticable a measure accepted as part of the natural order by the peasants of Picardy, Champagne and Lorraine" (p. 59).

Bloch's thinking is deeply spatial; he is frequently drawn to imagine how the social practices he describes would be distributed on a map of France. Thus: "In the present state of our knowledge, a distribution map would show the following as areas of enclosure: the whole of Brittany, ... Maine; Perche; the bocages of Poitou and Vendée; most of the Massif Central, ... Bugey and the Pays de Gex; and finally the Basque lands of the extreme south west" (p. 59). As Friedman (1996) points out, the discipline of historical geography had become important in French academic circles in the late nineteenth century, and Bloch was certainly influenced by Paul Vidal de la Blache and his followers.

Interestingly for the period, Bloch takes issue with other historians' efforts to account for regional differences in terms of ethnicity or race. Thus he takes up earlier efforts to explain differences in agrarian regime on the basis of *Volkgeist*: "'Race' and 'people' are words best left unmentioned in this context; in any case, there is nothing more elusive than the concept of ethnic unity. It is more fruitful to speak of types of civilization" (p. 62). I would interpret his points here as demanding a more disaggregated account: an account that looks for a more fine-grained analysis of geography, local practice, inherited agrarian regime in our historical efforts to account for specific regional outcomes.

2.2.4 Comparative History

One of Bloch's most important contributions was to reinvigorate the idea of "comparative history." Bloch believed that we could understand French feudalism better by putting it into the context of European legal and property regimes; and more broadly, he believed that the careful comparison of agrarian regimes across time and space could be an important source of insight into human societies. Moreover, he did not believe that the cases needed to be sociologically connected. He thought that we would learn important new truths by comparing medieval French serfdom with bonded labor in Senegal in the twentieth century, and one of the innovations developed in Bloch's editorship of *Annales d'histoire économique et social* was precisely his openness to this kind of comparison.[5]

What is "comparative history"? Most basically, it is the organized study of similar historical phenomena in separated temporal or geographical settings. The comparative historian picks several cases for detailed study and comparison, and then attempts to identify important similarities and differences across the cases. Theda Skocpol's treatment of social revolution is a case in point (Skocpol, 1979); Skocpol is interested in examining the particulars of the French, Chinese, and Russian Revolutions in order to discover whether there are similar causal processes at work in these three cases. Other possible comparative research projects might include—

- Slave-based agriculture in Rome and the antebellum United States South
- Rituals of royal healing in medieval France and Bali
- Religious pilgrimages in Islam and Christianity
- Periods of rural unrest in Britain and Malaysia
- Modern economic development in England, France, and China
- Frontier societies in nineteenth-century North America and seventeenth-century Russia
- Feudal legal institutions in eastern and western Europe
- Processes of urban development in London, Mumbai, and Berlin

What is the intellectual purpose of comparative history? What might we expect to learn through careful examination of sets of cases like these? What sorts of knowledge can comparative historical research provide? There might be several goals.

First, we might imagine that some of these phenomena are the effect of *similar causal processes*, so comparison can help to identify causal conditions and regularities. This approach implies that we think of social structures and processes as being part of a causal system, where it is possible to identify recurring causal conditions. This seems to be Skocpol's approach in *States and Social Revolutions*, though she later extends her views in an article mentioned below. Researchers often make use of some variant of Mill's methods in attempting to discover significant patterns of co-variation of conditions and outcomes.

Second, we might have a theory of *social types and subtypes* into which social formations fall. The purpose of comparison would be to identify some of the

[5] Bloch's early ideas about comparative history are presented in his 1928 article, "Toward a Comparative History of European Societies," (Bloch, 1953); see also William Sewell, "Marc Bloch and the Logic of Comparative History" (Sewell, 1967).

sub-types of a general phenomenon such as “slave economy”. This sounds pretty much like the approach that Comte and Durkheim took; it corresponds to a social metaphysic that holds that there are finitely many distinct types of society, and the central challenge for sociology is to discover the structural characteristics of the various types.

Third, we might have a fundamentally *functionalist view of social organization*, along with a basic repertoire of social functions that need to be performed. We might then look at religious systems as fulfilling one or more social functions—social order, solidarity, legitimacy—in alternative ways. Comparison might serve to identify functional alternatives—the multiple ways that different social systems have evolved to handle these functional needs.

Another possible purpose of comparative history is to attempt to discover *historical and social connections* across separate historical settings. For example, examining different methods of labor control in different fascist countries in the 1930s may give us a basis for assessing some of the forms of influence that existed between these movements and governments. And Victor Lieberman’s comparative study of the rise and fall of state power in France and Burma falls in this category as well (Lieberman, 2003).

An important application of comparative history stems from the increasing availability of similar *quantitative data across widely separated geographical settings*. Demographic and economic data from Europe, North America, China, Japan, and India now permit detailed comparison of demographic and economic processes in these various settings, and sophisticated quantitative techniques are now allowing comparative researchers in these fields to arrive at significant reassessment of received views about fundamental social processes at the local and regional level. Malthusian ideas about Asian and European population processes have been challenged on the basis of more fine-grained data now available; likewise, standard assumptions about the standard of living in Europe and Asia have been re-examined. Historical demography and economic history have been especially enriched by a surge of rigorous work along these lines; we will return to these examples in Chapter 7, including especially the example of the Eurasia Project on Population and Family History.

Finally, we might have a social metaphysics that emphasizes *contingency and difference*. This perspective differs from the first several ideas, in that it looks at structured comparative study as a vehicle for identifying *difference* rather than underlying similarity. Examining the histories of Berlin and Delhi may shed a great deal of light on the range of social forces and historical contingencies that occurred in these ostensibly similar cases of “urbanization”. Here the goal of comparison is more to discover alternatives, variations, and instances of path dependency. Charles Sabel and Jonathan Zeitlin’s analysis of alternative forms of capitalist development in “Historical Alternatives to Mass Production” illustrates this possibility (Sabel and Zeitlin, 1985; see also Sabel and Zeitlin, 1997).

So there are a number of different intellectual purposes we might have in undertaking comparative historical research. How have other historians and social scientists understood these issues? Theda Skocpol and Margaret Somers address precisely this issue in "The Uses of Comparative History in Macrosocial Inquiry" (Skocpol and Somers, 1979). Their analysis highlights three distinct models of analysis that can underlie comparative inquiry:

> There are, in fact, at least three distinct logics-in-use of comparative history. One of them, which we shall label comparative history as *macro-causal analysis*, actually does resemble multivariate hypothesis-testing. But in addition there are two other major types: comparative history as the *parallel demonstration of theory*; and comparative history as the *contrast of contexts*. Each of the three major types of comparative history assigns a distinctive purpose to the juxtaposition of historical cases. Concomitantly, each has its own requisites of case selection, its own patterns of presentation of arguments, and—perhaps most important—its own strengths and limitations as a tool of research in macrosocial inquiry. (Skocpol and Somers, 1979, p. 175)

R. Bin Wong offers a different view of the value of comparison in historical studies in his important comparative study of Chinese economic and political development (Wong, 1997). Wong argues that comparison allows the historian to discover what is distinctive about a particular series of historical developments. Features which perhaps looked inevitable and universal in European economic development look quite different when we consider a similar process of development in China; we may find that Chinese entrepreneurs and officials found very different institutions to do the work of insurance, provision of credit, or long-distance trade. Likewise, elements that might have been taken to be *sui generis* characteristics of one national experience may turn out to be widespread in many locations when we do a comparative study.

Ultimately it seems that there two fundamental intellectual reasons for being particularly interested in historical comparisons. One is the hope of discovering recurring social mechanisms and structures. This is what Charles Tilly seems to be about in his many studies of contentious politics. And the second is the hope of discovering some of the differentiating pathways that lead to significantly different outcomes in ostensibly similar social settings. The first goal serves the value of arriving at some level of generalization about social phenomena, and the second serves the goal of tracing out the fine structure of the particular.

2.2.5 New Understandings of China's Cultural Revolution

Let us consider a more current example that raises questions about the nature of history. An important area of current historical research in China has to do with arriving at a better understanding of China's Cultural Revolution. Recent research on the extent of violence during the Cultural Revolution has been one stimulus to this renewed emphasis. The prevailing assumption among China historians was that violence during the Cultural Revolution was relatively limited and incidental, rather than wide-spread and orchestrated. However, Song Yongyi, a Chinese-born American scholar and participant in the Cultural Revolution, has created a large database on the events of the Cultural Revolution, including especially an effort to document the killings and massacres that occurred during this period (Song, 2008).

Song and other contemporary researchers assert that deliberate mass killings were much more extensive during the Cultural Revolution than previous accounts have indicated. Song estimates that more than 50,000 people were killed during the purge of the Mongolian Communist Party alone, and he attributes to an internal party document a figure of 1.72 million deaths during the period of the Cultural Revolution (Song, 2008). Similarly, sociologist Yang Su carefully documents deliberate massacres in Guangdong, Guangxi, and Hubei involving thousands of innocent people (Su, 2006). So what is the truth of the matter? Was the Cultural Revolution much more violent than it has previously been understood to be?

The question is relevant to the philosophy of history because it raises important questions about historical knowledge and understanding. A vast amount has been written about the Cultural Revolution—by western scholars and by Chinese people who participated in the period or were victims of its violence. Tony Chang's 1999 annotated bibliography of documents and reference works in English includes over a thousand references (Chang, 1999), and dozens of memoirs of Red Guard cadres and victims have been published in English, including Yuan Gao's *Born Red* (Gao, 1987). We have both first-hand accounts and careful academic scholarship that document many aspects of this period of China's recent history. So in one sense, we are in a position to know a lot about this period of China's history. And China scholars have asked the "why" question as well—why did it take place? For example, Roderick MacFarquhar's multivolume history of the period, culminating in *Mao's Last Revolution*, goes into great detail about the politics that surrounded the Cultural Revolution (MacFarquhar and Schoenhals, 2006).

We might want to say, then, that the history of the Cultural Revolution has been written. But as Song Yongyi demonstrates, this would be incorrect, in two ways. First, the scope of the violence and the ways in which it was perpetrated—the military and political institutions that were involved deeply in the transmission of the violence across China—these factual aspects of the period of 1966–1976 are still only partially known. And there is reason to believe that the remaining areas of ignorance are likely to substantially change our interpretation of the events. In brief, it seems likely that the scope of violence and killings is substantially greater than what historians currently believe, and the degree of deliberate political control of the instruments of disorder is greater as well. So the simple factual question, what happened? is still to be answered in many important areas. More would be known if the authorities were to make the official archives available to scholars; but this has been a highly sensitive and secretive subject since 1989. Researchers like Song have been arrested and jailed in China for their efforts to gather materials from publicly accessible sources (Rosenthal, 2000).

Even more challenging than the factual story, though, is the explanatory story. We do not yet have a good understanding of why this period of upheaval took place; what the social and political causes were, what the institutions were that facilitated or hindered the spread of disorder, and how these events aided or impeded the political agendas of powerful figures and factions in China.

So the history of the Cultural Revolution still remains to be written. Fortunately, a new generation of scholarship is emerging that promises to greatly deepen our understanding of this period of recent Chinese history. Important new perspectives are offered in Joseph Esherick, Paul Pickowicz, and Andrew Walder's recent edited volume, *China's Cultural Revolution As History* (Esherick et al., 2006). The research presented in this volume differs from the previous generation of research in several important ways. First, it pays much more attention to the question of organized violence, as noted above. Second, it is much less concerned with formal structures of party organization, ideology, and command, and more concerned with the social realities that China experienced during this decade. Third, several scholars make a strong effort to push down into the local and regional experiences of the Cultural Revolution. For example, Xiaowai Zheng's essay on the Red Guards at Qinghua University delves into the specific local issues and strategies of contending groups of students, and she makes extensive use of oral history interviews of people who were participants in the movement at the relevant time. Yang Su makes use of recently available archives from communes and districts in the three provinces he studies, to get a more accurate understanding of the episodes of mass killing that took place in these provinces. And Jiangsui He pushes aside the rhetoric of "evil landlord" to get a better understanding of the persecution and death of one particular Shaanxi man, Ma Zhongtai, and the social and village relations that framed his political persecution. In each case we get a more granular understanding of the processes and human experiences that constituted the Cultural Revolution, and we are in a better position to be able to conceptualize and explain this large, complex historical event.

The current rethinking that is underway about China's Cultural Revolution presents us with a very real question of historical epistemology: how much can we ultimately know about a vast and important event, for which there are voluminous archival sources and surviving witnesses? Can we hope to come to a "final" and approximately true interpretation of these events? And can we learn something important about social movements and political institutions from this history?

2.3 Narratives of History

Representing history often takes the form of creating a *narrative* of events. Complicated things happen: riots occur, military coups take place, governments collapse. The happenings consist of a myriad of events and actions, many social actors, and a range of political interests and grievances. We want to know what happened; who did what; and who is responsible for the course that events took. It is one of the tasks of historians, journalists, and commentators to arrive at accounts of complicated things that answer many of these questions. And we want those accounts to be objective, truthful, and unbiased. Each account is a creative act of selection and narrative construction; the analyst has to sort out the evidence that is available to him or her and arrive at a chronology and a causal interpretation that makes sense, based on the evidence.

People sometimes imagine that history is narrative, full stop.[6] This is not the case; there certainly are important forms of historical writing that do not take the form of narrative. At least as important in much historical scholarship is what might be called "synchronic history"—research aimed at exploring the texture and interrelatedness of persons, practices, and institutions of a given time in the past. But let us consider some of the logical and pragmatic features of narrative, since there is no disputing that this is one important variety of historical representation.

What is a narrative? Most generally, it is an account of the unfolding of a series of events, along with an effort to explain how and why these processes and events came to be. A narrative is intended to provide an account of how a complex historical event unfolded and why. We want to understand the event in time. What were the contextual features that were relevant to the outcome—the settings at one or more points in time that played a role? What were the actions and choices that agents performed, and why did they take these actions rather than other possible choices? What causal processes—either social or natural—may have played a role in bringing the world to the outcome of interest? (For example, the Little Ice Age beginning in the sixteenth century pushed Europe's population into different patterns of cultivation and fishing, with major consequences for subsequent developments; (Fagan, 2000). So this natural event would play a significant role in the narrative of population change during this century.)

So a narrative seeks to provide hermeneutic understanding of the outcome—why did actors behave as they did in bringing about the outcome?—and causal explanation—what social and natural processes were acting behind the backs of the actors in bringing about the outcome? And different narratives represent different mixes of hermeneutic and causal factors. Some are primarily actor-centered and interpretive—who said what, who influenced the decisions, the reasons and motives that ultimately prevailed with the president and top national security officials. A key goal of the narrative is to clarify the reasoning, motives, and dynamics among decision-makers that led to the outcome. Other historians, treating the same topic, may give greater importance to large features of the international environment, the economic and material factors that influenced the course of affairs.

Narratives about specific momentous decisions affecting war and peace have an important feature in common: they single out a fairly brief historical moment and focus on the proximate actions and causes that created the outcome. This is an instance of "micro-history"—an effort to explain and understand an important but

[6] If there is a unifying theme to the philosophy of history in the past 15 years, it is the "linguistic turn" represented by Frank Ankersmit and others: the idea that narrative is the key distinguishing form of historical representation, and that the rhetorical and linguistic features of narrative should play a key role in the philosophy of history (Ankersmit and Kellner, 1995). On this approach, we should attempt to understand historians' writings in something like the way that we analyze literature. The approach taken in this book is one that is closer to the social sciences; my approach emphasizes the cognitive and semantic content of historical knowledge. The key issues are to be able to provide good interpretations of the causal analysis of social processes and empirically supportable interpretations of historical actors that play central roles in historical explanation.

bounded event. Is it possible to construct narratives of more extended historical processes? Certainly it is. Consider histories of World War II, the Ottoman Empire, or the Qing Dynasty. These are each large complexes including thousands of events and conditions over an extended period of time. Histories of these topics often take the form of chronologically organized presentations of occurrences and conditions, with a narrative storyline that attempts to hold these events together in a single story. There may also be an effort to break down the history topically or regionally—"War in the Pacific; North Africa; Western Europe" or "Technology; Intelligence; Supply and Industry; Command; Genocide". But for the history to take the form of a narrative, there needs to be an organized effort to weave the account into a somewhat coherent story; a series of intertwined events and conditions leading eventually to an outcome.

A crucial and unavoidable feature of narrative history is the fact of selectivity. The narrative historian is forced to make choices and selections at every stage: between "significant" and "insignificant", between "sideshow" and "main event", and between levels of description. (Is World War II better described at the level of generals and policy-makers or infantrymen and factory workers?)

It has to be acknowledged that there are often multiple truthful, unbiased narratives that can be told for a complex event. Exactly because many things happened at once, actors' motives were ambiguous, and the causal connections among events are debatable, it is possible to construct inconsistent narratives that are equally well supported by the evidence. Further, the intellectual interest that different observers bring to the happening can lead to differences in the narrative: one observer may be primarily interested in the role that different views of social justice played in the actions of the participants; another may be primarily interested in the role that social networks played, so the narrative is structured around network connections; and a third may be especially interested in the role of charismatic personalities, with a consequent structuring to the narrative. Each of these may be truthful, objective, unbiased—and inconsistent in important ways with the others. So narratives are underdetermined by the facts. And there is no such thing as an exhaustive and comprehensive telling of the story—only various tellings that emphasize one set of themes or another. That said—it is entirely possible that a given event will have provided enough factual data in the form of witness reports, government documents, YouTube videos, etc., that the main sequence of events, cast of actors and responsibility for events are unambiguous.

Another crucial feature of the genre of narrative history is the tension between structure and agency. Historians differ about where to set the balance between constraining structures and choosing agents. Partially this is a difference of opinion about the relative weight of various kinds of historical factors; but it is also a disagreement about what is interesting—choices or background conditions.

What are the criteria of success for a historical narrative? To start, there is the issue of the factual claims included in the account. A narrative of Abraham Lincoln's presidency that gets the names of the members of his cabinet wrong will not do well in critical judgment of other historians. Second, there is the overall persuasiveness and foundation in evidence of the interpretations of actions that are offered. Third, the causal claims that the account advances will be tested for their empirical and logical foundations. If the claim is made that some aspect of Andrew Jackson's presidency was influenced by the fragility of current banking institutions, we will want to assess whether this financial feature could be judged to have this result in the circumstances.

These are criteria that relate directly to the epistemic status of the many claims that the narrative advances. In addition, it is plausible that we evaluate narratives according to non-evidentiary criteria: the coherence of the story that is told, the degree of fit between "our" interest in the historical moment and the content of the narrative, and the degree of "lean" comprehensiveness the author provides. Does the author provide enough of the right sorts of details to make the story comprehensible, without overwhelming the reader with a thicket of extraneous facts?

Some of these criteria are clearly epistemic, having to do with evidence and credibility. But others are more aesthetic and interest-based, having to do with how well the account fits our expectations and interests. And this fact seems to set a bound on the degree to which one account is objectively superior to another.

2.3.1 Selectivity: China at War

Consider a mid-range example of historical research: Stephen MacKinnon's book, *Wuhan 1938* (MacKinnon, 2008). MacKinnon offers a short account of the suffering that China experienced during the anti-Japanese war (1937–1945) through the lens of the defense of the city of Wuhan in 1938. MacKinnon focuses on the strategically and historically crucial role that Wuhan played in the unfolding of Japan's war of conquest over China. Wuhan is a tricity on the upper Yangzi, including Hankou, Hanyang, and Wuchang in close proximity at the juncture of the Han and Yangzi rivers. In 1938 it had a combined population of roughly two million, and hundreds of thousands of refugees soon crowded into the city. The location of Wuhan along the Yangzi placed the city in a central position from the point of view of Japanese war planning: capturing Wuhan would leave central China open to rapid conquest. After the rapid fall of Shanghai and other coastal cities, it was expected that Wuhan would fall quickly as well. In fact, the defense of Wuhan was much more effective than previous efforts had been, and the Chinese military was successful in delaying Japan's offensive into the interior by a crucial 10 months. When it eventually fell, Republican forces were able to fall back to Chongqing, and though the Japanese subjected the wartime capital to intensive bombing, they did not succeed in capturing the city. So the prolonged defense of Wuhan set the stage for a turning point in the Chinese resistance to Japan.

MacKinnon provides a schematic military history of the Japanese assault on Wuhan. But the book is not primarily an exercise in military history. Instead, MacKinnon gives focused attention to the civilian part of the story: the burst of journalism and political debate that took place in the city, the great expansion of social services for orphans and displaced persons, and the mobilization of students and other young people in support of the war effort. The cultural experience of Wuhan is as important a part of the story as the military events.

The topic of Wuhan and wartime China is inherently interesting and important. But it is also valuable from the point of view of historiography. Consider the choices that a historian must face in setting out to write a history of an event of the scope of Wuhan 1938. This event is more localized and limited than "the French Revolution" or "British colonialism in South Asia." At the same time, it is far more complex and multi-stranded than events such as "the assassination of President Lincoln" or "MacArthur's decision to cross the Yalu". The Wuhan story involves millions of people, military organizations of great complexity, movements of population, rapidly changing political circumstances, the creation of dozens of newspapers, and shifts in popular culture. And the consequences of the Wuhan episode are complex and unexpected as well. So the historian is forced to decide which threads he or she will focus on; what she wants to explain; and how much of the story to attempt to tell.

Consider the wide range of questions that could be posed about this piece of China's history: What were the actions and deployments of the Japanese and Chinese military forces in the middle Yangzi region during 1938? What was the nature of the human experience of civilians in Wuhan during the period of assault, bombardment, and destruction? How did circumstances of Guomindang leadership and power relationships influence the behavior and deployment of the Chinese military? What role did Communist forces and leaders play in the defense of Wuhan? What influence did the defense of Wuhan have on later events in the conduct of the war? How was the battle of Wuhan captured in popular memory in China? What influence did this historical moment have on future developments of politics or culture?

So one could try to use available historical sources to tell a fairly straightforward factual narrative; one could give an interpretation of the actions and choices of the leaders and generals; one could attempt to reconstruct the experiences and memories of ordinary Chinese people who lived through these events; and one could offer an analysis of historical causation: X led to Y, Y had important consequences Z. The point here is a simple one: each of these approaches is a different kind of historical reasoning and presentation, and each involves a somewhat different kind of historical reconstruction. It is possible to interweave these approaches; but their foundations in evidence and reasoning are fairly distinct. So many histories of Wuhan could be written; and they might all be grounded in roughly similar bodies of historical evidence.

2.3.2 Narrative and Bias

The accusation of bias is a particularly troubling one for a historian. What we want from the historian and the journalist is easily described, though achieved with difficulty. We want an account that provides an accurate and truthful narrative of the events, based on the best available factual and historical information. We want an account that avoids the biases of the actors, including especially those of the most powerful actors who have the greatest capacity to shape the story—the government, the military, and the major parties. We want an investigator who is able to question his or her own initial assumptions—sympathy for the underdog, patient acceptance of the government's good intentions, or whatever. And we want a narrative that provides a balanced *synthesis* of the many events of the time period into a storyline with a degree of coherence: what the major events were, what choices were made by the actors, what the motivations of the actors were, and perhaps—who acted responsibly and who acted recklessly or out of narrow self-interest.

Examples of complex events supporting multiple narratives are easily found: the taking of the Bastille, the Haymarket Square riot in Chicago, the return of Franco to Malaga, or the decision of General MacArthur to cross the Yalu River in Korea. Virtually *every* historical event is a complex happening; so the problems raised here are endemic to historical interpretation.

We can raise the question of objectivity at two locations: the investigator and the narrative. So let us begin with the narrative itself—what do we want in a good comprehensive piece of historical writing that tells this story accurately and fairly? We want an account that lays out the causes, events, and actions that made up this period of time. We want to know what organizations and leaders took what actions at what time, to call forth what organized responses. We want to know what key decisions the government made. We want to know how the prime minister and the police and military deliberated about responses to massive demonstrations. We want to know how the several occasions of mob violence against officials and offices transpired. We want to know the crucial details of the final hours of confrontation between the military and the crowd, and the degree of violence that transpired at that point.

And what do we want from the investigator of a complex happening in Bangkok, Chicago, or Madrid? We want a commitment to arriving at the most truthful account of the story possible; a commitment to considering the full range of empirical and factual evidence available; and an ability to tell the story without regard to one's antecedent affinities and loyalties. It should not be a partisan's story; rather, it should be a *factual* story, based on critical reading and assessment of the available evidence. In order to arrive at such an account, the honest reporter needs to exercise critical good sense about the sources and the interests that the conveyors of the information have: the biases of the government, the press, and the parties as they provide evidence and interpretation of the events. And we want this account to be as free as possible of the interfering influences of bias and political interest. We want an honest and comprehensive synthesis, not a one-sided spin.

Both goals are possible. The standards and values associated with both good historical writing and good journalism lead at least some investigators to exert their talents and integrity to do the best job they can to use the evidence to discover the details of the story. Not all journalists are equally committed to these standards; that is why we prefer the I. F. Stones to the Jayson Blairs of the world. But enough are committed that we have a good likelihood of sorting out the realities and responsibilities of the complex happenings that surround us through their objective, fact-based reporting.

2.3.3 History, Memory, and Narrative—Recent China

What is the relation between "history", "memory", and "narrative"? We might put these concepts into a crude map by saying that "history" is an organized and evidence-based presentation of the processes and events that have occurred for a people over an extended period of time; "memory" is the personal recollections and representations of individuals who lived through a series of events and processes; and "narratives" are the stories that historians and ordinary people weave together to make sense of the events and happenings through which a people and a person have lived. We use narratives to connect the dots of things that have happened; to identify causes and meanings within this series of events; and to select the "important" events and processes out from the ordinary and inconsequential.

If we think that "history" should be informed by the ways in which historical events were experienced by individuals, then we must also address the question of how to use the evidence of memory as a prism for attributing subjective, lived experience to the people who lived this history. If we are interested in the Great Leap Forward famine years in China in 1959–1961, for example, we need to know more than the timeline of harvest failure or the map of grain distress across China; we need to know how various groups experienced this time of hardship. And for this we need to have access to documents and interviews reporting the experience of individuals in their own words; we need to have access to memory.

A particularly valuable body of work on China's recent history is currently underway, in the form of careful use of oral histories, memoirs, and other expressions of personal memories of some of China's most dramatic chapters of its history since the 1930s. C. K. Lee and Guobin Yang have presented some excellent examples of this work in *Re-envisioning the Chinese Revolution: The Politics and Poetics of Collective Memory in Reform China* (Lee and Yang, 2007). The book contains research contributions that draw out important new insights into the Cultural Revolution, the Great Leap Forward, the changing conditions of women, cinema, the experience of ethnic minorities, and the occurrence of violence and disorder in the past 60 years in China's history. Especially interesting are contributions by Paul Pickowicz and Guobin Yang.

In "Rural Protest Letters: Local Perspectives on the State's War on Tillers" Paul Pickowicz describes an extensive collection of interviews and private writings of a single Hebei peasant leader, Geng Xiufeng, written between the 1950s and the 1990s. Geng's writings often take the form of protest letters, addressed to leaders extending from local party officials to Chairman Mao himself. Geng also maintained a journal in which he recorded his observations of the effects of various state-directed reforms of agriculture—and the inimical effects these reforms had on peasant standard of living. Geng was a peasant activist and leader in the 1940s in support of rural cooperatives, as a practical mechanism for improving agriculture and improving local peasants' standard of living. And he turns out to be an astute and honest observer of the twists and turns of policy disaster (rapid collectivization of agriculture), corruption, and disregard of peasants' welfare by the CCP. (This latter is the meaning of Pickowicz's phrase, "the state's war on tillers.") Pickowicz had conducted a number of interviews with Geng in the 1970s and 1980s, and was greatly surprised to learn that Geng had written dozens of protest letters and had accumulated a multi-volume memoir that chronicled many of these social observations about change in North China. The content of these writings is fascinating; but even more important is the evidence they offer of the astute abilities possessed by ordinary Chinese people in observing and criticizing the processes of change that enmeshed them. These manuscripts offer Pickowicz and the reader a window into the consciousness of some ordinary rural people as China's history enveloped them; and they make evident the fact that Chinese peasants were not mere passive instruments, but rather practical, observant, and sometimes wise thinkers about revolution and reform.

Guobin Yang's article, "'A Portrait of Martyr Jiang Qing': The Chinese Cultural Revolution on the Internet" touches the other end of the information spectrum—not handwritten letters and reflections penned in the 1950s, but over 100 contemporary websites devoted to archiving and chronicling the Cultural Revolution. There are widely divergent stories that can be told in defining the Cultural Revolution as an episode of history: an excess of leftism, a deliberate use of power by China's leaders against each other and against society, a period of social hysteria, or even "still a good idea." (The latter is the theme taken by the website incorporated into Yang's title—"A Portrait of Martyr Jiang Qing." This is one of the few publicly available websites that Yang discovered that continues to glorify Madame Mao and her fellow radicals.) Yang demonstrates that we can learn a lot about how the current generation views the Cultural Revolution—and the strands of disagreement that continue to divide opinion about its causes and meanings—by examining in detail the editorial judgments and online commentaries that accompany these online "exhibition halls".

The use of photography and cinema to represent memory—both individual and collective—is an important theme in the volume. The photographs included in the exhibitions Yang discusses often represent a "struggle" session against "class enemies," capture a particular moment in time—for example, two particular men, exposed to a particular crowd. But in its particularity a photo also emblemizes scenes that were common throughout China during the Cultural Revolution. And, presumably, it triggers very specific personal memories for individual Chinese people who lived through the Cultural Revolution, whether as victims, Red Guards, or bystanders. As David Davies notes in "Visible Zhiqing: The Visual Culture of Nostalgia among China's Zhiqing Generation" (Lee and Yang, 2007), no photograph stands wholly by itself. But some photos have the directness and honesty needed to stand for a whole dimension of historical experience—in this case, the violence and humiliation perpetrated against teachers, scholars, and officials by zealous mobs of Red Guards and their followers. In this way the photo can faithfully capture one important strand of the history of this period.

The editors have provided a particularly valuable contribution with the innovative thinking the volume provides about the nexus of experience, identity, and history. The editors and contributors are very sensitive to the fact that there is no single "Hebei experience" or "Chinese women's experience"; instead, the oral history materials permit the contributors to discern both variation and some degree of thematicization of memory and identity. Another important contribution of the volume is the emphasis it offers to the idea of the agency involved in memory. Memories must be created; agents must find frameworks within which to understand their moments of historical experience. "As people grope for moral and cognitive frameworks to understand, assess, and sometimes resist these momentous changes in their lives, memories of the revolution thrive" (Lee and Yang, 2007, p. 1). A third and equally important thrust of the volume is the persuasive idea that memories become part of the political mobilization possibilities that exist for a group. Groups find their collective identities through shared understandings of the past; and these shared understandings provide a basis for future collective action. So memory, identity, and mobilization hang together.

2.3.4 Age Cohorts and Historical Experience

These examples from recent Chinese history raise another important point for the historian: the importance and salience of *age cohorts* within history. It is worth reflecting a bit on how absolutely tumultuous China's history has been since the Communist Revolution in 1949. The Great Leap Forward and consequent famine—1958–1960, in excess of twenty million famine deaths. The Cultural Revolution—1966–1976, in excess of 1.5 million deaths by violence, many times that number of maimed and ruined lives. The Democracy movement and Tiananmen Square and its dramatic suppression—1989, unknown thousands of victims. And since the early 1980s, economic reforms, rapid growth, and a substantial degree of social transformation.

If we consider these events in terms of age cohorts, the historical experience of almost every recent Chinese generation has been a traumatic one. Chinese men and women born in 1930 were children during the anti-Japanese war and teenagers during the Revolution, and they experienced famine, chaos, civil violence, and economic reform in the remainder of their lives. The generation born in 1950 experienced the GLF famine as children, they were the teen-aged militants in middle school who formed the Red Guards, they experienced years of rustication in the countryside in the 1970s, they returned to universities after the end of the Cultural Revolution in 1976, they participated in or observed the tumult of Tiananmen Square as they approached their forties, and they participated in China's economic reforms in their forties and fifties. This is an astounding quantity and pace of historical change for a single cohort to experience.

The children of the 1950 generation were born in 1970. They were born in the middle of the one-child policy. These children largely escaped the violence of the Cultural Revolution. Tiananmen Square was a reality for them in their teens. Their generation has been at the center of the dynamism of entrepreneurial China, with broadened opportunities in education and business. They have some of the expertise and comfort with the Internet that allows them to bridge to the China of the twenty-first century. And their standard of living—for urban people anyway—is dramatically improved over that of the previous generation. And of course the generation of 1990 is the youth generation of today. This is the generation that will set China's course for the next half century, and it appears to be quite different from previous cohorts.

These generations surely created vastly different mentalités for themselves—different ideas about politics, equality, morality, and social stability. The ideologies of each generation were shaped and burned by the super-heated political struggles through which they passed. And surely their thoughts about what China should become, what standards of fairness should be respected, and how they should live their lives, are very deeply affected by their generational experiences. So twentieth-century Chinese history was experienced and narrated by these cohorts in very different ways.

2.3.5 Maps and Narratives

There is an intriguing analogy between narratives and maps. Both are ways of organizing a great deal of factual knowledge about the world. Both involve selection and choice on the part of the designer. And each is itself an encapsulated form of social or historical knowledge. It is worthwhile examining the analogy briefly.

To start, it is obvious that maps are selective representations of the world. They represent an abstraction: a representation of a complex, dense reality that signifies some characteristics while deliberately ignoring other aspects. The principles of selection used by the cartographer are highly dependent on the expected interests of the user. Topography will be relevant to the hiker but not the motorist. Location of points of interest will be important to the tourist but not the long-distance trucker. Location of railroad hubs will be valued by the military planner but not the birdwatcher. So there is no such thing as a comprehensive map—one that represents all geographical details; and there is also no such thing as a truly "all-purpose" map—one that includes all the details that any user could want.

We also know that there are different schemes of representation of geography—different projections, different conventions for representing items and relationships, etc. So there is no objectively best map of a given terrain. Rather, comparing maps for adequacy, accuracy, and usefulness requires semantic and pragmatic comparison. (Here the word "semantic" is used in a specialized sense: "having to do with the reference relationship between a sign and the signified.") Semantically, we are interested in the correspondence between the map and the world. The conventions of a given cartography imply a specific set of statements about the spatial relations that actually exist among places, as well as denoting a variety of characteristics of places. So there is a perfectly natural question to ask of a given map: is it representationally accurate? This sort of assessment leads to judgments like these: This map does a more accurate job of representing driving distances than that one, given the rules of representation that each presupposes. This map errs in representing the relative population sizes of Cleveland and Peoria. These are features that have to do with the accuracy of the correspondence between the map and the world.

The pragmatic considerations have to do with how well the representation or its underlying conventions conform to how various people want to use it. Maps are particularly dependent on pragmatic considerations. We need to assess the value of a map with respect to a set of practical interests. How well does the map convey the information about places and spatial relationships that the user will want to consult? How have the judgments about what to include and what to exclude worked out from the point of view of the user? Pragmatic considerations lead to judgments like these: this mapping convention corresponds better to the needs of the military planner or the public health official than that one. The pragmatic questions about a map have to do with a different kind of fit—fit between the features and design of the map and the practical interests of a particular set of users. Do the conventions of the given cartography correspond well to the interests that specific sets of users have in the map?

Here is the point of this discussion: are there useful analogies between the epistemology of maps and the cognitive situation of historical narratives? Several points of parallel seem particularly evident. First, narratives are selective too. It is impossible to incorporate every element of a historical event or natural process into a narrative; rather, it is necessary to select a storyline that permits us to provide a partial account of what happened. This is true for the French Revolution; but it is also true for a much more limited event, for example, the resignation of Richard Nixon.

Second, there is a parallel point about veridicality that applies to narratives and theories as much as to maps. No map stands as an isolated representation; rather, it is embedded within a set of conventions of representation. We must apply the conventions in order to discover what "assertions" are contained in the representation. So maps are in an important sense "conventional." However, given the conventions of the map, we can undertake to evaluate its accuracy. And this is true for narratives as well; we can attempt to assess the degree of approximate truth possessed by the construction. Are the statements about the nature of the events and their sequence approximately true? (Given that an account of the French Revolution singles out class interests of parties within the narrative, has the historian correctly described the economic interests of the Jacobins?)

And third, the point about the relevance of users' interests to assessment of the construction seems pertinent to narratives as well. The civil engineer who is investigating the collapse of a building will probably find a truthful analysis of the thermodynamics of the HVAC system unhelpful, even though it is true. The human rights investigator investigating police violence during a demonstration will probably become impatient at a narrative that highlights the sequence of street noises that were audible during the demonstration, rather than the descriptions and actions of the participants and groups during the relevant time.

When it comes to narratives, there is another value dimension that we want to impose on the construction: the idea of explanatory adequacy. A narrative ought to provide a basis for explaining the "how and why" of historical events; it ought to single out the circumstances and reasoning that help to explain the actions of participants, and it ought to highlight some of the environmental circumstances that influenced the outcome. A scientific theory is intended to identify some of the fundamental causal factors that explain a puzzling phenomenon—the turbulence that occurs in a pot of water as it approaches the boiling point, for example. So when we say that a narrative is an abstraction, part of what we are getting at is the idea that the historian has deliberately excluded factors that do not make much of a difference, in order to highlight a set of factors that do make a difference.

Chapter 3
Historical Concepts and Social Ontology

This chapter takes up a specific task: to identify and analyze some of the ontological and conceptual conditions that must be satisfied in order for historical analysis and inquiry to be feasible. How does the historian need to *think* as he or she formulates a discursive representation of a period of history? What assumptions does the historian make about the structures and entities that make up the social world? And what sorts of conceptual schemes are needed in order to permit the historian to do his or her work of comparison and explanation? Are there "big structures" in history? And do "big structures" fall into kinds or universals that recur in different contexts?

3.1 Ontology and Explanation

Historians often focus their research and analysis on large historical happenings—the Peloponnesian War, the collapse of the Qing Empire, the Iranian Revolution. And they analyze these large, complex historical realities into a collection of historically extended entities and structures—formations such as riots, revolutions, classes, social organizations, ideologies, and states. This in turn requires that they aggregate large ensembles of actions, events, and properties into entities, structures, or processes of intermediate scale (Tilly, 1984). Further, they often attribute causal powers to these structures—they make assertions such as "the costs of military expansion brought about a fiscal crisis". And they attempt to establish explanatory relations—causal, interpretive, functional—among and within some of the historical formations that they identify. This description brings to the foreground two metaphysical questions: What assumptions do we make when we treat a historical ensemble as a thing or as a historical entity? And what is involved in categorizing and analyzing historical things; what *types* of entities exist in the historical realm? The first question has to do with "things," and the latter question has to do with categories or types into which we can classify the "things" we are concerned with.[1]

[1] For good recent treatments of philosophical approaches to the study of ontology, see Loux (1976), Strawson (1963), and Quinton (1973). Writers in the philosophy of social science who have raised questions about social ontology include Giddens (1979), Gould (1978), Ollman (1971), Ruben (1985), and Elster (1989a).

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These are all questions of social ontology. And they are unavoidable questions if we are to have a coherent conception of historical knowledge. When we ask "Why do revolutions commonly occur in agrarian settings?" or "Why do capitalist economies suffer periodic crises?", we imply that there are such things as revolutions or capitalist economies, and that there are multiple instances that constitute a significant group or type of social occurrence. Minimally, then, these questions imply that we can identify individual historical events as "revolutions"; that we can identify a group of events under the rubric "revolution"; and that we can pose the question whether there are underlying causal, structural, or agency features that these events share. Large-scale historical inquiry thus implies that we must be able to identify historical "things" and subsume these things under "concepts." And it raises the question of whether the historical concepts that we use refer to "kinds" or "universals". Much of what follows will attempt to clarify these two points.

Ontology is the abstract theory of the nature of the entities, properties, and relations to which one refers in a given domain of discourse or science (Quinton, 1973). To provide a social ontology is to answer questions like the following:

- What sorts of social entities exist?
- To what extent are there stable, continuing, and comparable social entities within a given social order over extended space and time? Is there such a thing as the "American state" or "East Asian trading regime"?
- To what extent does a given social concept identify a range of phenomena with common internal nature? That is: to what extent does a given concept refer to a "social kind"? Is a riot in contemporary Thailand the same type of thing as a riot in sixteenth-century France? Is "bride sale" the same social custom in England and China?
- To what extent do abstract social concepts cleanly divide patterns of activity in the various social contexts? For example, the western concept of the political excludes the religious; how does that work for Iran, Bali, or India? The "economic" excludes the normative; how does that work in societies in which charity is an important feature of economic transaction?
- To what extent is a given concept deeply rooted in a particular historical example—with the likely result that the concept is not readily transportable to other historical contexts? Perhaps "feudalism" and "capitalism" fall in this category, as does the "theatre state of Bali."

3.1.1 Things

Let us first address the issue of "things" or historical individuals—entities, concrete institutions, particular structures (Strawson, 1963). Somehow the historian needs to be able to identify the "things" he or she wants to refer to in the historical construction. What are the historical entities that need to enter into the account? This means specifying an ontology of things that are the subject matter of investigation.

Concepts are constructed to identify and analyze entities into groups of "similar" things. Historians work to identify, classify, and conceptualize particular or singular events as instances of a related group of things or occurrences. "This was a revolution, that was an episode of extended banditry."[2] (Note, however, that a given item can be differently characterized or classified, and that this is often an essential feature of the story.) Once an event is classified as an X, we can ask whether X's have important properties or regularities in common, and then we can offer a social science analysis of the case and the class of cases. Or we can inquire into the specific causal and narrative properties of the particular case.

What logical features must "things" satisfy in order to be things? In order to identify the individuals in a given domain, we need to address some or all of the following issues.

- Criteria of the entity's identity over time (e.g. is the French state in 1930 the same state as the French state in 1830?)
- Part-whole relations: Is X part of Y? (e.g. was racial struggle in Martinique part of the French Revolution?)
- Demarcation criteria between entities (e.g. is there a single French national working class, or are there several distinct regional working classes?)
- Criteria of classification; operationalization; theoretical definition of concepts. This means specifying the criteria or guidelines for judging that x is a Y; this protest is a riot. (E.g. are the American state and the English state of the 1980s both liberal democracies?)

An ontology of things requires, then, that we be able to identify features of persistence and continuity. We must be able to offer reasonably clear criteria of reidentification over multiple stages. And we must be able to provide reasonably clear boundaries for the entity.

Questions about identity are particularly difficult for historical entities. Two large sets of questions confront us about a given social entity. First, what are the social "threads" that suffice to unify a range of social actors, institutions, and places into a single unified historical entity (that is, what are the criteria of identity for a "single social entity"). Is "China during the Han Dynasty" one unified social formation; or is it a congeries of semi-independent regional economies, cultures, and social orders? Does China during the "Warring States" period have a different ontological status than during the Ming Dynasty? Is "the Chinese imperial state" a single historical entity over the 4,000 years of Chinese political history? And second, at what level of description is it credible that we can reidentify "the same" institutions or practices in separate historical formations? Is there some quality of "state-ness" that is possessed by the French absolutist state, the Chinese imperial system, and the Indian polity?

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[2] Early years of the Chinese Revolution have raised exactly this type of question: was it rebellion? Was it banditry? Or was it revolution in the making? Relevant sources include Bianco (1971) and Perry (1980).

In view of these difficulties, what can we say about the question, what things exist in the historical realm? That is, what purported entities satisfy these requirements? There is a wide range of choices in answer to this question, from the spare to the ontologically demanding. On the spare end, we can describe a social ontology in which only individuals with beliefs and goals exist. At the demanding end of the spectrum, we can describe a social ontology in which "states," "modes of production," "mentalités," "religions," and "political cultures" exist. Below I will offer a social ontology that falls on the spare end of the spectrum. On this approach, what exists is the socially constructed individual, within a congeries of concrete social institutions. This conception will be described under the rubric of "methodological localism."

3.1.2 Events

Historians face some of the same problems in individuating events as they do in individuating structures. What is a historical event? Is the American Civil War an event, or is it a collection of events? Consider events at a wide range of levels of aggregation: the assassination of Lincoln, the battle of Gettysburg, the American Civil War, the Reconstruction Era. These configurations of historical events range from the narrow and limited to more and more aggregated and comprehensive configurations: more people, more time, more space, more complexity, and more ambiguity. Is there a rational basis for delineating and defining historical events?

We might define a happening as an incident in which one or more persons act, singly or in concert. Happenings are then objective occurrences in space and time. History is a congeries of happenings, in which individuals and groups constitute goals, arrive at identities, and engage in internal development and action. And we might say that larger historical events—riots, financial crises, religious movements, revolutions, wars—are constructs created by the historian and the social scientist as an analytical tool for drawing together a constellation of happenings. So a large historical event—the seizure of power by the Bolsheviks during the Russian Revolution, say—can be understood as a constellation of happenings, drawn together conceptually in an act of historical unification. Large events are composed of small events.

What is the status of a large historical event? Are events objectively given, or are they constructed in the representations of persons, storytellers, and historians? Something is objectively given; things happen. Stones are thrown, speeches are made, children go hungry, within a specific period of time and in a given geographical domain. The question is whether these speeches, acts of violence, moments of hunger and anger, aggregate objectively into "events". One period of time in France might be variously characterized as "famine with attendant social violence," "incipient rebellion in the context of widespread hunger," or "ecological crisis creating hardship and collective violence". That is to say: the higher-level historical event must be constructed by observers, participants, and interpreters—even granting that the materials of which the large event is composed are themselves objective historical realities. And of course there is the fact that a single human action is amenable to multiple to multiple interpretations; so even this grain of historical reality is at least partly constructed rather than given.

For example, the French Revolution began in 1789. It was caused by conflict between the aristocracy and the monarchy. Eventually it developed into violent conflict in every region of France. It created more lasting change in French society than did the Russian Revolution. These statements purport to refer to an extended but unified historical event, the French Revolution. This event is assigned a place within a causal system, being caused by one set of factors and having causal consequences for other factors. It is considered a suitable topic for comparison with other such events (the Russian Revolution).

But what does the historical reality of the French Revolution consist in? Notice, to begin, that the revolution comprises a huge constellation of events and actions, both small and large. Were any of these events "the definitive moment" in the revolution—the decisive event that constituted the constellation as a revolution rather than a "period of unrest"? Is it possible to distinguish clearly between core events and peripheral, minor events—not to speak of events that are wholly unrelated to the revolution? Most radically, would it be possible to construct the events of 1789 in such a way that no revolution occurred at all (as some revisionist economic historians argue that the "industrial revolution" did not occur (O'Brien and Keyder, 1978), or that the Great Wall of China did not exist (Waldron, 1990))?

One possible answer to these questions is to reply that the events directly associated with the overthrow of the monarchy and the establishment of a different form of political power constituted the essence of the revolution. But what if later historians were to conclude that the transfer of power was illusory, and that the same interests in society continued to govern? In fact, if the monarchy had been restored in 1830 we might reasonably say that "no revolution occurred, only an interruption of monarchy." Would either of these possibilities represent a refiguring of the historical picture in which the seizure of power is minor and background rather than major and foreground? And would this not be a basis for doubting that "a revolution occurred in 1789"?

It seems best to understand "the French Revolution" as an intellectual construction—one possible way of knitting together the congeries of events that occurred during this time in France. Some constructions of these facts are more plausible than others, so it is possible to have rational dispute about the alternative construals of the constellation of events. But there is no essential fact of the matter that a revolution occurred in France in 1789. This doesn't derogate the status or facticity of the constituent events. But it does assert that the historian's act of composing events and actions into a large historical structure is an act of construction rather than recognition.

This is not to say that large historical events have no historical reality; it is to say that there is a contestable and elastic relationship between the positing of an event and the underlying happenings that made it up. When Peter Perdue (Perdue, 2005) treats the history of China, Russia, and the peoples of Inner Asia, he walks carefully through the series of diplomatic calculations, negotiations, tributes, campaigns, battles, retreats, and shifting alliances that constituted the three-way relationship between Qing, Russia, and the Mongol Empire. He relies on historical documents that record various conversations and decisions; various historical movements of troops; various victories and defeats by the several players and leaders. Various things happened; and the historian can pull these happenings together into a somewhat orderly representation that sheds light on the causes, constraints, opportunities, and contingencies that permitted events to develop in this way rather than that. In Perdue's case, he provides an eventful narrative of development of the struggle over Inner Asia, and he emphasizes the contingency of the linkages of these events in historical context.

There is a further complication about events, in that we can ask how they were conceptualized, understood, and analyzed by participants, and we can ask how they are best conceptualized for the purpose of historical explanation. It is likely that there is a substantial difference between "folk understandings" and after-the-fact analytical conceptualizations. Perhaps the typical Roman citizen would not have described his society as a republic, and yet it may be that reference to the Roman Republic is an effective way of bundling together a set of historical agencies and structures that historical explanation favors. Did ordinary Chinese peasants in 1936 know that they were living through a "revolution" in its early stages? Surely not.

3.2 Concepts and Kinds

Let us turn now to concepts and categories—the vocabulary in terms of which historians analyze and investigate the historical given. What sorts of concepts are to be found in historical thinking? And how do these concepts correspond to the social world? Do individual historical entities fall into natural classes, types, or kinds? Are there social universals that recur across historical contexts and settings?

We can begin with a simple question: what is a typical act of historical conceptualization? What is a concept? It is an element of language that serves to attribute characteristics or properties to an entity. Consider the sentence, "Fido is an aggressive dog." "Fido" is a proper name in this sentence; it refers to a unique individual. "Dog" is a category of animals to which Fido belongs. And "aggressive" is a behavioral property that Fido is said to possess. Systems of concepts allow us to classify and organize the entities we identify around ourselves. A conceptual scheme is an orderly set of concepts on the basis of which we classify entities into groups. So concepts do at least two different kinds of work for us: they specify what types of entities exist around us (and perhaps how they relate to each other); and they serve to provide a basis for assigning properties and relations to those entities. The classification scheme of species and genus is a systematic example of an effort to classify a complex reality into an orderly system of concepts. We might say that common nouns identify the kinds of things that exist, and adjectives identify the properties that object may have.

How do categories, concepts, and theories come together in historical analysis? Concepts are of course essential to social knowledge. The heart of social inquiry has to do with coming up with concepts that allow us to better understand social reality: for example, racism, patterns of behavior, fascism, free market, class consciousness, ethnic identities. Theory formation in the social sciences largely consists of the task of constructing concepts and categories that capture groups of social phenomena for the purpose of analysis. This analysis of historical ontology provides a productive suggestion. For each ontological category, it is the task of social theory to provide further theories and classifications among the things included in this category. So Marx distinguishes among "mentalités" between the distinctive worldviews of the bourgeoisie, the proletariat, and the peasantry; Durkheim distinguishes among social structures between "organic" and "mechanical" solidarity; Tönnies distinguishes among social organizations between *gemeinschaft* and *gesellschaft*; Marx distinguishes among economic systems the feudal, capitalist, and socialist modes of production; and so on indefinitely. Theories provide a substantive hypothetical basis for classification and analysis. The further classification provided by social theory is intended to provide a basis for successful explanation of the behavior of the individuals, groups, and structures to which the classifications and theories apply.

A concrete historically given society may be identified as a historical individual, a concrete social formation. Here we might think of the social, economic, political, and cultural realities of Britain in 1800 as the British social formation of the early nineteenth century. "Capitalism" is a concept of economic organization that is intended to encompass multiple concrete social-historical formations—Britain 1800, France 1900, United States 1950, Japan 1970,... "Mode of production" is a concept of classification for types of economic systems: capitalism, feudalism, bureaucratic socialism,.... So there is a rising hierarchy of concepts within a scheme of classification.

Historical concepts, then, are deployed in order to classify historical entities within groups or classes. What is in common among the things classified under a concept? There is a range of possibilities: we may define a concept in terms of a set of necessary and sufficient conditions; in terms of a set of symptoms or observable features; in terms of a cluster of properties; in terms of a common structure; or in terms of common causal properties.

3.2.1 Historical Ontology of the French Revolution

Consider a concrete and familiar example of historical conceptualization: historians offering an account of the French Revolution from 1789 to 1799. What are the historical entities to which historians need to refer in constructing a history of these events? Consider, to start, Albert Soboul's construction of the making and carrying out of the French Revolution (Soboul, 1977, 1989). He begins his short history with a chronology of "principal events" (Soboul, 1977)—roughly 150 events from February 1787 to November 1789. He refers to happenings at a variety of levels of scale in this chronology, from actions by a small number of individuals to events encompassing hundreds of thousands over a dispersed geography. Here is a partial list:

> Assembly of the Notables, Calonne dismissed, abortive reform of Parlements, "Day of the Tiles" in Grenoble, rural and urban unrest increases, riot in the Baubourg St.-Antoine, fall of the Bastille, The Great Fear, Assembly moves to Paris from Versailles, Church lands nationalized, mutiny and repression of garrison at Nancy, Louis XVI's attempted flight from the country, foundation of the Feuillants Club, massacre of the Champ de Mars, mounting unrest caused by food prices, uprising at Paris overthrows the Monarchy, massacre of prisoners in Paris, execution of Louis XVI, murder of Marat, . . .

and so on through crowd violence, state action, military movements, rise and fall of revolutionary leaders, and so on. Some of these are events in a specific time and place enacted by a small group of people (mutiny, storming of the Bastille); others are regionally diffuse collections of such local actions (the Great Fear, for example); and others are actions of officials, leaders, and generals. So clearly Soboul's historical ontology includes "events", which perhaps we can define as "actions by individuals and groups, both large and small." (This definition captures most but not all the items in Soboul's chronology.)

What about social entities beyond the level of actions by individuals? Here are some of the sorts of higher-level structures to which Soboul refers in his account: the Revolution itself, as a thing that exists in history, has causes, and has a "nature"; but also a larger category within which the French Revolution is one instance—"revolution". He refers to earlier social orders (the seigneurial system and the privileged orders of feudal society); a new social order (liberal democracy, bourgeois economy); large historical structures such as feudalism and capitalism; and a specific category of revolution (bourgeois revolution). Several of these concepts fall in the general category of a "type of social and economic organization"—what Marx refers to as a "mode of production." (Here is Soboul's paraphrase of the idea of feudalism: "a concept of social and economic history, defined by a particular form of property ownership and by a system of production based on landed property, preceding the modern system of capitalist production" (Soboul, 1977, p. 3)). He refers to different forms of the social management of labor (*corvée* labor, slave labor). He refers to specific periods of governance in French history—for example, the Capetian monarchy; and he refers to a type of governance—the absolutist monarchy. He refers to classes—the bourgeoisie, the landlord class, the peasant class. This part of Soboul's historical ontology incorporates large stretches of Marx's social theory of the structures that constitute society: forces and relations of production, property systems, modes of production, superstructures such as the state and the church. Soboul refers to subordinate social organizations within existing society—the officer corps, the Church. He refers to economic and demographic processes—population increase, trends of price movements for grain. He refers to features of political consciousness on the part of various social actors—hopes, fears, revolutionary spontaneity (p. 38). And he refers to political groups and clubs—the Girondins ("spokesmen of the commercial bourgeoisie"; p. 87), Montagnards and Jacobins (bourgeois but appealing to common people; p. 88), and Sans-Culottes (the organized representatives of the common people; p. 100).

So Soboul's historical vocabulary is a rich one, in that he refers to historical things and structures at a variety of levels. We can paraphrase Soboul's social ontology in these terms: the French populace of the 1770s consisted of a geographically dispersed collection of persons enmeshed social, economic, and political structures. These structures can be analyzed in a fair amount of detail (along the lines of Marx's theories of the mode of production as a system of forces and relations of production). The circumstances of life and opportunity created for different people by these structures in turn defined them as "classes" with interests and motivations. Peasants, artisans, landowners, lawyers, and government officials had very different views of the social world, and their political behavior was accordingly different as well. The political struggles that constituted the turmoil of the years of revolution derived from contests over power among different groups of people, mobilized by different organizations with different social, economic, and political interests. In other words, Soboul works with a "proto-theory" of what a society is, how it works, and how individuals are influenced in their ordinary conduct; within the context of this scheme, the task of the historian is to discover some of the specific features of those social relations and features of consciousness, and explain the small and large events that combined to bring about "the Revolution".

Now how about Tocqueville? What ontology does he presuppose in his own history of the French Revolution (Tocqueville, 1955)? There are some elements in common with Soboul in Tocqueville's account. The French Revolution is a "great event", and it reflects the passing of the old order—a specific set of political, economic, and social relations that can be investigated in detail. De Tocqueville assumes that France has embodied distinct social structures—the old regime, the modern post-revolution social and economic system, the system of land ownership and inheritance. He provides a schematic taxonomy of the French political structure—Monarchy, Estates-General, administration, the post-revolutionary French state, the centralized grasp of the Controller-General (p. 62). He identifies a number of key French institutions—the Church, etc. And he refers to systems of ideas as influential social causes—"The philosophical conceptions of the eighteenth century have rightly been regarded as one of the chief causes of the Revolution" (p. 6). But ordinary mentalités have historical import as well—the mindset of the French peasant is an important part of his story (p. 31). More than Soboul, Tocqueville's narrative is focused on the actions and actors; why they did what they did, how their mentalités shaped their choices. His story is less about structures and class conflict and more about desire—a desire for reform, a desire for transformation of the existing order. But his background ontology includes the categories of social, political, and economic structures.

Let us turn now to Simon Schama's conceptualization of the history of the French Revolution in *Citizens* (Schama, 1989). Schama is a highly original thinker when it comes to conceptualizing history—witness his motif of landscapes as a way of thematizing swatches of European history (*Landscape And Memory* (Schama, 1995)), or his crossing of history and fiction in *Dead Certainties: Unwarranted Speculations* (Schama, 1991). So what is distinctive about his history of the French Revolution?

To begin, his history breaks radically from earlier approaches to the Revolution. Schama distances his account from both materialist accounts like that of Albert Soboul and more orthodox approaches such as that of Tocqueville. Schama doubts the reality of large, impersonal historical forces; he doubts the historical reality of the concept of great classes in society; and he focuses instead on the thoughts and actions, often barely rational, of the individuals great and small who contributed to the period. He puts several of these points very clearly in the introduction:

> The 'bourgeoisie' said in the classic Marxist accounts to have been the authors and beneficiaries of the event have become social zombies, the product of historiographical obsessions rather than historical realities. . . . Continuities seem as marked as discontinuities. Nor does the Revolution seem any longer to conform to a grand historical design, preordained by inexorable forces of social change. Instead it seems a thing of contingencies and unforeseen consequences (not least the summoning of the Estates-General itself). (Schama, 1989, p. xiv)

Schama resists the totalizing impulses of many other historical narratives, the intellectual tendency to characterize the Revolution in the most general and comprehensive terms. For Schama, the grand locales (France, Paris, and the countryside) and great actors (monarchy, bourgeoisie, peasantry) are less important than the "local passions and interests" of the regions, provinces, and villages. The upshot of this preference is clear: rather than aggregating the events of the period into a single grand process of Revolution, Schama emphasizes the separateness and particularity of the component processes and local realities. In the extreme, France did not have a revolution, but instead the territory encompassed a number of parallel social and economic processes in different places, experienced differently by individuals in those places. (But of course this strategy of disaggregation can be applied at each lower level as well: instead of a "counter-revolution" in the Vendée, there were actions, church-burnings, massacres, and other kinds of organized violence in the various villages and towns of the region.) (This discussion relates directly to the issues raised above concerning the definition of "things" and "events.")

It is also worth pointing out that the sorts of events that Schama pays attention in the book to are very different from those highlighted by Soboul and Tocqueville. Schama focuses on things like commemorations, ceremonial elephants, public memories, and bits of architecture. It is the particular but revealing element or action that Schama finds most interesting. He pays attention to philosophy, theatre, the *Comédie-Française*, paintings, leaders (potted and sane), and extravagant events (the lofting of a balloon by Etienne Montgolfier to a crowd of 100,000 comes in for a few paragraphs). And he is uninterested in constructing a logical narrative according to which events A, B, and C are constitutive of the Revolution, and this event led to that event, which led in turn to the final event.

So Schama's ontology consists of actors, cultural meanings, shifting circumstances, and contingent outcomes. He gives little attention to "structures," organizations, and forces of history, and pays much more attention to the individuals whose states of mind and action constituted the period of violence and upheaval we refer to as the "French Revolution." It is a sort of cultural commentary on the moments of the time period—rather than a sustained effort to make sense of it all, to provide an explanation in terms of causes, classes, or structures. In fact, the Revolution as an integrated event largely disappears from his account, in place of a congeries of happenings and productions.

The overall impression of this approach to historical analysis is that it amounts to a "deconstruction" of the Revolution in favor of a large number of interesting and contingent but inherently lesser processes and events. Schama dissolves the Revolution as a grand historical event, and uses the noun simply as a handy label in terms of which to refer to a heterogeneous and sometimes disparate and confusing series of processes.

It is a fair question to ask, what precisely does Schama's account allow us to do? What intellectual insights does the reader gain? What kind of historical question is it intended to answer? *Citizens* certainly does not amount to an explanation of the events of the French Revolution, if by that we mean a narrative that identifies specific causes that brought about the outcome. In fact, it is hard to find examples of causal reasoning or causal attribution in the book at all. So causal explanation, based on discovery of large social structures and forces, is not the goal of the book.

Further, it does not seem to try to answer a question about the coherence of the period: "How did these events hang together?", since quite a bit of the narrative is intended to demonstrate precisely how incongruous were many of the various events, symbols, and actions. So the book isn't intended to let the reader say to himself or herself, "So that's what it was all about; that's the kind of thing the French Revolution was." The book certainly does not attempt to boil the Revolution down to a few simple formulas.

Somewhat more persuasive is the idea that perhaps it is intended as a work of cultural interpretation, more analogous to art criticism than to orderly, logical storytelling. Here the historian undertakes to locate a few specific moments and artifacts that bring to light some of the features of mentalité of the participants within the tangled skein of actions. (This would bring it into a parallel with Darnton's history of the Great Cat Massacre—interpretive ethnography of the moment rather than causal explanation of a large historical event (Darnton, 1984).)

But finally, perhaps the aim is more radical than any of these possibilities. Maybe Schama is interested most fundamentally in leading the reader to think very differently about the task of the historian and the content of historical knowledge. (This seems to be the goal of others of Schama's books—for example, *Dead Certainties* (Schama, 1991) with its pointed skepticism about fact-based narratives.) Perhaps Schama's most basic point is ontological: there is no such thing as coherent, logical, orderly, causally structured human history. Instead, all there is, is a congregation of separate threads, processes, contingencies, actions, choices, ideologies, and freakish accidents that ultimately do not add up to a coherent whole.

3.2.2 A Tabulation of Historical Ontology

This inventory of three approaches to the study of the French Revolution suggests the beginnings of a taxonomy of historical ontology. Consider this tabulation of the different types of historical concepts that we have encountered. The first row represents a set of ontological categories; the second row represents a definition of each category; and the following rows represent particular instances of the ontological category (Table 3.1).

And, as we have seen in this short discussion of the French Revolution, different historians make use of different aspects of this large ontological scheme, with different weights. Soboul's account emphasizes I, III, V, and VI; de Tocqueville's account emphasizes I, II, III, and IV; and Schama's account makes do largely with I, II, and IV. (Marx's historical materialism focuses almost wholly in III and VIII—the forces and relations of production.) The ontological concepts here—events, individuals, structures, mentalités, etc.—encompass general ideas of the types of entities to which the historian refers; they provide a sort of high-level grammar and semantics for the historical imagination.

3.2.3 Do Historical Categories Capture Social Kinds?

We can distinguish broadly between two interpretations of scientific concepts: nominalism and essentialism. According to the nominalist, concepts are a human linguistic convenience through which we break down complex phenomena into distinct entities. Concepts are necessary for science, but, according to the nominalist, they should not be understood as "carving nature at the joints" or as identifying real bits of the world. Essentialism (or realism) maintains, for at least some scientific concepts, that concepts succeed in identifying ontologically real entities, structures, and properties; and that good concepts are an essential step along the way toward formulation of scientific truths about the world. "Phlogiston" failed to identify a real kind of entity, whereas "oxygen" succeeded.

Whether or not nominalism is an acceptable general approach to the meaning of scientific concepts is an unresolved question. However, the approach has a central benefit in that it alerts us to an important error in scientific reasoning, the error of reification. Reification consists in the social scientist's assumption that, because he has a concept of X, that X really exists and has an underlying coherent essence. Because the concept of feudalism can be applied to Britain, Japan, and China, the historian may be led to assume that there is a common essence among these. But I find that it is better to regard these terms as nominalistic groupings; they are more like ideal types or descriptive concepts than kind terms.

A particularly important view of the interpretation of scientific concepts invokes the idea of a "natural kind" (Cartwright, 1983, 1989; Putnam, 1975). What is a kind? We may refer to a "kind" as a group of things that share fundamental properties—structural, essential, causal. When "things" fall into groups that share deep, explanatorily relevant properties, we refer to the groups as "kinds". For example,

Table 3.1 Historical ontology

I	II	III	IV	V	VI	VII	VIII
Events	Individuals and groups	Structures, organizations, institutions	Mentalités	Processes	Conditions	Patterns	Technologies
Things that individuals and groups do	The agents of history	Social context of historical change; standing and coercive sets of social relations	How agents think and represent their world	Social-causal mechanisms	Standing conditions that influence action	Observed trends over extended time	Viniculture
Convening the Estates	Jacobins	Feudalism, capitalism	Ideology	Fiscal crisis	Period of poor harvests	Rising discontent among peasants	Heavy plough
Storming the Bastille	Peasants	Property system	Peasant mentalité	Militarization	Threat of war by England	Population increase	Railroad
Collapse of the monarch	Aristocrats	State fiscal system	Patriotism	Corruption	Availability of educated technicians	Spread of heresy	Steam power
The Great Fear	Social class	Guild organization	Philosophy of liberty	Extension of transport		Association between the real wage and the fertility rate	Rifled cannon
Execution of the king	Political party or clique	Church	Working class conscious-ness				Glass making
		Monarchy					

"metal" constitutes a kind; "plastic" does not. "Gold" is a kind; "mud" is not. The question about social kinds, then, is this: are revolutions, riots, or kinship systems "social kinds"? That is, do social entities fall in groups of things that share deep, explanatory properties? And do "higher-level" social entities constitute social kinds? Do the categories of state, class, taxation system, religion, Islam, etc., constitute social kinds?

Examples of what might have been thought to be social kinds might include concepts such as these: proletariat, underclass resentment, revolutionary situation, racism, liberal representative states, fascism, feudalism, bureaucratic state. But I hold that these are not kinds in the strong sense that philosophers of the natural sciences have in mind. Rather, they are plastic, variable, opportunistic, individually specific instantiations across a variety of human contexts. We need to be able to identify some topics of interest, so we need language and concepts; but we must avoid reifying the concepts and thinking they refer to some underlying discoverable essence. (Think of how Chuck Tilly conceptualizes riot, rebellion, and resistance in terms of "contentious politics." Rightly, he avoids the idea that there is one common thing going on in these instances across time, history, and place; his goal is to identify a medium-sized body of causal mechanisms that bundle together in various contexts to give rise to one signature of contention or another.)

I take the view that complex structural concepts such as "state," "early modern state," "feudalism," or "free market economy" should be understood nominalistically, and should not be understood as referring to specific social essences or kinds. They are more akin to ideal types than to natural kinds. There are paradigm cases that correspond closely to the ideal type (nineteenth-century Britain, thirteenth-century France). But there is no regulative social system, capitalism or feudalism, that captures the institutions of a given society and transforms those institutions in the direction of a pure capitalist or pure feudal system. So I urge a nominalist understanding of social structure concepts—especially at the higher levels of description. There may be pure cases of feudalism in history; there are certainly many mixed cases; and the utility of the concept of feudalism is in focusing our thought in a first approximation as we begin analysis of the institutional specifics of a novel social order. What is real in the novel social order, however, is not its feudal character, but rather the specific set of institutions and organizations that are currently embodied and through which individuals exercise agency.

On this approach, a given social formation may be said to consist of specific social, economic, and political institutions; mentalités and systems of beliefs and values; and higher level structures that are composed of these institutions, practices, and mentalités. Social formations possess brute quantitative characteristics (population levels, population density, urbanization rates, nutritional levels of the population, educational levels of the population); and characteristics of individual behavior (levels of communal spirit or group identification, levels of violence). Agents act within the context of these structures; and their actions both reproduce and modify the structure. At any given time, agents are acting in ways that affect future states of the system while being prompted or constrained by existing structures and mentalités; and agents are being shaped by these structures and mentalités in ways that influence their future actions. Finally, the social formation is subject to "exogenous" influences: climate change, war, natural events (disastrous or favorable), the appearance of singular and exceptional individuals.

This approach concludes that there are no social kinds in the strong sense. Rather, there are social constructs that succeed in identifying groups of phenomena that share important common features. There are "cities," "riots," "states," and "kinship systems." But these concepts do not identify groups of entities that share a common essence, and they do not identify "kinds" or universals. Rather, they form heterogeneous groupings of contingently configured institutions and structures. The groupings of such entities do not have shared essences that would allow us to infer from one such element to the next. My view is that higher-level abstract social categories are non-essentialist concepts that pick out clusters of institutions based on observable features and paradigm instances. They do not constitute kinds.

What then is the ontological status of concepts such as "feudalism," "state," "free market", "bureaucracy", or "public goods problem"? These concepts represent idealized and abstracted theories of certain paradigm instances of particular configurations of institutions and relations. Once we have identified a specific interlocking set of institutions, it is possible to infer the institutional logic that these institutions produce. And this in turn gives us a theoretical basis for understanding concrete historical circumstances; to the extent that the abstract assumptions defining "feudalism" apply to a particular instance of Meiji Japanese society, we can use the historical model to explain or predict some of the developments that can be expected in the Japanese case. It is also true, of course, that the historical case does not exactly fit the model; so the model's behavior may in turn not exactly fit the historical developments. But it is often possible to discern the effects that the model predicts. Examples of the latter might include state, citizen, domain of politics, or bureaucracy. These might be called "institution-specific" concepts, in that they reflect historically contingent configurations of institutions that may or may not recur in other contexts.

This ontology is a sparse one, in the sense that it denies the existence of social kinds or universals. However, I maintain that comparative social and historical research can proceed on this basis, and that we come closer to identifying social kinds as we move downward along the slope of aggregation. Whole-society terms—"feudalism," "authoritarianism"—are further from being social kinds than are disaggregated terms such as "free-rider problem," "revenue extraction institution," or "free market." Feudalism and capitalism are not part of the furniture of the social world, whereas relations and institutions are.

The discovery of causal processes is essential to social explanation—not the discovery of high-level uniform categories of social events or structures. We explain social outcomes best when we can uncover the causal mechanisms that gave rise to them. However, most social ensembles are the result of multiple causal mechanisms, and their natures are not common, simple, or invariant. "States" embody mechanisms of social control. But as Tolstoy said about unhappy families, every state manages its contention in somewhat different ways. So we cannot and should not expect common causal properties across the class of "states". And this is directly relevant to the central point here: the "state" is not a social kind, and there is no simple theory that encapsulates its causal properties.

This approach has specific implications for the conduct of the social sciences. For example, political science and the study of different types of states: we can identify common mechanisms, sub-institutions, building blocks, etc., that recur in different political systems. And we can offer causal explanations of specific states in particular historical circumstances—for example, the Brazilian state in the 1990s. But we cannot produce strong generalizations about "states" or even particular kinds of states—for example, "developing states". Or at least, the generalizations we find are weak and exception-laden. Rather, we must build up our explanations from the component mechanisms and institutions found in the particular cases.

So here is a moderate form of realism that is well suited to the nature of the social world: be realist about social mechanisms but not about social kinds. And be a nominalist about social concepts. There are no macro or molar-level social kinds.

This approach to social ontology focuses on the level of the socially situated individual. Individuals exist; specific institutional arrangements exist; and specific ensembles of institutions exist. Note also what this list does not include: state, feudalism, market, Christianity. What, then, about higher-level social entities—economies, states, cultures? Into what sorts of structures or entities do these "elements" compose at broader levels of social functioning? On the ontology being advanced here, those higher-level entities are the sum of the congeries of socially situated individuals and institutions that exist at a given time. Higher-level structures supervene upon individuals and institutions.[3] Let us say that the comprehensive social entity at the macro-level is the social formation. It consists of a particular set of practices, organizations, and institutions at a particular stretch of time, through which human agency flows. Social formations, further, embody complexes of institutions that we denote as "state", "military regime", "market", "family", and other medium- and large-scale structures.

It is reasonable, on this approach, to affirm the existence of social structures like "the seventeenth-century French absolutist state", "the American industrial system", or "the Soviet military system"—if we note carefully the subordinate status that these higher-level concepts have. These social entities exist in the particular concrete forms that make them up in a particular time and place: the institutions that create rules, powers, and opportunities; the assignment of powers and restrictions to particular officers; the material factors and objects that embody various elements of these systems; the assumptions and values that individuals bring to their interactions with these institutions, and the like. In all instances the social entity is constituted by the social constructed individuals who make it up, through their beliefs, values, interests, actions, prohibitions, and powers.

These organizations and institutions constitute larger systems that can be termed "political," "economic," "demographic." But the latter set of terms—"state,"

[3] See Yaegwon Kim's exposition of this concept (Kim, 1984, 1993).

"market," "economic sphere", "religion"—should be regarded as nominal and provisional rather than essential. The French state, the British state, and the Indian polity all exist; but "the state as such" does not. Institutional configuration is plastic in its development and relatively sticky in operation. We can regard specific social formations as constituting distinctive regimes: distinctive and interlocking systems of institutions, norms, and groups that persist over time and through which agents pursue their goals. Moreover, given well-known processes of social feedback and selection, institutional settings will come over time to be adjusted so as to constitute a coherent system of institutions for accomplishing the social purposes of the society in question.

Another reason for skepticism about the availability of concepts that single out "social kinds" is a fundamental feature of social life: the plasticity and mutability of human institutions and social relations. The mutability and variety of social institutions—and therefore the inappropriateness of an essentialist view of "capitalism", "city", or "clientelism"—follows from a universal feature of human social agency. At any given time agents are presented with a repertoire of available institutions and variants (along the lines of Tilly's point about a repertoire of strategies of collective action or Bourdieu's analysis of social practice). The contents of the repertoire is historically specific, reflecting the examples that are currently available and that are available through historical memory. And the repertoire of institutional choices for Chinese decision makers was significantly different from that available in early modern Europe.

3.3 Methodological Localism

So the ontology that I defend comes down to socially constituted agents within social relations and institutions, possessing a set of material needs and purposes and a set of norms, beliefs, and goals that constitute the ground of their agency. These institutions convey individuals to the accomplishment of their purposes and embody various forms of power, production, and reproduction. And these institutions and practices in turn form larger configurations of institutions, practices, and organizations that we refer to as "states," "economic systems," "demographic regimes," and the like. It is then an empirical and contingent discovery when we discern important commonalities among the institutions of several distinct social formations—for example, similar systems of land tenure or systems of revenue extraction. The following, then, constitutes a simple social ontology:

- Individuals, relations, institutions exist
- Individuals have agency within constraints
- Institutions evolve to satisfy individual and collective purposes
- Institutions and organizations have powers
- Institutions have properties of organization and functioning

A coherent social ontology can now be formulated: individuals in social relations exist. Individuals in social relations constitute institutions that exist (that is, that persist and maintain their properties for extended periods of time). Configurations of institutions form higher-level complexes that we describe as large social structures: political systems, economic systems, cultural systems. And these higher-level structures too possess the qualities of persistence and continuity over significant periods (and surviving the comings and goings of the individuals who constitute them at a specific time) that permits us to say that they exist as durable social entities. This ontology places the level of "thing"-ness in the social realm close to the level of individuals in social relations and practices.

This view can be articulated more fully in terms of a theory of social ontology that I refer to as *methodological localism* (ML) (Little, 2006). This theory of social entities affirms that there are large social structures and facts that influence social outcomes. But it insists that these structures are only possible insofar as they are embodied in the actions and states of socially constructed individuals. The "molecule" of all social life is the socially constructed and socially situated individual, who lives, acts, and develops within a set of local social relationships, institutions, norms, and rules. With methodological individualism, this position embraces the point that individuals are the bearers of social structures and causes. There is no such thing as an autonomous social force; rather, all social properties and effects are conveyed through the individuals who constitute a population at a time. Against individualism, however, methodological localism affirms the "socialness" of social actors. Methodological localism denies the possibility or desirability of characterizing the individual extra-socially. Instead, the individual is understood as a socially constituted actor, affected by large current social facts such as value systems, social structures, extended social networks, and the like. In other words, ML denies the possibility of reductionism from the level of the social to the level of a population of non-social individuals. Rather, the individual is formed by locally embodied social facts, and the social facts are in turn constituted by the current characteristics of the persons who make them up.

This account begins with the socially constituted person. Human beings are subjective, intentional, and relational agents. They interact with other persons in ways that involve competition and cooperation. They form relationships, enmities, alliances, and networks; they compose institutions and organizations. They create material embodiments that reflect and affect human intentionality. They acquire beliefs, norms, practices, and worldviews, and they socialize their children, their friends, and others with whom they interact. Some of the products of human social interaction are short-lived and local (indigenous fishing practices); others are long-duration but local (oral traditions, stories, and jokes); and yet others are built up into social organizations of great geographical scope and extended duration (states, trade routes, knowledge systems). But always we have individual agents interacting with other agents, making use of resources (material and social), and pursuing their goals, desires, and impulses.

At the level of the socially constituted individual we need to ask two sorts of questions: First, what makes individual agents behave as they do? Here we need accounts of the mechanisms of deliberation and action at the level of the individual. What are the main features of individual choice, motivation, reasoning, and preference? How do these differ across social groups? How do emotions, rational deliberation, practical commitments, and other forms of agency influence the individual's deliberations and actions? This area of research is purposively eclectic, including performative action, rational action, impulse, theories of the emotions, theories of the self, or theories of identity.

Second, how are individuals formed and constituted? Methodological localism gives great importance to learning more about how individuals are formed and constituted—the concrete study of the social process of the development of the self. Here we need better accounts of social development, the acquisition of worldview, preferences, and moral frameworks, among the many other determinants of individual agency and action. What are the social institutions and influences through which individuals acquire norms, preferences, and ways of thinking? How do individuals develop cognitively, affectively, and socially? So methodological localism points up the importance of discovering the microfoundations and local variations of identity formation and the construction of the historically situated self.

So far we have emphasized the socially situated individual. But social action takes place within spaces that are themselves socially structured by the actions and purposes of others—by property, by prejudice, by law and custom, and by systems of knowledge. So our account needs to identify the local social environments through which action is structured and projected: the inter-personal networks, the systems of rules, the social institutions. The social thus has to do with the behaviorally, cognitively, and materially embodied reality of *social institutions*. An institution, we might say, is an embodied set of rules, incentives, and opportunities that have the potential of influencing agents' choices and behavior.[4] An institution is a complex of socially embodied powers, limitations, and opportunities within which individuals pursue their lives and goals. A property system, a legal system, and a professional baseball league all represent examples of institutions.[5] Institutions have effects that are in varying degrees independent from the individual or "larger" than the individual. Each of these social entities is embodied in the social states of a number of actors—their beliefs, intentions, reasoning, dispositions, and histories. Actors perform their actions within the context of social frameworks represented as rules, institutions, and organizations, and their actions and dispositions embody the causal effectiveness of those frameworks. And institutions influence individuals by offering incentives and constraints on their actions, by framing the knowledge and information on the basis of which they choose, and by conveying sets of

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[4] "Institutions are the humanly devised constraints that structure human interaction. They are made up of formal constraints (for example, rules, laws, constitutions), informal constraints (for example, norms of behavior, conventions, self-imposed codes of conduct), and their enforcement characteristics. Together they define the incentive structure of societies and, specifically, economies" (North, 1998, p. 247).

[5] See Brinton and Nee (1998), Ensminger (1992), North (1990), and Knight (1992) for recent expositions of the new institutionalism in the social sciences.

normative commitments (ethical, religious, interpersonal) that influence individual action.

It is important to emphasize that ML affirms the existence of social constructs beyond the purview of the individual actor or group. Political institutions exist—and they are embodied in the actions and states of officials, citizens, criminals, and opportunistic others. These institutions have real effects on individual behavior and on social processes and outcomes—but always mediated through the structured circumstances of agency of the myriad participants in these institutions and the affected society. This perspective emphasizes the contingency of social processes, the mutability of social structures over space and time, and the variability of human social systems (norms, urban arrangements, social practices, . . .).

This approach highlights the important point that all social facts, social structures, and social causal properties depend ultimately on facts about individuals within socially defined circumstances. Social ascriptions require *microfoundations* at the level of individuals in concrete social relationships. According to this way of understanding the nature of social ontology, an assertion of a structure or process at the macro-social level (causal, functional, structural) must be supplemented by two things: knowledge about what it is about the local circumstances of the typical individual that leads him or her to act in such a way as to bring about this relationship; and knowledge of the aggregative processes that lead from individual actions of that sort to an explanatory social relationship of this sort.[6] So if we are interested in analysis of the causal properties of states and governments, we need to arrive at an analysis of the institutions and constrained patterns of individual behavior through which the state's characteristics are effected. We need to raise questions such as these: How do states exercise influence throughout society? What are the institutional embodiments at lower levels that secure the impact of law, taxation, conscription, contract enforcement, and other central elements of state behavior?[7] If we are concerned about the workings of social identities, then we need to inquire into the concrete social mechanisms through which social identities are reproduced within a local population—and the ways in which these mechanisms and identities may vary over time and place. And if we are interested in analyzing the causal role that systems of norms play in social behavior, we need to discover some of the

[6] We may refer to explanations of this type as "aggregative explanations." An aggregative explanation is one that provides an account of a social mechanism that conveys multiple individual patterns of activity and demonstrates the collective or macro-level consequence of these actions.Thomas Schelling's *Micromotives and Macrobehavior* (Schelling, 1978) provides a developed treatment and numerous examples of this model of social explanation.

[7] An excellent recent example of historical analysis of Chinese local politics illustrates the value of this microfoundational approach: "But the villages were not totally out of the government's reach; nor was the subcounty administration necessarily chaotic, inefficient, and open to malfeasance. In fact, during most of the imperial times, the state was able to extract enough taxes to meet its normal needs and maintain social order in most of the country. What made this possible was a wide variety of informal institutions in local communities that grew out of the interaction between government demands and local initiatives to carry out day-to-day governmental functions" (Li, 2005, p. 1).

specific institutional practices through which individuals come to embrace a given set of norms.[8]

The microfoundations perspective requires that we attempt to discover the pathways by which socially constituted individuals are influenced by distant social circumstances, and how their actions in turn affect distant social outcomes. There is no action at a distance in social life; instead, individuals have the values that they have, the styles of reasoning, the funds of factual and causal beliefs, etc., as a result of the structured experiences of development that they have undergone as children and adults. On this perspective, large social facts and structures do indeed exist; but their causal properties are entirely defined by the current states of psychology, norm, and action of the individuals who currently exist. Systems of norms and bodies of knowledge exist—but only insofar as individuals (and material traces) embody and transmit them. So when we assert that a given social structure causes a given outcome, we need to be able to specify the local pathways through which individual actors embody this causal process. That is, we need to be able to provide an account of the causal mechanisms that convey social effects.

It is evident that methodological localism implies a fairly limited social ontology. What exists is the socially constructed individual, within a congeries of concrete social relations and institutions. The socially constructed individual possesses beliefs, norms, opportunities, powers, and capacities. These features are socially constructed in a perfectly ordinary sense: the individual has acquired his or her beliefs, norms, powers, and desires through social contact with other individuals and institutions, and the powers and constraints that define the domain of choice for the individual are largely constituted by social institutions (property systems, legal systems, educational systems, organizations, and the like).[9] Inevitably, social organizations at any level are constituted by the individuals who participate in them and whose behavior and ideas are influenced by them; sub-systems and organizations through which the actions of the organization are implemented; and the material traces through which the policies, memories, and acts of decision are imposed on the environment: buildings, archives, roads, etc. All features of the organization are embodied in the actors and institutional arrangements that carry the organization at a given time. At each point we are invited to ask the question: what are the social mechanisms through which this institution or organization exerts influence on other organizations and on agents' behavior?

Methodological localism has numerous intellectual advantages. It avoids the reification of the social that is characteristic of holism and structuralism, it abjures social "action at a distance," and it establishes the intellectual basis

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[8] "Explanations of social norms must do more than merely acknowledge the constraining effects of normative rules on social action. Such explanations must address the process that culminates in the establishment of one of these rules as the common norm in a community. One of the keys to the establishment of a new norm is the ability of those who seek to change norms to enforce compliance with the new norm" (Knight and Ensminger, 1998, p. 105).

[9] Hacking (1999) offers a critique of misuses of the concept of social construction. This use is not vulnerable to his criticisms, however.

for understanding the non-availability of strong laws of nature among social phenomena. It is possible to offer numerous examples of social research underway today that illustrate the perspective of methodological localism; in fact, almost all rigorous social theorizing and research can be accommodated to the assumptions of methodological localism.[10]

This set of views captures much of what I have come to believe is most fundamental about social ontology. First, it is scientifically important for historians and social scientists to arrive at a more adequate understanding of the social ontology that underlies their work, and such an ontology can be reasonably simple. The socially constituted agent within a set of social relations and institutions provides us a rich basis for characterizing social phenomena, and permits us to hypothesize higher-level structures and institutions as well.

Second, higher-level social structures exist; but they have their properties solely in virtue of the specific practices, rules, and arrangements that constitute them at a time and within a group of people. Higher-level structures are composed of the individuals, networks, and sub-institutions that coordinate and constrain the actions of persons throughout the scope of the social structure.

Third, macro-social entities exercise causal properties through the individuals who constitute them at a given time. Social entities convey causal properties through their effects, direct and indirect, on individuals and agency.

Fourth, social structures and institutions are plastic over time and space. We need to exercise great caution in postulating high-level abstract structures that recur across instances—state, mode of production, protestant ethic, Islam. Social institutions, structures, and practices "morph" over time in response to opportunism and power by the participants.

3.4 The Heterogeneous Social

Heterogeneity is a very basic characteristic of the domain of the social. And this makes a big difference for how we should attempt to study the social world "scientifically". How do the concepts of heterogeneity and homogeneity apply to the social world?[11]

Let's start with some semantics. A heterogeneous group of things is the contrary of a homogeneous group, and we can define homogeneity as "a group of

[10] Examples of theories and analyses contained in current comparative and historical social science research may be found in McDonald (1996) and Mahoney and Rueschemeyer (2003). Many of these examples illustrate the fecundity of the approach to social analysis that emphasizes the "socially constructed individual within a concrete set of social relations" as the molecule of social action. See also Sewell (2005) for a treatment of historical change that is compatible with this approach.

[11] Peter Katzenstein and others have begun to provide a way of understanding the role of social-science theory in historical explanations that is very consistent with these points about heterogeneity; they make use of the idea of "analytic eclecticism" (Sil and Katzenstein, 2009).

fundamentally similar units or samples". A homogeneous body may consist of a group of units with identical properties, or it may be a smooth mixture of different things, consisting of a similar composition at many levels of scale. A fruitcake is non-homogeneous, in that distinct volumes may include just cake or a mix of cake and dried cherries, or cake and the occasional walnut. The properties of fruitcake depend on which sample we encounter. A well mixed volume of oil and vinegar, by contrast, is homogeneous in a specific sense: the properties of each sample volume are the same as any other. The basic claim about the heterogeneity of the social comes down to this: at many levels of scale we continue to find a diversity of social things and processes at work. Society is more similar to fruitcake than cheesecake.

Heterogeneity makes a difference because one of the central goals of positivist science is to discover strong regularities among classes of phenomena, and regularities appear to presuppose homogeneity of the things over which the regularities are thought to obtain. So to observe that social phenomena are deeply heterogeneous at many levels of scale, is to cast fundamental doubt on the goal of discovering strong social regularities. Let us consider some of the forms of heterogeneity that the social world illustrates.

First is the heterogeneity that can be discovered within social categories of things—cities, religions, electoral democracies, social movements. Think of the diversity within Islam documented so well by Clifford Geertz (Geertz, 1968); the diversity at multiple levels that exists among great cities like Beijing, New York, Geneva, and Rio (institutions, demography, ethnic groups, economic characteristics, administrative roles, ...); the institutional variety that exists in the electoral democracies of India, France, and Argentina; or the wild diversity across the social movements of the right.

Second is the heterogeneity of social causes and influences. Social events are commonly the result of a variety of different kinds of causes that come together in highly contingent conjunctions. A revolution may be caused by a protracted drought, a harsh system of land tenure, a new ideology of peasant solidarity, a communications system that conveys messages to the rural poor, and an unexpected spar within the rulers—all coming together at a moment in time. And this range of causal factors, in turn, shows up in the background of a very heterogeneous set of effects. (A transportation network, for example, may play a causal role in the occurrence of an epidemic, the spread of radical ideas, and a long, slow process of urban settlement.) The causes of an event are a mixed group of dissimilar influences with different dynamics and temporalities, and the effects of a given causal factor are also a mixed and dissimilar group.

Third is the heterogeneity that can be discovered across and within social groups. It is not the case that all Kansans think alike—and this is true for whatever descriptors we might choose in order to achieve greater homogeneity (evangelical Kansans, urban evangelical Kansans, ...). There are always interesting gradients within any social group. Likewise, there is great variation in the nature of ordinary, lived experience—national holidays of independence have a different cultural meaning for middle-class French families celebrating *quatorze Juillet*, for

Californians celebrating July 4, and for Brazilians enjoying *Dia da Independência* on September 7.

A fourth form of heterogeneity takes us within the agent herself, when we note the variety of motives, moral frameworks, emotions, and modes of agency on the basis of which people act. This is one of the weaknesses of doctrinaire rational choice theory or dogmatic Marxism, the analytical assumption of a single dimension of motivation and reasoning. Instead, it is visible that one person acts for a variety of motives at a given time, persons shift their motives over time, and members of groups differ in terms of their motivational structure as well. So there is heterogeneity of motives and agency within the agent.

These dimensions of heterogeneity make the point: the social world is an ensemble, a dynamic mixture, and an ongoing interaction of forces, agents, structures, and mentalités. Social outcomes emerge from this heterogeneous and dynamic mixture, and the quest for general laws is deeply quixotic.

Where does the heterogeneity principle take us? It suggests an explanatory strategy: instead of looking for laws of whole categories of events and things, rather than searching for simple answers to questions like "why do revolutions occur?", we might instead look to a "concatenation" strategy. That is, we might simply acknowledge the fact of molar heterogeneity and look instead for some of the different processes and things in play in a given item of interest, and the build up a theory of the whole as a concatenation of the particulars of the parts.

3.4.1 Variation

Variation within a social or historical phenomenon seems to be all but ubiquitous. Think of the Cultural Revolution in China, demographic transition in early modern Europe, the ideology of a market society, or the experience of being black in America. We have the noun—"Cultural Revolution"—which can be explained or defined in a sentence or two as an extended social phenomenon of mobilization and conflict that took place in China from 1966 to 1976; and we have the complex underlying social realities to which it refers, spread out over many cities, villages, and communes across China (Esherick et al., 2006). Or consider another general noun, "demographic transition," defined as a period in which a population experiences abrupt decline in mortality, followed by a decline in fertility. Using a variety of statistical methods, historical demographers can document the occurrence of a demographic transition in different periods in Sweden, Italy, Britain, and China. And it turns out that there are both common features and distinguishing characteristics that emerge from detailed study—differences in timing, differences in social composition, differences in the mechanisms bringing these changes about.

In each case there is a very concrete and visible degree of variation in the factor over time and place. Historical and social research in a wide variety of fields confirms the non-homogeneity of social phenomena and the profound location-specific variations that occur in the characteristics of virtually all large social phenomena. Social nouns do not generally designate uniform social realities. These facts of local and regional variation provide an immediate rationale for case studies and comparative research, selecting different venues of the phenomenon and identifying specific features of the phenomenon in this location. Through a range of case studies it is possible for the research community to map out both common features and distinguishing features of a given social process.

This description focuses on locational variation in processes—village to village, country to country. But social scientists often also highlight variations across social segments *within* a given location: class, race, gender, religion, occupation. Do sharecroppers have a different fertility profile over time than the wealthy in a particular region at a particular time? Are there significant differences in survival strategies for distinct groups defined by race or ethnicity in a city or a group of cities?

This situation of variation and case-specific research raises a number of challenging questions. One is the question of whether the phenomenon designated by the noun is one integrated social reality, with varied expressions across locations, or whether instead the different locations are simply loosely similar but independent occurrences. Simon Schama's radical question—was there a French Revolution, or were there simply a congeries of periods and locations of disturbance?—illustrates this question, as does a parallel question about the reality of the revolutions of 1848.

A second major question is the challenge of discovering causal and social mechanisms connecting the various social locations encompassed by the phenomenon. How did the activism and ideology of Cultural Revolution spread from Beijing to Nanjing and other locations? How did activism spread from city to rural locations? How did local circumstances cause changes and variations in the political movement? How much path dependency existed in the spread of revolutionary ideas and strategies?

There is a more epistemic set of questions as well, concerning generalizability. Fundamentally, if there is substantial variation across locations and instances of a given phenomenon, then to what degree can we say anything about the phenomenon as a whole? And what does the study of one location allow us to say about the larger processes? Does study of the Tsinghua student Red Guard movement tell us anything about Red Guard mobilization in other places? Or is it simply one of many different and contingent developments of contentious politics during the period? Can we generalize from case studies and comparative research?

This is where the appeal to social mechanisms seems once more to be highly relevant and helpful. If we work on the assumption that any large social process—the dispersed locations of contention associated with the French Revolution, say—is the compound result of a set of underlying causal social mechanisms, and if we hypothesize that many of these mechanisms are in play in some places but not in others; then we can explain both similarity and difference in the occurrence of the phenomenon across time and place. Detailed investigation based on comparative historical research may reveal:

- There is a common set of conditions across the regions (e.g. famine or drought)
- There are common causes that mobilize people in many separate places (tax protests, land confiscations)
- There are common political traditions

- There is substantial inter-location communication and influence
- There are no large institutional or circumstantial variations that would drive significant variations in outcomes across locations

Now the work of historical investigation can be put in these terms: identify some of the social mechanisms that evidently recur in various locations; identify some of the mechanisms that lead to significantly different results in some places; and identify some of the cross-location mechanisms that are at work to secure a degree of synchrony and parallel in the developments observed in different locations (communication systems, networks of leaders, dissemination of activists). Case studies and comparative research permit both a degree of generalization and an explanation of variation.

In other words, the intellectual strategy here is to disaggregate the large social factor into the results of a larger number of underlying mechanisms; and then to attempt to discover how these mechanisms played out differently in different settings throughout the range of the French Revolution, protoindustrialization, or ethnic conflict in South Asia. Significantly, this is exactly the strategy of research and explanation that Charles Tilly was led to in his emphasis on discovering the component social mechanisms that underlie social contention (McAdam et al., 2001).

3.4.2 The Heterogeneous Social Whole

These points demonstrate that our social ontology needs to reflect the insight that complex social happenings are almost invariably composed of multiple causal processes rather than existing as unitary systems. The phenomena of a great social whole—a city over a 50-year span, a period of sustained social upheaval or revolution (Iran in the 1970s–1980s), an international trading system—should be conceptualized as the sum of a large number of separate processes with intertwining linkages and often highly dissimilar tempos. We can provide analysis and theory for some of the component processes, and we can attempt to model the results of aggregating these processes. And we can attempt to explain the patterns and exceptions that arise as the consequence of one or more of these processes. Some of the subordinate processes will be significantly amenable to theorizing and projection, and some will not. And the totality of behavior will be more than the "sum" of the relatively limited number of processes that are amenable to theoretical analysis. This means that the behavior of the whole will demonstrate contingency and unpredictability modulo the conditions and predictable workings of the known processes.

The point here is that social and historical knowledge requires recognition of the inherent heterogeneity of social phenomena and a fertile effort to find ways of segmenting this heterogeneous reality that shed light on social causation and patterns of behavior. And it is important to recognize that any level of granularity of analysis could be further partitioned and more fully described—sometimes with important insight. There is no "fundamental" or "optimal" level of analysis and description that captures the whole of Chicago. Instead, anthropology;

sociology, and political science can continually pursue the upward and downward research journey of discovering meaningful group-level patterns or regularities, and pressing into a deeper understanding of the diversity of the phenomena under study.

A social whole—the city of Chicago, for example—is a densely various empirical reality. At virtually every level of scale there is variance with respect to social characteristics—income, health status, ethnic or social identity, political adherence and preference, age, race, or occupation. Neighborhoods differ from each other—but equally, we find variance within neighborhoods as well. Given this fact of radical non-homogeneity of social characteristics, what is involved in arriving at knowledge about such a reality?

It is evident that we are forced to arrive at generalized descriptions, at some level of scale or granularity. It is neither feasible nor explanatory to provide a "fully" detailed description of a population, individual by individual. Instead, our challenge is to arrive at some ways of segmenting the population into groups that will prove felicitous in revealing causal connections among attributes or circumstances. Groups may be defined with unlimited range: geographically, occupationally, racially or ethnically, educationally, politically, and so on indefinitely. We can then observe and measure the distribution and means of various characteristics across these groups (attitudes towards the Patriot Act, for example) and we can consider whether there are meaningful differences across groups with respect to these characteristics. Finally, we can try to find causal explanations of these differences. Are Arab-Americans more distrustful of national security laws than Asian-Americans? Are poor people more prone to asthma than affluent people? Are doctors more favorable to higher taxes than skilled-trades workers? What factors might causally explain these differences?

Consider the example of the development of a large city over time. The sorts of subordinate processes that might be involved include—

- The habitation dynamics created by the nodes of a transportation system
- The dynamics of electoral competition governing the offices of mayor and city council
- The politics of land use policy and zoning permits
- The dynamics and outcomes of public education on the talent level of the population
- The demographic and economic circumstances of the periphery, influencing in-migration to the city
- Economic development policies and tax incentives emanating from state government
- Dynamics of real estate system with respect to race
- Employment and poverty characteristics of surrounding region

Each of these processes can be investigated by specialists—public policy experts, sociologists of race and segregation, urban politics experts. Each contributes to features of the evolving urban environment. And it is credible that there are consistent patterns of behavior and development within these various types of processes. This justifies a specialist's approach to specific types of causes of urban change, and rigorous social science can result.

But it should also be recognized that there are system interdependencies among these groups of factors. More in-migration of extremely poor families may put more stress on the public schools. Enhancement of quality or accessibility of public schools may increase in-migration. Political incentives within the city council system may favor land-use policies that encourage the creation of racial or ethnic enclaves. So it isn't enough to understand the separate processes individually; we need to make an effort to discover these endogenous relations among them.

But over and above this complication of the causal interdependency of recognized factors, there is another and more pervasive complication as well. For any given complex social whole, it is almost always the case that there are likely to be additional causal processes that have not been separately analyzed or theorized. Some may be highly contingent and singular—for example, the many effects that September 11 had on Manhattan. Others may be systemic and important, but novel and previously untheorized—for example, the global information networks that Saskia Sassen emphasizes for the twenty-first century global city (Sassen, 2001).

The upshot is that a complex social whole exceeds the particular theories we have created for this kind of phenomenon at any given point in time. The social whole is composed of lower-level processes; but it isn't exhausted by any specific list of underlying processes. Therefore we should not imagine that the ideal result of investigation of urban phenomena is a comprehensive theory of the city—the goal is chimerical. Social science is always "incomplete", in the sense that there are always social processes relevant to social outcomes that have not been theorized.

What analytical frameworks are available for capturing this understanding of the compositional nature of society? There are several, converging from different quarters. First is the idea of looking for microfoundations for observed social processes (Little, 1998). Here the idea is that higher-level social processes, causes, and events, need to be placed within the context of an account of the agent-level institutions and circumstances that convey those processes. Second is the method of causal mechanisms advocated by McAdam, Tarrow, and Tilly, and discussed extensively in Chapter 5 (McAdam et al., 2001). Put simply, the approach recommends that we explain an outcome as the contingent result of the concatenation of a set of independent causal mechanisms (escalation, intra-group competition, repression, ...). And third is the theory of "assemblages", derived from some of the theories of Gilles Deleuze, with its ontology of separable sub-processes "assembling" into larger social wholes. (Manuel De Landa describes this theory in *A New Philosophy of Society: Assemblage Theory and Social Complexity* (De Landa, 2006).)

Philosophers and social scientists working from the perspective of "system complexity" bring a different but helpful perspective to this topic. Particularly interesting are the contributions to *Hierarchy in Natural and Social Sciences* (Pumain, 2006). Here is how Denise Pumain frames the approach: "Hierarchy is a type of systemic organization into levels that are ordered with reference to criteria of a normative character, and fully or partially subordinated by relationships of power, influence, or control. Many intermediate situations are observed between strictly designed hierarchies where levels are distinct and communication restricted to vertical top-down command, and more flexible structures where the levels are not easily recognizable and the network circulation of information reveals unequal degrees of accessibility or control only after a detailed analysis" (p. 1). Saskia Sassen raises similar issues of hierarchy in her *Sociology of Globalization* (Sassen, 2007). A crucial part of her analysis is her effort to overturn the assumption of "linearity" and hierarchy among levels of analysis—the line of thought that assumes that neighborhoods are encompassed by cities, which fall within regions, which fall within states, which fall within international relations. She argues repeatedly and effectively that this linear scheme does not work for today's global relationships. The local neighborhood may be implicated in extra-national relations of immigration, crime, and trade that make it a global place. More importantly, what she calls "global cities" have crucial relationships at many levels in these supposed hierarchies—local, national, and supra-national. So the question of scale cannot be defined within a simple hierarchy of relationships of locality, nationality, and globality.

Each of these ideas gives expression to the important truth of the heterogeneity principle: that social outcomes are the aggregate result of a number of lower-level processes and institutions that give rise to them, and that social outcomes are contingent results of interaction and concatenation of these lower-level processes. In each case, we have the idea that the social entity is composed of underlying processes that take us back in the direction of agents acting within the context of social and environmental constraints. And we have a premise of causal openness: the behavior of the whole is not fully determined by a particular set of subordinate mechanisms or assemblages.

3.4.3 What Cities Have in Common

Given the heterogeneous nature of social groupings, we might imagine that every social entity must be considered as a unique individual. This is not the case, however. There are mid-level features that entities such as riots, cities, or bureaucracies possess in common, and it is a valuable exercise for historians and social scientists to discover some of these common features. So let us examine one such category a little more closely: the city.

The "city" is a pretty heterogeneous category, encompassing human places that differ greatly with each other and possess a great deal of internal social heterogeneity as well. Size, population structure, economic or industrial specialization, forms of governance, and habitation and transportation structure all vary enormously across the population of cities. And so New York (2000), Chicago (1930), Rome (200), Mumbai (2008), Beijing (1800), Lagos (1970), London (1600), and Mexico City (1990) are all vastly different human agglomerations; and yet all are "cities". Ancient, modern, medieval, developed, and underdeveloped—each of these places represents an urban concentration of population and habitation.

Given these many dimensions of difference, we can reasonably ask whether there are any shared urban characteristics or processes. Is there anything that a scientific or historical study of cities can discover? Is there a body of observation and discovery that might constitute the foundation for a sociology of cities?

Several points emerge quickly when we pose the question in these terms. First, a city is by definition a dense concentration of human inhabitants in a limited space. Human beings have material needs that must be satisfied daily: fresh water, food, shelter, clothing, and fuel, for example. Rural people can satisfy many of these needs directly through access to land, farms, and other natural resources. Urban populations are too dense to support individual or family self-sufficiency. So cities have this in common: they must have developed logistical systems for supplying residents with food, clean water, sanitation, and other basic necessities. This is the insight that motivated von Thünen in the development of central place theory (Thunen, 1826); equally it underlies William Cronon's analysis of Chicago (Cronon, 1991) and G. William Skinner's analysis of urban hierarchies in China (Skinner, 1977a).

A second feature of cities is the unavoidable need for value-adding, non-agricultural production within the city. This means activities such as manufacturing, artisanal production, and the provision of services for pay. The residents of the city need to gain income in order to have "purchasing power" to acquire the necessities of life from the countryside. This implies a social organization that supports employment and occupations. So cities share this characteristic as well: they are grounded systems of production and exchange, permitting labor, production, circulation of commodities, and consumption. (Reasoning something like this underlay the analysis of Chicago into "ecological zones" pioneered by Chicago School sociologists Park and Burgess; Park et al., 1967.)

Given that cities are inherently spatial, the economic characteristics just mentioned also imply a circulation of persons throughout the city. And this implies that cities must possess some organized system of transportation. This may be walking pathways, roads for carriages, streetcars, buses, or subways and railroads. But economic activity and production requires circulation of people, and this implies urban transportation. But, as Sam Bass Warner showed, transport systems in turn create new patterns of residence and work in their wake (Warner, 1969).

A third common urban characteristic has to do with inequalities of power, influence, and property. Human populations seem always to embody significant inequalities along these lines. But advantages of power and wealth can be almost automatically transformed into facts of urban geography by the *nomenklatura*, the elites. So cities are likely to bear the signature of the social inequalities of wealth and power that are interwoven in their histories. The attractive locations for homes and gardens, preferred access to amenities such as water and roads, even locations favored from the point of view of pest and disease—these locations are likely to be stratified by wealth and power. (Engels' description of the habitation patterns of bourgeoisie and proletariat in Birmingham and Manchester are illustrative; Engels, 1958.)

Fourth, cities require formal systems of governance and law. Village society may succeed in establishing stable social order based on informal norms and processes.

But cities are too large and complex to function as informal arrangements. Instead, there need to be ordinances for public health and safety, maintenance of public facilities, land use processes, and rules of public safety. Absent such governance, it is inconceivable that a city of one hundred thousand or a million would succeed in maintaining the delicate patterns of coordination needed for the continuing wellbeing of the residents.

So cities can be predicted to possess a variety of forms of social, political, economic, and geographical organization. Cities are not formless concentrations of humanity; rather, they are functional systems that can be investigated in depth. And here is the historical reality that permits this analysis to escape the charge of functionalism; the social systems that cities currently possess are the result of designs and adaptations of intelligent, strategic actors in the past. This means that they may be markedly non-optimal; they are likely to be skewed towards the interests and comfort of elites; but they are likely to *work* at some level of success.

These observations do not exactly answer the original question in a tidy way; they do not establish specific forms or characteristics that all cities share. But they do define a set of existential circumstances that cities must satisfy, and they pose in turn a series of questions about social organization and function that are likely to shed light on every city. We might look at this discussion as suggesting a matrix of analysis for all cities, corresponding to the large social needs mentioned here. How does a given city handle logistics, provisioning, local economic activity, transportation, land use, governance, public order, sanitation and health, and inequalities? What are the organizations and systems through which these central and inevitable tasks are accomplished? And then, perhaps, we may find a basis for classifying cities into large groups, based on the similarities that exist at this level of structure and organization. (For example, reasoning something like this leads G. William Skinner to distinguish between administrative and commercial cities in late Imperial China; Skinner, 1977a.)[12]

3.5 Conclusions

Do social structures have the features of permanence, demarcation, and reidentification that allow us to call them "things"? Are social structures more like molecules or clouds? Do states, societies, crowds, organizations, institutions, mobs, or classes exist? Are there social things?

Yes, there are individual social things we refer to as states, crowds, institutions. But no, these individuals do not form social kinds. The things we refer to as "states" or "crowds" do not have underlying essences that permit us to infer to new cases.

[12] A highly interesting contemporary attempt to identify some of the heterogeneous and non-linear aspects of social change in cities is found in *Complexity Perspectives in Innovation and Social Change* (Lane et al., 2009).

In particular, I offer reasons for doubt about social kinds. Terms like feudalism, proto-industrialization, revolution should be understood nominally, not essentially or realistically. They do not refer to a real and unchanging class of instances. Rather, they serve to pick out historical instances that show similarities and differences to paradigm cases. We can be realist about social things—relations, institutions, practices, organizations—in particular settings, but nominalist about the groups of such things across contexts.

This position represents a very sparse ontology. Social things exist, but they do not constitute social kinds of things. A social order existed in northern France in the thirteenth century that can be classified as "feudal". The social order existed; feudalism does not.

Is this approach enough for the purposes of historical analysis and explanation? I believe so. The historian compares complexes of social relations and institutions that perform certain social functions; and he/she compares, differentiates, and analyzes these complexes. There are "states," "economies," and "religions"; but they are heterogeneous groups of social things that share properties in fluid and changeable ways, depending on underlying features of structure and agency that produce these properties.

Chapter 4
Large Structures

The previous chapter came to the conclusion that we need to exercise a good deal of caution when we analyze history in terms of large structures such as the feudal mode of production, the Industrial Revolution, or the proletariat. It is important not to reify our concepts of large structures, and it is important to recognize the characteristics of heterogeneity and plasticity that attach to even the most visible and persistent of social structures. This chapter looks in greater detail at some of the intellectual challenges that must be addressed when we postulate a role for large structures in history.

What are the central assumptions we make in designating something as a social structure? Several ideas appear to be core features in our ordinary understanding of this concept. A social structure consists of rules, institutions, and practices. A social structure is socially embodied in the actions, thoughts, beliefs, and durable dispositions of individual human beings. A social structure is effective in organizing behavior of large numbers of actors. A structure is coercive of individual and group behavior. A social structure assigns roles and powers to individual actors. A social structure often has distributive consequences for individuals and groups. A social structure is geographically dispersed. Social structures can cause social outcomes involving both persistence and change.

We might try to reduce these intuitions to a definition: a social structure is a system of geographically dispersed rules and practices that influence the actions and outcomes of large numbers of social actors.

What are some examples of putative large structures? There are several that readily come to mind: a nation's economic system, its property and inheritance system, its system of law, legislation, and enforcement, its system of government, taxation, and policy-making, its educational system, religious organizations and traditions, the composite system of organizations that exist within civil society, the political systems of fascist states, and the norms and relations of the family.[1] These are all

[1] "Large structures" are a natural subject of interest in international relations theory. Some theorists are constructing approaches to international institutions that encompass both large causal reach and significant heterogeneity across instances; for example, in the concept of "analytic eclecticism" (Sil and Katzenstein, 2009).

D. Little, *New Contributions to the Philosophy of History*, Methodos Series 6,
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systems of regulation and practice that have extensive scope across a large geographical area and a significant population, and there is a significant degree of visible "sub-structuration" through which the large structure extends its influence across its scope.

The scope of action of the putative structure is crucial in this discussion. The background presupposition is that a great structure encompasses a large population and territory. So we would not call the specific marriage customs that govern a small group of Alpine villages but extend no further a "great structure." And it is further assumed that the hypothesized structure possesses a high degree of functional continuity and integration; there are assumed to be concrete social processes that assure that the structure works in roughly the same way throughout its scope to regulate behavior.

The idea of a "great structure" thus requires that we attend to the contrast between locally embodied institutions showing significant variation across time and space, and the supposedly more homogeneous workings of "great structures." We need to be able to provide an account of the extended social mechanisms that establish the effects and stability of the great structure. If we cannot validate these assumptions about scope, continuity, and functional similarity, then the concept of a "great structure" collapses onto a concatenation of vaguely similar institutions in different times and places.

Several points emerged in the previous chapter that need to be emphasized here. First, social phenomena are ultimately the aggregate result of the behavior of socially constituted persons who are acting within the context of locally embodied institutions. If there are regularities within the social realm, they derive from common features of individual agency, pervading systems of coercion and incentive, common features of institutions, and common processes of aggregation of effects. Second, this implies that historians should always keep in mind the real underlying behavioral and institutional settings that constitute the social processes or patterns they are interested in. It also implies that historians should expect plasticity and heterogeneity of social processes. Finally, any social entity must possess microfoundations in human purposiveness and actions. There is no such thing as a social entity that lacks human embodiment—any more than there are works of art that lacks material embodiment. Social entities "supervene" upon human individuals (Kim, 1993; Zahle, 2007).

This point also applies to any statements we might make about the putative causal powers of a social entity. So claims about the causal properties of social structures must be supplemented by a theory of the microfoundations of those powers. How does an extended social structure exert influence over the actions of located individuals?

And there is a final parallel point about claims about the geographical scope and coherence of a social entity. If we want to maintain that an entity exercises influence as a coherent and extended entity, we need to be able to specify the mechanisms through which this takes place. How does the Federal state exert its control and influence over the vast scope of the United States and its population? What are the subordinate organizations and structures through which the great structure operates to influence local behavior? How do central executives ensure that their decisions and policies are faithfully exercised? The necessity of a significant degree of nested sub-organizations within a great structure plainly raises the possibility of what political scientists call "principal-agent" problems; lower-level functionaries exert their independence and act in ways that are contrary to the decisions and policies of the central agencies.

So, with these qualifications about the unavoidable need for providing microfoundations—are there large social structures? To fit the bill, a great structure should have some specific features of scope and breadth. It should be geographically widespread, affecting a large population. It should have roughly similar characteristics and effects on behavior in the full range of its scope. And it should be persistent over an extended period of time—decades or longer.

On the positive side, it is possible to identify social mechanisms that secure the functional stability of certain institutions over a large reach of territory and time. A system of law is enforced by the agents of the state; so it is reasonable to assume that there will be similar legal institutions in Henan and Sichuan when there is an effective imperial government. A system of trading and credit may have centrally enforced and locally reinforcing mechanisms that assure that it works similarly in widely separated places. A normative system regulating marriage may be stabilized by local behaviors over a wide space. The crucial point here is simply this: if we postulate that a given structure has scope over a wide range, we need to have a theory of some of the social mechanisms that convey its power and its reproduction over time.

Several of the instances offered above fit the terms of our provisional definition. They are large complexes of rules and practices that influence behavior and outcomes. And it is straightforward to begin to provide a description of the microfoundations upon which they exist: the social components through which these structures are embodied and through which they exercise influence on individuals and groups. The United States federal government functions as a system of branches of government, each with its own departments governed by formal and informal rules. And the "reach" of the state down to the local and individual level is secured by the socially implemented forms of power that are locally expressed (bank inspectors, law enforcement agencies, tax auditors, . . .). This is an example of a large social structure that operates through a high degree of formal institutionalization. But some of the examples mentioned above depend primarily on informal mechanisms—the workings of widespread beliefs and attitudes, along with a diffused willingness of individuals to "enforce" the requirements of the structure. Structures relying primarily on informal mechanisms include the Indian marriage system or the English class system.

So the existence of great structures is ambiguous. Yes—in that there are effective institutions of politics, economics, and social life that are real and effectual within given historical settings, and we have empirical understanding of some of the mechanisms that reproduce these structures. But no—in that all social structures are historically rooted; so there is no fixed, "essential" nature of a state or economy can be expected to persist indefinitely. Instead, political and economic structures may be expected to evolve differently in different historical settings. And a central task of historical research is to discover both the unifying dynamics and the differentiating expressions which these abstract processes take in different historical settings.

We can get a better grip on this issue by looking at a few concrete cases.

4.1 Is France a Nation?

A "structure" concept that plays a particularly important role in historical analysis is the idea of "nation". Let's look at this concept more closely through an example. Is France one nation? What makes it so? And what are the large socio-cultural factors that led to modern France? These are the questions that Emmanuel Todd raises in *The Making of Modern France: Ideology, Politics and Culture* (Todd, 1991). Todd is one of this generation's leading historians in France, and his conception of the challenge of history is worth studying. I would call him a "macro-historian", in that he is interested in large processes of change over extended stretches of space (for example, the extension of industry across the map of France from 1850 to 1970, or the patterns of religious dissent from the twelfth to the twentieth centuries), and he singles out characteristics of family structure, demography, literacy, and religion as a set of causal factors that explain the patterns of historical change that he uncovers.[2]

Todd's starting point seems exactly right: the "nation" is not a particularly salient level of analysis for making sense of large historical change. Social, economic, and political developments should not be presumed to always unfold at the level of the nation; both sub-national, regional processes as well as supra-national (global) processes are historically crucial. He puts forward a simple but apt criterion for choosing a level of analysis for historical inquiry: "one has to observe the social and economic behaviour of the human beings in question and discover their scale in order to define closed and homogeneous groups which then can be called society X or economy Y" (Todd, 1991, p. 7). And in fact, he argues that "France" is better understood as a configuration of regions and zones than as an integrated national system. As he puts the point, "one can represent France as a heterogeneous and open area in which social, economic and political forces emerge, spread and establish themselves quite independently of the central power and of the overall national structure" (p. 8). And: "Notions of 'French society', 'French economy', 'French industry', 'French working class' are to some extent myths" (p. 7). (It is interesting to observe that this is one of G. William Skinner's central insights into Chinese history as well, especially in his analysis of the historical relevance of "macroregions" in China; Skinner, 1977b.)

So what are the patterns and causal factors that have given rise to "modern France" in Todd's reckoning? Crudely, Todd argues that there are large regional

[2]Eugen Weber's *Peasants into Frenchmen* is another central approach to the question of the formation of France as a unified nation (Weber, 1967).

patterns of culture, demography, and property that created distinct dynamics of change across eight centuries of French history. The southern half of France is characterized by complex family systems with several generations in the same household and a low rate of reproduction, in contrast to the nuclear families of the north and their higher rate of reproduction. The family values of the southern region gave greater importance to literacy and education than the nuclear (and larger) families of the north. And family structure, patterns of inheritance, and land tenure are in turn highly relevant to the formation of large patterns of ideology. (A similar logic is expressed in another of Todd's books, *The Explanation of Ideology: Family Structure and Social Systems*; Todd, 1985.)

The central analytical device in Todd's argument is a fascinating series of maps of France coding the 90 *départements* of France by such variables as the percent of women holding the *baccalauréat*, the percentage of priests accepting the *serment constitutionnel* (revolutionary loyalty oath) in 1791, or the percentage of workers in a given industrial sector. The maps display striking geographical patterns documenting Todd's interpretation of the large historical patterns and their underlying anthropological and geographical causes. At the largest scale, he argues for three axes of historical causation: a north-south axis defined by family structure that creates differentials of literacy and population growth; an east–west axis defined by the diffusion of industry from northern Europe into eastern France and across the map from east to west; and a political pattern different from both of these, extending from Paris at the political center to the periphery in all directions. The following is a good example; Todd is interested in observing the degree of "religiosity" across France around the time of the Revolution, and he uses the percentage of priests who accepted the oath of allegiance demanded by the Revolutionary government as a measure. The resulting map reveals conspicuous patterns; the periphery and the south stand out as nonconformist.

Todd also argues that there is a causal order among the large social factors he singles out. Family structure is causally relevant to literacy and education level; literacy is relevant to religious dissent and the emergence of Cathars, Waldensians, and Protestants; family structure is relevant to reproductive rates that are in turn relevant to the spread of industry; and traditions of inheritance are relevant to a region's receptiveness to the ideology of the Revolution. And the patterns created by these causal processes are very persistent; so the southern belt of high-literacy départements of the twelfth century coincides almost exactly with the pattern of high incidence of baccalauréats and doctors in the late twentieth century.

A particularly interesting part of Todd's analysis is his effort to map out the agrarian regimes of pre-revolutionary France (the *ancien régime*). He observes that this has not been done by existing studies of French rural society, and that there is no suitable statistical data on the basis of which to do so for the eighteenth century in any case. However, he makes use of the first census in 1851 to infer back a century in order to arrive at an analysis into four categories: large estates with hired labor, peasant proprietorship, tenant farming, and share-cropping. And using the mid-nineteenth century census data he constructs a map that demonstrates the distribution of these property forms across the expanse of France.

Todd's analysis shows that the large estates are concentrated in the center of France, including Paris; while peasant proprietorship (sometimes combined with share-cropping) predominates in the southern tier. And these patterns conform closely to the distribution of family structure and fertility. Todd argues that these patterns showed substantial continuity before and after the Revolution (p. 61). In other words, there is a very substantial overlap between agrarian regimes and the anthropological-demographic patterns discussed earlier. Todd then uses these geographical patterns to explain something different: the pattern of de-christianization that took place over the century following the Revolution. Basically, de-christianization is associated with the regions involving a large number of landless workers, whereas this cultural process was least virulent in regions of peasant proprietorship. Todd summarizes this way:

> The link between family and agrarian system will help us to understand why dechristianization gained ground, from 1791 onwards, in regions of large farms and share-cropping, and met with resistance in provinces where tenant farming and peasant proprietorship were predominant. This proposition can, moreover, be reformulated thanks to equivalences between family types and agrarian systems. Dechristianization spread in regions where the family structure was egalitarian nuclear or community, but failed in provinces where the family was stem or absolute nuclear. (Todd, 1991, pp. 66–67)

In other words, Todd hopes to provide an explanation of ideology and religion in terms of a set of demographic and social characteristics that are distributed differentially across regions.

Todd's work is striking for its effort to cross genres, incorporating geography, anthropology, and sociology into the formation of large interpretations of French history. And it is striking for the scale of the canvas that he attempts to paint.

4.2 A Modern World System?

Immanuel Wallerstein created a huge stir in the 1970s with the publication of *The Modern World-System* (Wallerstein, 1974). The book is an intellectual masterpiece, synthesizing a vast range of fundamental literature on the economic history of Europe and the world. We could look at the book as the first serious and extended effort to theorize globalization—a term that barely existed at the time of publication. Or we could look at it as a general theory of colonialism—an account of the pathways and influences through which the metropole dominated and exploited the periphery. It is worth looking back at this work today to tease out some of the guiding assumptions about history, sociology, and globalization it reflected.

The concept of "world system" is itself a key component of our current understanding of globalization, in that it captures the idea of causal interconnectedness across the globe among major organizations, firms, populations, and states.

Wallerstein observes that earlier social scientists had usually centered their analysis at the level of the political unit—the nation-state; whereas his own approach is different:

> This book makes a radically different assumption. It assumes that the unit of analysis is an economic entity, the one that is measured by the existence of an effective division of labor, and that the relationship of such economic boundaries to political and cultural boundaries is variable, and therefore must be determined by empirical research for each historical case. Once we assume that the unit of analysis is such a "world-system" and not the "state" or the "nation" or the "people", then much changes in the outcome of the analysis. (Wallerstein, 1974, p. xi)

But what, more exactly, did he mean by a system? Did he imagine something analogous to a mechanical system in which the relations among the parts were governed by a few simple laws? He seems to suggest this possibility when he asks the question, "What do astronomers do? As I understand it, the logic of their arguments involves two separate operations. They use the laws derived from the study of smaller physical entities, the laws of physics, and argue that... these laws hold by analogy for the system as a whole. Second, they argue a posteriori. If the whole system is to have a given state at time **y**, it most probably had a certain state at time **x**" (p. 7). Here he seems to suggest that social systems are tied together by the working of governing laws—a particularly unconvincing starting point.

But Wallerstein's practice as a sociologist is far more defensible than this language would suggest. He was in fact sensitive to causal heterogeneity, contingency, and variation in the systemic relations he meant to capture—particularity as well as universality. So he doesn't actually treat the modern world system as if it were analogous to a set of gravitational objects governed by fixed laws of nature.

The clue to an answer to his working definition of a system is found in his definition of scope in terms of an "effective division of labor": a set of regions constitute a system in his framework if there is significant exchange and dependence among various of the regions for products, people, knowledge, skills, and resources from other regions. If Europe, Asia, or the Americas had been "autarkic" in 1700—that is, if one or more of these continental regions had been a closed economy and society making no substantial use of products, knowledge, resources, or people from other regions—then there would not have been one "world system" but rather several independent macro-regional systems. And Wallerstein explicitly affirms this point late in the book:

> By saying that in the sixteenth century there was a European world-economy, we indicate that the boundaries are less than the earth as a whole. But how much less? We cannot simply include in it any part of the world with which "Europe" traded. In 1600 Portugal traded with the central African kingdom of Monomotapa as well as with Japan. Yet it would be *prima facie* hard to argue that either Monomotapa or Japan were part of the European world-economy at that time. And yet we argue that Brazil (or at least areas of the coast of Brazil) and the Azores were part of the European world-economy. (Wallerstein, 1974, p. 199)

So in postulating the concept of world system as a framework for analysis of the modern period (let's say 1600), Wallerstein is putting forth some important assumptions; he is indicating his judgment that there was significant and necessary exchange among virtually all accessible places on the planet. There were economically meaningful movements of resources, people (emigrants and slaves), crops (cotton, sugar), finished products, and ideas throughout the system of places defining the system of transport and trade. This in turn implies that we cannot properly understand the workings of the regional economy without taking into account its exchange relations with other regions—or in other words, we need to place the regional economy into the system of international division of labor in which it is located. And in fact, historians like Kenneth Pomeranz make a substantial case for the empirical accuracy of that judgment (Pomeranz, 2000; Pomeranz and Topik, 1999).

If we begin with this assumption—the idea of the substantial interdependence of continental regions in the early modern period—then we are naturally drawn to the question, what were the terms of trade? Was exchange among regions mutually beneficial, as trade theory would have it? Or was it extractive and exploitative, as the theory of colonialism would have it? This is where Wallerstein makes substantial use of the core-periphery framework in his analysis.

> The periphery of a world-economy is that geographical sector of it wherein production is primarily of lower-ranking goods... but which is an integral part of the overall system of the division of labor, because the commodities involved are essential for daily use. The external arena of a world-economy consists of those other world-systems with which a given world-economy has some kind of trade relationship... what was sometimes called the "rich trades." (Wallerstein, 1974, pp. 199–200)

Wallerstein was particularly interested in interconnections between places that were the expression of power and commerce. Core and periphery are linked by relations of subordination—military and economic domination, leading to the persistent disadvantage of the latter in favor of the former. These features define the "general attributes of a colonial situation" (Wallerstein, 1974, p. 5).

This analysis lays a theoretical and historical foundation for a theory of globalization. Wallerstein writes late in the book:

> One of the persisting themes of the history of the modern world is the seesaw between "nationalism" and "internationalism." I do not refer to the ideological seesaw... but to the organizational one. At some points in time the major economic and political institutions are geared to operating in the international arena and feel that local interests are tied in some immediate way to developments elsewhere in the world. At other points of time, the social actors tend to engage their efforts locally, tend to see the reinforcement of state boundaries as primary, and move toward a relative indifference about events beyond them. (Wallerstein, 1974, p. 147)

Where has the effort to theorize globalization gone in the 35 years since Wallerstein's book appeared? A particularly important contemporary voice on this subject is that of Saskia Sassen. Her book *A Sociology of Globalization* (2007) represents a current cutting-edge effort to provide a vocabulary and set of theoretical premises in terms of which to understand the global interconnectedness that characterizes the contemporary world. And she wants to provide a *sociology* of these processes—that is, she wants to provide a theoretical vocabulary and a set of hypotheses about the causal mechanisms that are involved that are adequate to the problem of describing and explaining the workings of this system. One thing this means is providing a framework within which the empirical details and structures of global networks can be investigated. Another key point in her approach is her attention to differentiation across institutions and mechanisms, a point we will return to below. Finally, Sassen is particularly interested in the networks of communication, finance, and service organizations that constitute the fabric joining what she calls "global cities". But in this book Sassen broadens considerably the angle of view in order to consider social networks at many levels of scale, including sub-national as well as supra-national.

So it is not unreasonable to judge that current efforts to analyze the networks of exchange of people, goods, and information that constitute the field of globalization studies represent a natural intellectual heir to Wallerstein's project in the *Modern World System*.

4.3 Revolutions of 1848

Let us turn now to a case where the question of the historical reality of a large structure is a real issue: the revolution of 1848. The revolutions of 1848 were the stage upon which the "spectre haunting Europe" danced. Karl Marx, Mikhail Bakunin, Alexandre Herzen, Alexis de Tocqueville, and numerous other critical observers of Europe's trajectory looked at 1848 as a moment of continent-wide social and political revolution. Mike Rapport's *1848: Year of Revolution* is a very interesting effort to synthesize the movements and events of the year in a specific attempt to try to assess the degree to which events in Vienna, Berlin, Paris, Milan, and dozens of other European cities hang together as a "year of revolution" (Rapport, 2009).

One reason that the book is so interesting is that the period itself is fascinating—the events, the social movements and causes, the mechanisms through which social contention spread and intensified, and the personalities who were drawn into engagement and commentary. Three men whose writings have influenced our thinking about the period—Tocqueville, Herzen, and Bakunin—are only a sliver of the powerful and enduring personalities who played important roles during the critical weeks and months of unrest in a variety of cities. Another reason for the interest of the book is Rapport's effort to separate out some of the causes and claims that led to mass protest in city after city—relief of impoverishment, anger at the impersonal economic relations of the time, and the claims of ethnic and national groups for self-determination. Fundamentally, Rapport suggests that mobilization and political demands flowed from two basic issues: the crushing poverty that segments of urban society experienced at mid-century, exacerbated by financial crisis and crop failures (Paris, Berlin), and the demand for political autonomy for national and ethnic groups (Italy, Germany, Czechoslovakia, Hungary). Finally, the book is distinguished by its effort to treat the full canvas of unrest and violence across much of the continent—not simply focusing on France, as one is sometimes inclined to do in thinking about 1848.

Tocqueville's *Recollections: The French Revolution of 1848* is a particularly intimate view of the events in Paris in spring, 1848 (Tocqueville, 1970). Tocqueville was a Deputy of the National Assembly and an aristocrat, and in January 1848 he gave a prescient speech in the Chamber of Deputies:

> I believe that right now we are sleeping on a volcano... can you not sense, by a sort of instinctive intuition... that the earth is trembling again in Europe? Can you not feel... the wind of revolution in the air? (Tocqueville, 1970)

In *Recollections* he chronicles his own experiences only a few months later, walking the streets of Paris during the street fighting in February 1848. He offers the sharpness of an attentive observer, with a sociologist's effort to see the underlying social alignments that the events he describes reflect.

Marx's writings of the events of February and June in France are more analytical and more political at the tactical level. Marx's face-to-face experience of the events was more fleeting than Tocqueville's—Rapport recounts Marx's rather unsuccessful efforts as a political speaker, attempting to raise class consciousness (p. 231). (Blanqui and Proudhon both seem to have been more successful in this vein.) But Marx followed the events carefully through available journalism, and he made every effort to interpret the comings and goings in a way that made sense to him from the framework of historical materialism and politics as class conflict. Here is how Marx described the outcome of the bloody June repression of the revolution in Paris:

> The Paris workers have been overwhelmed by superior forces; they have not succumbed to them. They have been beaten, but it is their enemies who have been vanquished. The momentary triumph of brutal violence has been purchased with the destruction of all the deceptions and illusions of the February revolution, with the dissolution of the whole of the old republican party, and with the fracturing of the French nation into two nations, the nation of possessors and the nation of the workers. The tricolour republic now bears only one colour, the colour of the defeated, the colour of blood. It has become the red republic. (N.Hr.Z., 29 June 1848)

One of the most interesting questions about 1848 is also the most basic: were these disturbances "revolutionary," or were they something different and perhaps less historically significant over the long sweep of the century? Were perhaps the "February days" better described as simply a short period of civil unrest and plebeian rioting; and were the "June days" simply a show-down with a state and military increasingly willing to use force to exert its will? And might we think that it is best to look at Berlin, Milan, Vienna, and Paris in 1848 as largely separate social upheavals brought together in a relatively short period of time, but lacking the internal connections that would constitute a large revolution? In other words, was 1848 really a "year of revolution", as Rapport says in his subtitle, or was it less dramatically, a year of unrest, rioting, and eventual political change?

One reason for posing the question in these terms is the fact that the concept of "revolution" is a very imposing one. When we think of "revolutions," we think of the great examples—France 1789, Russia 1917, China 1949. We think of organized revolutionary parties; mass movements; political contest over control of the state; a program of fundamental social and economic change; and eventual seizure of state power. Against this sweeping set of unifying ideas, one might say that 1848 never reached this threshold of significance and unity.

But perhaps this way of putting the question gets it backwards. Perhaps it is the "great" revolutions that need a second look—as Rapport suggests somewhere in a single sentence. Perhaps it is the Russian Revolution that has been over-dramatized, and the widespread social and political upheavals of 1848 are more genuinely revolutionary than the seizure of power by the Bolsheviks in one corner of Europe. The upheavals across Europe in 1848 are continental in scope; they involve a confluence of related claims (for autonomy for national groups, for poverty relief, for a democratic voice in government); and they did in fact result in "regime change" in Italy, France, Austria, and Germany. And, as Rapport, Tocqueville, and Marx seem to agree—by June 1848 in France, at least, there was a polarization around class lines and the primacy of the social question.

So it is a simple question, really: were there any "revolutions of 1848"? It seems most defensible to treat nouns that describe great historical events in a nominalistic way: they serve to draw together a complex social and historical reality around an interpretive principle, rather than designating a real, extended historical entity. This position does not question the reality or objectivity of the events and happenings of 1789 or 1848; but it questions the idea that these events add up unambiguously and objectively to a "revolution".

4.4 Explaining Fascism

Turn now to a fourth example of a large structure that has gained a great deal of historical attention: the rise of twentieth-century fascism and dictatorship. F. L. Carsten (1967), Karl Bracher (1970), and Michael Mann (2004) frame a major question for twentieth century history: why did fascism come to power in so many states in Europe in the 1930s? These studies raise a definitional question—what is fascism—and they demonstrate that this apparently semantic issue requires careful historical and theoretical analysis. Arriving at a good definition of fascism is itself an empirical and historical task. In engaging these topics, we are forced to ask a set of causal questions: How do the fascisms of Europe relate to important social forces in the early twentieth century (for example, the role of great social classes in conflict)? And what is involved in explaining fascism (the role of analysis and theory)? Further, these studies demonstrate the need for a nuanced treatment of the variety and diversity of human institutions—issues raised elsewhere under the topic of "heterogeneity".

There are clearly a number of different explanatory questions we might have in mind: why did fascist movements emerge and gain popular support in the first three decades of the twentieth century? Why did these movements prevail in several countries and not in others? (This version parallels Skocpol's question about revolutions; Skocpol, 1979.) Why did fascist states develop the political institutions they did in Germany, Italy, and Spain? How did fascist states and leaders exercise power? What prevented the rise of powerful fascist movements on France and Britain—in spite of the presence of ultra-nationalist leaders and organizations?

These are all different questions—even if there are relations among them. A particularly central question concerns the factors that were conducive to the emergence of extremist beliefs and organizations in certain periods and what factors favored the growth and power of some of these movements. This is a bundle of questions about the conditions that favor collective mobilization and ideological formation on a mass society. It is the sort of research question that Charles Tilly and other scholars of popular mobilization have been concerned with.

Another set of questions about the course of fascism has more to do with institution building and state formation. Given the goal of creating powerful stare institutions within the general framework of fascist ideas and goals, what institutional and organizational possibilities existed? Here we might refer to the repertoire of mass organization that fascist "revolutionaries" brought to their movement, as well as the historical and practical options that existed. This area of inquiry may provide a basis for answering questions about the particular nature of fascist political institutions.

Finally, the distinct question of why it was that fascist movements and leaders were able to defeat democratic movements and states requires that we identify some of the circumstances that weakened democratic regimes. This may be a wide range of factors: challenges of war, ideological conflict with communists and other critics of the state, and the economic circumstances of the great depression. (These fall in the same category as the circumstances that Skocpol brings forward as being relevant to the success or failure of revolutions.)

It would appear that social scientists and historians have better tools for addressing the issue of successful mobilization than the institutional or causal conditions surrounding seizure of power and state building. Schematically, we might consider a causal narrative along these lines: Conditions that favor fascism include the presence of a marginalized group of young people who are subject to great economic insecurity, including demobilized war veterans; an ideology that combines nationalism, ethnic suspicion, and disaffection from established social institutions and values, and a compelling narrative of how and why this group ought to wield power. To this we might add a few propitious international conditions: the threat of war, a widening economic crisis, and a broad view that the modern state isn't up to handling these challenges.

This approach sketches out a view of what might be a basis for an explanation of the rise of fascist social movements. Here we have singled out several causal-social factors that facilitate popular mobilization and the politicization of social movements. What it doesn't yet explain is why and in what circumstances these movements are likely to grow powerful enough to challenge the existing state structure; this remains for another discussion.

Of special interest for us are the conceptual questions that the historian of fascism must entertain. Is fascism a particular social system (dictatorship with such-and-so attributes)? Or was it first and foremost a historically distinctive political and social movement with characteristic values and ideology (violence, nationalism, anti-communism)? Is it a historically specific moment, or is it a systemic development stimulated by some structural feature of modern society (deadlocked conflict between workers and the bourgeoisie)? Crudely—is fascism a social formation, an ideological complex, a social movement, or a type of government apparatus? And our efforts of explanation will depend on what sort of answer we give to these ontological questions.

These alternative definitions of fascism would give rise to very different explanatory challenges. And in fact, there is a wide variety of explanatory and causal questions that can be considered: Why did the fascist movements arise? Why did they gain a mass following? How did the social realities of capitalism affect the emergence and form of fascism? How important were the particular qualities and ideas of Hitler, Mussolini, or Franco in the evolution of fascism as a social system? Why did fascist dictatorships take the form they did? Why did official and affiliate group violence take the virulent forms that it did? How did fascist governments maintain power? Did these governments gain "legitimacy" and support in their populations? Is there a characteristic "pattern of development" for fascist regimes, or are their political histories deeply contingent on events and persons? Are Germany, Italy, and Spain variants of one social form, or are they simply independent social systems possessing some family resemblances in ideology, propaganda systems, and propensities for violence?

We might also consider whether analysis and explanation need to occur at a lower level altogether—not "why fascism?" but rather, "why the Iron Guard in Romania", "why this or that feature of Italian fascism", "why this particular feature of Spanish state-military relations in Franco's fascism?". Here the point might be that there are no general or comprehensive explanations of the emergence and development of fascism in all the places it occurred; no common causes that were always or usually instrumental; but rather that each national history needs to be treated in its own terms. But as Carsten's study demonstrates, this would be somewhat too skeptical; there certainly were some large international and national forces that facilitated fascist mobilization and seizure of power in many different countries.

The historical phenomena of fascism are interesting and important, because they represented powerful social forces, movements, and governments that had great influence on events in the twentieth century. And their dynamics and causes are obscure and controversial. We would like for historians to have something substantive and illuminating to say about the causes and trajectory of fascism. And this requires significant conceptual and theoretical work.

4.5 Generalizations

Historical generalizations are often suspect: "The Renaissance encouraged innovative thinking," "The Qing state stifled independent commercial activity," "The open frontier created a distinctively American popular culture." The problem with statements like these is their sweep; among other things, they imply that the Renaissance, the Qing state, or American culture were essentially uniform social realities, and they erase the forms of variation that certainly existed—and that often constitute the most interesting of historical discoveries. So grand generalizations in history are problematic.

But then we have to ask a different sort of question. Specifically—what kinds of generalizations *are* possible in history? If we cannot answer this question constructively, then historical research loses much of its interest and purpose. If historical knowledge were limited to statements about specific actors in concrete local circumstances, it would have roughly the interest of a police report. Rather, the historian needs to aggregate his/her understanding of the available evidence into statements about larger agglomerations: villages, towns, and cities; crowds, classes, and professions; assemblies, riots, and movements. Moreover, we would like to be able to make something larger of the historian's findings—something that sheds light on broader social realities and trends. And each of these requires generalization: statements that extend beyond the particular instances that are presented by the historical record. Emmanuel Le Roy Ladurie's micro-history of the tiny village of Montaillou (Le Roy Ladurie, 1979b) is worth considering in this context. His opening lines raise the question of generalization:

> Whoever wishes to know the peasant of the old or very old regimes, does not aim at grand syntheses—regional, national, or continental: I think of the work of Goubert, Poitrineau, Fourquin, Fossier, Duby, Bloch... What is always missing is the direct aspect: the witnessing, without intermediary, how the peasant presents himself. (Le Roy Ladurie, 1979b, p. 1)

Le Roy Ladurie gives a treatment of the history of a very specific, small place—a specific group of village actors in a short time period. Their stories are told through the records of Inquisition investigations. So one might say—it is all very particular knowledge about this specific time and place. But if so, what makes it historically meaningful or valuable? How does it extend our historical knowledge and imagination?

There appear to be several different ways in which a concrete micro-study can achieve the broader significance that it needs to qualify as a genuine contribution to historical understanding.

One possibility is that the micro-study is somehow "representative" of larger social realities at the time. One might read *Montaillou* as being representative of many other remote places in fourteenth-century France—so the description of this place might serve to generalize to other parts of France. And what does this mean? It means, presumably, that the historian arrives at true statements about Montaillou that are also true of other villages at other times. (Though the author's cautions against "grand synthesis" seem to count against this use of his findings.)

Another possibility is diachronic generalization: the historian may have identified, under the "microscope" of detailed study of these decades in Montaillou, the crossing and emergence of historical patterns and changes that themselves have broader significance over time. The mental significance of Catholicism for rural people, for example, may have been undergoing change over a period of centuries;

we might take the Montaillou snapshot as one instant in time of the larger historical trend.

A third possibility is at the level of concepts of behavior and agency. The historian may grapple for ways of extending his/her vocabulary of action and thought for actors in the past; the micro-study may suggest a new set of categories in terms of which to understand the forms of action and thought that were possible for fourteenth-century common rural people. It is certainly an important question for the historian, to ask "why do people act as they do?" in specific historical settings—the outposts of the Roman empire, village India, or sixteenth-century London; and the micro-study may serve to broaden the range of answers we have for this fundamental question. This intellectual task is not one of "generalization", but rather one of "specification"—specification of the broad range of variation that is possible within historical reality.

This may all come down to a truism: there is an irresolvable tension for historians between "specification of the local" and "generalization over trends". Too much generalization, and we lose the point of historical research—we lose the tangible granularity of real people and social settings in history, and the surprising singularities that historians like Le Roy Ladurie or Robert Darnton are able to put in front of us. Too little generalization, however, and the research becomes pointless—just a specification of a collection of actions and outcomes for which the existing historical record happens to provide some information. We want both from good historical writing: an adequate attention to specificity and some degree of projectability and insight into broader questions.

4.5.1 Similarity and Difference

Comparisons across social and historical settings (England against the Yangzi region, or France against Russia) naturally provoke questions of similarity and difference. Comparison of different settings may illustrate that there are common processes or structures at work, or comparative research may lead us to conclude that the large processes that are of interest (economic development, political development, proliferation of religious ideas) are highly distinctive in their different historical settings (France vs. England, France vs. Japan, Morocco vs. Indonesia). So we can ask the question, to what extent are there *common social causal processes* at work in the historical experiences under comparison? Candidates for common social processes include: population dynamics ("high fertility regimes dampen economic growth"), property relations ("share-cropping regimes dampen technological innovation"), state institutions and policies ("predatory states inhibit business growth"). Or alternatively, we might consider whether the historical experience of different settings is highly particular, path-dependent, and context-sensitive—with the result that we would not expect to find causal regularities across cases.

A related but distinct question arising from comparative research has to do with the possibility of common institutions and structures in separate historical settings.

To what extent are there *common structures* in the several historical settings ("fascist movement," "market," "demographic regime")? There is the intriguing possibility that there are cross-context similarities in certain areas of social life: a small set of solutions to the problem of collecting taxes, a small set of alternatives in the use and management of land, a small set of possible ways of organizing the human family, etc. Candidates might include: share-cropping regimes, tax farming fiscal systems, feudal parcelization of political/military power, market systems of wage labor. At a higher level, economic historians have sometimes singled out "Protestant ethic," "liberal state," "private property," or "competitive market" as common structures that recur in a variety of settings. Comparative economic and political research can give grounds for answering this type of question as well. It can lead us to formulate more specific, perhaps more differentiated, theories of political, social, or economic institutions, and can give an empirical and historical basis for identifying some institutional forms or processes as recurring across a number of historical settings.

The other end of the "similarity-uniqueness" spectrum is also theoretically possible in comparative economic history. We might find that England, France, China, India, and Japan all had highly distinct patterns of development, with locally particular institutions and very different patterns of development over time. We therefore need to ask this question: To what extent are developments in China or Western Europe (or England and the Yangzi Delta) unique and particular—the consequence of highly contingent factors in the single context? This could be true in several different ways. It could be that there is very little overlap of institutions and causes in the two experiences; it might be, hypothetically, that religious and cultural factors are primary in one setting while population and property factors are primary in the other setting. Second, it could be that similar factors are in play in each case, but that, for reasons having to do with path dependency, the mix of primary factors is quite different in the two settings. Third, we could have a high degree of individual variation in the composition and nature of basic institutions—states, agrarian regimes, religious institutions. On this scenario, the statement that "the state is an important causal factor in England but not the lower Yangzi core" is misleading, in that it suggests that there is an important set of institutional or functional similarities defining the "states" in the two cases. But if states differ in their institutional makeup as much as do the sixteenth-century French monarchy and the Balinese theatre state—then the reference to the "role of the state" is more likely an instance of reification rather than rigorous causal analysis.

One plausible position that emerges from sustained comparative economic history is a more "layered" approach to the question of generality and particularity of institutions and structures. It might be that high-level institutions ("state") are complexes of characteristics and functions that are in sum unique to the setting; so the "absolutist state" or the "theatre state" are not portable historical constructs. However, if we push the analysis down a level and single out specific state institutions and capabilities (revenue collection, regulation, military policy, use of technical experts, ability to project power throughout the dominion), we arrive at a set of constructs that permit genuine and fruitful comparison across historical contexts. On this approach, comparison should attempt to bring the level of analysis down a level or two in order to arrive at discoveries of similarity and difference across historical settings.

This suggests that the best hope we have for generalizations about large historical processes such as economic development is not at the level of wholes—regions or nations. Rather, what we can hope to do is to discover a number of recurring processes and mechanisms—political, demographic, technology, institutional, and economic—that can be identified and studied in multiple historical cases. In this category of recurring processes and mechanisms, one might include "proto-industrialization," "scissors crisis," "high level equilibrium trap," "state fiscal crisis," and "rapid urban growth"—along with dozens of other comparable social and economic processes. These are mid-level social processes and mechanisms that correspond to specific opportunities or situations of persons and groups in a developing society, and they can arguably occur in historically separate cases. And actors will adjust their behavior in relation to these processes in their particular settings, to pursue their goals. Finally, some of these processes will aggregate in particular historical settings—often in novel ways—to give rise to a particular historical trajectory. (Notice that this is methodologically very similar to the picture that McAdam, Tarrow and Tilly paint about the possibility of generalizations about contentious politics; McAdam et al., 2001.)

4.6 Predictions

Let us turn now to another large question about history: to what extent is it possible to make predictions about large historical processes? To what extent is it possible to predict the course of large-scale history—the rise and fall of empires, the crises of capitalism, or the ultimate failure of twentieth-century Communism? Does a good understanding of important processes and changes in the past give a basis for forecasting the likely future of similar processes in the future?

In spite of their reputations as historical determinists, Hegel and Marx each had their own versions of skepticism about "learning from history"—in particular, the possibility of predicting the future based on historical knowledge. Notwithstanding his view that history embodies reason, Hegel is famous for his idea in the *Philosophy of Right*: "When philosophy paints its grey in grey then has a shape of life grown old. By philosophy's grey in grey it cannot be rejuvenated but only understood. The owl of Minerva spreads its wings only with the falling of dusk" (Hegel, 1967). And Marx puts the point more sarcastically in the *Eighteenth Brumaire*: "Hegel remarks somewhere that all great world-historic facts and personages appear, so to speak, twice. He forgot to add: the first time as tragedy, the second time as farce" (Marx, 1974). Marx's remarks to Vera Zasulich about the prospects for communist revolution in Russia are instructive in this context: "I thus *expressly* limited the 'historical inevitability' of this process to the *countries of Western Europe*" (Marx and Engels, 1975). Both Hegel and Marx, then, cast specific doubt on the idea that history presents us with general patterns that can be projected into the future.

This is a view I agree with very profoundly: history is contingent, there are always alternative pathways that might have been taken, and history has no general plan. (The idea of path dependency comes into this discussion; Page, 2006.) So—no grand predictions in history.

But then we need to ask a different sort of question. Specifically—what kinds of predictions or projections *are* possible in history? And what is the intellectual base of grounded historical predictions? Here are a few predictions that seem to be supportable:

- Labor unrest in China will intensify over the next 10 years.
- The Alsatian language is likely to disappear as a functioning medium of communication in Alsace within the next 50 years.
- Social unrest will continue to occur over the next decade in Thailand.
- Large and deadly technology failures will occur in Europe and the United States in the next decade.
- Social movements will arise more frequently and more adaptively as a result of the use of social media (twitter, blogs, facebook, email).

Several things are apparent when we consider these predictions. First, they are limited in scope; they involve small-scale features of the historical drama. Second, they depend on specific and identifiable social circumstances, along with clear ideas about social mechanisms connecting the present to the future. Third, they are at least by implication probabilistic; they indicate likelihoods rather than inevitabilities. Fourth, they imply the existence of *ceteris paribus* conditions: "Absent intervening factors, such-and-so is likely to occur." But, finally, they all appear to be intellectually justifiable. They may not be true, but they can be grounded in an empirically and historically justified analysis of the mechanisms that produce social change, and a model projecting the future effects of those mechanisms in combination.

It is worth exploring the logic and function of prediction. Fundamentally, it seems that prediction is related to the effort to forecast the *effects of mechanisms and interventions*, the *projection of existing trends*, and the *likely strategies* of powerful social actors. We often want to know what will be the net effect of introducing X into the social environment. (For example, what effect on economic development would result from a region's succeeding in increasing the high school graduation rate from 50 to 75%?). We may find it useful to project into the future some social trends that can be observed in the present. (Demographers' prediction that the United States will be a "majority–minority" population by 2042 falls in this category.) And we can often do quite a bit of rigorous reasoning about the likely actions of leaders, policy makers, and other powerful actors given what we know about their objectives and their beliefs. (We can try to forecast the outcome of the 2009 impasse between Russia and Ukraine over natural gas by analyzing the strategic interests of both sets of decision-makers and the constraints to which they must respond.)

The heart of prediction is our ability to identify dynamic processes and mechanisms that are at work in the present, and our ability to project their effects into the future. To arrive at a supportable prediction about a state of affairs, we might possess a theory of the dynamics of the situation, the mechanisms and processes that interact to bring about subsequent states, and we might be able to model the future effects of those mechanisms and processes. A biologist's projection of the spread of a disease through an isolated population of birds is an example. *Modest* predictions are those that single out fairly humdrum current processes in specific detail, and derive some expectations about how these processes will play out in the relatively short run. *Grand* predictions, on the other hand, purport to discover wide and encompassing patterns of development and then to extrapolate their civilizational consequences over a very long period. A modest prediction about China is the expectation that labor protest will intensify over the next 10 years. A grand prediction about China is that it will become the dominant economic and military superpower of the late twenty-first century. We can have a fair degree of confidence in the first type of prediction; whereas there are vastly too many possible branches in history, too many "countervailing tendencies," too many accidents and contingencies, that may occur to give us any confidence in the latter prediction.

Ceteris paribus conditions are unavoidable in formulating historical expectations about the future, because social change is inherently complex and multi-causal. So even if it is case that a given process, accurately described in the present, creates a tendency for a certain kind of result—it remains the case that there may well be other processes at work that will offset this result. The tendency of powerful agents to seize opportunities for enhancing their wealth through processes of urban development implies a certain kind of urban geography in the future; but this outcome might be offset by a genuinely robust and sustained citizens' movement at the city council level.

Social predictions are generally *probabilistic*. A probabilistic prediction specifies the range of outcome scenarios that are most likely: "Given current level of unrest, these outcomes are likely: rebellion 60%, everyday resistance 30%, resolution 10%." The fact that historical predictions are generally probabilistic is partly a consequence of the fact of the existence of unknown *ceteris paribus* conditions. But it is also, more fundamentally, a consequence of the fact that social causation itself is almost always probabilistic. If we say that rising conflict over important resources (X) is a cause of inter-group violence (Y), we don't mean that X is necessarily followed by Y; instead, we mean that X raises the likelihood of the occurrence of Y.

So the question is, what kinds of predictions can we make in the social realm? And what circumstances limit our ability to predict? A few points seem relatively clear.

Specific prediction of singular events and outcomes seems particularly difficult: the collapse of the Soviet Union, China's decision to cross the Yalu River in the Korean War, or the onset of the Great Depression were all surprises to the experts.

Projection of stable trends into the near future seems defensible—though of course we can give many examples of discontinuities in previously stable trends. Projection of trends over medium- and long-term is more uncertain—given the likelihood of intervening changes of structure, behavior, and environment that will alter the trends over the extended time.

Predictions of limited social outcomes, couched in terms of a range of possibilities attached to estimates of probabilities and based on analysis of known causal and strategic processes, also appear defensible. The degree of confidence we can have in such predictions is limited by the possibility of unrecognized intervening causes and processes.

The idea of forecasting the total future state of a social system given information about the current state of the system and a set of laws of change is entirely indefensible. This is unattainable; societies are not systems of variables linked by precise laws of transition.

So two conclusions seem justified. First, there is a perfectly valid intellectual role for making historical predictions. But these need to be modest predictions: limited in scope, closely tied to theories of existing social mechanisms, and accompanied by *ceteris paribus* conditions. And second, grand predictions should be treated with great suspicion. At their best, they depend on identifying a few existing mechanisms and processes; but the fact of multi-causal historical change, the fact of the compounding of uncertainties, and the fact of the unpredictability of complex systems should all make us dubious about large and immodest claims about the future. For the big answers, we really have to wait for the owl of Minerva to spread her wings.

4.7 The New “Meso-History”

Where do these considerations take us? Do they lead us to abandon the aspirations of large-scale history? Or do they suggest the possibility of a “meso-history” which attributes causal importance to social structures, while at the same time recognizing the cautions that we have surfaced? I believe that the latter is the case. The conception of large-scale historical change that is worth defending is what I will call “conjunctural, contingent, meso-level explanation” (CCM). *Conjunctural*, because at every point there are a range of independent factors present that are salient to the choices and outcomes which will take place—each of which has its own history of emergence, contingency, and reproduction. *Contingent*, both because a given structural configuration still leaves room for strategic choice by actors, and because particular conjunctions of factors are not themselves historically determined. And *meso-level*, in that the most useful explanatory causal factors are those that fall at an intermediate level of generality and specificity—not “capitalism” but “market relations,” not “the modernizing state” but the polity. This approach allows for a middle way between grand theory and excessively particularistic narrative.[3]

[3]Clayton Roberts (Roberts, 1996) draws attention to this point in his analysis of the role of “covering laws” in historical explanations. He argues that covering laws are not available for large macro events, but are available in relation to smaller scale processes that underlie historical causation—what I refer to as causal mechanisms throughout this book.

Putting these three features together brings us to an important limitation on the possible reach of large-scale history: compelling, rigorous large-scale historical explanation will never resemble Laplacean mechanics or Marxist historical materialism, with predictable and inevitable outcomes. And good meso-historical explanations will not take the form of single-variable explanations of any sort (“forces and relations of production in the last instance” or technological determinism). Finally, large-scale historical explanation will unavoidably need to be responsive to local circumstance and contingency. The presence of certain large-scale factors which are commonly associated with outcome X will *not* guarantee that X occurs in this circumstance too. Rather, a compelling large-scale explanation will be local in its analysis of circumstance, and large-scale in its recognition of the common workings of certain general factors (population increase, extension of markets, technological change, etc.).

At the same time, the CCM view postulates a firm alternative to the subjectivist historiography that implicitly asserts the full plasticity of historical process. Given the conjunction of factors in place at a certain time, certain futures are more likely than others, and certain pathways of development are inaccessible. The challenge for the large-scale historian is to uncover the sometimes obscure ways in which structural conditions make certain futures likely and others entirely inaccessible. Charles Sabel, Robert Brenner, and Marc Bloch all provide concrete explanations of specific large-scale historical transitions that were contingent and conjunctural. As we will see in Chapter 6, Charles Sabel (Sabel and Zeitlin, 1997, 1985) particularly emphasizes the contingency and variability of economic organization. Robert Brenner (1976) emphasizes the conjunctural character of agricultural revolution in England (new agricultural technology, specific property relations, specific local relations of power) (Chapter 7). Marc Bloch (1966) emphasizes the utility of explanations of agricultural change in medieval France based on middle-level concepts and analyses (soil types, forms of peasant community, plough technology).

This approach thus suggests the value and feasibility of a level of historical analysis that locates itself in the middle range—hence “meso-history.” Here we may think of examples of causal hypotheses that link one type of familiar structure, common across a group of societies, with another familiar form. For example, consider the discovery that population and settlement follows the structure of the system of transportation, and more generally, that the imperatives of central place theory explain patterns of settlement in many or all societies. This observation is a valid meso-level historical generalization, and one that will find expression in different ways in differing social contexts.[4]

The approach to meso-history indicated here depends heavily on the notion that there are common social structures with similar causal properties in different historical settings. This assumption depends upon the availability of appropriate

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[4]Consider Cronon (1991), Skinner (1964), and Warner (1969) for powerful applications of this insight to rural China, nineteenth-century Chicago, and early twentieth-century Boston respectively.

social theory to indicate the causal mechanisms that give rise to such structures and through which the effects of these structures flow. Is there a compelling theoretical basis for this assumption? Can we bring forward convincing reasons for expecting that there will be sufficient similarity in structure and function among institutions and structures that have evolved in separate social contexts, to give rise to the possibility of significant similarities of causal profile? The current focus on common causal mechanisms provides a modest basis for optimism that there are these sorts of similarities across social contexts; this is a point that is discussed more fully in Chapter 5.

Consider the example of sharecropping as an institution governing access to the land and division of the risks and revenues created by cultivation. This is an institution of property relations in land that has emerged in many separate historical contexts (Netting, 1993). And it is an arrangement that is directly salient to participants, given the circumstances of risk, need, and interest that affect the powerful and the cultivator, on the one hand, and the circumstances of traditional agriculture and technology, on the other. Therefore it is not surprising that this institution has been re-invented in countless contexts.

We can therefore expect that existing societies will possess a range of institutions that serve a handful of functions—

- Economic—production, exchange, income generation, savings and investment
- Political—regulation of public order, enforcement of agreements, establishment of the conditions of economic activity (currency, banking and credit, standards of health and safety in products), collection of revenues, establishment of public infrastructure (water, roads)
- Social—educational institutions, institutions of social solidarity (religion, associations)

Social institutions thus emerge as the result of individuals striving (sometimes cooperatively, sometimes competitively) to solve existential problems. And as institutions emerge, they are often "captured" by opportunistic individuals and groups who can exploit them for their own purposes. Social institutions thus have a deep potential for "morphing" into new shapes and configurations (another reason for doubting the strongest variants of technological, materialist, or cultural determinism).[5]

We can further predict that these various institutions will be subject to specific forms of pressure and erosion. For example, given that institutions work through specific agents and given that these agents have private purposes as well as role-defined purposes, we can predict that there will be a tendency toward "rent seeking," corruption, and capture. Likewise, "principal-agent" problems are predictable, in which subordinates within an institution make use of their powers for purposes other

[5] See North (1990) and Ostrom (1990) for rational choice constructions of the development of institutions.

than those intended by the superior. But likewise, because other agents can anticipate these consequences, we can predict the emergence of preventive checks on the use of position and power for personal ends.

This blend of agency theory and materialism takes us to the point of being able to assert the likelihood of the development of similar institutions in different societies. But it does not take us the whole way to an ability to predict (or explain on first principles alone) the course of a given historical period. The reason for this has ultimately to do with human agency. Historical change proceeds through agents’ interests and needs. Institutions and structures exist at particular points in time as the cumulative evolved result of agents’ previous efforts to satisfy their needs and interests. Institutions are therefore more like artifacts than natural kinds; they are the result of many individuals’ purposive actions and unintended effects. To the extent there are common features of institutions this derives from “parallel evolution”—a particular feature is a commonly accessible solution to a common existential problem—or the result of diffusion of organizational themes and ideas (transmission of governing styles and strategies).

Once a group of institutions exist in a particular setting, they constrain the future choices open to agents; so they become part of the causal field within which historical change proceeds. But it would be misleading to attribute primacy to the institutions; rather, institutions are themselves the artifact of the agents (collectively over extended sweep of time). So we can generalize Thomas Hughes’s point concerning technological momentum to speak of “institutional momentum”: institutional configuration is plastic in its development and relatively sticky in operation. This analysis can be understood as the social contract argument writ large. The general approach is to identify a common existential situation for a group of agents within the material circumstances of human life; identify a salient and accessible solution; and infer that this institutional arrangement will recur again and again.

It is also important to bear in mind that, at any given time, agents are presented with a repertoire of available institutions and variants (along the lines of Charles Tilly’s point about a repertoire of strategies of collective action; Tilly, 1986). The contents of the institutional repertoire is historically specific, reflecting the examples that are currently available and those that are available through historical memory. This highlights one of the reasons for the institutional differences that Wong identifies between the political histories of Europe and China; the repertoire of institutional choices for Chinese decision makers was significantly different from that available in early modern Europe.

Where does CCM stand on the question of historical inevitability or historical necessity? CCM implies directedness and intelligibility within historical process, without inevitability or uniqueness. Given that a new water transport option becomes available, trade should increase along this pathway. But other factors may intervene—from banditry to limitations on demand. So we can make only qualified predictions about the direction of future developments.

Chapter 5
Causal Mechanisms

This chapter takes a specific objective: to identify and analyze the philosophical and conceptual conditions that are involved in postulating causal relations among meso-historical entities, structures, and processes. What is the nature of the causal relations among structures and entities that make up the social world? What sorts of mechanisms are available to substantiate causal claims such as "population pressure causes technological innovation," "sharecropping causes technological stagnation in agriculture," or "limited transport and communication technology causes infeudation of political power"? What are the causal mechanisms through which social practices, ideologies and systems of social belief are transmitted? How are structures and practices instantiated or embodied, and how are they transmitted and maintained? Do causal claims need to be generalizable? How do historians identify and justify causal hypotheses?

The general answers I offer flow from a very simple perspective that was developed in the preceding two chapters. Social structures and institutions have causal properties and effects that play an important role within historical change (the social causation thesis). They exercise their causal powers through their influence on individual actions, beliefs, values, and choices (the microfoundations thesis). Structures are themselves influenced by individuals, so social causation and agency represent an ongoing iterative process (the agency-structure thesis). And hypotheses concerning social and historical causation can be rigorously formulated, criticized, and defended using a variety of tools: case-study methodology, comparative study, statistical study, and application of social theory.

Historians and historical sociologists are commonly interested in providing causal explanations of large historical outcomes: revolutions, social contention, state formation, the spread of religious ideas, and many other sorts of phenomena. Often these research efforts depend on the Millian idea, "same cause, same effect," which unfolds into a theory of causal inquiry based on methodical comparison of cases (Goldstone, 2003; Goldthorpe, 1997; Kiser and Hechter, 1991; Lichbach and Zuckerman, 1997; Mahoney, 1999; Mahoney and Rueschemeyer, 2003; Ragin, 1987, 1998; Skocpol and Somers, 1979). This approach is contrasted to the quantitative methodologies of causal analysis that depend on discovery of correlations among variables in large datasets.

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Here I will make the case that the discovery of historically specific causal mechanisms is feasible, rigorous, and explanatory. Second, it will be argued that it is possible to provide a rigorous interpretation of the "metaphysics" of social causal mechanisms, working through the structured circumstances of choice of socially constructed actors. This approach makes good use of the new institutionalism, in that the new institutionalism emphasizes the causal powers and differentiating influences of specific institutional arrangements. This approach provides an alternative to a narrowly empiricist search for governing social laws or generalizations as the basis for social explanations. But equally it represents an alternative to idiographic narrative. The chapter will attempt to establish the coherence and plausibility of the social-mechanism approach to research and explanation in historical sociology.

5.1 A Range of Causal Questions

Consider a range of causal questions and hypotheses that have arisen within historical and comparative sociology.

- What causes ethnic violence (Horowitz, 1985)?
- What caused ethnic violence in Rwanda?
- What caused twentieth-century revolutions (Wolf, 1969)?
- What caused the Nicaraguan revolution?
- Why did revolution unfold as it did in the Canton Delta in 1911 (Hsieh, 1974)?
- What factors explain the East Asian economic miracle (Vogel, 1991)?
- Why was the political party of labor more successful in the UK than the US (Przeworski, 1985)?
- Why is infant mortality significantly lower in Sri Lanka than Brazil or Egypt (Drèze and Sen, 1989, 1995)?
- Why do millenarian cults occur in the post-colonial world (Adas, 1979)?
- Why was agricultural technology stagnant in late imperial China (Elvin, 1973)?
- Why do social tastes and styles change as they do (Lieberson, 2000)?
- Why did the New England Patriots win the 2003 Super Bowl (Lieberson, 1997)?
- Why did the political culture of corporations remain powerful among French workers in the nineteenth century (Sewell, 1980)?
- Why did the heavy wheeled plough diffuse in the geographical pattern that it did in medieval France (Bloch, 1966)?

And here are some typical historical causal claims, both singular and general:

- Population increase causes technological innovation (Boserup, 1981).
- A free press within an electoral democracy causes a low incidence of famine (Drèze and Sen, 1989, 1991).
- The fiscal system of the *ancien régime* caused the collapse of the French monarchy (Soboul, 1989).

- Transport systems cause patterns of commerce and habitation (Skinner, 1964–1965).
- New market conditions cause changes in systems of norms (Popkin, 1979).
- A new irrigation system causes changes in family organization (Pasternak, 1978).
- Concentrated urban demand causes development of an infrastructure to support a flow of timber and grain into the metropolis (Cronon, 1991).
- The principal-agent problem represented by cattle herding in Kenya causes the emergence of the practice of bridewealth (Ensminger, 1992).
- Citizens' shared sense of justice causes stability of existing legal system (Rawls, 1993).
- Availability of large financial resources and a favorable regulatory/governmental environment in the city of Chicago were necessary conditions for the development of a regional electricity system in Chicago in the 1910s and 1020s (Hughes, 1983).

We can learn a great deal about causal inquiry by reflecting briefly on a number of these examples. There is a common thread among these examples, in that each question directs inquiry towards the question, "What are the causal conditions that give rise to a given social or historical outcome?" But there are a number of important differences among these examples as well. Some are about a category of outcome ("twentieth-century revolution" or "ethnic violence"), whereas others are about a historically specific outcome (the Nicaraguan revolution, the Rwandan genocide, the 2003 Super Bowl). Some are about large and publicly salient events, structures, and mentalités (states, revolutions, political cultures); others are about small-scale and unnoticed social characteristics (the frequency of first names).

These examples illustrate a number of different patterns of causal relations among social entities, structures, and outcomes. We have—

- change in structure causes change in behavior
- change in structure causes change in norms
- change in structure causes change in structure
- persistence of norms causes persistence of structure
- persistence of structure causes persistence of norms
- change of material resources leads to change of norms and practices
- change in population or density causes change in structure
- change in population or density causes change in process (e.g. technological innovation)

This chapter focuses on the idea that social causation is constituted by concrete causal mechanisms linking one set of social circumstances to another, and that historical and social inquiry into social causation needs to be designed in recognition of this fact. Two central conclusions are key: that it is possible to provide "theories at the middle range" of some causal mechanisms that occur in multiple social and historical settings—which can be used to explain similarities and contrasts among broadly comparable historical outcomes; and that it is possible to identify concrete and historically specific causal mechanisms at work in large sociological processes (single-case causal analysis)—which then provides a basis for explaining the large historical outcome or condition.

Let us expand upon several of the causal stories offered above to get a better idea of the nature of these causal hypotheses.

A new irrigation system causes changes in family organization. Rural society in pre-1930 Taiwan featured a "joint-family" system, in which a parent and married sons would continue to live together and farm their holdings together rather than dividing into two or more nuclear families. After the 1930, however, a trend toward divided families began and has continued until the present. Why did this change in family structure occur? It is often believed that family structure is a deeply idiosyncratic feature of a given culture. But Burton Pasternak attempts to show that the joint-family system in the Taiwan rice economy is a prudent arrangement for the organization of farm labor, given the uncertainties of rainfall (Pasternak, 1978). Pasternak offers this model of the domestic economy. Rice must be transplanted within 20 days and can only be transplanted if there is enough water. The model family contains two married brothers (A and B) and A's son. The family owns 2 ha (5 acres) and two water buffalo. As a joint family the unit can manage field preparation and transplanting in 19–22 days. As two divided units A and his son can manage 1 ha in 17–20 days, but B needs 22–25 days. This means that his rice crop will often fail. If there are fewer than 10 days of rain, both families will lose the crop. If there are fewer than 15 days of rain, A will survive and B will not. In times of water crisis, the joint family has enough labor to plant a crisis crop (sweet potatoes), but the divided families do not. Therefore, if cropping depends on rainfall, the joint family is substantially more secure. After the Japanese removed this uncertainty by creating a large irrigation system in the 1930s, the joint-family practice began to disappear. With irrigation the water supplies are much more secure, and crisis is therefore less likely. Under these circumstances there are incentives for dividing the family and fewer economic reasons not to do so. Once the imperative to protect against catastrophic crop failure due to inadequate labor supply was diminished, the normal frictions of social life (between sisters-in-law, for example) led to a division of families. Thus Pasternak explains the change in family structure as the effect of changing circumstances of the rural economy—the availability of reliable irrigation water. The mechanism postulated is the adaptive, purposive behavior of the actors involved.

A free press within an electoral democracy causes a low incidence of famine. Drèze and Sen offer a careful study of India's experience of hunger and famine since Independence (Drèze and Sen, 1989). Sen had previously offered a careful study of the great Bengal famine of 1942 (Sen, 1981). In their study of post-independence India they find the interesting fact that India, little less poor than it was in the 1940s, had nonetheless not experienced another widespread famine since independence. Why was this? They offer a simple theory along these lines: India was an electoral democracy in which the Congress party needed to compete for electoral support on a regular basis. India also possessed a vigorous free press with numerous newspapers and a tradition of prompt and unencumbered news investigation. Occurrence of famine anywhere in India would be a very significant failure for the governing party. This combination of circumstances gave the government, and the party in power, a large political incentive to implement institutions that would prevent the occurrence of famine: early warning systems, stockpiles of grain, and a responsive government emergency system. These mechanisms are effective in preventing famine. Governments therefore pursued their political interests by adopting these mechanisms; and the absence of famine during the period is the effect of this adoption. Sen and Drèze note the important contrast to the experience of China during the Great Leap Forward: information indicating the existence of widespread hunger and impending famine was available to the central government in the fall of 1959, but the government took no effective emergency measures for a full year. There was little public notice of famine outside of affected areas, and the government had little to fear from the public because its hold on power did not depend on electoral processes. (That is: in a broadly similar material and population setting, a polity without electoral politics and a free press *does* suffer from a major famine.)

Citizens' shared sense of justice causes stability of existing legal system. Barrington Moore points out that a system of law cannot easily depend exclusively on fear of punishment (Moore, 1978). The supervisory power of the state is limited. Citizens, on the whole, comply with the law in a voluntary fashion. What are the factors that serve to render a legal system stable? Moore points to a social fact: when there is a widespread belief that the legal system is fair and just, individuals will have a motivation to comply. Likewise, when citizens believe that the system of law is unjust and unfair, or is used for the benefit of some over others, they will have a motivation to resist. In other words, the social fact that "most citizens regard the existing legal system as fair" causes the stability of the existing legal system. Symmetrically, the social fact that "many citizens regard the legal system as *un*fair" has the potential to cause destabilization of the legal system—the central point of Moore's argument.

5.2 Causal Realism

These examples show that causal explanations are ubiquitous in meso-history. What is involved in asserting a causal relation among historical factors—for example, that "a free press" causes "lower incidence of famine"? Many historical explanations depend on a position that we can describe as "causal realism". The central tenet of causal realism is a thesis about the reality of causal mechanisms or causal powers. Causal realists maintain that we can only assert that there is a causal relationship between X and Y if we can offer a credible hypothesis about the sort of underlying mechanism that might connect X to the occurrence of Y. The sociologist Mats Ekström puts the view this way: "the essence of causal analysis is. . . the elucidation of the processes that generate the objects, events, and actions we seek to explain" (Ekstrom, 1992, p. 115). Authors who have urged the centrality of causal mechanisms for both explanatory and ontological purposes include Nancy Cartwright

(1989), Jon Elster (2007), Rom Harré and Edward H. Madden (1975), and Wesley Salmon (1984). (Hedstrom and Swedberg's collection on mechanisms in the social sciences is a key source on this topic; Hedström and Swedberg, 1998.)

Aage Sørensen summarizes a causal realist position for sociology in these terms: "Sociological ideas are best reintroduced into quantitative sociological research by focusing on specifying the mechanisms by which change is brought about in social processes" (Sørensen, 1998, p. 264). Central to an adequate explanatory theory is the specification of the mechanism that is hypothesized to underlie a given set of observations. "Developing theoretical ideas about social processes is to specify some concept of what brings about a certain outcome—a change in political regimes, a new job, an increase in corporate performance.... The development of the conceptualization of change amounts to proposing a mechanism for a social process" (pp. 239–240). Sørensen makes the critical point that one cannot select a statistical model for analysis of a set of data without first asking the question, what in the nature of the mechanisms we wish to postulate to link the influences of some variables with others? It is necessary to have a hypothesis of the mechanisms that link the variables before we can arrive at a justified estimate of the relative importance of the causal variables in bringing about the outcome.

A particularly important recent effort to make use of causal mechanisms as a foundation for social research is found within the literature on social contention—the occurrence of medium- and large-scale episodes of contention in a variety of social settings. Charles Tilly, Doug McAdam, and Sidney Tarrow have applied framework of causal mechanisms with a great deal of rigor in *Dynamics of Contention* (McAdam et al., 2001) and a volume of associated research. They provide a simple definition of mechanisms: "a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations" (McAdam et al., 2001, p. 24). And processes are concatenations of mechanisms: "regular sequences of such mechanisms that produce similar (generally more complex and contingent) transformations of these elements" (p. 24). "We employ mechanisms and processes as our workhorses of explanation, episodes as our workhorses of description. We therefore make a bet on how the social world works: that big structures and sequences never repeat themselves, but result from differing combinations and sequences of mechanisms with very general scope" (p. 30). They summarize their theoretical ambitions concisely: "Our aim is not to construct general models of revolution, democratization, or social movements, much less of all political contention whenever and wherever it occurs. On the contrary, we aim to identify crucial causal mechanisms that recur in a wide variety of contention, but produce different aggregate outcomes depending on the initial conditions, combinations, and sequences in which they occur" (p. 37).

What is a causal mechanism? Consider this formulation:

A causal mechanism is **(i)** a particular configuration of conditions and processes that **(ii)** always or normally leads from one set of conditions to an outcome **(iii)** through the properties and powers of the events and entities in the domain of concern.[1]

[1] This is an extension of the formulation offered in Little (1991, p. 15).

Mechanisms bring about specific effects based on the properties of the substrate of processes and events in this domain. For example, "over-grazing of the commons" is a mechanism of resource depletion in the context of a non-regulated community of users (Hardin, 1968). We can reconstruct precisely why this would be true for rationally self-interested actors in the presence of a public good: rational agents use more of the "free" public resource to increase their own private consumption, and this behavior aggregates to over-use of the public resource. This is how we specify condition (iii) for the overgrazing mechanism. Further, it is the case that, whenever the conditions of the mechanism are satisfied, the result regularly ensues; in any case where the dominant motive for agents is rational self-interest, we can expect that a common resource will be over-used.

So we do not need to postulate "laws of society" in order to see how social causation might work. Instead, we can directly identify the features of purposive action within given structures that make the mechanism work. Human actions and refrainings are the "stuff" of social causation, and features of human agency underwrite the "necessity" of social mechanisms. So we can properly understand a claim for social causation along these lines: "C causes E" means "there is a set of causal mechanisms working through features of structured agency that convey circumstances including C to circumstances including E." It follows from this analysis that mechanisms implicate regularities. But these regularities are low-level and may not be observable in macro-level social behavior (for example, because of the mixing of several causal processes and the possibility of countervailing mechanisms in play). So they do not serve to play the role of a set of governing laws of society, analogous to laws of nature.

The discovery of social mechanisms often requires the formulation of mid-level theories and models of these mechanisms and processes—for example, the theory of free-riding. These theories and models are "theories of the middle range" in much the sense that Robert Merton meant to convey when he introduced the term (Merton, 1963): accounts of the real social processes that take place above the level of isolated individual action but below the level of full theories of whole social systems. Marx's theory of capitalism illustrates the latter; Jevons's theory of the individual consumer as a utility maximizer illustrates the former. Coase's theory of transaction costs is a good example of a mid-level theory (Coase, 1988): general enough to apply across a wide range of institutional settings, but restricted enough in its claim of comprehensiveness to admit of careful empirical investigation. Significantly, the theory of transaction costs has spawned major new developments in the new institutionalism in sociology.

So this provides an answer to the fundamental question: explaining a social outcome or pattern involves providing an account of the social-causal mechanisms that typically bring it about, or brought it about in specific circumstances. But what is the nature of the substrate of social causation? What do social mechanisms consist of? What makes them operate in the patterned and regular ways that we hypothesize for them?

The general nature of the mechanisms that underlie sociological causation has been very much the subject of debate. Two broad approaches may be identified: agent-based perspectives and social-influence theories. The former follow the strategy of aggregating the results of individual-level choices into macro-level outcomes; the latter attempt to identify the factors that work behind the backs of agents to influence their choices. Thomas Schelling's apt title *Micromotives and Macrobehavior* captures the logic of the former approach, and his work profoundly illustrates the sometimes highly unpredictable results of the interactions of locally rational behavior (Schelling, 1978). Jon Elster has also shed light on the ways in which the tools of rational choice theory support the construction of large-scale sociological explanations (Elster, 1989b). The second approach, the social-influence approach, attempts to identify socially salient influences such as race, gender, educational status, and to provide detailed accounts of how these factors influence or constrain individual trajectories—thereby affecting sociological outcomes. These should not be understood as being contradictory approaches; rather, they each direct explanatory inquiry at different parts of the same nexus of socially situated agency. The first set of approaches pays primary attention to the motives and reasonings of agents within a given set of constraints; while the second set gives more attention to the broad social factors that influence individual agency.

How do social mechanisms work? The basics are fairly clear: individuals have goals, values, and beliefs, they exist within social and natural constraints, and their actions *bring about* a variety of social outcomes. But how do features of "agents within structures" bring about social outcomes? We can give a somewhat more detailed analysis of some of the ways that social facts might cause other social facts by surveying a wide sample of causal explanations from history and the social sciences. This approach leads to an open-ended list of kinds of social mechanisms.

1. *Rational-intentional mechanisms.* Why do empires establish a policy of rotating senior military officials? Because emperors want to avoid the creation of warlords.
2. *Imitation mechanisms.* Why did the no-huddle offense become so common in the National Football League in the 1980s? Because it was successful for a few teams, and others copied the offense in the hope that they too would win more games.
3. *Conspiracy mechanisms* (covert strategems of the powerful). Why did the United States move away from passenger railroads as the primary form of intercity transportation? Because powerful actors took political actions to assure that private automobiles would be encouraged as the primary form of transport.
4. *Aggregative mechanisms* (aggregate consequences of individual-level strategies). Why does technological innovation occur continuously within a market-based society? Because each firm is constantly looking for lower-cost and higher-value-added methods of manufacturing, and these individual efforts aggregate to an industry trend towards innovations in products and technologies.
5. *Mentality mechanisms* (behavior is changed by changing beliefs and attitudes). Why were so many Quaker men conscientious objectors at great personal cost during World War II? Because their religious beliefs categorically rejected the violence in war and they refused to participate in this immoral activity.

6. *Social network mechanisms* (information and norms proliferate through concrete sets of social relationships among individuals). Why was the Soviet military system less adaptive in combat than the Israeli military system? Because information flow among officers and troops was more rapid and more bidirectional in the latter than the former.
7. *Evolutionary mechanisms.* Why does the level of firm efficiency tend to rise over time? Because the net efficiency of a firm is the product of many small factors. These small factors sometimes change, with an effect on the efficiency of the firm. Low efficiency firms tend ultimately to lose market share and decline into bankruptcy. Surviving firms will have features that produce higher efficiency.
8. *Filtering mechanisms.* Why are passengers on commercial aircraft better educated than the general population? Because most airline passengers are business travelers, and high-level and mid-level business employees tend to have a higher level of education than the general population.
9. *Critical mass mechanisms.* A new social networking site experiences slow growth for the first 18 months of operation until it reaches N users; it then takes off with rapid growth for the next 18 months. We attempt to explain this change by arguing that N is a critical mass of users, stimulating much more rapid growth in the future.
10. *Path-dependency mechanisms.* Why do we still use the very inefficient QWERTY keyboard arrangement that was devised in 1874? Because this arrangement, designed to keep typists from typing faster than the mechanical keyboard would permit, was so deeply embodied in the typing skills of a large population and the existing typewriter inventory by 1940 that no other keyboard arrangement could be introduced without incurring massive marketing and training costs.

This is not intended to be an exhaustive list of types of social causation, and there is some overlap among these types. The first four examples fall roughly into the broad category of agent-centered explanations; the next three examples illustrate the social-influence model; and the final three examples illustrate "system-level" features of the environment of social change (selective filtering of events, the mathematics of critical mass, and the momentum of prior social choices). There are no doubt another dozen examples of explanatory schemata that could be adduced as well. What this list illustrates, however, is that there are a variety of ways, both direct and indirect, through which social causation can be conveyed from one set of social facts to another. They all involve the same basic ontology of social causation—agents acting within structures leading to social outcomes—but the nature of the pathway from cause to effect is different in the various types.

This approach places central focus on the idea of a causal mechanism: to identify a causal relation between two kinds of events or conditions, we need to identify the typical causal mechanisms through which the first kind brings about the second kind. What, though, is the nature of the relations that constitute causal mechanisms among social phenomena?

On the methodological-localist approach, the causal capacities of social entities are to be explained in terms of the structuring of preferences, worldviews, information, incentives, and opportunities for agents. The causal powers or capacities of a social entity inhere in its power to affect individuals' behavior through incentives, preference-formation, belief-acquisition, or powers and opportunities. The micromechanism that conveys cause to effect is supplied by an account of the actions of agents with specific goals, beliefs, and powers. Social entities can exert their influence, then, in several possible ways.

- They can alter the incentives presented to individuals.
- They can alter the preferences of individuals.
- They can alter the beliefs of individuals. (Constraints on knowledge; ideology)
- They can alter the powers or opportunities available to individuals. (Social structures and institutions)
- They can confer power on some agents relative to other agents.

Social causal ascriptions thus depend on common characteristics of agents (e.g., the central axioms of rational choice theory, or other theories of practical cognition and choice). I would assert, then, that the most basic foundations for social causal explanation are stories about the characteristics of typical human agents within specific institutional settings. The causal powers of a particular social institution—a conscription system, a revenue system, a system of democratic legislation—derive from the incentives, powers, and knowledge that these institutions provide for participants. Social entities thus possess causal powers in a derivative sense: they possess characteristics that affect individuals' behavior in simple, widespread ways. Given features of the common constitution and circumstances of individuals, such alterations at the social level produce regularities of behavior at the individual level that eventuate in new social circumstances.

Emphasis on causal mechanisms for adequate social explanation has several beneficial effects on sociological method. It takes us away from easy reliance on uncritical statistical models. But it also may take us away from excessive emphasis on large-scale classification of events into revolutions, democracies, or religions, and toward more specific analysis of the processes and features that serve to discriminate among instances of large social categories. Charles Tilly emphasizes this point in his arguments for causal narratives in comparative sociology (Tilly, 1995). He writes, "I am arguing that regularities in political life are very broad, indeed transhistorical, but do not operate in the form of recurrent structures and processes at a large scale. They consist of recurrent causes which in different circumstances and sequences compound into highly variable but nonetheless explicable effects" (Tilly, 1995, p. 1601).

We do a poor job of understanding industrial strikes if we simply collect a thousand instances and perform statistical analysis on the features we've measured against the outcome variables. We do a much better job of understanding them if we put together a set of theories about the features of structure and agency through which a strike emerges and through which individuals make decisions about participation—the mechanisms that commonly arise in the social processes of industrial contention. Analysis of the common "agent/structure" factors that are relevant to mobilization will permit us to understand individual instances of mobilization, explain the soft regularities that we discover, and account for the negative instances as well.

5.3 Examples of Social Mechanisms

It is useful at this point to offer a deliberately heterogeneous list of social processes that have served as hypotheses about social mechanisms in the social sciences:

- Freerider problems undermining effective collective action (Olson, 1965)
- Logic of prisoners' dilemma explaining defection of Catholic villages in colonial Vietnam (Popkin, 1979)
- The market mechanism as an explanation of price equilibria among independent producers, traders, and consumers
- Sørensen's model of the mechanisms of career and income (Sørensen, 2001)
- Practical cognition errors underlying common forms of social action (Kahneman et al., 1982)
- Political entrepreneurship as a mechanism leading to ethnic conflict (Kohli, 1990)
- The pre-famine mechanism (Sen, 1981)
- "Stereotype threat" as a mechanism underlying black-white performance gap (Steele and Aronson, 1995)
- The "ratchet effect" as a mechanism of change in social tastes (Lieberson, 2000)
- Pattern of recruitment into a labor union as a mechanism of union radicalism (Kimeldorf, 1988)

Transport systems have the causal capacity to influence patterns of settlement; settlements arise and grow at hubs of the transport system. Why so? It is not a brute fact, representing a bare correlation of the two factors. Instead, it is the understandable result of a fuller description of the way that commerce and settlement interact. Agents have an interest in settling in places where they can market and gain income. The transport system is the structure through which economic activity flows. Proximity to the transport system is economically desirable for agents: they can expect rising density of demand for their services and supply of the things they need. So when a new transport possibility emerges—extension of a rail line, steamer traffic farther up a river, or a new shipping technique that permits cheap transportation to offshore islands—we can expect a new pattern of settlement to emerge as well.

Consider, for a second example, Robert Klitgaard's treatment of efforts to reduce corruption within the Philippine Bureau of Internal Revenue (Klitgaard, 1988). The key to these reforms was implementation of better means of collecting information about corruption at higher levels of organization and administration. This innovation had a substantial effect on the probability of detection of corrupt officials, which in turn had the effect of deterring corrupt practices. This institutional arrangement has the causal power to reduce corruption because it creates a set of incentives and powers in individuals that lead to anti-corruption behavior.

A third example of social explanation that illustrates the importance of disaggregating social processes onto underlying conjunctions of agency and structure, and the contingency of the social causal processes that result, is found in a large literature on the study of social movements. The literature on "political opportunity structures" emphasizes the contingency of mobilization of social movements depending on the array of opportunities that exist at a given time. Sidney Tarrow summarizes the approach in these terms: "Rather than focus on some supposedly universal cause of collective action, writers in this tradition examine political structures as incentives to the formation of social movements" (Tarrow, 1996a: 41, p. 41). The openness to contingency characteristic of this approach parallels the approach to contentious politics offered in (McAdam et al., 2001).

5.3.1 Transportation as a Large-Scale Historical Factor

Let's now look at a particularly interesting kind of large structure with important causal properties: transportation. Transportation systems are mundane but pervasive, and they seem to represent a good example of a distributed structure with significant system-wide effects. For example, the settlement patterns of suburban Boston in the early twentieth century depended crucially on the pace and geographical location of the extension of the street car system from downtown Boston into the less developed environs (Warner, 1969). Prior to the extension of the trolley line into Roxbury, Newton, and other Boston suburbs, these areas were home to the affluent and powerful of Boston who could afford to maintain a horse and buggy for transportation. Once the trolley reached these areas, however, it was possible for working families to choose to live in these suburbs and travel to work in Boston by trolley. This created demand for a new kind of housing—smaller, cheaper, and more densely packed. This increase in population density in turn triggered the emergence of a new set of businesses in these areas—green grocers and other suppliers of daily necessities. Warner puts the point this way:

> At any given time the arrangements of streets and buildings in a large city represents a temporary compromise among such diverse and often conflicting elements as aspirations for business and home life, the conditions of trade, the supply of labor, and the ability to remake what came before. (Warner, 1969, p. 15)

The theory of historical causation under consideration here emphasizes the structuring role of intermediate factors (of which transport is a good example) and the importance of contingency—e.g. policy choices made at a specific point in time that structure future developments. Transportation systems appear to offer important examples of both points: that transportation represents a causal factor that influences social developments in very similar ways across many social and historical settings; and that there are crucial contingencies that influence the unfolding of a given transport system (Chicago rather than St. Louis, steam traction rather than electric motors, a rail network designed for military needs for mobilization rather than efficient economic activity throughout the country). Finally, transportation represents a factor, unlike climate, in which there is an internal process of development that can be studied using the methods of the history of technology, the history of business organization, and the tools of the new institutionalism. Transportation has its own internal history that can be analyzed and theorized with profit. And careful study will demonstrate that there are important structural and institutional differences in the way in which transportation technologies are implemented that themselves have important historical consequences across contexts, as demonstrated in Frank Dobbin's treatment of the differences in the state and regulatory contexts in France, England, and the US in the early implementation of railways (Dobbin, 1994).

The idea to be considered is something like this: the system of transportation available at a given time creates a framework of opportunities and constraints that have deep causal consequences for historical development. It creates opportunities for individuals within the context of a specific but evolving set of economic arrangements and institutions. It creates the pathways through which people, goods, and ideas flow within and across societies—and these movements themselves have consequences. The system of transportation facilitates a certain kind and intensity of military power. It creates the feasibility of a certain kind and intensity of state-society relations (e.g. fiscal and police powers). It is possible to provide an abstract framework in terms of which to analyze transportation systems. And the implications that come along with this abstract framework may facilitate our understanding of phenomena that seem distant from transportation.

Transportation is a contingent historical product because its emergence and the particular features of its underlying assemblages of technologies and institutions themselves emerge though contingent processes. So the development of a particular system of transport is a contingent process of innovation and refinement, and the consequences of the establishment of the transport system are sometimes unexpected and radical.

Transportation has deep effects on social development, including the pattern and pace of the extension of settlement, the course of economic development (by enlarging regional and national markets and lowering costs of delivery), and facilitating the flow of ideas, bodies of knowledge, and innovations. Let us turn, then, to some of the factors and mechanisms through which transport influences history. We can attempt to categorize the effects of transport by exploring likely effects flowing from the transport of *goods*, *people*, and *ideas*.

The flow of goods that is effected by a transport system leads to market expansion (increasing availability of goods over a larger region), market integration (price correlations across space), greater commercialization (more production for the market as a result of broader and more predictable markets for goods), broader patterns of consumption, and diffusion of technology (as new potential users are exposed to new products, tools, and processes).

Easier movement of people creates equally important and equally visible effects. Long distance migration depends on transportation; symmetrically, an increase in transport efficiency and convenience will predictably increase the volume of migration. At the more local scale (inter-village, inter-city) improved transport increases the ability of people to seek employment, goods, and services at greater distance—thereby creating the possibility of ring settlements around higher-level places. Improved efficiency of the movement of people has important effects on the state and other dispersed organizations. If it takes the representative of the Emperor 14 days to travel from Beijing to Hankow, the ability of the Emperor to control events is clearly limited. When rail travel shortens this trip to 2 days, the administrative grasp of the state is enhanced. And if it takes a week to move reliable troops into position in defense against rebels, clearly the state's ability to control rebellion is weak.

The movement of ideas that is facilitated by more effective transport is equally important. The movement of ideas depends on the movement of people and goods, but the effects are important and independent. The circuits of White Lotus teachers and martial arts instructors brought heterodox ideas to many parts of rural Shandong in the late Qing—with dramatic effects in the production of millenarian rebellion. The distribution of newspapers in the American West by rail allowed for a form of national unity that would otherwise have been impossible. The diffusion of new farm machinery and the cultivation techniques that accompanied depended profoundly on the network of railroads that crossed the west.

What are the obvious implications of new transport capabilities? First, patterns of settlement are plainly organized by available transportation facilities. Population grows around nodes and termini of transport systems. Second, transportation opportunities determine the extension and integration of markets for a variety of goods and services. If it costs 10 cash to produce a picul of rice but 100 cash to transport it 20 miles; then the grain market will not be very extensive. Third, there are obvious military consequences created by transportation networks. If armies are forced to march to their stations and carry their food, weapons, and water with them; the effective range of an army is limited. Slightly less obvious are the consequences for the diffusion of people and ideas that are created by a transport network. If New York newspapers are carried by the east–west train, then all the settlements along the way are potentially influenced by ideas, political trends, and styles represented in the pages of the newspaper. And if union organizers or anti-tax activists are extended in their reach by the existence of a new rail link between St. Louis and New Orleans—then we can expect a surge of political activity along these lines.

What are the non-obvious consequences? Can more efficient transport impede economic development? It can, in that it can readily extinguish local production when extra-local products begin to show up. It can spread crime, when criminals can make more efficient use of transport; the crime spree of Bonnie and Clyde would not have been possible without the automobile. (There is a letter in the Henry Ford Museum from Clyde Barrow to Mr. Henry Ford thanking him for the high-speed automobile the couple used.) More prosaically, the Interstate highway system facilitates the smuggling of drugs and untaxed tobacco from south to north, and it is possible to track the spread of sexually-transmitted diseases from truck stop to truck stop along major trucking routes.

Transport is also a limiting and/or facilitating condition of economic integration. The cost of transport per unit of a good plays a critical role in the regional scope of markets for the good. Some of the obvious factors: high mass, low price commodities—for example, grain—will have limited markets in circumstances of inefficient transport. Innovation of a low price mass transport option will abruptly increase the regional scope of markets for that good. Transport cost is less significant as a limiting factor for high-value, low bulk goods—tea, spice, and electronic components. Perishable commodities have a similar logic. Slow transport severely limits the scope of perishable commodity markets such as fruits and vegetables.

Are there unanticipated and perverse consequences that can emerge as a result of enhancement of service? Cities that are bypassed by new routes lose economic vitality—for example, Worcester, Massachusetts suffered economically when the Massachusetts Turnpike was routed so as to avoid passing near the city. Some traffic specialists maintained that the third harbor tunnel in Boston would increase congestion, by giving the public the impression that it will be more convenient to drive to the airport. Speeding up the velocity of travel on the tributary roads may lead to staggering traffic jams on the trunk road—with greater lost time overall. And it turns out that transportation systems display surprising system effects—for example, non-linear patterns of rail congestion.

Let us consider the question of causal influence. Transportation is particularly important in the view that I offer of social explanation under the framework of methodological localism. Individuals make choices, large and small, within the context of the space of opportunities and powers that are available to them. And transportation constitutes one particularly fundamental such source of opportunities and powers. Transportation is a factor that creates an institutional logic for the individuals, organizations, and structures within a society at a specific moment in time, imposing constraints and creating opportunities for them to achieve their goals. Traders exploit the opportunity to push further up a river when motorized boats become available, and people choose to settle in more remote places. Fishermen push further out into deep ocean when more seaworthy ships become available (Fagan, 2000). Smugglers take advantage of the wheel wells of aircraft. And so forth. Using the framework of methodological localism, we can understand the historical dynamics of a social setting that are created by the transport network along these lines:

> Individuals have a set of purposes; movement of people and goods influences their ability to achieve these purposes; individuals will adapt opportunistically to the opportunities and constraints created by the transport system; and large social patterns (e.g. patterns of settlement, market integration) emerge as the consequence of the large number of independent actions and choices made by individuals in the population.

How does the transport system influence historical events? It does so as a causal mechanism embedded within the opportunities and constraints available to actors. It presents actors with a specific set of opportunities and constraints as they pursue their plans and purposes. To the extent that the new option permits the actor to better achieve his goal, his behavior and choices will change accordingly. This is especially true with regard to residence, employment, and business activity. But it also extends in the direction of technology change. We can expect some actors to look for ways of taking advantage of the new technology—of refining, perfecting, or extending it. So we can expect entrepreneurial activity to take place around the implementation of the system. Likewise, we can expect agents of the state to seize opportunities of interest in and around the transport system—e.g. as a powerful tool for military mobilization. The transport system is thus a locus for individual agency.

These effects all derive from the purposeful choices of individuals. Equally interesting are the unintended consequences of a particular direction of transport technology—the creation of isolated suburban communities, the transport of criminal activity, the social inertia behind the automobile, the values and lifestyle choices that emerge as a result of suburbia.

5.4 Many Small Causes

When large historical events occur, we often want to know the causes that brought them about. And we often look at the world as if these causes too ought to be large, identifiable historical factors or forces. Big outcomes ought to have big, simple causes.

But what if sometimes the historical reality is significantly different from this picture? What if the causes of some "world-historical events" are themselves small, granular, gradual, and cumulative? What if there is no satisfyingly simple and macro answer to the question, why did Rome fall? Or why did the American Civil War take the course it did? Or why did North Africa not develop a major Mediterranean economy and trading system? What if, instead, the best we can do in some of these cases is to identify a swarm of independent, small-scale processes and contingencies that eventually produced the outcome?

Take the fall of Rome. Rather than there being a single large cause for this catastrophe, it is possible that the collapse of the empire resulted from a myriad of very different contingencies and organizational features in different parts of the empire: say, logistical difficulties in supplying armies in the German winter, particularly stubborn local resistance in Palestine, administrative decay in Roman Britain, population pressure in Egypt, and a particularly inept series of commanders in Gaul. Too many moving pieces, too much entropy, and some bad luck in personnel decisions, and administrative and military collapse ensues. Alaric sits in Rome.

What an account like this decidedly lacks, is a story about a few key systemic or environmental factors that made collapse "inevitable". Instead, the account is a dense survey of dozens or hundreds of small factors, separated in time and place, whose cumulative but contingent effect was the observed collapse of Rome. There is no simple necessity here—"Rome collapsed because of fatal flaw X or environmental pressure Y"—but instead a careful, granulated assessment of many small and solvable factors.

But here is a different possible historical account of the fall of Rome. An empire depends upon a few key organizational systems: a system of taxation, a system of effective far-flung military power, and a system of local administration in the various parts of the empire. We can take it as a given that the locals will resent imperial taxation, military presence, and governance. So there is a constant pressure against imperial institutions at each locus—fiscal, military, and administrative. In order to maintain its grip on imperial power, Rome needed to continually support and revitalize its core functions. If taxation capacity slips, the other functions erode as well; but slippage in military capacity in turn undermines the other two functions. (Tilly, 1990 and Mann, 1986 offer theories of large premodern states and empires along these lines.)

And now we have a possible basis for a satisfyingly simple and systemic explanation of the fall of Rome: there was a gradual erosion of administrative competence that led to increasingly devastating failures in the central functions of taxation, military control, and local administration. Eventually this permitted catastrophic military failure in response to a fairly routine challenge. Administrative decline caused the fall of Rome.

Neither of these stories—the "many small causes" story or the "systemic administrative failure" story—may in fact be historically credible. But either *could* be historically accurate. And this is enough to establish the central point: we should not presuppose what the eventual historical explanation will look like. There is no reason to expect a priori that large events will conform to either model. It may be that some great events do in fact result from a small number of large causes, while others do not. So the point here is one about the need to expand our historical imaginations, and not to permit our quest for simplicity and generality to obscure the possibility of complexity, granularity, and specificity when it comes to historical causation.

5.4.1 Causes of the Chinese Revolution

Let us consider a question fundamental to twentieth-century world history: why did the Chinese Communist Revolution succeed? Was it the result of a few large social forces and structures? Or was this a case of many small causes operating at a local level, aggregating to a world-historical outcome?

It should first be noted that the CCP's path to power was rural rather than urban. The Guomindong (GMD) had effectively expelled the CCP from the cities in 1927 and had detached the Communist Party from urban workers. (Note that this runs directly contrary to the expectations of classical Marxism, according to which the urban proletariat is expected to be the vanguard of the revolution. A massive contingency intervened—Chiang Kai-shek's ability to wipe out the urban Communist movement in the Shanghai Massacre in 1927.) Further, the turning point in the fortunes of the CCP clearly occurred in the "base areas" during the Sino-Japanese War (1937–1945): the areas of rural China where the CCP was able to establish itself as the dominant political and military force opposed to the Guomindong and the

Japanese Army. The success of the revolution, therefore, depended on successful mobilization of the peasantry in the 1930s and 1940s. How are we to account for its success?

This question has naturally loomed large in Western discussions of the Chinese Revolution since 1949. Two influential theories offer political culture and class conflict as causes of revolution, and neither of these high-level theories appears to be altogether satisfactory. A more plausible analysis refers to the local politics of class. Rather than postulating a single large causal factor, it is more plausible to understand CCP success as a concatenation of a number of small causes and advantages, deployed with skill and luck to a successful national victory.

Consider first a theory based on political culture. Chalmers Johnson argued in *Peasant Nationalism and Communist Power* (Johnson, 1962) that the CCP succeeded in mobilizing peasant support during the Sino-Japanese War because (a) peasants were nationalistic and patriotic, and determined to expel the Japanese, and (b) the CCP was the organization that showed the greatest military and organizational ability to oppose the Japanese military presence in China. Johnson maintained that the CCP downplayed its social program (class conflict, land reform, etc.) during the Anti-Japanese War, in the interest of a united front against the Japanese, and that its social goals played little or no role in its mobilizational successes. Peasants therefore supported the CCP out of nationalism, and were, perhaps, unpleasantly surprised at the social program that emerged after the defeat of the Japanese. This theory made a feature of political culture—nationalist identity—the central determinant of largescale collective action.

Mark Selden, an American sociologist, advanced a very different view of the CCP's success in *The Yenan Way in Revolutionary China* (Selden, 1971). He offered a class-conflict model, according to which Chinese rural society possessed an objectively exploitative class structure in opposition to which the CCP successfully mobilized support. Landlords, moneylenders, and the state exploited the peasantry by extracting rent, interest, and taxes. The CCP provided a program of social revolution aimed at overthrowing this exploitative order, and peasants followed this program, and supported the CCP, in order to pursue their class interests.

Johnson's theory has not stood the test of time very well, in part because there is a dearth of evidence to support the idea that ordinary Chinese people did in fact possess the nationalistic identity and political commitments that the theory postulates. Serious weaknesses in Selden's argument are substantial as well, however. Selden assumed that the realities of exploitation and class are relatively transparent, so that peasants more or less immediately perceive their class interests. And he assumed that collective action follows more or less directly from a perception of class interests: if there is a plausible strategy for furthering class interests through rebellion (i.e., the CCP), then peasants will be disposed to do so. However, the social reality of China was much more complex than this story would allow, with region, lineage, and village society existing as a more immediate social reality for most rural people than class and exploitation. So neither Johnson nor Selden provide a framework within which a fully satisfactory theory of the revolution can be constructed.

(Lucien Bianco discusses many of these rural complexities in *Peasants without the Party*; Bianco, 2001.)

A more convincing view has been offered by a third generation of historians of the Chinese Revolution. One of those historians is Yung-fa Chen in *Making Revolution: The Communist Movement in Eastern and Central China, 1937–1945* (Chen, 1986). Chen offers an explanation of the CCP's mobilization successes that depends upon a micro-level analysis of the local politics created in Eastern China as a result of local social arrangements and the Japanese occupation. Methodologically his approach is microfoundational and localistic rather than sweeping and monocausal. And Chen's main findings disagree in some important ways with both Johnson and Selden.

The main elements of Chen's analysis are these. First, he confirmed the Marxist view that the CCP had a coherent social program (land reform and fundamental alteration of rural property arrangements), and that the CCP made this program a central part of its mobilization efforts. This program implicitly defined a form of class analysis of rural Chinese society into poor peasants, middle peasants, rich peasants, and landlords, and endeavored to sharpen conflicts among these. Second, though, Chen rejected the view that these rural class relations and oppositions were fully transparent to participants, needing only the appearance in the village of a few ideologically correct cadres to mobilize peasant support. Rather, Chen held that the wide variety of rural social relations—lineage, family, religious organization, patron-client, friendship—worked as powerful brakes on the emergence of class consciousness. So a determined program of class-consciousness raising was needed, which the CCP attempted to provide through its "speaking-bitterness" sessions.

And, Chen maintained, peasants were highly skeptical of the ability of outside organizations to protect them against the wrath of local powers (landlords, officials) once the military threat had disappeared. A central problem of mobilization, then, was to create a local organization and militia that was capable of fending off Japanese and GMD military attack; that was sufficiently stable as to lend confidence that peasants could rely on it in the future; and to put forward a social program that would leave it well-positioned to begin the process of socialist reform through land reform, reform of credit institutions, and ultimately collectivization of agriculture and industry.

The heart of Chen's analysis depends on the assumption that peasants are rational political actors, and will support a political organization only if they judge that (a) it will support their local interests and (b) it will be powerful enough to support its local followers. (This has a lot in common with Samuel Popkin's arguments in *The Rational Peasant*; Popkin, 1979.) Chen then considers available data on a large number of local communities in Eastern China during the war years in the base areas of the revolution, and finds that the CCP did a skillful job of satisfying both requirements. It was effective in creating military and political organizations capable of protecting local interests; and it was effective in communicating its class analysis to peasants in sufficient degree to lead to support for its revolutionary social program. But, contrary to the nationalist thesis offered by Chalmers Johnson, he argues that the CCP was very skillful in avoiding direct military confrontation with the Japanese Army.

Another impressive effort to provide a new reading of aspects of the Chinese Revolution is provided by Odoric Wou in *Mobilizing the Masses: Building Revolution in Henan* (Wou, 1994). Focused on Henan Province, Wou attempts to uncover the complex set of factors that permitted the Communist Party to mobilize mass support for its program. He emphasizes organizational and political factors in his account: the strategies and organizational resources through which the CCP was able to move ordinary workers and peasants from concern with local interests to adherence to a national program. Wou provides fascinating detail concerning Communist efforts to mobilize miners and workers, Red Spears and bandits, and peasants in Henan Province.

Wou makes plain the daunting challenges confronting Communist cadres in their efforts to mobilize support at the village level: mistrust of outsiders, the entrenched political power of elites, and the localism of peasant interests in the region. Wou describes a social-political environment in the countryside that is reminiscent of Philip Kuhn's account of the situation of local militarization during the Taiping Rebellion in eastern China—one in which elite-dominated militias had evolved as an institution of self-defense against bandits and sectarian organizations (Kuhn, 1970a).

One of the most interesting and surprising findings that Wou puts forward is his contention that mobilization in Henan was not centered in remote and backward border areas, but rather included both remote and commercialized peasant villages (Wou, 1994, p. 129). This is somewhat inconsistent with Chen's analysis, who focuses precisely on the tactical advantages of remoteness offered by the base areas.

Wou also makes an effort to crack the riddle of peasant mentality in China. Are peasants inherently conservative? Are they latently revolutionary, awaiting only the clarion call of revolution? Both, and neither, appears to be Wou's assessment (Wou, 1994, p. 161). Wou finds a popular equalitarianism within Chinese peasant culture that provides a basis for Communist mobilization around an ideology of redistribution (p. 151); but equally he finds an entrenched hierarchicalism within Chinese popular culture that made subversion of elite power more difficult for Communist cadres (Wou, 1994, p. 135).

Wou also considers the political environment created for the CCP by the Sino-Japanese War. (This is the period treated by Chen.) Guomindang power virtually collapsed in Henan Province, and the Japanese occupied eastern Henan in 1938. The three-way struggle between the Japanese, the Guomindang, and the Communist Party gave the Party new opportunities for mobilization against both its enemies. Here Wou makes the important point that structural circumstance—military fragmentation of society, in this case—only provides the opening to successful mobilization, not its sufficient condition. The organizational and strategic competence of the CCP was needed in order to make effective use of these new opportunities for mobilization. Successful play of the game of coalition politics gave the CCP important advantages during this period, and created a position of strength that contributed substantially to post-war success of the movement.

A central tenet of Wou's analysis is the importance of Communist efforts to improve material conditions of life for the populations it aimed to mobilize. Famine relief, formation of production cooperatives, and revival of the silk industry represented efforts by the Party to demonstrate its ability to provide tangible benefits for local communities (Wou, 1994, pp. 314–326). These efforts had at least two beneficial effects: they provided material incentives to prospective followers, and, less tangibly, they enhanced confidence among villagers in the competence and endurance of the Party.

Both Chen and Wou make important contributions within a third generation of historical scholarship and interpretation of the Chinese Revolution. Their accounts are to some extent complementary and to some extent inconsistent—as one would expect in detailed efforts to answer profound questions about causation. And both accounts share an important historical insight: it is crucial to push down into the local village circumstances of social life and mobilization that the CCP faced as it attempted to generate commitment and support for its movement if we are to understand why it succeeded in mobilizing support from millions of rural people.

5.5 General and Specific Causal Hypotheses

It is worthwhile noticing that we can ask causal questions at two extremes of specificity and generality. We can ask why the Nicaraguan Revolution occurred—that is, what was the chain of circumstances that led to the successful seizure of power by the Sandinistas? This is to invite a specific historical narrative, supported by claims about causal powers of various circumstances. And we can ask why twentieth-century revolutionary movements succeeded in some circumstances and failed in others—that is, we can ask for an account of the common causal factors that influenced the course of revolution in the twentieth century. In the first instance we are looking to put forward a causal hypothesis about a particular event; in the latter we are seeking a causal explanation concerning the behavior of a class of events.

Take the idea that the outbreak of hostilities in World War I was caused by the assassination of Franz Ferdinand of Austria in 1914. This claim might be supported by identifying a chain of events that proceeded from the assassination, to decisions in various capitals, to the mobilization of troops, to the outbreak of fighting. The assassination was the spark that led to the conflagration. But this is a purely singular chain of events, and there is no regular connection between occurrences of this set of events and the outbreak of war. The sequence of causal links in this story involves pure contingency at many stages. Assassinations don't generally cause wars; sometimes they do and sometimes they don't. Events in the category of "political assassination" do in fact have a set of causal powers—through the influence that a political assassination can have on powerful decision-makers and public opinion. But there is no single mechanism that links assassinations to the outbreak of war.

Much inquiry in the social sciences has to do with singular causal processes (historical outcomes): individual revolutions, specific experiences of modernization and development, specific histories of collective action. Charles Tilly's career-long treatment of the collective political behavior of the French is a case in point; Tilly attempts to identify a characteristic tradition of French political action, and attempts to identify the historical occurrences which gave this tradition its specificity (Tilly, 1986). But Tilly is also interested in identifying common social mechanisms of contention; and this allows him to identify general causes as well as singular causes.

Historical investigation and "process tracing" permit us to analyze particular singular causal sequences—for example, "a floating iceberg caused the sinking of the Titanic." This kind of singular historical analysis permits discovery of the causal mechanisms and contingent happenings that were involved in the production of the event to be explained.

A general hypothesis about causation is based on a discovery of a pattern across a number of similar cases. For example, Theda Skocpol's *States and Social Revolutions* (1979) attempts to discover causal regularities leading to the occurrence of revolution that emerge from study of a small number of particular revolutions, and Jeffrey Paige's *Agrarian Revolution* (Paige, 1975) offers a large-N study of cases of revolution and rebellion to attempt to discover common causal conditions. And through either type of study we might arrive at evidence supporting general causal claims like these: "the occurrence of subsistence crises is a causal factor in the occurrence of rebellion," "a strong state inhibits the occurrence of rebellion," and "international crises make rebellions more likely."

To assert that A's are causes of B's is to assert that there is a typical causal mechanism through which events of type A lead to events of type B. Here, however, we must note that there are rarely single sufficient conditions for social outcomes; instead, causes work in the context of causal fields. So to say that revolutions are causally influenced by food crisis, weak states, and local organization, is to say that there are real causal linkages from these conditions to the occurrence of revolution in specific instances. If we have enough cases, then these causal mechanisms will also produce some regularities of association between the hypothesized causal factors and the outcome; but without a large number of cases these regularities will be difficult or impossible to discern.

To what extent is such a causal analysis of a unique event explanatory, rather than merely true? The account is explanatory if it identifies influences that commonly exert causal power in a variety of contexts, not merely the case of the French in 1848 or Russia in 1917. And a case study that invokes or suggests no implications for other cases, falls short of being explanatory.

I will put it forward as a methodological maxim that a causal assertion is explanatory only if it identifies a causal process that recurs across a family of cases. A historical narrative is an answer to the first sort of question ("why did this particular event come about?"); such a narrative may or may not have implications for more general causal questions. A true causal story is not always explanatory.

There is another issue raised by this topic of general and particular causal hypotheses, which has to do with the idea of "over-determination." Return to the case of World War I. It might be argued that there were broad structural forces at work that were steadily increasing the likelihood of war throughout 1912–1914—deepening economic and geographical conflicts of interest among the great powers, large-scale military planning by various governments, and a worsening arms race, for example; so war was "inevitable" with or without the spark created by the assassination of the Archduke. If this event had not occurred, some other instigating event would have cropped up; so the conflagration was inevitable. On this interpretation, the assassination of the Archduke was a critical part of the actual pathway leading to the outbreak of war; but there were many other hypothetical pathways that would have led to the same result. So it is the background structural conditions that were the real and substantive causes of World War I—not the contingent and accidental fact of the assassination in 1914.

5.6 Causal Reasoning in Meso-History

Here, then, we can come to a set of conclusions. Social entities exercise causal powers through their capacity to affect the choices and behavior of the individuals who make up these entities, and through no other mechanism. Once the ground is cleared along the lines delineated by the notion of meso-history—emphasizing both the importance for the historian of the particular contingencies of a specific historical context and the causal efficacy of the broad structures and processes that are in play—the challenge for the historian of large processes is more apparent. It is to seek out the specific institutions, structures, and processes that are embodied in a given historical setting; to identify the possibilities and constraints that these structures create for agents within those settings; and to construct explanations of outcomes that link the causal properties of those structures to the processes of development that are found in the historical record. Finally, it is useful for the historian of large processes to explore the space of "what might have been"—the space of contingent alternative developments that were equally consistent with the configuration of large structures and particular circumstances at a given time.

We can come to several concluding observations about causal explanations in history.

- Social entities exercise causal powers through their capacity to affect the choices and behavior of the individuals who make up these entities, and through no other avenue.
- Social processes should be expected to demonstrate a significant level of contingency, path-dependency, and variability, given the multiple types of causal mechanisms, institutional variations, and features of individual agency that come together to bring about a given outcome.
- We should not expect to discover strong "social laws" or governing social generalizations across social phenomena and settings. Instead, the most we should expect are the exception-laden regularities that derive from "common structures of agency" in multiple social settings.

- It is possible to offer valid and justified causal explanations of singular events, by discovering through historical and empirical research the traces of the causal mechanisms that brought about these events.
- A central intellectual role for empirical theories in the social sciences and social psychology—"theories of the middle range"—is the formulation of descriptions of typical causal mechanisms in social circumstances of "socially situated agents".

It is difficult to discern a valuable intellectual role for sweeping social theories intended to apply to all social settings—general theories of social organization and change. Instead, we should recognize the contingency and variability of the social world, and look rather to contextually defined social relations and the causal mechanisms that derive from them.

Chapter 6
History of Technology

Technological change is a key issue for the philosophy of history, because technology is itself a complex social process, involving the influence of many social factors (economic, scientific, political, organizational, educational). So arriving at a history of a particular technology—e.g., electric power, inertial navigation, or medieval ship construction—is itself a challenging and important task for historians. And second, technological change is itself often invoked as one of the large causal factors that account for other important large social outcomes—e.g., population increase, the incidence of war and peace, or environmental change. We need to be able to provide an account of the metaphysics of the causal properties allegedly possessed by technology systems. It is worthwhile to examine both sets of problems in the context of the philosophy of history.

Let us canvass, to start, how the history of technology intersects with meso-history. It does so in several ways:

- Technology constitutes a large "structural force or condition" commonly invoked in macro-historical accounts (e.g., Lynn White's analysis of the stirrup (White, 1962) or Marc Bloch's analysis of the wheeled plough (Bloch, 1966)).
- Technological change is itself a complex historical process, invoking other large-scale structural factors, such as population, education, market circumstances (e.g., Ester Boserup's argument that technological change derives from rising population density and consequent pressure on natural and biological resources; Boserup, 1981).
- A technology is embedded in a specific set of educational, research, and financial institutions that significantly influence the pace and direction of technology change.
- Technological changes are often said to have important meso-level social consequences, distinct from their primary purposes (e.g., extension of a transport technology into new periurban areas may stimulate a distinctive pattern of population growth and settlement patterns; Warner, 1969; Skinner, 1964).

Some historians imagine that new technologies force other kinds of social changes, or even that a given technology creates a more or less inevitable process of development in society. Marx is sometimes thought to offer such a theory, for

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example, when he holds that the forces of production create changes in the relations of production. This view is referred to as technological determinism (Boserup, 1981; Smith and Marx, 1994). The reasoning underlying this interpretation of history goes something like this. A new technology creates a set of accessible new possibilities for achieving new forms of value: new products, more productive farming techniques, or new ways of satisfying common human needs. Once the technology exists, agents or organizations in society will recognize those new opportunities and will attempt to take advantage of them by investing in the technology and developing it more fully. Some of these attempts will fail, but others will succeed. So over time, the inherent potential of the technology will be realized; the technology will be fully exploited and utilized. And, often enough, the technology will both require and force a new set of social institutions to permit its full utilization; here again, agents will recognize opportunities for gain in the creation of social innovations, and will work towards implementing these social changes.

Even cursory examination of current work in the history of technology refutes this idea. All major technologies demonstrate deep contingencies when we examine their development in detail. Why do we have alternating current rather than direct current in the outlets in our homes today? Because Thomas Edison mounted a major public relations campaign involving the eloctrocution of cats in order to defeat the advocates of direct current. There are many examples of potentially productive technologies that failed to come to full development after discovery (for example, the water mill in the ancient world and the Dvorak keyboard in the contemporary world). There is nothing inevitable about the way in which a technology will develop—imposed, perhaps, by the underlying scientific realities of the technology; and there are numerous illustrations of a more complex back-and-forth between social conditions and the development of a technology. And we have numerous examples of contingent and unexpected social consequences emerging from the advent of a major new technology. So technological determinism is no more persuasive than any other mono-causal theory of historical change.

Rather, it is more credible to regard technology as one among a number of meso-level social factors that influence historical development at a given time. We are well advised to approach the history of technology with the ideas of contingency and conjunction in mind: historical outcomes are almost always the result of multiple sets of conjunctural causes, and the results are highly contingent and path-dependent.

For more credible interpretations of the relationships that exist between technology and historical change, we can consider the work of some very insightful historians.

6.1 History of Electric Power

Let us examine an important detailed study in the history of technology: Thomas Hughes's groundbreaking book, *Networks of Power: Electrification in Western*

Society, 1880–1930 (Hughes, 1983).[1] Hughes has done much in the past 30 years to provide a new foundation for the history of technology, and this work on the history of electric power is among his most important contributions. Hughes constructs a complex narrative that leads from the important scientific discoveries and inventions in the 1880s which created the possibility of using electricity for power and light; through the creation of complex organizations by such systems builders as Thomas Edison and Elmer Sprague to solve the many technical problems which stood in the way of successful implementation of these technical possibilities; to the establishment of even larger social, political, and financial systems through which systems builders implemented the legal, financial, and physical infrastructure through which electricity could be adopted by large cities and regions.

Along the way Hughes demolishes several important misconceptions about the history of technology. He refutes, first, the notion that there was an *inevitable logic* to the development of electric power. At various points in the story he tells, there are choices available that do not have unique technical solutions. The battle of the systems (direct versus alternating current) is one such example; Edison's work proceeded on the basis of a technology of direct current, whereas the industry eventually adopted the technology of alternating current. Each choice posed technical hurdles that required solution; but there is good reason to believe that the alternative not taken could have been adopted with suitable breakthroughs along the other path. The path chosen depends on a set of social factors—popular opinion, the press, the orientation of professional engineering schools, the availability of financing, and the intensity of intellectual resources brought to bear on the technical problems that arise by the research community.

Second, Hughes establishes that, even when the basic technology was settled, the *social implementation of the technology*, including the nature and pace of adoption, was profoundly influenced by non-technical factors. Most graphically, by comparing the proliferation of power stations and power grids in London, Paris, and Chicago, Hughes demonstrates that differences in political structure (e.g. jurisdiction and local autonomy) and differences in cultural attitudes elicited markedly different patterns of implementation. Chicago shows a pattern of a few large power stations in the central city; London shows a pattern of myriad small stations throughout the metropolitan area; and Paris shows a pattern of a few large stations along the Seine in the periurban areas of the city. Moreover, these differences in styles of implementation can have major differences in other sorts of social outcomes; for example, the failure of London to implement a large-scale and rational system of electric power distribution meant that its industrial development was impeded; whereas Chicago's industrial output increased rapidly during the same time period. These patterns of build-out, and the urban geography that they helped to create, were the effect of

[1] The history of technology as a discipline has been particularly fruitful in the past 20 years. Historians in this field have moved substantially beyond the conception of technological change as a series of stages of technical design and implementation, to focus on the social constitution of the process of technological change. Thomas Hughes has played a central role in this revival, as has the journal *Technology and Culture*.

social and political factors at the level of municipal government, regional financial institutions, and the like.

Third, Hughes sheds light on the social and individual characteristics of *invention and refinement* that occur internal to the process of technological change. He describes a world of inventors and businesses that was highly attuned to the current challenges that stood in the way of further progress for the technology at any given time. Major hurdles to further development constituted "reverse salients" which then received extensive attention from researchers, inventors, and businesses. The designs of generators, dynamos, transformers, light bulbs, and motors each presented critical, difficult problems that stood in the way of the next step; and the concentrated but independent energies of many inventors and scientists led frequently to independent and simultaneous solutions to these problems.

Fourth, Hughes makes the point that, in the instance of this technology at least, the development of the technology was inseparable from the establishment of "massive, extensive, vertically *integrated production systems*," including banks, factories, and electric power companies (Hughes, 1983, p. 5). "The rationale for undertaking this study of electric power systems was the assumption that the history of all large-scale technology—not only power systems—can be studied effectively as a history of systems" (p. 7). The technology does not drive itself; and it is not driven (exclusively) by the technical discoveries of the inventor and scientist. Rather, the eventual course of development and implementation is the complex result of social pulls and constraints, as well as the inherent possibilities of the scientific and technical material.

Finally, Hughes introduces the important concept of "*technological momentum*". By this concept he means to identify the point that a large technology—transportation, communication, power production—once implemented on a wide scale, acquires an inertia that is difficult to displace. Engineers and designers have acquired specialized knowledge and ways of approaching problems in the field; factories have been established to build the specialized machines and parts needed for the technology; and investors and banks have embedded their fortunes in the physical implementation of the technology. "Business concerns, government agencies, professional societies, educational institutions, and other organizations that shape and are shaped by the technical core of the system also add to the momentum" (p. 15).

Hughes demonstrates several important lessons for large-scale historical explanation. First, through his detailed account of a complex 50-year international process of design and implementation, he shows that large-scale events can be explained, and that a variety of large-scale structural factors are pertinent to the outcomes. Second, he demonstrates the important scope of agency and choice within this story. Outcomes are contingent, and individuals and local agents are able to influence the stream of events at every point. And finally, through his concept of technological momentum he provides a constructive way of thinking about the social influence of technology itself within the fabric of historical change—not as an ultimate determinant of outcomes, but as constraining and impelling set of limitations and opportunities within the context of which individuals strategize and choose.

6.2 Alternative Forms of Industrial Organization

Turn now to a second important example of contemporary meso-history relevant to technology change: research by Charles Sabel and others on alternative modes of industrial organization in European economic history. There is a conventional line of thought in economic history that emphasizes the inevitability of certain broad characteristics of economic change and institutional organization in any pre-modern economy.[2] It is the familiar storyline of industrial revolution in Western Europe. Rising agricultural productivity stimulated population growth and permitted the increase of non-agricultural population. Demand for consumption goods increased as a result of this population increase—leading to rising prices for common consumption goods. These price changes stimulated more extensive production for the market; they also created an incentive for technological innovation (resulting in rising productivity of labor). Machine production was a predictable response to these commercial and financial changes, eliciting innovations in power technology and leading to an increase in the scale of production (from workshop to factory). Factory production elicits greater technological innovation, greater division of labor, and a rising capital-labor ratio; these changes in turn require expansion in the scope of production. Mass production based on low-skill labor, extensive use of specialized machines, and extensive use of non-biological sources of power follow.[3] This is the narrative of Marx's *Capital* (Marx, 1977), and also underlies the Fordist interpretation of the American industrial system.

However, recent work in economic history suggests strongly that this story is significantly too inevitabilist. Population, prices, and technology are all highly pertinent to the economic pathway experienced by Western Europe; but they do not determine either the institutions through which economic activity takes place or the outcome of economic development. And the stylized history of Western Europe's economic transformation that the story represents is deficient in failing to recognize the very great degree of variation there was in basic economic institutional arrangements. Contingency rather than necessity, and diversity rather than uniformity, appear to be the dominant features of much recent economic history—even in Europe and North America.

In "Historical Alternatives to Mass Production" Sabel and Zeitlin (1985) argue against the idea of the historical inevitability of mass manufacture, both theoretically and empirically. They argue that historically feasible alternatives exist—in particular, the alternative of flexible production, short runs, specialized products, flexible machinery, and skilled artisanal and engineering labor. The argument in this essay is

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[2]There has been lively work on the issue of the nature and causes of economic development in the early modern European economy in the past 20 years. Especially central is the question of the causal origins of self-sustaining growth in the early modern period of European development. Early expressions of work in this area include Deane (1979), Feinstein (1981), Deane and Cole (1967), and Postan (1972). Important contributions to the more recent literature include Crafts (1985), Jones (1988), Floud and McCloskey, (1981), and O'Brien and Keyder (1978).

[3]Deane and Cole (1967) provide a representative narrative along these lines.

that political and class factors produced the imperative toward mass manufacture—not the technical characteristics of new technologies, or the efficiencies and cost structures of the various alternatives. Mass production techniques in textiles spelled the doom of the weavers in the 1820s; this is an instance of a clear efficiency-based explanation for the dominance of one system over another. But there were historically feasible alternatives to factory production in many industries—glass, silk, watches, metal working, machine goods—where skilled artisanal production could have successfully competed with techniques of mass manufacturing. In *Worlds of Possibility* (1997) they expand this point by demonstrating even broader "strategic" variability within existing forms of industrial organization—substantial levels of hedging on the part of managers, and substantial effort to influence the competitive environment. And in fact, Philip Scranton demonstrates that much of the history of American factory manufacturing took the form of "flexible manufacturing" rather than mass manufacturing through at least the 1960s (Scranton, 1997). Locomotives, specialized metal-working tools, jewellery, and furniture all were produced in factories involving skilled labor, flexible manufacturing, and batch industrial processes.

Sabel and Zeitlin, then, emphasize contingency and agency within the process of technology development, economic growth, and institution-building: there were historically feasible alternatives in the organization of production with modern technologies; and in fact, managers, workers, and planners exploited these contingencies so that the alternative forms in fact prospered in various settings. They emphatically contest the sense of iron necessity in outcomes of economic processes, relative to the standard approach to the history of industrialization of Europe and America.

Sabel and Zeitlin's case is important for several reasons. First, it offers a striking and persuasive alternative to the standard view of European economic history—that traditional techniques of production and modes of economic organization based on skilled labor, small manufacture, and traditional techniques, were inevitably replaced by factory production, the application of specialized tools and machinery, and the de-skilling of industrial labor. Proletarians replaced artisans, and factories replaced specialized shops. And second, more generically, it significantly challenges a dominant paradigm of understanding large-scale historical change—as a cumulative and sweeping process through which one form comprehensively replaces another, based on the technical or economic superiority of the successor. Sabel and Zeitlin argue instead for a conception of social change that emphasizes flexibility and multiplicity of forms—factories, specialized machine shops, large-scale rigid units and small, flexible operations—governed by strategic decision-makers who deliberately chose a range of options well-designed to secure their interests. At any given time, a number of alternative economic institutions are in use (types of firms, for example, with types of technology and forms of labor skill), and very significantly different forms may be viable simultaneously and indefinitely. An ecological metaphor, in which many different organisms exploit different niches within one environment, fits this picture better than the notion of economic competition and the inevitable success of one particular type. This portrait is important, because it may lead us to doubt, or at least inspect with newly critical eyes, the blanket statements that we sometimes find about "feudal institutions" or "traditional agriculture" or "early capitalism."

The detailed scrutiny of these forms of contingency and diversity within European economic history is highly productive. It leads us to recognize the multiplicity of forms of adaptation that are available in many historical cases; and at the same time, it serves to identify some of the structural factors that impel the process of change in one direction rather than another.

6.3 Railroads as a Historical Cause

The development of the railroad was a transformative technology in the nineteenth century. Here at least one might wish to argue that the development of the technology was inevitable, and its effects on social organization were predictable. However, neither of these assumptions is correct. There were many branching possibilities in the development of the technology itself; and historians and social scientists have demonstrated that the transportation potential represented by the technology had rather different effects on the social geography of different countries as they developed their rail networks.

An important example of a study of the social-technological development of the railroad is offered in William Cronon's fascinating history of *Chicago, Nature's Metropolis: Chicago and the Great West* (Cronon, 1991). Cronon tracks the effects that the extension of the rail network had on the city of Chicago and the region surrounding it into Illinois, Wisconsin, Iowa, and Michigan. Cheap, reliable rail transport between Chicago and New York created large markets for grain and beef. This gave incentives to farmers and traders to organize their activities in such a way as to take advantage of the profits newly available in these markets. But Cronon points out that transportation by railroad of large volumes of grain required a reorganization of the market institutions that were used: the establishment of grain elevators along the rail lines, the establishment of a grading system for qualities of grain being sold by farmers to elevator operators, and the establishment of a futures market for grain and beef. And he observes that entrepreneurs recognized the gains that could be achieved by developing these institutions and carried them forward. So the technology "needed" a reorganization of the market for grain; entrepreneurs recognized an opportunity for profits in achieving this reorganization; and the necessary social innovations occurred.

The central causal mechanisms in this instance are the market demand created by rising population in the Northeastern United States (Boston and New York), and Chicago's favorable location for rail and water transport to points east. Concentrated urban demand causes development of infrastructure and flow of timber and grain. Residents in the urban eastern United States need food, so rising population creates rising demand for grain. Rising demand gives economic incentive to distant producers to increase production. And it gives economic incentives to commercial agents to organize infrastructure (warehouses, railyards, grain elevators, exchanges) that permit efficient and large-scale trade in grain. Goods need to be transported from the point of production to the point of consumption—thereby creating an economic incentive for transport providers to establish transportation infrastructure (railroads, terminals, rolling stock). Greater availability of goods transported by effective transportation, in turn, provides incentive to new residents and traders to choose Chicago over Peoria—leading in turn to rising population and consequent demand.

It is worth noting that the processes described here have, in turn, additional unintended and unexpected consequences. Dense population causes more frequent public health problems. Effective transportation systems create constituencies of working class people who can be mobilized to politics and union activity (e.g. the Pullman strike). More intensive inter-regional transportation can have the effect of spreading disease more rapidly. Effective communication systems cause the more rapid diffusion of ideas, innovations, and social movements—which in turn cause changes in technology, politics, and patterns of consumption.

In analyzing and viewing the development of great metropolitan regions—the restructuring of economic activity throughout the region (crops, forestry, manufacturing, the movement and circulation of people and goods, the proliferation of new and more diverse and specialized enterprises)—we see a great and powerful process. We see the invention of new inter-locking business institutions and practices; new patterns of consumption; and new secondary technologies that fill the niches created by the new regional flows.

Thus transport technology innovation plays a key role in these patterns of regional development. So a major technology plays an important causal role in historical outcomes. But is it an instigating cause; an important explanatory variable; or a predictable and obvious necessary condition (and therefore not of special explanatory interest)? We might say that it is the economic causes—population, demand, and markets—that elicit the innovations and adoptions of the new technology and that transport is simply an intervening variable. It seems clear from Cronon's account that rail transport played a somewhat autonomous causal role in the process. If the investments had not been made in Chicago's rail infrastructure; if siting decisions had been made differently crossing the Midwest; and if supporting innovations (futures grain markets, grain elevators) had not been forthcoming, then Chicago's economic and social development would have been very different.

We can also ask the question of contingency in the Chicago story. How much path-dependency do we find in this story? Was it the circumstances of the location of the terminus and the initial structure of the network that led to the development of the metropolis? Or were the circumstances of pre-existing water transport (Great Lakes), along with geography linking east and west, sufficient to select Chicago over other possible hubs? This appears not to be the case; there were other important cities that had the same strategic opportunity of water transport in the Great Lakes (Milwaukee, Toledo, Cleveland). So Chicago's emergence as the premier city of the region was a contingent event.

Might we imagine that Chicago's pre-eminence was established once the rail system arrived? Was the business and ecological transformation of Chicago's region sound inevitable, given the extension of the rail system into Chicago? Once again, contingency comes into the story. The build-out of the American rail network was itself a highly contingent matter; the major east–west lines could have been placed in numerous alternative routes, including a network that would have made St. Louis the major rail nexus in the center of the country. Second, the policy environment within which the American rail network developed represented another major form of contingency. As Frank Dobbin demonstrates, England, France, and the United States possessed very different "policy cultures", and these differences created substantial differences in the way in which the basic technology of the railroad was exploited in the three national settings (Dobbin, 1994). And third, there are multiple social solutions that would work roughly as well as the institutions of the grain elevator and the futures market for solving the business challenges of mass transport and marketing of grain. The solutions that emerged in Chicago were therefore contingent as well.

Let's consider more closely one aspect of this historical contingency: Frank Dobbin's analysis of the historical processes associated with the extension of railroad technology in different settings (Dobbin, 1994). Dobbin's research is intended to address a relatively limited historical puzzle. A powerful new technology, the railroad, was developed in the first part of the nineteenth century. The nature and characteristics of the technology were essentially homogeneous across the national settings in which it appeared in Europe and North America. However, it was introduced and built out in three countries—the United States, Britain, and France—in markedly different ways. The ways in which the railroads and their technologies were regulated and encouraged were very different in the three countries, and the eventual rail networks had very different properties in the three countries. The question for explanation is this: can we explain the differences in these three national experiences on the basis of some small set of structural or cultural differences that existed among the three countries and that causally explain the resulting differences in build-out, structure, and technical frameworks? Or, possibly, are the three historical experiences different simply because of the occurrence of a large but cumulative number of unimportant and non-systemic events?

Dobbin attempts to explain these differences in implementation as a consequences of differences in what he calls "policy culture" in the three countries. He argues that there were significantly different cultures of political and industrial policy in the three countries that led to substantial differences in the ways in which government and business interacted in the development of the railroads. "Each Western nation-state developed a distinct strategy for governing industry" (Dobbin, 1994, p. 1). The laissez-faire culture of the United States permitted a few large railroad magnates and corporations to make the crucial decisions about technology, standards, and routes that would govern the development of the rail system. The regulated market culture of Great Britain favored smaller companies and strove to prevent the emergence of a small number of oligopolistic rail companies. And the technocratic civil-service culture of France gave a great deal of power to the engineers and civil servants who were charged to make decisions about technology choice, routes, and standards.

These differences led to systemic differences in the historical implementation of the railroads, the rail networks that were developed, and the regulatory regimes that surrounded them. The US rail network developed as the result of competition among a small number of rail magnates for the most profitable routes. This turned out to favor a few east–west trunk lines connecting urban centers, including New York, Boston, Chicago, and San Francisco. The British rail network gave more influence to municipalities who demanded service; as a result, the network that developed was a more distributed one across a larger number of cities. And the French rail network was rationally designed to conform to the economic and military needs of the French state, with a system of rail routes that largely centered on Paris.

This example illustrates the insights that can be distilled from comparative historical sociology. Dobbin takes a single technology and documents a range of outcomes in the ways in which the technology is built out into a national system. And he attempts to isolate the differences in structures and cultures in the three settings that would account for the differences in outcomes. He offers a causal analysis of the development of the technology in the three settings, demonstrating how the mechanism of policy culture imposes effects on the development of the technology. The inherent possibilities represented by the technology intersect with the economic circumstances and the policy cultures of the three national settings, and the result is a set of differentiated organizations and outcomes in the three countries. The analysis provides abundant documentation of the social mechanisms through which policy culture influenced technology development; the logic of his analysis is more akin to process tracing than to the methods of difference and similarity in Mill's methods.

The research establishes several important things. First, it refutes any sort of technological determinism, according to which the technical characteristics of the technology determine the way it will be implemented. To the contrary, Dobbin's work demonstrates the very great degree of contingency that existed in the social implementation of the railroad. Second, it makes a strong case for the idea that an element of culture—the framework of assumptions, precedents, and institutions defining the "policy culture" of a country—can have a very strong effect on the development of large social institutions. Dobbin emphasizes the role that things like traditions, customs, and legacies play in the unfolding of important historical developments. And finally, the work makes it clear that these highly contingent pathways of development nonetheless admit of explanation. We can identify the mechanisms and local circumstances that led, in one instance, to a large number of firms and hubs and in the other, a small number of firms and trunk lines.

Cronon and Dobbin illustrate several different aspects of technological causes and technological contingency in their accounts of the railroad. Their work illustrates several fundamental points about the role of technology in historical change. A new technology creates new opportunities for power, wealth, efficiency, or productivity; so a new technology can be a powerful force for social and economic change. Governments, farmers, entrepreneurs, and corporations have a complicated set of incentives that lead them to consider developing the new technology. So new technologies certainly function as effective historical causes. The development of a technology, however, introduces deeply significant elements of contingency. The term "path-dependency" is an accurate description of the process of the development of a major technology. Third, a technology is both influenced by social factors in the society in which it is developed, and also influences the future direction of social factors in the society. Thus technology is both cause and effect of social change. And, finally, it is evident that the study of the history of technology is inevitably a study of social processes and institutions as much as it is a study of machines and inventions. Technology is a social product, shaped by the needs and powers that exist in society as much as it is shaped by scientific imagination and discovery.

6.4 Water Transport in China

Water transport was a crucial factor in China's economic and spatial development. Many parts of China were very richly provided with networks of rivers; these were supplemented by canals to provide low-cost transport throughout relatively large spaces. And China's major rivers provided the possibility of long-distance commerce based on low-cost river transport. Water transport, according to G. William Skinner, established the structure within which "macro-regional" economies emerged; and the social transactions and behavior of people throughout China were very much structured by these economic networks (Skinner, 1964, 1977b). The role of transport in late Imperial China was thus of great importance for the development of the size and spatial distribution of population, the reach of the state, and the ability of the state to maintain social order. The grain trade provided for more intensive population development, the movement of troops helped to secure public order, and the movement of officials and messengers was essential to the imperial state's ability to impose its will on the periphery.

Skinner's insights have generated a very fertile program of research for the China field. His own analysis of the marketing hierarchies of pre-modern China sets the social context for much of the subsequent study that scholars have provided for subjects as diverse as urbanization, religion, and rebellion.

Winston Hsieh (Hsieh, 1978) provides an interesting example of how these factors come together in explanation of an important historical episode—the rapid and patterned diffusion of rebellion in the Canton Delta in 1911. His narrative depends on transport in several important ways. The population density of the lower Canton Delta was made possible by the availability of low cost bulk transport through the water network of the delta. This permitted farmers to specialize in export rice; commercialization proceeded intensely, and the population density of the region rose sharply. Another important effect occurred in the small city of Shih-Ch'i; its western districts grew rapidly in urban intensity in the final decades of the nineteenth century, while the other parts of the city declined. Hsieh attributes this pattern of growth to the importance of ferry and steam shipping on the Shih-ch'i Sea (Hsieh, 1978, p. 129). But the income created by low-cost transport was challenged by another transportation innovation—the establishment of the Canton-Kowloon railroad in

1906, which allowed rice merchants to bring Thai rice directly into competition with the rice harvest of the lower Canton delta.

Hsieh argues that transport and marketing hierarchies provide critical explanatory variables for the timing and pattern of mobilization of Republican rebellion in the Canton Delta in 1911. Transport constituted a longstanding structural variable that created population density and population interests that were vulnerable to crisis—and therefore provided a population ready to be mobilized when crisis hit. And the marketing routes that had been established through local markets also provided the networks through which agents of mobilization—sectarians, martial arts instructors, millenarianists—would travel and mobilize.

6.5 Agriculture and the Natural Environment

Consider now another large example of meso-history with a technological core. In this case the subject has to do with the relationship between agriculture (a fundamental human technology) and the natural environment. (Population growth comes into the story as well; expansion of agricultural capacity permitted expansion of China's population.) Mark Elvin's title*, The Retreat of the Elephants: An Environmental History of China*, is brilliantly chosen to epitomize his subject: the human causes of longterm environmental change in China over a 4000 year period of history (Elvin, 2004). How many of us would have guessed that elephants once ranged across almost all of China, as far to the northeast as what is now Beijing? And what was the cause of this great retreat? According to Elvin, it was the relentless spread of agriculture and human settlement.

In other words, human activity in farming and water management changed the physical environment of China in such a profound way as to refigure the range and habitat of the elephant. "Chinese farmers and elephants do not mix." This story provides an expressive metaphor for the larger interpretation of environmental history that Elvin offers: that environmental history is as much a subject of social history as it is a chronology of physical and natural changes. Human beings transform their environments—often profoundly and at great cost.

This analysis complements some of Elvin's arguments in "The High-Level Equilibrium Trap"—the idea that Chinese agriculture had reached a stage of development by the late imperial period in which technique had been refined to the maximum possible within traditional technologies, and population had increased to the point where the agricultural system was only marginally able to feed the population (Elvin, 1972). So further productivity advances were impossible; the technology had been finetuned to the point that only advanced scientific research could further increase grain productivity. This is what he refers to as a "high-level equilibrium trap." He returns to something rather similar to this idea in *Retreat of the Elephants* by offering a theory of environmental exhaustion ("Concluding Remarks"): a measure of the degree to which population increase and economic growth have placed greater and greater pressure on non-renewable resources.

This is now a familiar story, when we consider the anthropogenic influences on global warming in the past 50 years. What Elvin's book demonstrates is that human activity is an integral part of the story in the long sweep of history as well. Nowhere is this fact more evident than in Elvin's treatment of the perennial problem of water management in China. Seawalls, canals, dikes, drainage, irrigation, desalinization, and reservoirs were all a part of China's centuries-long efforts at water control. And each of these measures had effects that refigured the next period in the water system—the course of a river, the degree of silting of a harbor, the diminishment of a lake as a result of encroachment. (Peter Perdue tells a similar story about the fortunes of Hunan's Dongting Lake (Perdue, 1987); the lake's boundaries shrink as opportunistic farmers fill it in.) The waterscape of late Imperial China was very much a moving picture as human activity, deliberate policies, technology innovations, and hydrology and climate interacted. There is a particular drama in seeing a centuries-long history of magistrates attempting to control the hydrology of the great rivers and deltas of the Yangzi and Yellow Rivers, to counteract silting and flooding and the massive problems that these processes entailed. Here the local officials made their best efforts to absorb the history of past interventions and their effects in order to design new systems that would obviate silting and flooding. This required planning and scientific-technical reasoning (Elvin, 2004, p. 137); it required large financial resources; and, most importantly, it required the mobilization of vast amounts of human labor to build dikes and polders. But always, in the end, the water prevailed.

Elvin's history is fascinating in a number of ways. He is an innovative writer of history, bringing new materials and new topics into Chinese historical research. His interweaving of agriculture, population growth, technology, and environmental change is masterful. He combines economic history, cultural history, and natural history in ways that bring continual new flashes of insight. He makes innovative use of literature and poetry to try to get some inklings into the attitudes and values that Chinese people brought to the environment. And he returns frequently to the dialectic of population growth and resource use—a rising tempo of change that imposes more and more pressure on the natural environment.

6.6 Warfare: The Franco-Prussian War

When we consider the role of technology in history, we are brought to consider some of the basics of human civilization: farming, control of the natural environment, transportation, manufacturing. But warfare and the technologies associated with largescale violence are also largescale historical factors. So let us now consider an example drawn from recent French history: the defeat of France in the Franco-Prussian War in 1870. This is an episode that is often explained in terms of technology (railroads and armaments); but, as we will see, the situation was more complex.

The rapid, bloody, and total defeat of the French army by the Prussian army in 1870–1871 was an enormous and unexpected shock to France and to Europe.

Since the Napoleonic Wars it was taken as given that France's armies were powerful, well-equipped, and well generaled. But the Prussian army quickly defeated French armies across eastern France, from Wissembourg to Sedan, with massive loss of life on the French side. And the collapse of the army was rapidly followed by the siege of Paris and the Paris uprising leading to the establishment of the Commune of Paris and eventually its bloody suppression. So this period of 2 years was a critical moment in France's history in the nineteenth century. Michael Howard's 1961 history*, The Franco-Prussian War: The German Invasion of France 1870–1871* (Howard, 1961), is probably the most comprehensive book in English on the Franco-Prussian War. Here is how Howard expresses the comprehensiveness and shocking totality of France's defeat:

> The collapse at Sedan, like that of the Prussians at Jena sixty-four years earlier, was the result not simply of faulty command but of a faulty military system; and the military system of a nation is not an independent section of the social system but an aspect of it in its totality. The French had good reason to look on their disasters as a judgment. The social and economic developments of the past fifty years had brought about a military as well as an industrial revolution. The Prussians had kept abreast of it and France had not. Therein lay the basic cause of her defeat. (Howard, 1961, p. 1)

So Howard's judgment of the causes of this massive military failure is ultimately technological and systemic. The technological changes to which he refers are familiar: the role that railroads could play in the logistics of nineteenth-century warfare (opportunities that needed to be recognized and incorporated into military plans and the design of operational systems); the advent of new infantry weapons (breech-loading rifles of greater range and speed of loading); and new advances in artillery. The Prussian army incorporated breech-loading rifles (the needle gun) as early as 1843; whereas the French (as well as the British and Austrian armies) retained the muzzle-loader until the 1860s. And the Prussian generals led major advances in artillery in the decades leading up to the Franco-Prussian war, with greater precision and fire power in their Krup guns.

Railroads played a key role in Prussia's mobilization and logistics. The Prussians were able to maintain coordination and organization of their rail system; whereas the French rail system quickly fell into disorder. Howard describes the military potential of railroads in these terms:

> Speed of concentration was only one of the advantages which railways provided. They carried troops rapidly to the theatre of war; and they enabled them to arrive in good physical condition, not wearied and decimated by weeks of marching. Armies needed no longer to consist of hardened regular troops; reservists from civil life could be embodied in the force as well.... Further, the problem of supplying large forces in the field was simplified. (Howard, 1961, p. 3)

And, most significantly, the vast challenge of supplying an army in the field was greatly facilitated by the presence of an effective and well-administered rail system. However, a rail system is not simply a collection of track, locomotives, and rail cars; it is an organized social system with intricate logistics, infrastructure, and planning. Howard takes the view that an important determinant of the outcome of the Franco-Prussian War was the administrative superiority of the Prussians over the French in the management, planning, and deployment of their rail resources. The French rail system was forced into sudden disarray by the attempt to rapidly mobilize a large civilian army. Troops and their equipment were separated, often forever. Mountains of matériel were to accumulate in depots without adequate logistical planning for how to deliver these weapons, ammunition, uniforms, and food to the field. There were sufficient war materials to support an army of adequate size; instead, "it was the chaos of the French mobilisation" that led to the disastrous failure of 1870. On Howard's account, then, the failure of the rail system is a very important cause of the shocking collapse of the French military during the Franco-Prussian war.

The systemic part of Howard's diagnosis is a failure of government: a failure to coordinate ministries and the bureaucracy of the military in pushing forward the reforms that would lead to effective incorporation of new technological possibilities into the order of battle and mobilization. The Prussian army made intelligent use of the General Staff as a learning organization; the French had no comparable organization.

Military failure is perhaps best viewed as a type of organizational failure as well. Elliot Cohen and John Gooch offer a clear analytical basis for trying to understand the military disaster of the Franco-Prussian War (Cohen and Gooch, 1990). Bad generals can cause military disasters; but Cohen and Gooch take the position that "human error" is an explanation we turn to too quickly when it comes to large failures. (Likewise, "pilot error" and "surgeon error" are too superficial in aviation and hospital failures.) Rather, it is important to look for the systemic and organizational causes of failure. They treat war as a complex organizational activity, and they attempt to discover the causes of military failures in a variety of kinds of organizational failure. They identify three basic kinds of failure: "failure to learn, failure to anticipate, and failure to adapt" (Cohen and Gooch, 1990, p. 26). And when these kinds of failure compound in a single period, it is likely enough that the result will be catastrophic failure.

Cohen and Gooch offer a "matrix of failure", partitioning "command level" (from president down to operating units) and "critical task" (communication of warning, appropriate level of alert, coordination) (p. 55); and they demonstrate how mistakes at various levels of command in the several critical tasks can cascade into "critical failures". The cases they analyze include the failure of American antisubmarine warfare, 1942; Israel Defense Forces on the Suez Front and the Golan Heights, 1973; the British at Gallipoli, 1915; the defeat of the American Eighth Army in Korea, 1950; and the French army and airforce, 1940.

It seems that the Cohen-Gooch framework can be usefully applied to the Franco-Prussian War. Each of the key failures occurred: failure to anticipate (especially, failure to anticipate the possible consequences of Prussia's rapid military modernization in the 1850s and 1860s; failure to anticipate the fatal consequences that would follow from the French declaration of war in July 1870); failure to learn (an almost total lack of ability on the part of the French general staff to make sense of the causes of defeat as they occurred in summer and fall 1870); and, most strikingly, a failure to adapt (essentially the same tactics were used at Sedan as had first been applied at Wissembourg; Howard, pp. 204–208).

Emile Zola's treatment of the war, *The Debacle: 1870–1871*, is not a piece of analytical history. Instead, it is a brilliant novelist's best effort to capture the horror and hopelessness of the campaigning in the summer and fall of 1870 from the point of view of the peasant, Jean Macquart. The confusion of endless marches in one direction and then the reverse; the misery of driving rain; the hunger of poorly provisioned campaigning; and the seemingly endless terror of artillery and rifle fire put the reader into the shoes of the foot soldier as he approaches his fate. The novel presents a textured and grim picture of the confusion of the march and the terrors of the battlefield:

> In Remilly there was a dreadful mix-up of men, horses, and vehicles jamming the street which zigzags down the hill to the Meuse. Half way down, in front of the church, some guns had got their wheels locked together and could not be moved in spite of much swearing and banging. At the bottom of the hill, where the Emmane roars down a fall, there was a huge queue of broken-down vans blocking the road, while an ever-growing wave of soldiers was struggling at the Croix de Malte inn (pp. 139–140)

And here, the fateful trap of Sedan, where the larger part of the French army was annihilated:

> The hundred thousand men and five hundred cannon of the French army were there packed together and hounded into this triangle. And when the King of Prussia turned westwards he saw another plain, that of Donchery, empty fields extending to Briancourt, Marancourt and Vrigne-aux-Bois, a waste of grey earth, powdery-looking under the blue sky, and when he turned to the east there was yet again, opposite the huddled French lines, an immense vista, a crowd of villages. . . . In all directions the land belonged to him, he could move at will the two hundred and fifty thousand men and the eight hundred guns of his armies, he could take in with one sweeping look their invading march. (p. 197)

It is an interesting question to ask: to what extent do the skills of the novelist complement the theories of the social scientist and the narratives and analysis of the historian, in helping us to come to a better understanding of the reality of the historical moment? Is Zola's novel a genuine addition to our ability to make sense of this period in France's history? Or is it simply fiction?

6.7 Technology and Culture

Technology is sometimes thought of as a domain with a logic of its own—an inevitable trend towards the development of the most efficient artifacts, given the potential represented by a novel scientific or technical insight. The most important shift that has occurred in the ways in which historians conceptualize the history of technology in the past 30 years is the clear recognition that technology is a social product, all the way down. And, as a corollary, historians of technology have increasingly come to recognize the deep contingency that characterizes the development of specific instances or families of technologies.

As we saw above, Thomas Hughes is one of the most important and prolific historians of technology of his generation. His most recent book, *Human-Built*

World: How to Think about Technology and Culture (Hughes, 2004), looks at technology from a very broad perspective and asks how this dimension of civilization has affected our cultures in the past two centuries. The twentieth-century city, for example, could not have existed without the inventions of electricity, steel buildings, elevators, railroads, and modern waste-treatment technologies. So technology "created" the modern city. (Or rather, technology made the modern city possible.) But it is also clear that life in the twentieth-century city was transformative for the several generations of rural people who migrated to them. And the literature, art, values, and social consciousness of people in the twentieth century have surely been affected by these new technology systems. So technology is a profound historical cause.

This level of analysis stands at the most generic perspective: how does technology influence culture? And how does culture influence technology? What Hughes demonstrates in so much of his work, though, is the fact that the most interesting questions about the "technology-society" interface can be framed at a much more disaggregated level. Recall some of the connections he suggests in his earlier book on the history of electric power (Hughes, 1983):

- Invention (by individuals with a very specific educational and cultural background)
- Concrete development of the artifacts within a laboratory (involving specific social relationships among various experts and workers)
- "Selling" the innovation to municipal authorities (for lighting and traction) and to industrial capitalists (for power)
- Finding investors and sources of finance for large capital investments in electricity
- Building out the infrastructure for delivery of electric power
- Government regulation of industry practices
- Development of an extended research capability addressing technology problems

Each part of this complex story involves processes that are highly contingent and highly intertwined with social, economic, and political relationships. And the ultimate shape of the technology is the result of decisions and pressures exerted throughout the web of relationships through which the technology took shape. But here is an important point: there is no moment in this story where it is possible to put "technology" on one side and "social context" on the other. Instead, the technology and the society develop together.

Hughes also explores some of the ways in which the culture of the machine has influenced architecture, art, and literature. He discusses photography by Charles Sheeler (whose famous series on the Ford Rouge plant defined an industrial aesthetic), artists Carl Grossberg and Marcel Duchamp, and architects such as Peter Behren. The central theme here is the idea that industrial-technological developments caused significant cultural change in Europe and America. Hughes's examples are mostly drawn from "high" culture; but historians of popular culture too have focused on the impact of technologies such as the railroad, the automobile, or the cigarette on American popular culture. See Deborah Clarke's *Driving Women: Fiction and Automobile Culture in Twentieth-Century America* for a discussion of the effect of automotive culture (Clarke, 2007).

Hughes does not consider here the other direction of influence that is possible between culture and technology: how prevailing aesthetic and cultural preferences influence the development of a technology. This has been an important theme in the line of interpretation referred to as the "social construction of technology" (SCOT). Wiebe Bijker makes the case for the social construction of mundane technologies such as bicycles in *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change* (Bijker, 1997; Bijker et al., 1987). And automobile historian Gijs Moms argues in *The Electric Vehicle: Technology and Expectations in the Automobile Age* (Mom, 2004) that the choice between electric and internal combustion vehicles in the early twentieth century turned on aesthetic and lifestyle preferences rather than technical or economic efficiency. This too is a more disaggregated approach to the question. It proceeds on the idea that we can learn a great deal by examining the "micro" processes in culture and society that influence the development of a technology. Technology is not an independent "driver" of history, but is rather itself a historical product densely interwoven with other social and cultural processes.

6.8 Observations from the Examples

The examples presented here are rich in numerous dimensions. Here I will draw out several central observations, as components of a historiography for "meso-history." Most importantly, Sabel and Zeitlin demonstrate that there were multiple feasible modes of economic organization involving different configurations of labor, capital, machinery, tools, product design, and business organization. So the course of western economic development was fundamentally contingent: it could have taken a variety of substantially different branches, consistent with economic, demographic, and political realities. Sabel and Zeitlin demonstrate that the stylized assumption that modernization entails mass manufacture, rigidly specialized machines and tools, and de-skilled labor is incorrect. It is therefore crucial for historians to resist the impulse toward an expectation of unique outcomes. More generally, this case alerts us to the significant degree of choice that exists at every historical moment. Agents choose among multiple feasible strategies, and competing strategies may co-exist for long periods of time. This means that the large-scale outcome is underdetermined by the structural configuration in place at a given time. At the same time, however, Sabel and Zeitlin demonstrate the significant power for constraining and impelling the directions that social change may take that is exerted by existing institutions. Available systems of finance and insurance influence the choices that manufacturers make about maintenance (Reynard, 1999); the political imperative of constraining naval costs impelled the early modern British Admiralty to adopt new architectural approaches to design and construction of ships of war (McGee, 1999); and the advent of the telegraph significantly altered the United States' ability to respond diplomatically to the Franco-Prussian War, in comparison to the equally serious French political crisis of 1848 (Nickles, 1999). The point of flexibility, then, is not that there are no powerful structural influences on the course of history at a given moment; it is rather that these forces are not ultimately determinative of the outcomes. But good explanation will unavoidably need to provide nuanced and theoretically informed analysis of these forces.

Thomas Hughes takes the point about the plasticity of history's course a step further by demonstrating the sensitivity of the course of technology development to the social and political environment. Technological possibilities and constraints do not by themselves determine historical outcomes—even the narrow case of a particular course of the development of a particular cluster of technologies. The technical and scientific setting of a particular invention serves to constrain but not to determine the ultimate course of development that the invention takes. A broad range of technical outcomes are accessible in the medium term. In place of a technological determinism, however, Hughes argues for technological momentum. Once a technology/social system is embodied on the ground, other paths of development are significantly more difficult to reach. Thus there are technological imperatives once a new set of technical possibilities come on the scene; but the development of these possibilities is sensitive to non-technical environmental influences (e.g. the scope of local political jurisdiction, as we saw in the comparison of British, French, and American electric power systems).[4] Further, however, Hughes's work illustrates the very significant gains that come from a micro-level study of the development of important social constructs such as electricity as a power source. By studying the laboratories, universities, banks, city councils, and legislatures through which the electric grid was created in the United States, we are in a very good position to observe both contingent and predictable aspects of the course of development of the technology.

Cronon and Dobbin illustrate a different aspect of the role that technology systems play within historical development: the degree of relative constraint that they create for future historical developments. This is a positive feature of meso-historical explanation: we can explain quite a bit of the changing pattern of economic geography of the upper Midwest by considering in detail the opportunities and constraints that were created by rail connections to the hinterlands and to the major population centers in the East.

Mark Elvin's analysis of China's environmental history illustrates another set of important observations about historical change. One has to do with the dialectic of human activity and natural resources. The availability of resources stimulates various kinds of activity; and these activities in turn influence the future availability of resources. Second, there is an intriguing feature of temporality in Elvin's account: the timeframe of action of farmers, officials, and emperors is different from the

[4]Essays in *Does Technology Drive History*? Shed important new light on the topic of technological determinism (Smith and Marx, 1994).

timescale of the effects that human actions bring about. So the century-long struggle between officials and the Yellow River is one that goes beyond the capacity of any particular official to perceive.

These insights suggest a series of negative maxims as well—historiographic blunders that large-scale history ought to avoid:

- Avoid single-factor explanations (e.g. technological determinism; Wittfogel and hydraulic despotism).
- Be suspicious of grand schemes of paradigmatic historical development (e.g. capitalist development, typical population transition).
- Be cautious in applying uncritically the paradigms and patterns of the European experience to other historical experiences (capitalism, the modernizing state).
- Recognize that historical junctures generally present a range of possible outcomes, depending on the choices of actors; so avoid explanations that impute "historical inevitability" to a particular outcome.[5]

Finally, it would appear that the conceptual framework of "assemblages theory" may be useful in discussing the history of technology and the role of technology in large historical processes (Latour, 2005). (See Manuel DeLanda's *A New Philosophy of Society: Assemblage Theory and Social Complexity* for a review of the theory; De Landa, 2006.) The framework is useful because technology is a social phenomenon that extends from one's own kitchen and household to the cities of Chicago or Berlin, to the global Internet and the international system of manufacturing and design. And similar processes of shaping and conditioning occur at the micro, meso, and macro levels. In other words, perhaps we can understand "technology" at the molar level, as a complex composition of activities and processes at many levels closer to the socially constructed individual. And the novelty provided by the sociology and history of technology is precisely this: to shed light on the mechanisms at work at all levels that have an influence on the aggregate direction and shape of the resulting technology.

[5]For a recent and powerful case for the contingency of a great event of the twentieth century, see Niall Ferguson's analysis of the origins of World War I (Ferguson, 1999b).

Chapter 7
Economic History

The history of a region or people encompasses a multitude of aspects of social life: culture, religion, political institutions, social movements, environmental change, technology, population—and the circumstances and processes of economic change that the region undergoes. One does not need to be a reductionist in order to observe that the economic circumstances a society experiences, and the processes of change that these circumstances undergo, have a profound influence on other aspects of social and cultural change. Improved agricultural productivity can support population growth; it can enhance the coercive power of state institutions; and it can make possible the flourishing of intricate institutions of religion and education. Likewise, the constraints created by slow or negative economic productivity growth in a region can stifle the development of other important social processes. So economic history, as a discipline within history more broadly, is a crucially important field of historical inquiry.

Yet the foundations of the discipline of economic history are still the subject of controversy. Economic historians do not yet agree on the role of mathematical economic models within their discipline, or the relationships that should obtain between quantitative and qualitative data, or the role of social theories of causal factors in explaining economic change, or the connections that should be established between economic historical research and other fields of social or cultural history. This chapter is a contribution to discussion of some of those foundational topics, with special focus on the agricultural history of China.

7.1 What Is "Economic History"?

Let us begin by considering a foundational question: what is the intellectual task of an "economic history" of a region or country? To start, we might say that the task of the discipline of economic history is to provide an *evidence-based description of the main economic characteristics of the country or region over a defined period of time*: the kinds and levels of agricultural and manufacturing products that are produced, the technologies and institutions through which production and distribution occurs, the size of the population, and the level of material well-being that is

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experienced by the population. And, second, the task of economic history is to arrive at *causal hypotheses that may serve as explanations of some of the patterns of economic change that are discovered*. This charge can be further broken down into the activities of description, synthesis, and explanation.

An economic history demands temporal differentiation; we need to identify the processes of change over time that occur for the many variables of interest. Ideally we would want time-series estimates for each of these groups of variables. But we also need to have spatial differentiation of these variables, in order to capture the crucially important regional variations that are present in economic development in an economy as large and physiographically diverse as China's or Europe's. As G. William Skinner's work demonstrates, we need to break these data down in spatial terms. Regions differ across China, and data that is averaged over large regions give misleading impressions of the processes of development that were underway (Skinner, 1964, 1977b, 1985). And in fact, one of the most interesting implications of recent Chinese economic history is the discovery that the definition of the regions under comparison makes a great deal of difference. North and South China had profoundly different agrarian economies, and findings that are based on data from one region may be highly misleading when generalized to another region. Likewise, comparisons between England and the lower Yangzi Delta are more revealing than comparisons between Europe and China. Schematically, then, we can picture the empirical core of an economic history as a three-dimensional data space representing a set of variables as they change over time and place.

If we succeeded in reaching reasonable estimates of these dimensions of economic activity and change, we would have a very substantial basis for resolving the breadth of disagreement that currently exists among economic historians about larger questions of interpretation of causes and processes. This would permit the field to move from the situation of "basic paradigm" disagreement to one of "normal science" investigation of more particular issues.[1] In other words, it should be possible to resolve many basic disagreements within the field of economic history with additional empirical research (for example, debates over the standard of living or over growth versus involution in various countries). And in fact, it appears that research in the past 10 years has provided a substantially better empirical grasp of most of these key dimensions of the economic history of much of Europe and Asia, especially with regard to population, farm productivity, and the standard of living.

It should be observed that economic history is a good example of "large social science," in the sense that there is a large and extended research community of scholars who contribute different pieces of the overall body of knowledge. No single scholar can do original research on every aspect of a region's economic history. Instead, there are specialized groups of scholars who focus their research on different aspects and levels of the problem, and others who synthesize these findings

[1] The distinction between paradigm shifts and normal science is the core of the history of science developed by Thomas Kuhn (1970b). The distinction is developed primarily on the basis of study of the physical sciences, but it seems relevant in this area of historiographic disagreement as well.

into larger historical constructions. This division of labor is cross-generational and cross-national. Thus the careful farm studies conducted by John Lossing Buck in the early twentieth century continue to provide the basis of more comprehensive synthetic work on the economic history of rural China in the early twenty-first century. And the historical population studies conducted by teams of researchers in over a dozen settings across Eurasia in the Eurasian Population Project provide the basis for rigorous testing of such high-level hypotheses as the Malthusian theory of population behavior (Bengtsson et al., 2004). In each case the specialized studies are eventually brought into higher-level interpretations by other economic historians.

There is a substantial and permanent role within this definition of the field for sustained, detailed empirical and archival investigation. The data that permit the historian to reconstruct economic facts—prices, wages, consumption levels—are usually obscure and difficult to reconstruct. And there is almost invariably a substantial degree of variation of economic activity and results throughout the given region. So the task of gathering and analyzing relevant evidence about economic phenomena and behavior is an intellectually challenging one. This work takes the form of estimating price movements, wage levels, demographic events, the timing and distribution of technological innovations, the rate and pattern of capital formation, patterns of trade, and so on; in other words, descriptive empirical work. Second, the economic historian is charged to identify salient patterns, whether of change or persistence. This might be called the work of synthesis, or the construction of more general and abstract narratives of the economic processes that were underway in the time and place under study. And third, the economic historian is asked to provide explanations of the patterns that the research tradition discovers. If an "agricultural revolution" occurs, if a prolonged period of technological stagnation takes place, if population trends change significantly—the economic historian is charged to attempt to arrive at an explanation of these large features of economic development. What are the background factors that caused the economic circumstance in question to occur?

7.1.1 Explanation in Economic History

What theoretical resources are available to the economic historian in support of explanations? It is evident that economic outcomes are the result of human behavior within the context of environmental circumstances and institutional settings. Social theories provide developed accounts of common social mechanisms—the processes and patterns that result from purposive human behavior within constraints (Chapter 5). Human behavior, however, is not rigidly segregated into "economic," "cultural," and "social" behavior; rather, behavioral outcomes are influenced by all these kinds of factors. So economic history cannot be restricted to the theories associated with neoclassical economics. Rather, the economic historian is obliged to examine the economic phenomena under study within the broader social and environmental context in which this behavior takes place. And that means that the economic historian must be as much a social historian, a sociologist, or an ethnographer as he is an economist; he needs to pay as much attention to the social and political context of economic trends as he does to the pure mathematics of equilibrium or the idealized workings of a market. This suggests that the economic historian needs to be mindful of the wide variety of social factors and causes that are potentially relevant in the explanation of economic outcomes.[2]

Explanation of patterns of economic development requires discovering causal relations among various sets of factors. Here the economic historian needs to have a deep understanding of a wide range of social theories and the social mechanisms that they describe; and he or she needs to be able to consider whether specific mechanisms are at work in specific historical circumstances. How did population pressure affect technological change? How did tenancy relations affect investment (and hence productivity)? How did tenancy relations affect average rural welfare? How did rural welfare levels affect population trends? If we are to arrive at explanations of the large historical processes that we discern in economic development, we need to consider a range of possible social mechanisms that might be shown to drive these processes. This is the function of social theories—and significantly, the role of theory is itself one of the central dimensions of disagreement among contemporary economic historians. Disagreements about social mechanisms are highly prominent across these debates. Consider a few of the major social mechanisms that have been invoked in recent debates:

- unregulated population growth, leading to unsustainable pressure on land and other resources (Chao, 1986; Elvin, 1973; Huang, 1990);
- competition and market institutions, leading to the spread of efficient techniques, the migration of labor, and the extension of trade (Brandt, 1989; Myers, 1970; Rawski, 1989);
- economic incentives at the level of the household or manager, leading to adjustment of techniques, inputs, and innovations so as to arrive at optimal distribution of labor and capital to produce outputs that maximize income and security (Huang, 1985; Schultz, 1964).
- sets of social property institutions that assign highly unequal bundles of powers, resources, and incomes to various participants including peasants, handicraft workers, landlords, merchants, or officials, leading to different economic strategies in different settings (Brenner, 1976; Lippit, 1987; Riskin, 1975);
- the economic effects of colonialism and empire in the form of cheap access to resources and labor (Frank, 1967; Pomeranz, 2000);

[2] Cliometrics, the school of thought that gave almost exclusive priority to economic modeling as a tool for economic history, has been convincingly criticized for its disregard of other historically important factors; Schabas (1995). Less single-minded economic historians from an earlier generation, such as Hicks (1969), Jones (1988), and Schumpeter (1947) offer examples of full historians who treat the history of economies in ways that give appropriate attention to the broad context of economic institutions and behavior. See Rawski (1996) for good recent discussions of the role of theory in economic history.

- the accident of the geographical distribution of mineral and energy resources (Goldstone, 1991; Pomeranz, 2000);
- processes of environmental change, both exogenous and endogenous (Elvin, 2004; Perdue, 1987)
- processes of warfare and militarization (Perdue, 2005; Tilly, 1990)
- cultural factors that influence economic behavior (Geertz, 1980; Lieberman, 2003)

This list could be extended, but the central point is this. These factors may or may not have causal significance in the economic history of particular regions, but each serves to identify a possible causal mechanism that has been invoked as a factor leading to economic change or stasis. Well-developed social theories give us a basis for demonstrating how various factors *could* be causally relevant. It is then the task of empirical, historical, and theoretical research to arrive at justified conclusions about causation.

In short, the task of explanation of a region's pattern of economic development depends on the work of social scientists who can identify some common middle-level processes that recur in different settings—economic behavior, family and reproductive behavior, incentives and opportunities presented to the wielders of monopoly coercive power. Crucial here is discovery of the specific institutional arrangements within which economic activity takes place. Explanation proceeds by showing how different institutional settings can lead these processes to significantly different outcomes. The general point is that institutions matter, and that institutional arrangements in different sectors may impose limits (or sometimes opportunities) that discourage or favor some pathways of development over others. Instead of an expectation of one grand course of development, we ought to expect a congeries of contingent, fluctuating path-dependent processes.

Moreover, it is entirely possible that different combinations of causal factors are of primary importance in different historical settings; historical change is conjunctural and contingent. (These themes are explored more fully in Chapter 5.) The general point is that institutions and circumstances matter, and that institutional arrangements in different times and places may impose limits or opportunities that discourage or favor some pathways of development over others. Instead of expecting one grand course of development, we ought to expect a congeries of contingent, fluctuating path-dependent processes. And it is the responsibility of the historian to use a variety of forms of inquiry and inference to assign credible weight to the various factors in different circumstances.

The "multi-threaded" character of economic phenomena also implies that the economic historian needs to be open to a variety of kinds of evidence that are pertinent to assessing economic circumstances. Marc Bloch's history of medieval French agriculture is a good illustration of the value of a broadly contextualized approach to a region's economic history. Bloch surveys the main social institutions and technologies that were in use, and attempts to explain some of the large patterns that became evident (for example, field shape and the geographical diffusion of the wheeled plough) (Bloch, 1966). Bloch's explanation invokes such varied factors as the nature of the soils across France, the availability and timing of technical innovations in the design of the plough, and the nature of village communities in different parts of France. And he makes use of an ingeniously wide range of historical sources to permit him to come to assessments of various economic and institutional facts.

7.1.2 Problems of Evidence

What problems stand in the way of achieving greater levels of consensus about the most basic characteristics of a region's economic history? It is evident, to start, that the available data concerning economic activities and volumes are inherently selective, biased, and error-prone. Historical population data are almost always incomplete, leading to a wide range of empirically possible estimates of population size and growth rates. Estimates of arable land, productivity, and cropping patterns are likewise available only through incomplete records—for example, local land tax records and surveys. So many of the basic economic parameters are only partially measurable through available archival data and observers' reports.

Further, it is clear that estimates of some of the important economic variables depend on estimates of others. For example, if we have good data on population size and trends, income distribution, and absolute levels of output, then we can construct credible estimates of real welfare. If we have good estimates of real wages then (making neo-classical economic assumptions) we can estimate labor productivity. If we have good estimates of the absolute amount of grain consumption and if we make assumptions about the direction and rate of change of consumption then we can make an estimate of demographic variables. And so on for various sets of these variables.

There are other epistemic problems that confront economic historians besides paucity and unreliability of data. An issue that divides much of the work in this field is the question of the relative importance of qualitative and quantitative data. Thomas Rawski (1989) and Loren Brandt (1989) make an appeal for investigation solely on the basis of available quantitative data—grain prices, maritime records detailing quantities of grain transported, official population data. On the other hand, as a historian, Philip Huang (Huang, 1985) pays substantial attention to a variety of non-quantitative sources: criminal case archives, the Mantetsu studies, the observations of rural welfare by interested observers. Does one data source have strict priority over the other, as Rawski and Brandt maintain; or is it mandatory for the historian to cross-check the inferential conclusions his or her quantitative analysis produces on the basis of qualitative data?

A related issue concerns the need for sociological detail in economic history. As Kathleen Hartford notes (Hartford, 1992), there is disagreement among contemporary historians about the relative autonomy of economic processes and economic behavior. Rawski and Brandt essentially assume that political and social institutions matter little to economic change. Huang believes, and Hartford emphasizes, that the specifics of institutions and political culture have a great deal of effect on the results:

whereas commercialization in the institutional context of eighteenth century English agriculture led to managerial agriculture, within the context of the normative and social situation of the lower Yangzi it led to more intensive family farming. And so we are faced with another important methodological question for the economic historian: how much institutional detail and sociological or even ethnographic detail is needed in order to account for processes of economic development in China?

Finally, we may question the validity of the theoretical frameworks and assumptions made by various authors. Quantitative analysis requires economic theory, on a more or less grand scale; in order to arrive at an estimate of labor productivity it is necessary to employ a set of assumptions that permit us to make inferences from a given set of data about population size, land acreage and output to conclusions about labor productivity. But what assumptions should we use? Rawski and Brandt are neoclassical economists. Their analysis depends on the equilibrium conditions of competitive markets. How well or poorly, however, do such assumptions fit the circumstances of the Chinese rural economy? Huang embraces no single economic theory; but he comes closest to a neo-Marxist theory, emphasizing surplus extraction and power rather than freely competitive markets. For all these theories, we need to ask the foundational question: how well do these theoretical assumptions fit the terrain of late-imperial China?

7.2 Aspects of China's Rural Economy

Consider a limited number of features of rural life that define the research tasks for China's economic history today. Abstractly, it is clear that an economic history of rural China should address several crucial sets of issues, and it is plausible to believe that additional empirical and theoretical research can narrow the range of disagreement about these factors. So let us begin by identifying several core dimensions of empirical and theoretical disagreement about China's economic history.

- *Demography.* What was the absolute population size and distribution at various time points during the period? What were the trends of population growth during the period? How much urbanization occurred during the period?
- *Inputs and technology.* How much land was under cultivation? What crops and products were in production? What fertilizer technologies were in use? How much irrigation was available, and what was the trend of extension of land and irrigation?
- *Property relations and control of labor.* What forms of tenancy and land ownership were in place? How were these arrangements changing during the time period? What forms of labor control were in use? Was there a tendency of change in the conditions and extent of wage labor?
- *Productivity.* What was the absolute size of the production of central commodities—rice, wheat, cotton? What were the factor productivities for land, labor, capital, or animal power? What trends existed in these quantities?

- *Prices and market conditions.* How much agricultural activity took place within functioning markets for crops, grain, textiles, and handicraft goods? What were the prices of these goods over time? How sensitive were farmers to changing market conditions?
- *Human welfare.* What were the income levels and food security of various groups: landless workers, smallholding peasants, tenants and other groups? How extensive were income inequalities within the economy? Where were economic surpluses going? What was the trend of real welfare and inequalities?
- *Causal factors.* What are the causal relationships that obtain between various large factors: technology, social relations, property systems, state, demographic regimes, and international relations?

It is evident that there are relationships among these dimensions. For example, we can make inferences about the standard of living if we have confident estimates of crop production, population size, and surplus distribution institutions. Equally, it is evident that there are different types of historical data that permit us to arrive at estimates of these several dimensions of economic history. For example, there are qualitative and forensic sources (gazetteers' and travelers' reports, skeletal measurements) that permit assessment of the standard of living that may provide evidence that either supports or contradicts the estimates that are produced on the basis of estimation of population, food output, and prices (Allen et al., 2005). This variety of kinds of evidence permits us to test the validity of some of our conclusions on the basis of the estimates produced of related variables by a different method.

7.2.1 Agricultural History

Economic history generally attempts to discover fundamental features of social organization and natural environment that impose constraints and imperatives on the rest of society. Farming is fundamental in this respect; the material basis of production is particularly visible, and the products of the farm system are fundamental to the survival of society. The standard of living of a traditional society is largely determined by agricultural productivity and the nature of the farming system—nutrition, for example, is essentially determined by the ratio of total grain output to population. Finally, virtually all traditional economies are primarily rural; so farm life defines the conditions of ordinary social existence for the majority of the population. So let us consider a brief analysis of the farm. (A. V. Chayanov's classic treatment of the peasant economy, *The Theory of Peasant Economy*, written in the 1920s, remains highly valuable (Chayanov, 1986). Robert Allen's lifetime of research on the English farm economy is highly insightful (Allen, 1992), as is Bozhong Li's work on the farm economy of the Lower Yangzi (Li, 1998).)

A farm is the basic unit of agricultural production. It represents the coordinated application of diverse factors of production in order to produce crops. The factors of production include labor; land; tools, implements, and machinery; fertilizers;

and water resources and irrigation techniques. Crops include both foods (e.g., rice, wheat, millet) and raw materials (e.g., cotton, soya beans). And farms may be organized for a variety of purposes: to satisfy a family's subsistence needs, to create a profit within a market system, or to provide employment for the greatest number of rural people.

Farms in different agrarian regimes may be characterized in terms of a set of technical and social features. On the technical side we need to know what the scale of cultivation is (farm size); what techniques of cultivation are employed; what varieties of crops and seeds are available; what types of farm tools and machines are used; what types of irrigation, if any, are in use; what varieties and skills of labor are employed; what types of fertilizers are used; and what forms of agronomic knowledge are available to the farmer. (We might reduce this variety of technical features to a spectrum ranging from low-technology to high-technology farming systems.) On the social side we need to know the purpose of cultivation (family subsistence or commercial profitability); the form of land tenure in place (fixed rent, sharecropping, smallholding, etc.); the forms of labor employed (slave, serf, family labor, hired labor); the forms of supervision employed; and the processes of income distribution embodied in the agricultural system.

The peasant farm represents a form of organization of the productive forces of rural society that is fundamental in many developing societies. The peasant farming system may be defined as a system in which agriculture is performed on peasant farms; the bulk of the population consists of free peasant cultivators; and agricultural surpluses are extracted through rent, interest, and taxation. A great deal of direct economic historical research has to do with efforts to discover the main characteristics of the farm economy in historical settings.

These features are the primary subject of research for agricultural historians such as Robert Allen and Bozhong Li. These features essentially determine the most important economic characteristics of the agricultural system. First, they determine the productivity of the farming system, whether measured in terms of land efficiency (output per hectare) or labor efficiency (output per man-day). For once we know the techniques of cultivation in a given ecological setting, it is possible to form relatively accurate estimates of output for a given input of land, labor, and capital. This set of considerations also determines what we might describe as the net rural product—the total agricultural product over and above the replacement cost of the factors of production. On this basis, Chinese historians such as Dwight Perkins attempt to estimate the overall wealth and income of late Imperial China, including estimates of quality of life for the majority of rural people (Perkins, 1969).

Second, the full description of the farming system will allow us to infer the system of surplus extraction in place; it will be possible to indicate with adequate precision how much surplus is generated and where it goes. Victor Lippit attempts to arrive at such an estimate for traditional China (Lippit, 1974). This in turn permits us to describe the system of rural class relations that correspond to a given farming regime. The agricultural surplus is taken in the form of rent, interest, and taxes; and the distribution of the surplus is a basic determinant of material interests across social groups in a developing society.

Dwight Perkins' *Agricultural Development in China* (Perkins, 1969) stands out in Chinese economic history for its effort to establish a baseline set of estimates of agricultural productivity, output, and social organization in China over a six-century period. Perkins tries to answer some of the fundamental factual questions about the basics of Chinese rural economic history: what were the patterns of population growth, growth of cultivated land, and growth of net output, in traditional China? How did the agricultural system serve to support the food needs of the population, and what were the chief tendencies of change that were visible over several centuries?

Perkins' study plays the role in China studies that Deane and Cole plays in English studies (Deane and Cole, 1967). Perkins attempts to provide estimates of population growth, cultivated land, and grain output for a period from the late fourteenth century through the mid-twentieth century. Perkins' central thesis is that China's population increased five- or six-fold between the late fourteenth and early nineteenth centuries, and that the agricultural system was able to keep pace with this increase in equal measure by expanding cultivated acreage and by raising the yield per acre (1969, p. 13).

Bozhong Li considers the question of technological change with respect to the agriculture of the lower Yangzi Delta in *Agricultural Development in Jiangnan* (Li, 1998); see also his contribution to *Living Standards in the Past* (Allen et al., 2005). And since this was the most important agricultural region in China for centuries, Li's findings are important. Li makes an important point about technological innovation by distinguishing between invention and dissemination. An important innovation may be discovered in one time period but only adopted and disseminated over a wide territory much later. And the economic effects of the innovation only take hold when there is broad dissemination. This was true for Chinese agriculture during the Ming period, according to Li:

> The revolutionary advance in Jiangnan rice agriculture technology appeared in the late Tang and led to the emergence and development of intensive agriculture composed of double-cropping rice and wheat. But this kind of intensive agriculture in pre-Ming times was largely limited to the high-fields of western Jiangnan. In the Ming this pattern developed into what Kitada has called the 'new double-cropping system' and spread throughout Jiangnan, but only in the late Ming did it become a leading crop regime. Similar were the development and spread of mulberry and cotton farming technologies, though they were limited to particular areas and cotton technology's advances came later because cotton was introduced later. Each had its major advances in the Ming. Therefore, technology advances in Ming Jiangnan agriculture were certainly not inferior to those of Song times which are looked at as a period of "farming revolution". (p. 40)

Li also finds that there was a significant increase in the number of crop varieties in the early Qing—another indication of technological development. He observes, "The later the date, the greater the number of varieties. For example, in the two prefectures of Suzhou and Changzhou, 46 varieties were found in the Song, but the number rose to 118 in the Ming and 259 in the Qing" (p. 40). And this proliferation of varieties permitted farmers to adjust their crop to local soil, water, and climate conditions—thus increasing the output of the crop per unit of land. Moreover, formal knowledge of the properties of the main varieties increased from Ming to Qing periods; "By the mid-Qing, the concept of 'early' rice had become clear and exact, and knowledge of 'intermediate' and 'late' strains had also deepened" (p. 42). This knowledge is important, because it indicates an ability to codify the match between the variety to the local farming environment.

Another important process of technology change in agriculture had to do with fertilizer use. Here again Li finds that there was significant enhancement, discovery, and dissemination of new uses of fertilizer in the Ming-Qing period.

> A great advance in fertilizer use took place in Jiangnan during the early and mid-Qing, an advance so significant that it can be called a "fertilizer revolution". The advance included three aspects: (a) an improvement in fertilizer application techniques, centering on the use of top dressing; (b) progress in the processing of traditional fertilizer; and (c) an introduction of a new kind of fertilizer, oilcake. Although all three advances began to appear in the Ming, they were not widespread until the Qing. (p. 46)

And the discovery of oilcake was very important to the increases in land productivity that Qing agriculture witnessed—thus permitting a constant or slightly rising standard of living during a period of some population increase.

There were also advances in the use of water resources. Raising fish in ponds, for example, became an important farming activity in the late Ming period, and pond fish became a widely commercialized product in the Qing. Li describes large-scale fishing operations in Lake Tai in Jiangnan using large fishing boats with six masts to catch and transport the fish (p. 62).

So Li's estimate of agricultural technology during the Ming period is that it was *not* stagnant; rather, there was significant diffusion of new crops, rotation systems, and fertilizers that led to significant increases in agricultural product during the period. "In sum, in the Jiangnan plain, land and water resources were used more rationally and fully in the early and mid-Qing than they had been in the late Ming" (p. 64).

Two points emerge from this discussion. First, Li's account does in fact succeed in documenting a variety of knowledge-based changes in agricultural practices and techniques that led to rising productivity during the Ming-Qing period in Jiangnan. So the stereotype of "stagnant Chinese technology" does not serve us well. Second, though, what Li does not find is what we might call "science-based" technology change: for example, the discovery of chemical fertilizer, controlled experiments in rice breeding, or the use of machinery in irrigation. The innovations that he describes appear to be a combination of local adaptation and diffusion of discoveries across a broad territory.

7.2.2 Assessment of Available Sources of Data

What kinds of historical and empirical data provide a basis for these sorts of estimates? For the pre-twentieth century period, Perkins' findings are largely based on primary research: Ming-Qing tax records on population and cultivated acreage, local gazetteers, agricultural handbooks published over past centuries, memorials by local officials to the Emperor, and so forth. Perkins also refers to a large volume of Chinese and Japanese research on the agrarian history of China. The local gazettes provide a great deal of information about the timing and location of markets; commodity prices; land tenure arrangements; and the activities of local elites.

One of the central challenges of agricultural history is to assess and evaluate the empirical data that happen to survive. One important method that is available to the researcher is to assess the consistency of one set of estimates with another. Perkins refers to some of the ways in which he attempts to check the validity of his sources: "I have, in fact, frequently judged the validity of data for the decades and centuries prior to the 1950's on whether these earlier figures were consistent with those for 1957 and with historical developments in the intervening periods" (Perkins, 1969, p. 10). And Perkins attempts to assess the consistency of his estimates of population and acreage: "If the pre-modern estimates of provincial population and acreage had been arrived at by arbitrary methods, one would expect yield data derived from such figures to rise in certain periods and fall in others with no apparent pattern. . .. But most of the estimates in Table II.3 for 1850 bear a close relation to the 1957 figures" (p. 20).

But it is glaringly obvious that there are very considerable problems of data in recent efforts to measure China's historical economic performance. This is a problem that confronts all research in economic history—witness the "standard of living" debate in English economic history of the 1960s (usefully analyzed in retrospect in Crafts, 1985). But the problems are particularly severe in the case of China. Ideally we want estimates of a variety of common economic variables: population size over time, urbanization rate, amount of land under cultivation, average crop yields, prevalence of various agricultural techniques and inputs, level of output of various export goods (cotton, silk), amount of commercialization, amount of transport of commercial goods, prevalence of landlessness, tenancy rates and rent levels, wages for various kinds of employment, productivity in various sectors of the economy. But these data are hard to come by; in practice there are only a few data sources that scholars have relied on to tease out into estimates of economic trends.

Consider some of the main data sources on which Chinese economic history depends. Population and production data are crucial. In some cases the imperial state itself collected relevant data (generally for tax purposes), and these sources are used extensively by all contemporary historians. But (as Joseph Esherick shows convincingly; Esherick, 1981) these data can be severely skewed as a result of strategic under- or over-reporting of data. Moreover, as Skinner shows in his analysis of Sichuan's population data, there are often internal inconsistencies that cast very profound doubt on the validity of official population and production data (Skinner, 1987). (For a comprehensive review of the state of Chinese demography see Lavely et al., 1990.)

A second major source of data, employed by Perkins as well as almost all contemporary Chinese historians, are the agricultural surveys undertaken by John Lossing Buck and his research teams in the 1930s. The Buck surveys collected a wide range of information: land usage, wage rates, tenancy rates, cropping patterns and so on. Here again, however, there are substantial problems of bias and completeness of coverage; Buck's surveys of land concentration, for example, excluded absentee landlords, thus underestimating land concentration (Esherick, 1981).

A third data source that has been extensively employed by several authors (including especially Philip Huang and Ramon Myers) are the Mantetsu surveys performed in North China by the Japanese South Manchurian Railway Company in the 1930s. These surveys offer village- and household-level surveys of economic activity in North China. They provide very extensive detail on the organization of the local economy in a variety of villages. Their limitations, however, are severe as well: only a relative handful of villages are studied (raising questions about the representativeness of the data); and the surveys are done at the behest of a conquering army—raising a different set of questions about bias of investigation and response. Joshua Fogel (1987) provides a useful description and assessment of the Mantetsu surveys.

The methodological problem, then, is this: for virtually none of the key economic variables describing rural economic development are the data sources fully adequate and fully credible. Instead, economic historians are unavoidably forced to combine sketchy and problematic data, assumptions about trends they cannot directly observe and assumptions about the economic system that derive from economic theory (e.g. the assumption that the real wage of unskilled labor is equal to the marginal product of unskilled labor), in order to arrive at estimates of other economic variables. The point to be emphasized here is not that the resulting estimates cannot be taken seriously. It is rather that the reader needs to follow the economic historian's reasoning very carefully, in order to arrive at an assessment of the overall credibility of a given claim.

7.3 Population History

Agricultural history goes hand in hand with population history. The productivity of agriculture imposes a fundamental constraint on population size; and in order to estimate the standard of living of a historical population, we need to be able to estimate both productivity and size. So historical demography is a key partner to historical studies of economic development.

Historical demography concerns itself with two families of questions—factual description of population behavior and explanation of the patterns that are observed. On the descriptive side, historical demographers are concerned with estimating the absolute population size of a group or region—Tuscany in 1400, New England in 1700, the Anasazi in 1000. And they are interested in measuring the underlying demographic rates—for example, natality, mortality, age of marriage, nutritional and health status, or sex ratios.

And, to make useful comparisons, these measurements need to be linked to the same variables over time and comparable populations over the same time period. This set of observations permits the discovery of trends and patterns—steady population growth in group A simultaneous with flat population size in group B; high rate of growth for C during 1700–1750 followed by slow growth in the next 50 years; high population density in agricultural region D compared with low density in agricultural region E.

The descriptive task is a difficult one. But it immediately raises a host of explanatory questions. What are the social mechanisms that conjoin to bring about the complex observed demographic outcomes? Population outcomes are the result of myriad choices by anonymous individuals; and yet the outcomes are highly patterned. The task of explanation arises when we try to make sense of the demographic behavior and outcomes that are discovered. What are the material and social circumstances that explain the trends we observe and the differences that emerge across populations?

Thomas Malthus (1798) offered an explanation of large population trends based on both material and social factors: the food-production capacity of the land inhabited and the norms and institutions regulating family formation and child-bearing. The central idea is that population tends to increase more rapidly than agricultural capacity—with the result that population pressure tends to push the standard of living to or below the subsistence threshold unless there are positive checks (leading to excess mortality) or preventive checks (leading to reduction in the fertility rate). Positive checks include famine and war; preventive checks include delayed marriage, prolonged lactation, and traditional forms of birth control. Malthus's theories suggest relating population size and growth rates to measures of economic prosperity—for example, measures of the real wage for farm laborers over time, measures of the fluctuations in availability of jobs, or measures of available land per unit of population. The classic Malthusian argument predicts that population growth will lag economic fluctuation: rising wages encourage higher fertility and falling wages discourage fertility. And if population continues to rise in declining economic circumstances, then eventually mortality will rise—again suppressing population growth.

The simple Malthusian theory, then, postulates that population expands to the limit created by available sources of food. So increases in farm productivity and increases in arable land should give rise to an increase in population, whereas a rising population density on a given territory will eventually lead to the positive checks of famine and nutrition-based mortality if not pre-empted by the preventive checks of reduced natality.

The question of scale, both temporal and regional, makes a large difference in the explanatory quest. We can frame the question of group demographic behavior from the micro- to the macro-levels—from a handful of Alpine villages in the sixteenth century (population 5,000) to the Yangzi delta in the eighteenth century (population 31 million). Micro-trends may have a different explanations than macro-trends. (This is a point that is well explored in the multilevel analytical framework for demographic processes offered by Daniel Courgeau, Robert Franck, and others; Courgeau, 2003, 2007.)

It might be maintained that the Malthusian theory is only well-suited to the largest scale: large territory and long timeframe. In the longterm it is almost tautological to observe that a population will expand to the limits created by environmental resources. But long before those limits are approached, there is ample space for the workings of social arrangements to modulate population behavior. Alternative economic and demographic institutions create different demographic profiles. Birth spacing (reflecting family choices and cultural values) can dramatically influence total fertility. Technologies of birth control—both traditional and modern—can move a population from high-fertility to medium- or low-fertility. So historical demography requires social explanation based on discovery of concrete and specific institutions in local circumstances, not simply broad-brush hypotheses about resource limits. And the choice of levels of analysis is crucial.

Recent studies of population behavior across the expanse of Eurasia confirm this point. The Eurasia Project on Population and Family History finds that there is a wide range of demographic patterns across the locales of Eurasia, from Finland to Italy to China to Japan. And the Malthusian picture works only at the coarsest level; in particular, it turns out that the Chinese population did not teeter on the edge of Malthusian crisis.

7.3.1 The Eurasia Project on Population and Family History

The Eurasia Project on Population and Family History offers an important example of comparative historical research across Eurasia (Bengtsson et al., 2004). Lead scholars in this project include James Lee, Cameron Campbell, Tommy Bengtsson, and George Alter, as well as many others. The project is an approach to historical demography that is intended to provide a substantially more detailed understanding of the historical trajectory of demography in different parts of the Eurasian land mass—population size, nuptiality, fertility, mortality, etc., as well as a more empirically constrained set of hypotheses about the causes of changes in these factors over time (Allen et al., 2005; Bengtsson and Mineau, 2008; Bengtsson et al., 2004). This research represents an important example of collaborative "large science" in the historical social sciences, in that it involves research teams at a number of centers in Europe, Asia, and the United States who have agreed on a set of standards on the basis of which to represent and analyze the demographic data they have collected. In the aggregate, the study makes use of 2.5 million longitudinally linked individual records in over a dozen locations across Eurasia. Research teams are focusing on detailed demographic records in community-sized locations across Eurasia, including Sweden, Belgium, Italy, Japan, and China, and these researchers aim to provide analysis of their findings that is sufficiently structured as to permit rigorous comparison across cases.

The researchers describe the project in these terms: "New data and new methods ... have begun to illuminate the complexities of demographic responses to exogenous stress, economic and otherwise.... Combined time-series and event-history analyses of longitudinal, nominative, microlevel data now allow for the finely grained differentiation of mortality, fertility, and other demographic responses by social class, household context, and other dimensions at the individual level"

(Bengtsson et al., 2004, pp. viii–ix). Their goal is an ambitious one; it is to provide detailed, analytically sophisticated multi-generational studies of a number of populations across Eurasia. The studies are intended to permit the researchers to probe issues of causation as well as to identify important dimensions of similarity and difference across regions and communities.

A key to this approach is to have assembled comparable micro-level longitudinal demographic data in different geographic locations across Europe and Asia. "[Our study] . . . is an international comparison of short-term mortality responses at the microlevel in past times. . . . It examines not just one people, but a variety of micro-populations in West Europe and East Asia; not just one period, but a period of 80–150 years" (Bengtsson et al., 2004, p. 4). The research groups have thus attempted to arrive at a micro-picture of the individual-level demographic events with individual, relationship, and household characteristics; to put together a timeline of "stress"; and to see how outcomes for people's demographic events behave in relationship to the patterns of change in environmental and economic characteristics. This allows them to probe the ways in which varying social and economic institutions—family structure, economic niche structure, state food subsidy practices—influence people in different places.

An important historiographic feature of this work is the decisions the researchers have made about the scale of groups to be studied. The work is not intended to tell the demographic story of the nations in which the communities under study exist, but rather to provide detailed snapshots of local populations over extended periods of time. Lee, Campbell, and Bengtsson write, "Our focus on comparison of communities, not countries, represents a new approach in social science history. While we generalize about certain aspects of human behavior, we do so within the cultural, economic, and historical contexts of specific communities, not at the national level" (Bengtsson et al., 2004, p. 7). The approach thus makes a novel contribution to the question of the scope or level of comparison. Rather than selecting continental-scale or national-scale comparisons, the authors have chosen to compare the demographic experience of relatively small regions across the face of Eurasia. This line of reasoning offers a novel answer to the important question posed above concerning the right "scale of comparison" that should be employed in comparative economic and demographic history.

This research is distinguished by several important characteristics. First, it attempts to combine aggregate and individual levels, building aggregate conclusions from analysis of individual-level data. Second, it is deeply comparative, in that these groups deliberately attempt to identify similarities and differences at both extremes of Eurasia and within Europe and Asia. Third, this research project examines the "large-scale processes and implications of the fertility and mortality transitions," and to do so in a way that permits comparison across regions. Finally, the research attempts to evaluate the large hypotheses (especially Malthusianism) concerning differences in demographic regimes between Europe and Asia. So the Eurasia Project on Population and Family History is both micro and macro, with a coherent method for joining the two levels; it is comparative; and it is intended to provide a basis for discovering causal relationships among factors that influence demographic outcomes. The multilevel methods of analysis developed by Courgeau and others are relevant here as well (Courgeau, 2003).

The Eurasia Project approach tends to downplay the "large" common institutions at the national level and to emphasize instead the local institutions that directly contact and constrain individuals. These researchers also emphasize the degree of variability that exists across societies and local settings with regard to demographic behavior: "Our efforts suggest that the grand narratives of classic behavioral theory overestimate the uniformity of human responses to exogenous forces" (p. ix). However, the studies successfully demonstrate the causal salience of political and social arrangements; differences in behavior are noted and are traced to differences in institutional settings. As Lee, Bengtsson, and Campbell write in their introduction to the series of volumes that will present the findings of the project, "These books demonstrate that patterns of demographic outcomes are determined by society, not biology" (Bengtsson et al., 2004, p. ix). Some of the local and regional social and cultural regimes that the authors identify and explore as causal variables in explaining outcomes include family structure, public provisioning systems for food emergency, wealth and class, and land tenure. Biology is certainly relevant to demographic outcomes. The genetics of reproduction, adaptability of species to changing environmental conditions, the "hard-wiring" of social emotions of cooperation and altruism all have effects on the demographic behavior of human populations. But the proximate causes of variations in fertility and mortality can be traced to variations in social institutions and conditions, not to differences across genotypes. Or at least this is the meta-conclusion that Lee, Bengtsson, and Campbell reach. (There is quite a bit of debate over these issues within the technical demographic literature; for example, Vetta and Courgeau, 2003.)

The issue of the relative importance of genetics and biological kinship, on the one hand, and social institutions and practices including family structure, on the other, in influencing fundamental demographic outcomes is an important one. As indicated above, Bengtsson et al. judge that "patterns of demographic outcomes are determined by society, not biology". However, in their later volume, *Kinship and Demographic Behavior in the Past* (Bengtsson and Mineau, 2008), they write: "Anthropologists, behavioral scientists, and evolutionary biologists traditionally study the sociocultural basis and consequences of kinship systems. Genetic and evolutionary theories, sometimes combined with social support theories, have been used to address mechanisms related to fertility, longevity, and nuptiality. The importance of genetic effects on fertility has been readdressed in recent research as operating through fecundity as well as motivation" (p. 4). This seems to imply that they now give more credence to genetic explanations of demographic outcomes. So which is it—social context or genetics?

It is plain from first principles that *both* genetics and social institutions are causally relevant to demographic outcomes such as fertility, longevity, or infant mortality. A given family structure (a social factor) may provide more adult attention to young children—thus leading to longer average lifespan and lower infant mortality. But likewise, a population with a higher frequency of a gene (a biological factor) that makes infants more resistant to potentially fatal infections will also display greater longevity and lower infant mortality. So it is easy to demonstrate that both genetic facts and social facts are relevant to demographic outcomes. But they may not be equally important in explaining *variations* across historical populations (Russo, 2008). It is possible that there is a very low level of genetic variation across the historical populations studied—which implies that demographic variations across these groups must be caused by social rather than biological or genetic factors. We know that there is substantial variation in social institutions and practices across populations, and we can discover plausible mechanisms through which these variations would bring about variations in demographic outcomes. So Bengtsson, Lee, and their colleagues are not forced to choose between biology and social context. Their preliminary judgment that institutional differences are most likely the proximate causes of demographic variations across populations remains supportable. It is an empirical question to discover whether there are differences in the frequency of relevant genes across historical populations to cause differences in demographic variables like fecundity or longevity.

Another important implication of this research pertains to the viability of the Malthusian hypothesis about macro-demography. The authors find that their results refute the Malthusian conclusions and generalizations about positive and preventive checks and Europe versus Asia. "Our project studies how changing economic conditions—food prices and wages—and different socioeconomic contexts—household, kin, and class composition—affect individual demographic outcomes. By comparing the patterns of demographic responses, we can understand better the socially and culturally conditioned decisions that families and individuals make as they struggle to cope with changing conditions" (Bengtsson et al., 2004, p. 5). They find that family practices, demographic institutions, and economic settings vary sufficiently across the map of Eurasia as to make it impossible to arrive at grand differentiating statements about European and Asian demography (or English and Chinese demography). In particular, they find that the evidence shows that Chinese demographic behavior resulted in fertility rates that were broadly comparable to those of Western Europe.

There are some important generalizations to be made about Europe and about Asia with regard to demographic patterns; but there are also cross-cutting similarities in some regions of Europe with some regions of Asia, and there are important differences across both Europe and Asia. As Lee, Campbell, and Bengtsson write, "Our efforts suggest that the grand narrative of classic behavioral theory overestimate the uniformity of human responses to exogenous forces. . . . Our work . . . bridges the apparent contradiction between two classes of social theory: one that emphasizes universalism and similarity . . . and one that emphasizes contingency and difference" (pp. ix, x). This finding illustrates a highly important common thread that emerges from comparative economic history in many areas—that regions contain substantial variation within their boundaries, and that these variations may sometimes be as significant as those across regions.

7.4 Comparative Economic History

Let us turn now to the challenge of making comparisons across large regional economies—Western Europe against East Asia, for example, or Great Britain against France. The intellectual motivation for such comparisons is fairly transparent: it may be possible to identify common features of economic change in widely different settings; and it may be possible to find differentiating conditions as well, leading to different outcomes. In other words, comparison permits institutional and causal analysis of economic development.[3]

Comparative economic historians have attempted to compare and interpret processes of change in large historically unified but distinct social orders; commonly, Europe and "elsewhere". A particularly important comparison is that between the economic, political, and social histories of early modern Europe and imperial China. Both were regimes with complex and reasonably effective states; agricultural systems that successfully provisioned mass populations; a cultural context which supported advancing levels of scientific understanding of nature (with the associated promise of technological innovation); and some level of mass manufacture (textiles, ceramics, metals). The impulse exists, then, to compare and contrast the large-scale processes of development and change that are to be found in those historical formations. Was there an impulse of state formation that can be discerned in Europe and applied to China? Were there similar population dynamics at work? Did market forces elicit a process of "proto-industrialization" in Europe and China?[4]

Much current discussion of economic history is at the level of comparisons across the regions of Eurasia. How should the comparativist proceed? First, it is important to have the best possible grasp of the empirical and factual experience of the two settings. The specialized economic history literatures of Europe and Asia provide factual evidence at a number of levels of scale: national economic performance, regional or occupational living standards (wages and prices), levels of trade, levels of transport, levels of output and output-per-unit, and so on. As noted above, these sources do not "speak for themselves"; the economic historian needs to exert himself in understanding the measurement issues that underlie the data, the regional variations that exist, etc. But important economic and social variables can be observed through existing economic history research at a variety of levels of aggregation. Economic historians such as Robert Allen have done highly valuable work in attempting to provide meta-level analysis of these data sets, to provide the

[3] The logic of comparative historical analysis has been a central topic of debate in the past decade. Particularly important are Adams et al. (2005), Kiser and Hechter (1991), Lichbach and Zuckerman (1997), Mahoney and Rueschemeyer (2003), McAdam et al. (1996), Ragin (1987), and McDonald (1996).

[4] The proto-industrialization literature has provided a powerful stimulus to recent research on the early character of economic transformation in Europe. Franklin Mendels describes this concept in these terms: " 'Proto-industrialization'—a period of rural industrialization with simultaneous bifurcation between areas of subsistence farming with cottage industry and areas of commercial farming without it" (Mendels, 1981, p. 176).

strongest basis for comparison across contexts; so Allen's detailed summaries of cost of living and wages across Europe and Asia provide a significantly more rigorous basis for cross-regional comparison (Allen, 2005, 2002; see also Goldstone, 2002 and Li, 2002a, 1998).

Second, a great deal of institutional detail is known about state policies, taxes, property systems, labor, etc., in a wide variety of settings across the Eurasian continental system and the smaller regions embraced within this system. Once again, this sort of historical knowledge can be gathered and aggregated at a range of levels, from the local to the regional to the national or continental; it is a substantive issue that the historian must address in choosing the "right" level of aggregation for the purpose of the current comparison. The comparative economic historian needs to have an expert acquaintance with the most credible accounts of the institutional settings within which communities, regions, and nations undertook their processes of economic development (Pomeranz, 2000; Wong, 1997).

These points underlines the importance of further *empirical and factual research* in resolving these hypotheses and debates about China's economic development. But there are also significant conceptual problems that need to be addressed as historians attempt to formulate summaries of the facts about population, output, productivity, standard of living, or real wage. Goldstone emphasizes the importance of discussing and motivating a set of data consistency standards that ought to guide the use of historical economic data. Goldstone distinguishes between micro and macro data and argues for the need to tie them together to establish consistency (Goldstone, 2002). Similarly, Robert Allen's close attention to establishing consistent accounting for measurement and comparison provides an admirable model of rigor.

What is the ideal product of a successful comparative economic history across regions or continents? We would like to have a reasonably detailed description of the empirical realities that were experienced through time and space in the field of study. We would like to have an account of the most important causal mechanisms that were in play in economic development in the cases under comparison. We would like to be able to construct narratives that illuminate the similarities and differences in the several regional experiences that we have considered. We would like to arrive at a clear understanding of some of the diversity and contingency that exists in these economic development trajectories. At the same time, we would hope that the results of historical comparison will shed light on the degree of systematicity or causal order that exists in economic and political development processes. There is variation, but there is also a degree of causal order. So each region's economic history is not *sui generis*; instead, productively, we find that there are causal similarities and contingent differences.

The knowledge that emerges from comparative research should offer insight into questions about *contingency and necessity* among large economic and political developments in East Asia or Western Europe. We should be open to the likelihood that there are substantial differences in processes across cases. Economic development embodies a complex configuration of factors and institutions, and represents a clear example of the role of contingency and path-dependence in historical change. Do we find that economic outcomes reflect a large degree of path-dependency and contingency? Do we observe substantial variation in the basic institutions through which economic and political functions are performed? Or is there convergence around "most efficient or effective" political and economic institutions? The results so far suggest a high degree of variation and contingency across Eurasia.

Rather than finding a "natural" process of economic development in the sequence, agricultural revolution => proto-industrialization => industrialization, we should be prepared to recognize and analyze a process that involves agricultural stagnation and advanced technology applications in different regions or sectors of the Chinese economy. Likewise, rather than presuming that the general logic of state formation "should" approximate that described in the rise of the absolutist state in Europe, we must be open to the discovery that the underlying dynamics of the Chinese state, military, and revenue system are functionally distinct. And indeed, Wong's account of the institutional setting of Chinese politics makes apparent why we should expect dramatically different polities in the two civilizations. Europe's politics were characterized by a polarity between the state and powerful non-political elite organizations; whereas China's imperial and Confucian system embodied a much more continuous and interrelated association between the state and elites. (Wong uses the intriguing concept of "self-similarity at many scales" from fractal theory to describe the structure of Chinese politics; Wong, 1997, p. 121.)

7.4.1 Scale and Scope of Comparison

A particularly important question for comparative economic history is the question of scale and scope of inquiry in these comparisons—the unit of comparison. Perhaps the most basic feature of any domain of social phenomena is its non-homogeneity, over many dimensions—location, time, ethnicity, urban-rural, etc. There was vast differentiation of institution, practice, environment, and outcome across Western Europe; so it is always important to keep in mind that patterns we identify in England are likely to have very different counterparts in Italy or Sweden. And likewise, China's economy and society are highly differentiated from north to south and from east to west—with the same caution that there is no single answer to the question, "What was the nature of China's economic development during the Ming period?" National comparisons—England compared with France or Japan, for example—elide many of these dimensions of difference. So the issue of scale is a methodologically and substantively important question to address: What are appropriate units of comparison?

There are a number of available possible answers: civilizational units (Europe and Asia), nations, macro-regions, meso-regions, or representative core regions. And the answer that we give needs to be motivated by a cluster of hypotheses about social mechanisms and institutions within the social activities bounded by the scale choice that we have made. What are the strands of interconnection that serve to bind a geographical area into an economic region—patterns of trade, language, common agriculture and manufacturing technologies, rural-urban networks of exchange of people and goods? Or should we look instead to boundaries of political jurisdiction—provinces, states, or empires? Is it better to compare larger and more complex units, or are we better served by attempting to define smaller units that are more homogeneous in their economic characteristics? As we will see in Chapter 8, Kenneth Pomeranz makes a sustained case for selecting comparable regional systems rather than "civilizational units" or nations (Pomeranz, 2000, pp. 7–8). He recommends comparison of England against the Yangzi Delta region of Eastern China, on the ground that populations and range of activities were roughly comparable at an early period of development. So the reason for this level of analysis is a preliminary assessment that there is greater likelihood that regions at this level of aggregation will be more meaningfully comparable to those in historically separate settings; that comparable (not necessarily similar) processes will be occurring; and that there is a greater degree of causal interconnectedness within the social system defined by these boundaries. Here the work of James Lee and his collaborators in the Eurasia Project is especially relevant, because these researchers attempt to disregard the national level altogether, and to focus on smaller human communities selected in different locations across Eurasia (Bengtsson et al., 2004). Their view is that meaningful comparison of demographic behavior is best construed at the level at which people and families interact with each other, with the environment, and with larger economic and social factors—at the community level.

It should also be emphasized that comparisons across regions must always be sensitive to intra-regional diversity. One thing we can say confidently is that there was substantial intra-regional diversity across Eurasia, at virtually every level, in levels and rates of change with respect to defining economic variables: standard of living, total output, output per capita, etc. Robert Allen's research (Allen, 2000) demonstrates this diversity for Europe; England, Scandinavia, and Italy show very different profiles of development, real wage, and institutional setting. Likewise, within China and across Asia it is possible to document a similar range of diversity. The agricultural systems of North China were substantially different from the rice economy of East China, and these differences were expressed in institutions and culture as much as in crops and technique. We can also say confidently that there were significant regional variations with respect to background institutions and conditions: environment, political institutions, market institutions, and social property systems governing land and labor.

It is useful to illustrate some of these features of comparative economic history by turning to a few important examples: Bin Wong's effort to provide a more balanced treatment of China's economic development, and Robert Brenner's effort to explain differences in agricultural development between England and France.

7.4.2 Alternative Pathways of Development in Europe and Asia

In *China Transformed* R. Bin Wong (Wong, 1997) offers a sophisticated approach to the problem of comparison across Europe and China. Wong believes that such comparisons are legitimate and fruitful; but he offers a convincing set of cautions about the conceptual and theoretical presuppositions that we bring to such an effort. His central point is a crucial one: we must not make the mistake of assuming that European developments and characteristics are the paradigm for history, and that Chinese developments will either reproduce this general template, or will be regarded as “a-typical.” He writes, “This book too aims to dislodge European state making and capitalism from their privileged positions as universalizing themes in world history, but it offers a new approach: comparison with the dynamics of economic and political change in a major non-Western civilization” (1997, p. 2). Against the general approach of taking European developments as paradigmatic—demographic transition, capitalist development, state formation—he argues that the comparativist needs to be prepared to identify large processes in any of the great civilizations as potentially insightful in application or contrast to the experience of others. He puts the point this way: “For historical trajectories to matter, there must be more than one. Western social theory has generally analyzed only that created by the twin processes of European state formation and capitalism. Western states and economies have histories that matter to the formation of the modern world. Other parts of the globe, according to the research strategies employed in most social science research, had no histories of comparable significance before Western contacts began to transform them” (1997, p. 3).

The purpose of *China Transformed* is thus to attempt to discern China’s own dynamic of transformation, its own historical trajectory and historical formations, with the aid of appropriate social theory. And Wong aims to illuminate European history by detailed consideration of an alternative historical course of development. For example, Wong carefully assesses the literature on proto-industrialization in Europe; finds that very similar processes of rural manufacture are present in both Europe and China; and argues that the causes of European “breakthrough” must therefore be sought elsewhere. More generally, he argues that similar processes of commercialization and population dynamics are associated with very different paths to (or away from) industrialization (Wong, 1997, pp. 46, 47).

What is “appropriate social theory”? The skeptic answers the question in a minimalist way: social theory is unavoidably associated with the paradigms of historical European development; even concepts like “state,” “market,” and “demographic regime” are invariably grounded in the European experience, so there is no legitimate basis for articulating a social theory that is truly cross-cultural and trans-historical. Wong does not accept this point, however. Rather, he aspires to a middle-level articulation of theory, identifying a set of processes that can be theorized and observed in very different social contexts. Population dynamics follow from the institutional setting of reproduction; it is therefore appropriate to theorize the consequences of several different “demographic regimes.” Individuals make calculating choices about costs and benefits of various options that they confront; therefore it is appropriate to theorize the consequences of prudent decision-making within several institutional settings. “Economic principles have a powerful capacity to order diverse economic experiences even as they prove inadequate to explain the multiple paths of Eurasian economic history and development” (1997, p. 11). Note the strategy here. It involves dropping down from the high-level description of the outcome (capitalist development) by focusing on the circumstances of human life and choice that drive multiple comprehensible paths and outcomes.

The upshot of Wong’s approach is this. Let us consider China’s historical development—economic, agricultural, political, social, military—in its own terms, but informed by the best available social theoretical insights and concepts; let us identify China's own "paradigms" of development, its own pathways of political development and economic change; and let us use those new-found paradigms to inflect our understanding of the processes of other parts of the world.[5] Finally, let us recognize that the stuff of social theory takes us a ways down the road of being able to explain particular pathways of historical development in a variety of contexts; but it does not permit us to make confident predictions about uniquely determined outcomes. In place of the overtones of inevitability—population increase, technological change, improvement in agricultural productivity—we get the sub-harmonics of diversity and contingency, and the recognition that historical outcomes are underdetermined by any particular and limited set of causal factors. And in fact, Wong argues that careful comparative study of the economic histories of different regions of Eurasia will establish this plasticity of outcome. For example, Wong carefully assesses the literature on proto-industrialization in Europe; finds that very similar processes of rural manufacture are present in both Europe and China; and argues that the causes of European "breakthrough" must therefore be sought elsewhere. More generally, he argues that similar processes of commercialization and population dynamics are associated with very different paths to (or away from) industrialization (Wong, 1997, pp. 46, 47).

Several important methodological points emerge from Wong's comparative study of Europe and China. First is a point about the role of social theory in historical inquiry. Wong recognizes that reliance on current social theory is inescapable in historical analysis (what else would provide the analytical basis for comparison and hypothesis?), but he emphasizes the importance of doing so with care and critical intelligence. As Susanne Rudolph puts the point, "At this stage we need fragile theoretical templates, made of soft clay rather than hard steel, that adapt to the variety of evidence and break when they do not fit" (Rudolph, 1987, p. 738). Crucially, Wong insists on the point that the researcher must be critical in extending ideal-typical concepts of structures and processes from the European context to an Asian context. More acutely, we need to find new ideal-typical configurations of institutions and processes in Asia (and other world civilizations), to add depth to our understanding of European history. Finally, Wong, like both other scholars whose work we have considered, emphasizes the plasticity of large historical developments. There are multiple contingent factors involved in any large historical process, and there is room for choice by agents at all points along the way.

7.4.3 Agricultural Revolution and Stagnation Within Europe

Comparison is useful within regions as well as across regions. And in fact, different parts of Western Europe demonstrated significantly different trajectories in the early

[5] Paul Cohen argued effectively along these lines in his call for a "China-centered" history of China in *Discovering History in China* (Cohen, 1984).

modern period. This kind of variation is the subject of an important debate initiated within European economic history in the 1970s and 1980s by Robert Brenner (Aston and Philpin, 1985; Brenner, 1976; Brenner, 1982). Sixteenth- and seventeenth-century England witnessed an agricultural revolution that involved massive changes in land tenure, the organization of production on farms, the techniques employed in farming, and the productivity of agriculture. Thus the sixteenth century represented a sharp change in English rural life: the emergence of the capitalist farm in place of small-scale peasant cultivation, the intensification of market relations, increase in population, and eventual breakthrough to capitalist development in town and country. The social consequences of this revolution were massive as well: smallholding peasant farming gave way to larger capitalist farms; hundreds of thousands of displaced peasants were rapidly plunged into conditions of day labor, first in farming and then in manufacture in towns and cities; higher farm productivity permitted more rapid urbanization and the growth of an urban, commercialized economy; and higher real incomes provided higher levels of demand for finished goods which stimulated industrial development. Thus the agricultural revolution was the necessary prelude to the industrial revolution in England.

This process poses at least two problems for historical explanation. First is a temporal question: why did breakthrough occur in England in the sixteenth century and not the fifteenth or the nineteenth? And the second is geographic: why did this process of agricultural development occur in England but *not* on the Continent? In particular, why did agrarian life in the French countryside remain relatively unchanged throughout this period? And why did eastern Europe slide into a "second feudalism" (Anderson, 1974)?

A variety of explanations have been advanced for these developments. Some economic historians (e.g., M. M. Postan and Emmanuel Le Roy Ladurie) have maintained that the cause of this process of change was an autonomous increase in either population or commerce or both. North and Thomas encapsulate this view in *The Rise of the Western World*: "The growth of population and the resultant expansion of organized markets and a money economy changed the basic conditions which had given rise to feudal society" (North and Thomas, 1973, p. 64). A key advocate of the "price and population" view is M. M. Postan. He writes, "Behind most economic trends in the middle ages, above all behind the advancing and retreating land settlement, it is possible to discern the inexorable effects of rising and declining population" (Postan, 1972, p. 30). Another proponent is Emmanuel Le Roy Ladurie, who writes,

> The tragic demographic situation of the fifteenth century—the scarcity of people—was the overriding fact that lend land settlement, economic life, and social relationships their peculiar coloration on the eve of the great advance of the modern period. (Le Roy Ladurie, 1974, p. 11)

Robert Brenner argues that these population-based explanations are inadequate, since these large-scale factors affected the whole of Western Europe, while capitalist breakthrough occurred only in Britain. Instead, Brenner holds that the differentiating causal factor is the particular character of social-property relations in different regions of Europe (particularly the conditions of land-tenure and associated forms of surplus extraction), the interests and incentives that these relations impose on the various actors, and the relative power of the classes defined by those relations in particular regions.

Brenner's explanation of these developments is thus based on "micro-class analysis" of the agrarian relations of particular regions of Europe. The processes of agricultural modernization unavoidably favored some class interests and harmed others. Capitalist agriculture required larger units of production (farms); the application of larger quantities of capital goods to agriculture; higher levels of education and scientific knowledge; etc. All of this required expropriation of small holders and destruction of traditional communal forms of agrarian relations. Whose interests would be served by these changes? Higher agricultural productivity would result; but the new agrarian relations would be ones that would pump the greater product out of the control of the producer and into elite classes and larger urban concentrations. Consequently, these changes did not favor peasant community interests, in the medium run at least. It is Brenner's view that in those regions of Europe where peasant societies were best able to defend traditional arrangements—favorable rent levels, communal control of land, and patterns of small holding—those arrangements persisted for centuries. In areas where peasants had been substantially deprived of tradition, organization, and power of resistance, capitalist agriculture was able (through an enlightened gentry and budding bourgeoisie) to restructure agrarian relations in the direction of profitable, scientific, rational (capitalist) agriculture. He writes,

> Under different property structures and different balances of power, similar demographic or commercial trends, with their associated patterns of factor prices, presented very different opportunities and dangers and thus evoked disparate responses, with diverse consequences for the economy as a whole. Indeed, ... under different property structures and balances of class forces ... precisely the same demographic and commercial trends yielded widely divergent results. (Brenner, 1982, pp. 16–17)

Brenner's thesis is thus that the central causal factor in the pattern of agrarian development in Europe—the factor which co-varies the most closely with patterns of development, regression, and stagnation—is the particular character of class relations in a region and the set of incentives and opportunities which the local property relations impose on the various actors.

This analysis depends upon the economic interests and political capacities of the various actors in a given region, especially the small cultivator and the landlord. Consider first the cultivator (peasant). His interests may be defined fairly simply: to achieve the most extensive possible property rights over the land which he farms; to keep rents at the lowest level possible; to reduce to a minimum the forms of labor services and other non-rent obligations he owes the lord; and to minimize tax obligations.

The cultivator's political capacities as an individual were quite limited; collectively, however, peasants in different regions of Europe developed significant powers of resistance to lordly exactions. Brenner identifies several different sorts of political resources available to peasant communities in some regions of Europe: a history of effective collective action (including peasant rebellion) in support of peasant interests and rights; complex and inefficient feudal relations in some regions involving conflicting lordly rights to a given community (thus permitting peasants to play off one lord against another); and peasant alliances with the monarch against landed property through which peasants were sometimes able to entrench and defend property settlements favorable to their interests.

The key to French developments in agrarian relations, for example, may be found in the extensive levels of power and solidarity which French peasant communities were able to achieve with respect to their "lords". "By the early fourteenth century, the peasants of northern France *had achieved effectively full property rights to the customary land* (fixed, minimal dues and right to inherit)" (Brenner, 1982, p. 50).

These property rights were ultimately grounded in peasant traditions of collective political action in France. A history of peasant collective action, political organization, and rebellion significantly enhanced the capacity of peasants to defend and extend their property rights. The property rights which peasants successfully established and defended in France led to technical stagnation in agriculture, however. This was true in part because peasant production was inherently low-yield; it took place on a small scale, using traditional and relatively inefficient techniques of cultivation. But peasants were also less able to make capital investments in their farms because of their vulnerability to surplus-extracting agencies above them. It was possible for other agencies—landlords and the state, particularly—to extract what small surplus that existed from peasant cultivators. Brenner writes,

> Thus the capitalist farmer was given a clear field: he had the incentive, the opportunity, and the power to effectively organize the farming process so as to produce a profit; and this gave him the incentive to pursue the path of technical reorganization of farming. Peasant agriculture was not wholly eclipsed: "This is not to say, of course, that peasant production was incapable of improvement. Thus small scale farming could be especially effective with certain industrial crops (for example flax) as well as in viticulture, dairying and horticulture. But this sort of agriculture generally brought about increased yields through the intensification of labour rather than through the greater efficiency of a given unit of labour input" (Brenner, 1976, p. 64).

It is important to Brenner's case to establish that the agricultural revolution was *not* the result of new technical advances in agriculture. Rather, alternative techniques and forms of organization of production that were more efficient were available for an extended period of time in England, but were not adopted until it was in the interest of a powerful class to incorporate them. Thus—against other economic historians—it was not newly discovered technological options that drove the agricultural revolution, but rather a new set of incentives and opportunities imposed on a powerful class.

This schematic representation of the strands of argument in the Brenner debate suggests competing causal diagrams:

- population growth => economic activity => sustained economic growth (Postan, 1972)
- weak peasant farmers, strong capitalist farmers => enclosure and farming innovations => rapid agricultural growth (Brenner, 1976)
- enhanced protections of property rights => incentive for profitable activity => sustained economic growth (North and Thomas, 1973)

But it seems clear in hindsight that these are false dichotomies. We are not forced to choose: Malthus, Marx, or Smith. Economic development is not caused by a single dominant factor. Rather, all these factors were in play in European economic development—and several others as well. For example, Pomeranz introduces the exploitation of the natural resources, energy sources, and forced labor of the Americas in his account of the economic growth of Western Europe (Pomeranz, 2000). And it would be possible to make a climate-change argument for this period of change as well. Moreover, each large factor (population, prices, property relations) itself is the complex result of a number of great factors—including the others on the list. So we should not expect simple causal diagrams of large outcomes like sustained economic growth.

The main historiographical conclusion which may be drawn from Brenner's arguments concerns the need for analysis of the microprocesses of social change, and the social structures and institutions which constitute the local environment of largescale economic and social processes. Brenner shows in one important case that these local structures can fundamentally alter the largescale causal factor (diverting the process from a stimulus to development toward stagnation or involution, for example). There is an important substantive contribution in this debate as well. Brenner highlights the centrality of a particular kind of local structure: the relations of class power and interest which are determined by existing property relations, on the one hand, and traditions and practices of joint political activity, on the other. And finally, the debate underlines the importance of explanatory pluralism. Historical processes are multi-causal, and we need multiple theories of causal mechanisms in order to account for them.

7.5 Contingency and Alternative Pathways of Development

Let us consider once again the intellectual returns that we can expect from comparisons across Eurasia. First, we would hope that the results of historical comparison will shed light on the degree of causal order that exists in economic and political development processes. There is variation across instances of economic development, but there is also a degree of causal order. So each economic history is not *sui generis*; instead, productively, we find that there are causal similarities and contingent differences. If we have the good fortune of finding that there are a relatively small number of factors that have prominent effects on the pace of economic development, we would hope to be able to support some causal generalizations across cases—for example, "Secure property systems enhance technology innovation," "Improvements in transport efficiency cause economic growth," "Increases in mobility of labor cause rising urbanization," or "Social insurance regimes increase economic growth." With appropriate attention to *ceteris paribus* conditions, it is credible that we might be able to identify some such causal generalizations.

At the same time, the knowledge that emerges from comparative research should provide answers to questions about contingency and necessity among large economic and political developments in East Asia or Western Europe. Do we find that economic outcomes reflect a large degree of path-dependency and contingency? Do we observe substantial variation in the basic institutions through which economic and political functions are performed? Or is there convergence around "most efficient or effective" political and economic institutions? The results discussed here suggest a substantial degree of variation, contingency, and path-dependency across Eurasia. There were multiple feasible pathways and institutional settings for economic and political development in the processes of population change, agricultural development, and institutional development within and across Eurasia. Wong, Lee, and Pomeranz show that there is ample room for contingency and agency within the historical experience: there were historically feasible alternatives in the organization of production with modern technologies; and in fact, managers, workers, and planners exploited these contingencies so that the alternative forms in fact prospered in various settings (Sabel and Zeitlin, 1997). This narrative casts doubt on the metaphors of iron necessity in development that have sometimes been used in earlier approaches to the history of industrialization of Europe. In place of the overtones of inevitability—population increase, technological change, improvement in agricultural productivity—we get more nuanced narratives of diversity and contingency, and the recognition that historical outcomes are under-determined by any particular and limited set of causal factors.

Further, comparative economic history can discover some common middle-level processes that recur in different settings—economic behavior, family and reproductive behavior, incentives and opportunities presented to the wielders of monopoly coercive power—and that different institutional settings can lead these processes to radically different outcomes. Moreover, there are interaction effects among the institutions that regulate the various common processes; thus the particulars of a given set of political institutions (designed, perhaps, to impede the ability of military commanders to challenge the emperor; Kuhn, 1970a) may impede development of effective financial institutions, and therefore impede the development of large-scale enterprises with large geographical scope. Peasant production—smallholding and tenant farming—may place a limit on improvements in agricultural productivity that constrain the state's fiscal capacity—and hence its ability to finance military or commercial infrastructure. Large-scale commercialization of a product sector—e.g. cotton textiles—may be so successful at producing large quantities at low price, that technological innovation is discouraged (Elvin, 1973).

A fundamental result of study of these important current historical debates is this. It is crucial to consider China's historical development—economic, agricultural, political, social, military—in its own terms, but informed by the best available social theoretical insights and concepts. We need to identify China's own "paradigms" of development, its own pathways of political development and economic change. And we need to use those new-found paradigms to shed new light on our understanding of the processes of other parts of the world.

We began by noting the intellectual importance of economic history, along with the explicit and implicit disagreements that exist among practitioners about the most basic assumptions that should lead research and theory in the field. Several things are clear. First, it is important to have a clear statement of the knowledge goals that motivate the discipline of economic history, at a range of levels; and the answers to this set of questions are not yet clear. What kind of knowledge should be the result of a sustained research program of economic history? Are there different knowledge goals at different levels of aggregation? Does the knowledge product of economic history research need to contribute to comparative economic history knowledge? Second, it is worthwhile to have an explicit treatment of some of the conceptual complexities that arise in the conduct of this field of research. Some of these complexities have been identified above: the importance of specifying appropriate regions for comparison, the importance of incorporating the variability of economic factors across place, time, and population into our thinking, the importance of institutions and customs in influencing economic outcomes, or the challenge of formulating an index to represent the "standard of living." Third, we have seen that there are large epistemological challenges within economic history, at the level of providing critical assessment of different kinds and qualities of empirical data. What are the challenges of arriving at confident knowledge of a given sort of low-level economic fact (for example, price and wage history, standard of living)? And what are the logical inferences through which data of this sort can be brought to bear on higher-level economic history findings (such as "the lower Yangzi Delta experienced a rising standard of living in the eighteenth century")? If we can arrive at more satisfying answers to questions such as these, it should be possible to make substantial progress in the primary task of better understanding the economic history of China as a result. Collaboration between philosophers and methodologically astute social scientists and historians should pay valuable rewards to the discipline of economic history—and to the ability of philosophers to think more rigorously about history and social change.

Chapter 8
The Involution Debate

This chapter discusses an important current debate that illustrates many of the indeterminacies and historiographical challenges that have been raised in earlier chapters: a debate over the nature of China's economic development since 1600. Was China on a path of steady growth or asphyxiating involution? Neither the facts, nor the institutional descriptions, nor the interpretations of these facts and descriptions, are yet settled. So the case presents an excellent opportunity to observe the historians at work.

8.1 China's Early Modern Rural Economy

China's rural economy was extremely poor; it was stagnant or even declining in per capita terms; and it embodied substantial inequalities of land, tenancy and security—or so conventional wisdom would have it. R.H. Tawney's bleak observations in the 1930s set the stage for much work on the economic history of the late imperial period in the 1960s and 1970s. Tawney emphasizes extortionate taxation and credit relations, warlordism, minute landholdings, poor soils, and population pressure as the chief causes of increasing rural misery in China. He wrote, "There is even some reason to believe that, with the increased pressure on the land caused by the growth of population, the condition of the rural population, in some parts of China, may be actually worse than it was two centuries ago. . . . It is difficult to resist the conclusion that a large proportion of Chinese peasants are constantly on the brink of actual destitution" (Tawney, 1966, pp. 71–72). Western scholarship in the 1960s and 1970s emphasized the poverty and stagnancy of the Chinese rural economy, thus confirming the broad outlines of Tawney's analysis. And this interpretation of Republican China echoes the Malthusian and Smithian interpretations of China's rural economy in the early-modern period (1600–1850), according to which population growth, limited resources, and stagnant technology doomed rural Chinese people to low and falling standards of living.

However, in the 1990s several important bodies of scholarship have challenged this conventional wisdom. Treating the last decades of the nineteenth century and the first 30 years of the twentieth century, Thomas Rawski (Rawski, 1989) and

D. Little, *New Contributions to the Philosophy of History*, Methodos Series 6,
DOI 10.1007/978-90-481-9410-0_8,

Loren Brandt (Brandt, 1989) argue for a substantial degree of growth in agricultural output, rural incomes and living standards. And in their important treatments of the longer duration of Chinese economic development, Kenneth Pomeranz (Pomeranz, 2000) and R. Bin Wong (Wong, 1997) argue that early-modern Chinese agriculture was roughly as productive in 1800 as that of its contemporary European farming, and that the standard of living in the countryside was comparable in China and England. Further, James Lee and his collaborators (Bengtsson et al., 2004; Lee and Campbell, 1997; Lee and Wang, 1999) have challenged the Malthusian interpretation of Chinese historical demography. They argue that China's population history shows moderate growth and socially regulated rates of fertility—thus contradicting the idea that population growth made modern economic growth impossible to achieve in China.

These more favorable views of the economic potential of early modern China have stimulated a vigorous debate. Against Pomeranz, Philip Huang (1990) argues that the rice economy of the Yangzi Delta was locked in a pattern of "involutionary growth" with little or no improvement in per capita output and living standards and a pattern of declining labor productivity. In a major critique of Pomeranz's interpretation, Huang (2002) and Brenner and Isett (2002) offer fundamental and sweeping criticisms of the empirical and theoretical case that Pomeranz advances; Pomeranz responds forcefully and at length in the same journal (Pomeranz, 2002), and James Lee and his colleagues rebut the demographic assumptions made by Huang (Lee et al., 2002).

Highly relevant to both debates is Bozhong Li's extensive body of work on agricultural inputs, outputs, costs, and rents in the family farm economy of the lower Yangzi Delta. Li provides a crucial empirical basis for assessing the claims in these debates (Li, 1998). Li provides for the Jiangnan region of China a body of empirical assessment that is comparable to the impact of the work of Robert Allen on the productivity of the English farm economy (Allen, 2005). These bodies of research permit evidence-based estimates of the standard of living in England and Jiangnan that provide the basis for some conclusions about involution, growth, or stagnation in these rural economies, especially in the early modern period.

These disagreements raise a number of important issues for China scholars more broadly: the nature and rate of agricultural development (output, productivity and application of new technologies), the direction and nature of change in rural welfare during the period, and the character and pace of social change during this period (rural to urban migration, land tenure change, concentration of landholdings). If the generally upbeat assessment offered by Rawski and Brandt is sustained, then a rather deep reassessment will be needed of the status of welfare and social change in China's countryside in the early twentieth century. If Huang's view is validated, then customary assumptions about the nature of economic development in an agrarian economy need rethinking.

This chapter focuses on these important dimensions of disagreement in the literature today about economic change in the Qing and early Republican period. The substantive issues may be summarized along the following lines. First are issues directly concerned with processes of development within the agricultural sector.

Was there significant productivity growth in Chinese agriculture during the period? Were there significant processes of technological change under way? Was commercialization stimulating greater efficiency and investment? To what extent did new communication and transportation technologies stimulate change in the rural sector? Second, each author is forced to arrive at assessments of China's population trends during the period, and there is significant controversy about China's historical demography. What is the best estimate of the rate of increase of the population? How much urban migration or inter-rural migration was occurring? How significant were large "positive checks" such as famine, disease, and warfare in China's population history? Third is the question of the status of structural economic transformation of the Chinese economy in the Republican period. To what extent was the proportion of agriculture to manufacturing and handicraft output changing during this period? How much growth of manufacturing and industrial employment was occurring? How extensive was the growth of commercialization of agriculture? How rapidly was modern industry eroding traditional manufacturing? Finally, there are a host of issues relating to the net effects of these various processes on the welfare of the rural population. What was happening to rents and wages? Was population pressure on resources placing increasing strain on the rural economy? Were rural incomes subject to greater instability? Are there available data that would indicate the direction of change of nutritional adequacy in the rural population? To put it crudely: was the rural population in a state of immiseration during the period? Was it holding its own? Or was there significant, if slow, improvement in rural welfare?

It is evident that there is a very wide range of disagreement across these several schools of thought on China's rural economy. The disagreements between Pomeranz and Huang, or Brandt and Lippit, are not over minor points of empirical detail; they involve fundamentally differing assessments of the overall nature and direction of Chinese economic change in the relevant periods. Moreover, these disagreements matter a great deal to our understanding of China's development in the twentieth century. To what extent is it possible to resolve these issues? What obstacles stand in the way of our reaching relatively definitive conclusions on these central economic issues? How much resolution is it possible to reach concerning the main economic characteristics of the Chinese rural economy?

8.2 Involution or Revolution in the Early Qing?

Let us begin by considering the "involution debate" between Pomeranz and Huang (and numerous other experts). Eurasian economic history has been dominated in the past several years by a sustained debate over the developmental status of late imperial China relative to England: was the early modern Chinese agricultural economy "involutionary," "stagnant", or "revolutionary"? This section considers the main features of this debate. Since there is a substantial range of empirical disagreement between the two perspectives, it is logical to hope for some degree of resolution through more detailed factual and empirical research.

The involution debate involving Pomeranz, Brenner, Huang, Lee, and others has been heated, complex, and sometimes illuminating (Brenner and Isett, 2002; Huang, 2002; Pomeranz, 2002; Wong, 2003, Lee et al., 2002; Goldstone, 2002; Li, 2002b). The debate has revolved around several important and somewhat independent dimensions. There is a core set of factual disagreements over the status of a number of important variables, including especially the comparative standard of living and the level of agricultural productivity. There is also a degree of disagreement over conceptual issues. How do we define "sustained economic development", standard of living, or productivity? What constitutes a good causal explanation? And there is disagreement concerning the causal and institutional factors that might be thought to have created "stagnation" or involution in China.

Philip Huang argues forcefully for the involutionary nature of China's rural economy. He maintains that China's agricultural economy in the late Qing and early Republican economy experienced extremely low levels of per capita productivity and was able to increase output only at the expense of ever-increasing inputs of labor per unit of output (Huang, 1990). The family-farming unit was one that was highly vulnerable to self-exploitation (the use of "free" family labor well past the point of reasonable marginal return), and the pressure of limited land, population increase, and technological stagnation resulted in falling productivity and stagnant to falling standard of living. According to Huang and his supporters, the Yangzi Delta was on an involutionary trajectory in the early-modern time period, involving Malthusian crisis (population exceeding food production), falling labor productivity, rising intensity of land use, falling marginal product, and falling living standards.

Huang's book covers a very long time horizon; he treats the Yangzi rural economy over a 600-year period, leading through the post-Mao reforms. He maintains that the Yangzi Delta economy was characterized by a system of subsistence-level farming based on peasant family production; "only in the 1980s did transformative development begin to come to the delta countryside, to result in substantial margins above subsistence in peasant incomes" (Huang, 1990, p. 1). Huang holds that this rural economy was heavily involuted, organized around self-exploiting family production. The stimulus of population increase led to intensive rather than productivity-enhancing growth, and the results were stagnant levels of welfare for the rural population. The farm family system drove out hired labor managerial farming because of low opportunity cost of family labor (p. 14). Thus in Huang's view the farm economy was characterized by "growth without development" (p. 11). It was highly involuted due to population pressure and did not show significant growth in productivity through this whole period. Agricultural output expanded just enough to keep pace with population increase, largely through intensification of production. "There was little or no expansion until the introduction of modern inputs after 1950" (Huang, 1990, p. 14). Finally, Huang rejects Rawski's and Brandt's arguments that living standards were rising appreciably around the turn of the twentieth century (pp. 137–143) (discussed below).

Kenneth Pomeranz disagrees profoundly with the involutionist interpretation when applied to the early modern period (1600–1800). In order to provide a more adequate comparative economic history, he proposes a detailed comparison between

England and Jiangnan in the early stages of the modern period. Pomeranz maintains, against the involutionists, that China's rural economy was roughly as productive as England's in 1700, and that the rural standard of living in the lower Yangzi region was approximately the same as that of rural England in the same period (Pomeranz, 2000). "It seems likely that average incomes in Japan, China, and parts of southeast Asia were comparable to (or higher than) those in western Europe even in the late eighteenth century" (Pomeranz, 2000, p. 49). Pomeranz holds that Huang gives too little attention to the importance of the differences betweens land-intensive and labor-intensive agriculture. Pomeranz agrees that China's economy did not emerge into a period of sustained modern economic development following the beginning of the eighteenth century (this is the significance of his title *The Great Divergence*); but he contests Huang's explanation for this fact (Huang's argument that involutionary agriculture prevented productivity-enhancing innovations). Pomeranz asserts that broad features of Yangzi Delta agricultural productivity, handicraft productivity, standard of living, and demographic behavior were generally similar across the two cases, and that economic "breakthrough" in the English case was the result of a highly contingent, non-systemic factor—the acquisition of significant natural resources and labor in the Americas.

There is an important conceptual point that must be emphasized in considering this debate. Both "revolution" and "involution" imply a sustained tendency to change: either dramatically rising labor-land productivity or gradually falling labor productivity. But there is a third logical alternative: generally flat productivity in the face of many other changing variables—new fertilizers, rising population, ecological challenges, falling land-labor ratios, technological changes, or environmental challenges. (Broadly speaking, this is the view advanced by Dwight Perkins, 1969.) This is a coherent and historically defensible position: that Chinese agriculture was neither leading to revolution, nor was it experiencing a longterm trend towards involution. It was instead stable and progressive, from the point of view of labor productivity, per capita output, and farm incomes. But is this position supported by the facts?

One thing we can say confidently is that there was substantial intra-regional diversity in levels and rates of change with respect to defining economic variables across Eurasia: standard of living, total output, output per capita, etc. Robert Allen's research demonstrates this diversity for Europe; England, Scandinavia, and Italy show very different profiles of development, real wage, and institutional setting (Allen, 2000). But likewise, it is possible to document a similar range of diversity within China and across Asia. We can also say confidently that there were significant regional variations with respect to background institutions and conditions: political institutions, market institutions, environment, and social property systems (governing land and labor). This degree of variation should lead us to expect significant differences in economic history as well across regions.

In the next several pages we will consider the central areas of disagreement among the participants in the involution debate: population trends, farm productivity, the level of the real wage, and the impact of differences in the institutions governing agriculture.

8.2.1 Population Trends

The issue of population dynamics is central to this debate. The involutionary interpretation depends heavily on the Malthusian view that China's population consistently showed rates of increase that pushed against the limits of agriculture and land. However, Pomeranz, Lee, and Li maintain that the lower Yangzi River basin was not characterized by a Malthusian crisis. Instead, they argue that China's demographic regime was stable and resulted in controlled fertility. James Lee and his colleagues maintain that more detailed study of China's demographic systems at the level of the family result in similar demographic outcomes to those experienced in early modern Europe (Lee and Wang, 1999; Bengtsson et al., 2004). Lee and Campbell conclude, "Even thought Huang (1990) and others have speculated that Chinese populations were distinguished from European populations by the elevated importance of mortality . . . , reductions in birth rates played a much more important role in slowing the increase of population in Daoyi than changes in the death rate. . . . Increases in Malthusian pressure not only triggered a demographic response, but a social one as well: processes of household formation were permanently transformed, fundamentally changing the social context of daily life in Daoyi" (Lee and Campbell, 1997, pp. 47, 49).

Lee, Bengsston, and Campbell find that their results cast doubt on the Malthusian conclusions and generalizations about positive and preventive checks in Europe versus Asia. "Our project studies how changing economic conditions—food prices and wages—and different socioeconomic contexts—household, kin, and class composition—affect individual demographic outcomes. By comparing the patterns of demographic responses, we can understand better the socially and culturally conditioned decisions that families and individuals make as they struggle to cope with changing conditions" (Bengtsson et al., 2004, p. 5). They find that family practices, demographic institutions, and economic settings vary sufficiently across the map of Eurasia as to make it impossible to arrive at grand differentiating statements about European and Asian demography (or English and Chinese demography). In particular, they find that the evidence shows that Chinese demographic behavior resulted in fertility rates that were broadly comparable to those of Western Europe.

8.2.2 Productivity

The behavior of agricultural productivity is crucial to this debate. How are we to attempt to resolve the disagreements involved here? Here the careful empirical work provided by Robert Allen and Bozhong Li is crucial to the debate. Bozhong Li's major studies of Jiangnan farming (Li, 1998, 2002a) provide much of the empirical base that is used by other scholars in attempting to arrive at estimates of farm productivity and rural incomes in the lower Yangzi region. And Li's studies contradict the assertion that labor productivity was declining in the early modern period in the lower Yangzi Delta. According to Li, the Chinese farm economy experienced steady to rising labor productivity and rising land productivity, resulting in a level standard of living for rural workers and farmers. After a careful analysis of the amount of labor employed over the course of a year in the several farming sectors, Li writes: "My conclusion is the opposite of the conventional view that 'heavy population pressure' reduced laobur productivity in farming in early and mid-Qing Jiangnan. The reduced size of Qing farms did not reduce per worker labour productivity on the farm. On the contrary, labour productivity rose" (Li, 1998, pp. 140–141). Finally, Li and Pomeranz observe that the two paths (England and Jiangnan) separated in the mid-eighteenth century, with sustained productivity increases in manufacturing and agriculture in England, and static or worsening productivity in Jiangnan.

Robert Allen contributes to the debate by assembling a detailed and historically rigorous framework for aggregating costs on historical farming systems (England and the lower Yangzi), and arriving at estimates of labor and land productivity, farm wage incomes, and farm family incomes (Allen, 2003). His farm model permits a consistent framework for estimating costs and outputs of Yangzi farming. His analysis supports detailed comparison of labor productivity in England and the lower Yangzi Delta, and his findings are two-fold. First, he finds that the overall level of farm labor productivity in the Yangzi Delta is a bit lower than that of England, but higher than several other regions of Europe; and second, he finds that this level of labor productivity is roughly constant between 1620 and 1820 (Allen, 2003, Table 5). In other words, his analysis contradicts the "involutionary" hypothesis of falling labor productivity during these centuries. He also contradicts the "revolutionary" thesis of rising productivity; he finds that gross output of rice per day of labor increased significantly between 1620 and 1820; but—contrary to Li—when we take into account the cost of beancake fertilizer, net output is roughly constant (Allen, 2003, p. 11). "Labour productivity in the Yangzi Delta was about 79% of that in England in 1800. While this was, of course, less than the English or Dutch achievement, it was considerably above that of most countries in Europe" (p. 11).

Allen's overall finding is supportive of the judgment that the rural Chinese standard of living was comparable to that of rural England, and that there is little evidence of productivity increase or decline in Chinese agriculture in the early modern period. There was significant change in the intensity of agriculture and fertilizer use (beancake); these changes led to rising output; and the cost of new inputs kept overall labor productivity roughly constant. And, most significantly, he finds that labor productivity was roughly unchanged through the two centuries between 1620 and 1820—a finding that contradicts the expectations of the involution theory. Thus Allen finds that neither the involutionary nor the revolutionary model is adequate to the Chinese data. This supports the view that Chinese agriculture was neither leading to sustained per-capita growth, nor was it experiencing a longterm trend towards involution. It was instead stable and progressive, from the point of view of labor productivity, per capita output, and farm incomes.

8.2.3 Real Wage Comparisons

Robert Allen's work on real wages in Europe and Asia provide a substantially stronger basis for empirical assessment of the question the rural standard of living than we have had hitherto. The central question here is, how did rural real wages compare in England and China? Allen is able to address this problem using the farm economy model developed in "Agricultural Productivity and Rural Incomes" (Allen, 2003). This model incorporates data on crops, prices, and labor expenditures for Yangzi and English Midlands farms. He is able to calculate estimates for family incomes in the two settings. He finds that the Yangzi Delta family income per day was 34.2d in 1620 and 21.0d in 1820; and the latter figure compares to 19.8d for the English Midlands (Allen, 2003, Table 8). These data indicate that Yangzi family income fell during these centuries but remained slightly higher than rural English family income in 1820. And based on trends in English rural wages reported in (Allen, 2005), we can infer that the Yangzi family income was measurably higher than its English counterpart in 1620. These points vindicate Pomeranz's claim that Chinese rural incomes were comparable to their English counterparts in the early modern period.

In "Real Wages in Europe and Asia" Allen (2005) provides a methodology that involves careful estimation of a "cost of living" index for England, India, Japan, and China. This index is based on a wage basket of staple food and clothing, for which there are very good price data in England and sporadic price data in China. He also provides a simpler index based on the price of a calorie of the basic foodstuff in each country. He then converts money wage data from several countries into a common real wage, and uses these estimates for England, India, Japan, and China to provide a quantitative answer to some of the most basic issues in the involution debate. Centrally, he concludes for the middle of the eighteenth century, that "using the price of a calorie as a deflator indicates that there was little difference in the standard of living of English, Chinese, and Japanese farm workers.... Asia did not lag behind Europe" (Allen, 2005). This estimate is for a time period that falls within the period of dispute between Pomeranz and Huang, and it clearly favors the Pomeranz position. Moreover, he finds that the Chinese standard of living rose substantially between 1700 and 1900: "The standard of living in the Yangzi rose by over 40% between the early eighteenth and early twentieth centuries" (Allen, 2005).

8.2.4 Institutional Settings

Throughout his writings Robert Brenner makes a causal argument about differences in the profile of economic development, based on the two kinds of differentiation noted here; he argues that high and low economic developers correspond to differences in social-property systems (Brenner, 1976, 1982). This is a simple causal argument with two foundations: first, an analysis of co-variation between outcomes and institutional settings, and second, an account of a possible social mechanism that shows why social-property systems of a certain sort should be expected to result in sustained economic growth. Brenner brings this perspective to bear in his contribution to the involution debate (Brenner and Isett, 2002). (Brenner's comparative treatment of English and French agrarian development is discussed briefly in Chapter 7.)

Brenner's (and apparently Huang's) explanation of English case, in contrast to China, involves three large factors: (a) Property relations permitted capitalist agriculture in England (Brenner, 1976, 1982). (b) Chinese demographic practices permitted high fertility, moderate mortality in China—leading to endemic population pressure on resources. And, (c) implementation of technological innovation was rapid in England as a result of the incentives for capitalist farmers. The result of this combination of factors is a steady increase in productivity in England, sustained improvement in the standard of living, and the gathering financial capacity of elites to invest in modernizing technologies in manufacturing. By contrast, Brenner characterizes China as witnessing erosion of the standard of living and a failure to introduce modern technologies and agricultural improvements; and by inference, the explanation of this outcome is the less favorable institutional setting that Chinese society created for innovation and investment in agriculture.

Pomeranz takes issue with both aspects of this theory. He disputes the premise that Chinese agriculture failed to make progress in implementing new technologies of irrigation, cropping, and fertilizers. And he disputes the thesis of "superior institutional setting" as an explanation of England's later economic takeoff. Instead, he argues that England shoots forward because of resources from the Americas, cotton and agriculture imports, extension of land in the Americas, and the exploitation of slave labor in the Americas. Here again Bozhong Li's analysis of farm productivity and the standard of living in the lower Yangzi appears to support Pomeranz.

8.2.5 Environmental Exhaustion

Mark Elvin provides a different basis of analysis of the "involutionary" character of Chinese economic development in his pathbreaking environmental history of China, *Retreat of the Elephants* (Elvin, 2004). Elvin closes his treatment of China's environmental history, and the history of agricultural development that is deeply entangled within this history, by offering a way of thinking about the level of "environmental pressure" within a given economy. Elvin introduces this concept as an alternative way of assessing the degree of intensity with which the Chinese farming system had developed in its use of labor and environmental resources; extremely high environmental pressure would imply something very similar to the high-level equilibrium trap he had hypothesized earlier in his writings (Elvin, 1972).[1] Elvin also argues that "environmental pressure" might have functioned as a formidable barrier to China's adoption of modern economic forms and manufacturing systems:

[1] Elvin's concept of the high-level equilibrium trap is discussed in Little (1998, Chapter 8).

the sunk costs of control of the environment made it difficult to consider adoption of an entirely different system of production.

Elvin attempts to begin the project of assigning a quantitative measure to "environmental pressure" by offering a definition. He singles out the quantity "cost of restoring existing resources to their prior level of output for the same level of input" and the ratio of this quantity to total output, and he suggests that we consider the rate at which this ratio changes over time (Elvin, 2004, pp. 455ff). An environment under severe "pressure" is one in which the cost of restoring it to its prior level of productivity exceeds the total output of the economy for that period. Elvin observes that innovations in technology, or the discovery of new external sources of resources, can dramatically change the degree of pressure experienced by a given economy; so a new water control technology can potentially greatly reduce the costs of restoration of the water system at the end of the production period. That said, the judgment that a given environment is under severe environmental pressure appears to represent an alternative basis for arguing for the conclusion that this economy is undergoing involution.

Elvin then asks the question whether there is a basis for comparing China and Europe according to this measure (Elvin, 2004, p. 460). Here he explicitly considers Pomeranz's claims about seventeenth century parity between England and the lower Yangzi, and he suggests that we have reason to judge that China was under substantially greater environmental pressure than Europe in the early modern period. He notes that the decisive empirical basis for establishing this conclusion is currently unavailable, but he argues that the evidence of contemporary observations and comparisons offered by Jesuit observers permits some preliminary conclusions. He offers this conclusion: "Overall, the Jesuit evidence ... makes a persuasive prima facie case that the 'pressure' of the late-imperial Chinese productive system on the natural environment ... was significantly heavier than that at least of France around the beginning of the modern era. This can probably be extended, though with less certainty, to other parts of northwestern Europe" (Elvin, 2004, pp. 469–470). Significantly, Elvin counts the cost of hydraulic maintenance work as a large component of the renewal cost for resources; other large components include the intensity of Chinese farming and the need for annual labor to replace soil fertility (because of the lack of fallow).

Elvin links this discussion to the involution debate, but we may question whether the circumstance of "environmental exhaustion" that he analyzes is significantly related to the condition of involution that Huang postulates. One line of thought serves to link the two conditions together: if we consider the example of an irrigation system that requires more labor for dredging of silt each year in order to produce the same output of grain, then we can infer falling productivity (grain/total labor input). So rising "environmental pressure" in this instance leads directly to falling labor productivity—or in other words, involution. Sustainability requires restoration of the production system to its initial level of productivity. If producers choose not to invest the full amount needed for restoration, then the production system will have lower productivity in the next cycle—with the consequence, once again, of involution in the technical sense (declining labor productivity). But the connection is not always so tight.

For, as Elvin notes, there are multiple ways of dealing with environmental pressure. As he emphasized in his earlier work on the high-level equilibrium trap (Elvin, 1973), innovations in technology and technique provide the means for pushing back the productivity frontier. But here Elvin's earlier conclusions are directly relevant to his analysis of environmental pressure; in his arguments surrounding the theory of the high-level equilibrium trap, Elvin argued that the Chinese production system had fully exploited the available repertoire of technological and technical innovations that could shift the system to higher productivity. And on this assumption, the conclusion that "rising environmental pressure entails falling labor productivity" is economically inescapable.

Consider briefly the treatment that Pomeranz provides of resources and environment. Pomeranz makes a great deal of the fact that European exploration and colonialism provided vast sources of natural resources into the control of European nations, including England. The "underground forests" of England's coal reserves, the "hidden acreage" of South American and Caribbean plantations, and the labor of colonial peoples all provided infusions of resources into the English economic system; and when these inputs are incorporated into the calculation of "environmental pressure" that Elvin provides, they have the effect of relieving environmental pressure.

So it would appear that Elvin is providing a conceptual basis for a new line of criticism of the thesis that England and China were in comparable economic situations at the beginning of the modern era. This approach is worthy of further empirical and historical investigation.

8.2.6 Conclusions on the Involution Debate

It is now possible to delineate some areas of best judgment with respect to the primary disagreements involved in the involution debate. Thanks to detailed and rigorous empirical work by Bozhong Li and Robert Allen, the situation of agricultural productivity and the real wage in England and the Yangzi delta is somewhat more clear today than it was when this debate originated. It appears reasonable to conclude with Robert Allen that the real wage for Yangzi peasants was roughly equal to that of English farm laborers in the seventeenth and eighteenth centuries. This finding supports Pomeranz and Lee in their assertion that conditions for ordinary people in England and China were roughly comparable.

Second, it seems reasonable to conclude on the basis of work by Bozhong Li and Robert Allen, that agricultural labor productivity was roughly comparable in these two regions as well. As Pomeranz emphasizes, we must take full account of the very different circumstances of agriculture in the two settings; but careful measurement by Robert Allen of the inputs and products of English farms, combined with Bozhong Li's analysis of the Jiangnan farm economy, suggests that farm productivity, measured in terms of working days per calorie-equivalent of grain, was comparable as well. These data do not support Huang's assertion of a longterm tendency towards falling labor productivity in the Chinese rice economy.

Third, the substantial progress that has been made in Chinese historical demography in the past decade effectively eliminates the crude Malthusian interpretation of Chinese population behavior. There was no unconstrained tendency towards population increase up to the carrying capacity of the land; instead, fertility rates and rates of population increase were essentially comparable to those of European populations. This finding too casts doubt on the involution hypothesis, since unrestrained population increase is the central causal mechanism that was hypothesized to push the process of involution.

These findings sound “final”; but, as Robert Allen emphasizes, the quality of the economic data that is available for measurement of productivity and real wages in Asia remains sketchy and questionable. The best evidence available today supports the summary conclusions rehearsed above; but it is also possible that subsequent research will call some of these specific findings into doubt.

What remains unresolved in the debate is the large causal question: what accounts for the “Great Divergence” between Western Europe and East Asia in the seventeenth and eighteenth centuries? Here the most promising perspective is that of R. Bin Wong, in his insistence on the necessity of pursuing an economic history that does not privilege the “master narrative” of western economic revolution. Instead, we need to attempt to identify the conjunction of circumstances in Western Europe and East Asia—environmental, international, political, demographic—that created the characteristic patterns of development in the two settings. And we need further historical and theoretical research to come to conclusions about the relative importance of a variety of causes of the “great divergence” between England and China around 1800.

8.3 Immiseration or Gradual Improvement in Republican China?

Let us turn now to a related debate that focuses on more recent Chinese history—the status of the Chinese rural economy since roughly 1900. This debate raises some of the same issues, but in a later and shorter period of Chinese economic history: the transition from the final years of the Qing empire into the early decades of the Republican period. Many observers have regarded this period as one of agricultural stagnation, falling real rural incomes, worsening tenancy relations, and increasing rural inequalities. These unfavorable economic developments are often taken as preparing the ground for the successful peasant revolution in China. In the 1980s several economic historians offered substantial criticism of this prevailing wisdom. Arguing from a neoclassical economic perspective, Thomas Rawski (1989), Ramon Myers (1970), and Loren Brandt (1989) have argued that the early Republican economy was more dynamic and forward-moving than this interpretation would suggest. According to these historians, agricultural productivity was rising, rural incomes were improving, and labor markets permitted a degree of social opportunity to the rural poor. These are important and controversial claims; if sustained, they require a significant reevaluation of the state and direction of change of the Chinese rural economy in the early twentieth century.

8.3.1 The Received View

Many observers have regarded the late Qing and Republican period as one of agricultural stagnation, stagnant or falling real rural incomes, worsening tenancy relations and increasing rural inequalities. These unfavorable economic developments are often taken as setting the stage for the successful peasant revolution in China: increasing rural misery gave peasants a strong motive to support a party that promised land reform and a program aimed at improving the lot of the rural poor. Dwight Perkins holds that China's rural economy in the early twentieth century was almost stagnant, with little or no per capita growth in gross domestic product. There was growth of output, but it occurred at essentially the rate of population increase—resulting in stagnant per capita incomes (Perkins, 1975a, 1975b, pp. 121–122). Perkins acknowledges that there was sustained growth in certain modern sectors (e.g. cotton textiles, transport, banking), but reminds us that agriculture and traditional manufacturing dwarfed the modern sector; and he argues that these sectors showed little or no growth (Perkins, 1975a, pp. 120–125). The benefits of modern-sector growth would only be realized in living standard improvement in later decades. Perkins' target is the position that held that living standards were falling during this period (represented by R. H. Tawney, 1966). Against this position, Perkins maintains that the balance of evidence suggests that this was not the case: "the view that the incomes of all or of the vast majority of the people were declining during the first half of the twentieth century is not supported by currently available evidence" (Perkins, 1975a, p. 124). Perkins also makes an effort to assess the direction of change in land concentration, tenancy and income distribution during the period. He holds that tenancy rates remained approximately the same during the period, and he denies that there was an abrupt increase in tenancy or landlessness during the early twentieth century (Perkins, 1969, p. 100).

Another important statement of the received view of the 1960s is Albert Feuerwerker's *The Chinese Economy, Ca. 1870–1911* (Feuerwerker, 1969). Feuerwerker's assessment too emphasizes economic stagnation: "Fundamental economic change and modern economic growth, however, in so far as they have been accomplished in twentieth century China, did not come of their own momentum out of the late-Qing economic system. They were preeminently the by-products of a new and possibly still tenuous political integration which itself was achieved only after decades of political strife, foreign invasion and civil war" (Feuerwerker, 1969, p. 1). Feuerwerker maintains that agricultural techniques remained roughly unchanged throughout the period (1880–1930s), with output increasing in pace with population growth through small increase in cultivated acreage (Feuerwerker, 1969, p. 3). He takes it as certain that rural living standards did not improve throughout the period, but doubts that evidence exists to demonstrate a significant decline in living standards (p. 5). Feuerwerker believes that tenancy rates probably did not increase in the early decades of the twentieth century, and he doubts that effective rent levels increased during the period (p. 14). He thus adopts roughly the same view as Perkins: that output approximately kept pace with population increase, with the result that average rural welfare remained about constant.

Scholarship in the 1970s focused more attention on distributive issues in the rural economy: the status of tenancy, landlessness, wage labor, peasant welfare and rural inequalities. Such authors as Mark Selden, Victor Lippit, Carl Riskin and Joseph Esherick argued that inequalities increased during the period. Mark Selden emphasizes the deterioration of living conditions in Shensi. He details the destructive effects of warlordism and famine in Shensi, and he argues that tenancy in Shensi increased substantially in the 1930s, accompanied by increasing landlessness (Selden, 1971, pp. 7–8). These worsening conditions are a central causal factor in Selden's analysis of the successes of Communist mobilization in Shensi. Likewise, Carl Riskin emphasizes the significance of income and land inequalities in the Chinese rural economy (Riskin, 1987, pp. 24–26). And Victor Lippit focuses attention on the disposition of the rural surplus: through rent, taxation and usurious interest rates the peasant was separated from the surplus available within the rural economy (Lippit, 1974). He argues that incomes based on these sources represented a significant portion of China's national income in the 1930s: rent (10.7%), farm business profits (3.4%) and rural interest payments (2.8%), for a total of 16.9%. Moreover, Lippit argues that, for reasons internal to China's rural elites, these incomes were not devoted to productive investment but elite consumption.

In short, the received view represents the Chinese rural economy as being largely stagnant during the early Republican period. Technological change in agriculture was sparse. Living standards for peasants were stagnant or falling. The main fissure of disagreement within the field concerned the causes of the stagnation. One school of thought (the *technological* school) held that the chief obstacles to development were technological and demographic; population pressure on resources led to an economy in which there was very little economic surplus available for productive investment. The other theory was the *distributional* school, which held that the traditional Chinese economy generated substantial surpluses that could have funded economic development, but that the elite classes used those surpluses in unproductive ways.

8.3.2 Revision

Brandt and Rawski focus their work on Chinese economic development in the late Qing and early Republican periods. They disagree about some issues; but they agree in rejecting many features of the received view. Consider first some of Thomas Rawski's central findings. Rawski argues that economic growth was significant and sustained in pre-war China. It was driven by modernization of transport, factory industry and commercial banking (Rawski, 1989, p. xx). Much of Rawski's book focuses on industrial growth, but he maintains that agriculture expanded in per capita terms as well. He estimates that agricultural growth averaged 1.5%—about 0.5% ahead of population growth. This process of growth led to sustained increase in output and income per capita (Rawski, 1989, p. 268), and this increase led to rising living standards. Rawski provides a new analysis of Buck's data on rural living standards, to support the conclusion that rural welfare was rising during the pre-war period (Rawski, 1989, pp. 287ff). He argues that there is good evidence of rising consumption of cotton cloth, which he takes to support the conclusion that living standards were rising (Rawski, 1989, p. 289). Rawski summarizes his findings relevant to the rural economy in these terms: "This study has produced a variety of direct and indirect evidence of increasing per capita output, income and living standards in large areas of rural China prior to the outbreak of the Pacific War in 1937" (Rawski, 1989, p. 320).

Loren Brandt shares many of Rawski's assumptions (Brandt, 1989). He holds that commercialization progressed rapidly during this period, bringing greater integration between domestic and international markets in rice, cotton, and other important commodities; and that commercialization in turn induced growth in agricultural output, improvement in the agricultural terms of trade, rising real incomes for farmers and laborers alike, and a probable overall reduction in the range of income inequalities in the countryside of central and eastern China. In fact, Brandt draws a parallel between the performance of the Chinese rural economy during this period of rapid commercialization and its performance during the period of the post-Mao rural reforms; in each case, he asserts, the gains were the result of greater market activity and specialization. He maintains that the early Republican period witnessed rising real incomes for farmers and laborers alike and a probable overall reduction in the range of income inequalities in the countryside of central and eastern China. Brandt uses these conclusions about real wages to argue that labor productivity increased between 40 and 60% during the time period (Brandt, 1989, p. 132)—suggesting that the rural economy was improving rather respectably during the period. And he argues that commercialization of the rural economy had the effect of significantly narrowing income inequalities in rural China (Brandt, 1989, p. 138), by increasing the demand and opportunities for labor. Finally, he denies that land concentration was increasing during this period, arguing that the relative share of income flowing to the bottom of the income distribution (tenant farmers, small owner-farmers, landless workers, peddlers, handicraft workers) improved during this period relative to landlords (Brandt, 1989, pp. 169–170).

Brandt's position depends on several premises: his argument for the extensive integration of rural China into the world economy, his argument that rural wages and labor productivity were rising in this period, and his argument that income inequalities probably improved somewhat throughout this period. How convincing are Brandt's arguments for these claims? Here I will maintain that the evidence that Brandt puts forward, while suggestive, falls far short of clinching his case, and the interpretation of the early twentieth century rural economy as static or worsening continues to be more credible.

8.3.3 Price Integration

Brandt makes the strongest case for his first point—China's extensive commercialization and integration into the world economy. Brandt concedes that only a small fraction of China's economy depended on internationally traded goods, but he argues that the small volume was sufficient to link commodity prices to international levels rather than domestic demand. Surveying rice price data for South China, Siam, Burma, India, and Saigon (the latter being the chief rice exporting markets in Asia), he finds that there are high and rising price correlations between South China and each of the major exporting markets (Brandt, 1989, p. 19). And he finds, further, that the interior Chinese economy showed similar integration with respect to rice prices. Without providing comparable detail from other locations, Brandt suggests that these results obtain as well in markets for cotton and wheat—supporting the contention that the Chinese rural economy was highly commercialized, reasonably competitive, and extensively integrated into the international economy.

Brandt's arguments here are fairly convincing. At the same time, this is the least novel portion of the argument; few would disagree with the conclusion that the Chinese rural economy was price-responsive and competitive in the period in question. And the well-documented shock to the Chinese economy produced by the Great Depression—through its disruption of cotton prices—would be unintelligible except on the assumption that Chinese cotton markets were integrated with international prices. (Philip Huang discusses this aspect of Chinese commercialization in *The Peasant Economy and Social Change in North China*; Huang, 1985.) So this line of thought is reasonably well grounded, but does not provide much support for the view that conditions in the countryside were improving.

8.3.4 Output

Let us turn now to a more controversial part of Brandt's argument: his contention that output outpaced population growth during this period (Brandt, 1989, pp. 106ff) and that rural real wages and labor productivity were rising significantly. Brandt argues, contrary to much received opinion, that per capita output was rising in the farm economy during this period: "Between the 1890s and 1930s, agricultural output in Central and East China increased more than two times the estimated rate of population growth of 0.6% per annum" (p. 178)—or in other words, a 1.2% increase in output, accumulating to an increase of 70% over the 45 year period. Is this a credible conclusion? Brandt holds that other interpreters have been misled by the fall in grain commerce flowing from the Middle and Upper Yangzi paralleled by a rise in foreign rice imports (p. 39). He believes that this shift represents a reorganization of Chinese agricultural markets rather than a decline in agricultural product. Because of shifts in international rice prices, South China came to import rice from Indochina and Siam for its urban population rather from than the Yangzi delta (p. 51). But Brandt estimates that this drop in rice trade between the Yangzi and South

China was more than matched by an increase in demand in Yangzi cities, resulting by the 1920s in an increase in demand of more than 20 million *piculs* of rice (p. 53).

This argument, however, depends entirely on estimates of rising demand (through rising urban and non-agricultural populations); it is unsupported by any direct estimate of rice output. Aggregate output is affected by two chief variables: amount of acreage sown and changes in labor productivity. Dwight Perkins judged that productivity remained constant and rice acreage declined between 1914–1918 and 1931–1937, resulting in a decline in domestic rice production of about 5.8% for China as a whole and 11.9% for East and Central China (Perkins, 1969, p. 276). However, these declines are offset by substantial increases in wheat cultivation (Perkins, 1969, p. 250), implying a small net increase in grain production. Brandt disputes Perkins's rice production data, largely on the ground that it is implausible that there was a drop in cultivated acreage in the early twentieth century. However, Perkins's data does not have this implication; Brandt ignores Perkins's data on wheat cultivation showing that wheat acreage increased more than the amount of decline in rice cultivation. And Brandt's positive case is weak, since he does not provide any direct evidence of rising rice output in the region, and (as he himself notes), there are alternative possible explanations that could account for the required increases in rice marketing (p. 54). His case here is unconvincing, therefore; his arguments do not establish that there was an increase of per capita rice output between 1915 and 1936. This does not show that there was *not* such an increase; it may have been so, but the data offered in this study does not establish it.

8.3.5 Real Wages

A crucial part of Brandt's argument is his analysis of farm wages. Brandt argues that real farm wages were rising during the period; that farm wages were closely linked to other forms of employment; and that it is reasonable to conclude on the basis of these points that rural welfare was rising during the period. The data that Brandt employs here take the form of scattered cross-sectional studies of wages for seasonal and long-term agricultural laborers. Upon inspection, this data is insufficient to the task, however. The Royal Asiatic Society compiled wage data for 1888 in fifteen places; there is cross-sectional data for about 700 counties for the 1930s; and the Buck surveys reported time series wage data for about 100 counties in the 1930s. Brandt converts the data from each of these sources into grain-equivalence wages (*piculs* of rice). Between three and four *piculs* of rice are required for subsistence. On the basis of the Royal Asiatic Society reports Brandt concludes that the grain equivalent of the cash component of the annual agricultural wage for the 1880s was about 5 *piculs*; for the 1930s he finds that the corresponding value was between 4.21 *piculs* (Sichuan) and 13.86 *piculs* (Shandong), with a mean of 9.87 *piculs* (Brandt, 1989, Table 5.2, pp. 114–115). This suggest a rough doubling of the rural real wage and an annual increase of about 1.5%—or does it? The argument is questionable.

First, these data sources are not particularly convincing, particularly for the earlier period. The 1888 estimate depends on a very small data set on the basis of which to estimate the level of the wage for rural China (fairly casual observations in fifteen locations, only six of which provide data on the annual wage). And since this data does not allow Brandt to estimate the value of in-kind payments (which were substantial), it is impossible to estimate the total value of the wage. The 1930s data are more extensive but show substantial variance, suggesting that the data is not particularly reliable (it is hard to imagine that the real farm wage—and on Brandt's argument, rural welfare as well—in Shandong would be three times that in adjacent Henan). Here too the problem of the value of in-kind payments arises; if in-kind payments declined in value in the later period (as would be expected with the advance of commercialization), then comparison of changes in the cash component overestimates the increase in the wage. If, for example, the value of in-kind payments declined from 60 to 40% of the wage, then a doubling of the cash wage represents only a 33% increase in the total wage. (Brandt considers the problem posed by in-kind payments, but does not take it seriously enough.) So it is hard to regard these data sets as establishing reasonable estimates of the farm wage for either period; the most they allow us to conclude is that it is unlikely that the real wage fell during this period.

The final source that Brandt analyzes on this topic is the time series data collected by John Lossing Buck in the 1930s (Buck, 1937b, a). This data was collected by a number of investigators in about 100 places in China for the time period 1901–1933. Investigators were asked to collect the recollections of three well-informed villagers in 1933 on the level of the cash farm wage for this time period. Brandt normalizes these cash estimates using his own price index and then computes growth rates for each place surveyed by regressing the resulting real wages against time. He finds a range of positive growth rates for twenty-one out of twenty-nine places, with an average rate of growth for all places of 0.9%. Over a period of 45 years this would result in a 50% increase in the real wage. If taken at face value this is a significant, though hardly startling, improvement in the real wage. However, it is difficult to take this finding at face value. First (as Brandt himself acknowledges), the data themselves are questionable, since they rely on the recollections of observers over a 30 years lapse of time. Second, this data reports only the cash component of the wage; so if there was a decline in the value of in-kind payments, this data will overestimate the rate of increase in the total wage. Finally, other researchers have arrived at substantially lower estimates of growth on the basis of the same data. Thomas Rawski analyzes the same data using the same regression technique but a different price series; his estimates for the provinces included in Brandt's study (Jiangsu, Zhejiang, Anhui, Jiangxi, Hupeh, and Hunan) imply average growth rates of –0.03% (1901–1933), 0.43% (1914–1933), and 0.13% (1925–1933). Aggregating these rates over a 45 years period, these values imply a *fall* of 1%, a rise of 21%, and a rise of 6% depending on the time period considered.[2] In the best case, then,

[2]Rawski (1989). Rawski too concludes that real wages were rising during the period, but more slowly than Brandt's estimate; he suggests an average annual rate of increase of about 0.4%.

Rawski's analysis implies a growth rate less than half that estimated by Brandt; in the worst case his data implies a slight drop in the real farm wage in East and Central China.

There are enough uncertainties in these calculations of the behavior of the real rural wage, therefore, to make Brandt's conclusion that the real wage was rising significantly largely unconvincing; it may have been so, but this data does not establish the point. If anything, reconsideration of this data appears to imply that any increase in the rural real wage was less than 0.5% per year over the 45 years period in question, and may have been zero. Rawski's estimate of an average rate of growth of the real wage of about 0.4% is more credible on the basis of this evidence; but the uncertainty of the available evidence affects his conclusion equally severely.

We might also consider what implications a slow rise in the real farm wage (if established) would have for the state of rural welfare. For it is possible for the farm wage to rise slowly while average rural income is falling—if, for example, there is less employment overall, fewer days worked, or a larger pool of unemployed or underemployed rural people. In other words, a slow improvement in the farm wage paid is consistent with the common perception of a general worsening of rural conditions in the first several decades of the twentieth century.

8.3.6 Productivity

What inferences about productivity does this analysis of farm wages permit? Brandt reasons along neoclassical lines: the wage is determined by the marginal product of labor; if wages are rising, we can infer that the marginal product is rising, from which Brandt infers in turn that the average product (a measure of productivity) was rising as well. And in a competitive labor market with few barriers between types of employment, the level of the farm wage ought to be closely correlated with the returns to other forms of labor—with the result that we can conclude that other forms of rural income were rising as well. On the basis of this line of reasoning, Brandt estimates that labor productivity increased between 40 and 60% during the time period (p. 132)—suggesting that the rural economy was improving rather respectably during the period.

Brandt also makes an attempt to provide an indirect estimate of changes in labor productivity by estimating population growth, agricultural labor force growth, and output; this permits him to infer a growth rate in labor productivity (pp. 130ff). Assuming that per capita consumption remained constant, Brandt estimates that labor productivity must have increased 16.5% between 1893 and 1933. This is a figure substantially lower than that implied by his analysis of real wage data (between 40 and 60%)—which might lead one to conclude that the real wage estimates are flawed. Brandt, however, does not draw this conclusion; instead he postulates that output must have risen more rapidly than population increase, leading to rising per capita consumption of rice. And he computes that a 50% increase in labor productivity would correspond to a 63% increase in output—an annual increase of 1.21%. This calculation is the basis for his conclusion that output increased at about double the rate of population increase in the period (0.6%). But note how highly conjectural this line of thought is; it would seem more reasonable to conclude that labor productivity did *not* increase as rapidly as Brandt's wage data implies. And if per capita grain consumption tended to decline during this period—as some observers believe that it did—then even the modest 16.5% increase in productivity disappears; a constant level of productivity implies a fall of 14% in per capita consumption, given the population data that Brandt employs.

A careful reading of Brandt's arguments on these points suggests, then, that the increase in labor productivity, if any, was small, and that Brandt's upbeat appraisal of the improving state of the rural economy during these decades is unsubstantiated.

8.3.7 Distributive Consequences

Turn finally to Brandt's interpretation of the distributive performance of the commercializing Chinese economy. He argues that commercialization of the rural economy had the effect of significantly narrowing income inequalities in rural China (p. 138), by increasing the demand and opportunities for labor. And he denies the common view that land concentration was increasing during this period. He maintains that the relative share of income flowing to the bottom of the income distribution (tenant farmers, small owner-farmers, landless workers, peddlers, handicraft workers) improved during this period relative to landlords (pp. 169–170). However, he provides surprisingly little support for this conclusion, devoting well over half the relevant chapter to a discussion of patterns of farm household behavior across large and small farms. He counts the increases in the rural real wage discussed above as probably raising the lower quintiles of income earners relative to the top quintile; as we found above, however, he appears to substantially overestimate the magnitude of this increase. Second, he doubts the common belief that land holdings became more stratified during this period, and he believes that the terms of tenancy had improved for the tenant by the 1930s, reducing the effective rent from about 50% of output to about 40% (Brandt, 1989, Table 6.20, p. 171)—thus improving tenant incomes at the expense of landlords. And he holds that the increasing opportunities for sideline activities (textiles, refining oils, sericulture, etc.) primarily benefited the poorest strata. These claims do not receive much empirical support, however. Almost all the investigations made in the 1930s suggest the reverse conclusions. For example, his discussion of the data about rural labor, landlessness, and tenancy is unconvincing. Brandt accepts the National Land Commission estimate (1934) that only 1.57% of rural households were pure farm-laborer households; Joseph Esherick (Esherick, 1981) shows convincingly, however, that this figure is substantially too low and argues for an estimate of 8% in this category (based on Chinese surveys and economic gazetteers from the 1930s), and Thomas Wiens reports an average of 10% (Wiens, 1982).[3]

[3] Philip Huang also makes an effort to estimate the extent of hired labor in North China, and arrives at a rough estimate of 14–17% of farm work being performed by hired labor (Huang, 1985).

8.3.8 Conclusion on Brandt

Brandt's arguments for improving productivity, output, real wages, and inequalities are unconvincing, and his view of the Chinese rural economy experiencing substantial improvement in these decades is unsubstantiated. In each case the empirical arguments that Brandt constructs are too soft to justify the strong conclusions that he draws. And Brandt's case is single-minded in its sole attention to available quantitative data on wages, prices, volume of trade, and the like. There is no attempt to buttress or test the economic interpretation that he offers through consideration of more qualitative information that is available concerning the state of the rural economy in these years (village studies, travelers' reports, and the like). Many readers will prefer an approach that makes an effort to construct an interpretation of the Chinese economy that balances quantitative and qualitative data; in this regard Philip Huang's work—which Brandt sharply criticizes—provides a better model.

8.4 A Puzzle

There is an apparent tension between the two debates we have considered here that ought to be addressed directly. In the first debate, our analysis supports the "non-involutionary" position of Pomeranz and Wong for the period of 1700–1850. We conclude with Pomeranz and Wong that the rural economy of the lower Yangzi was improving, that the standard of living was comparable to that of the rural population in England, and that the agricultural system was capable of incorporating improvements in technique leading to some rise in farm productivity. In the second debate, our analysis supports the "impoverishment" interpretation of the early twentieth century: farm productivity and output were outpaced by population, the standard of living for peasants and other rural people was falling, and the economic system was falling short of its central challenge of supporting a rising quality of life for its population. Are these conclusions inconsistent? Or are there important historical factors that distinguish between China's economic experience in the early modern period and the early Republican period?

Here it is worth recalling the severity and breadth of the economic, social, and environmental circumstances that China encountered in the first 40 years of the twentieth century. The century from 1850 to 1950 was one of unprecedented hardship and disruption for most of China. The Taiping Rebellion brought widespread devastation to China at mid-nineteenth century, at the cost of millions of lives and great destruction to the economic structure. Rebellion, civil war, and the period of warlordism brought additional destruction to most parts of China; these circumstances made coordinated economic efforts difficult, they interfered with inter-regional economic activity and trade, and they created local insecurity that made even small improvements in agriculture and manufacture difficult. And rampant, extortionate taxation under warlords increasingly impaired the ability of peasant families to satisfy their most basic needs. Further, China experienced severe economic costs in the form of reparations to foreign powers early in the century.

Following the Boxer War, the European parties forced reparations of 450 million taels of silver, and reparations to Japan following the first Sino-Japanese War amounted to payment of 230 million taels—compared to an annual Qing revenue of only 89 million taels. These vast amounts of resources were consequently not available for the project of modernization of China's economy. Finally, China experienced an unusual number of natural calamities during the first part of the twentieth century: changes of course and flooding of the Yellow River, flooding of the Yangzi in 1931 and 1935, and devastating droughts in North China in the 1930s and 1940s.[4]

Given this series of severe challenges to China's economic prosperity—the financial cost of reparations and foreign indemnities, the economic and political disruption created by warlordism in the early decades of the period, the stresses of wartime occupation by Japan beginning in the 1930s, and the cumulative costs of natural calamities—it is unsurprising that the farm economy would suffer and that the rural standard of living would fall. We might regard the "involution" of the twentieth century as a clear example of the contingency of economic history and the crucial role that non-systemic factors play. It was not an underlying "logic of development" that led to China's impoverishment in the first part of the twentieth century, but rather a series of historically contingent and tragic circumstances that combined to bring about impoverishment and decline for China's population.

8.5 Import for Chinese Studies

Why are these debates important for China scholars outside of the precincts of economic history? There are several important reasons. First, it has seemed important to many China historians to arrive at judgments about China's potential for autonomous economic development independent of western intervention. Were there economic institutions and processes at work within China's domestic economy in the late Qing that might, in other circumstances, have led to a process of modernization and change? Or was China caught hopelessly in a high-level equilibrium trap, from which it could be liberated only through some exogenous shock (Elvin, 1973)? Much of the import of Rawski's book is the conclusion that there were powerful processes of modernization and growth already at work in China in the 1880s. This conclusion supports a counterfactual historical judgment: if China's domestic and international circumstances had been somewhat different; if the Qing had survived in a reformist mode, or if the Republican revolution had installed an effective national government; if China had not been invaded by Japan; if China had not been drawn into civil war and the warlord era—then China might well have developed into a modernizing market economy. This conclusion is sympathetic to

[4]Conversations with Bozhong Li and his generous sharing of an unpublished manuscript permitted me to see the importance of the circumstances described in this section for interpreting the performance of China's rural economy in the early twentieth century.

those who offer a "China-centered" approach to the study of China (e.g. Cohen, 1984).

A second reason these debates should be of interest to China historians more generally has to do with the causes of the Communist revolution. Our construal of the Chinese Communist Party's successes in rural mobilization and ultimate seizure of power depends a great deal on our assumptions about the material welfare of the rural population. If things were bad and getting worse, then mobilization is easy to understand. If the economy was generally improving and if the results of improvement were being experienced as a generally rising standard of living, then we cannot cite immiseration as a cause of the revolution. And if (as Rawski and Brandt believe) the processes of commercialization and the extension of ever-more-efficient markets were undercutting the forms of pre-capitalist exploitation that existed in rural China (extortionate rents, bonded labor relations), then we cannot explain the success of mobilization as the consequence of the Chinese peasantry's willingness to challenge an exploitative and worsening social order. If, on the other hand, this benign view of the neoclassical school is unpersuasive, then the immiseration and worsening inequalities interpretation remains salient for our interpretation of the success of rural mobilization strategies.

One important result of study of these important current debates about China's economic history is this. Let us consider China's historical development—economic, agricultural, political, social, military—in its own terms, but informed by the best available social theoretical insights and concepts; let us identify China's own "paradigms" of development, its own pathways of political development and economic change; and let us use those new-found paradigms to inflect our understanding of the processes of other parts of the world. Finally, let us recognize that the hypotheses of social theory takes us a ways down the road of being able to explain particular pathways of historical development in a variety of contexts; but social theory does not permit us to make confident predictions about uniquely determined outcomes. In place of the overtones of inevitability—population increase, technological change, improvement in agricultural productivity—we get more nuanced narratives of diversity and contingency, and the recognition that historical outcomes are under-determined by any particular and limited set of causal factors. And in fact, Wong, Lee, and Pomeranz show that careful comparative study of the economic histories of different regions of Eurasia will establish this plasticity of outcome. For example, Wong carefully assesses the literature on proto-industrialization in Europe; finds that very similar processes of rural manufacture are present in both Europe and China; and argues that the causes of European "breakthrough" must therefore be sought elsewhere. More generally, he argues that similar processes of commercialization and population dynamics are associated with very different paths to (or away from) industrialization (Wong, 1997, pp. 46–47).

The comparative studies of Europe and China that are central to the involution debate invite us to reflect on the question of the role of social theory in historical inquiry. Wong recognizes that reliance on current social theory is inescapable in historical analysis (what else would provide the analytical basis for comparison and hypothesis?), but he emphasizes the importance of doing so with care and critical intelligence. As Susanne Rudolph puts the point, “At this stage we need fragile theoretical templates, made of soft clay rather than hard steel, that adapt to the variety of evidence and break when they do not fit” (Rudolph, 1987, p. 738). Crucially, Wong insists on the point that the researcher must be critical in extending ideal-typical concepts of structures and processes from the European context to an Asian context. More acutely, we need to find new ideal-typical configurations of institutions and processes in Asia (and other world civilizations), to add depth to our understanding of European history. Finally, Wong, like other scholars, emphasizes the plasticity of large historical developments. There are multiple contingent factors involved in any large historical process, and there is room for choice by agents at all points along the way.

Chapter 9
Mentalités

What role do socially shared ideas and identities play in historical causation? Large-scale historical causation commonly involves objective factors such as climate, demography, and natural resources; it involves as well reference to social structural factors such as political institutions, cities, or transportation networks. Is there a rigorous meaning to be assigned to the notion of "mentalité"—a broadly shared set of ideas, representations, and values within a given people? Do subjective factors such as paradigms, practices, or moral systems influence historical change? Is there such a thing as a "mentalité" of a people, group, or nation? Take a group of young people at an Iowa potluck supper and a group of young traders at the Chicago Board of Trade—is there a midwestern mentalité that they can be said to share? What factors might be comprised by such a concept? What forms of variation must we expect within a group sharing a mentalité? And what are the social mechanisms through which these hypothesized forms of shared experience and thought are conveyed?

This chapter will provide a contemporary account of the nature of "mentalité" and social identity and the legitimate role these ideas play in historical explanation. It will explore the question of how mentalités and identities are embodied in an individual and a population. In what do identities consist? How are they created and sustained? How do mentalités and identities influence history—what are some of the causal pathways (microfoundations) through which such a formation can influence historical outcomes? And the chapter will examine several important examples of historical explanation that turn on reference to socially shared ideas, norms, and practices. The practice of insightful historians in their treatment of factors such as these can go a long ways towards providing illuminating answers to these foundational questions.

Before we can confidently judge that mentalités play an important role in historical change, we need to raise the question of historical rigor: are the phenomena of mentalité, identity, and moral frameworks discrete and stable enough to admit of rigorous historical investigation? Can we be specific enough about a given topic of inquiry in this area to permit us to formulate achievable research goals? Are there historical or contemporary data that will permit us to recognize and track distinctive socially embodied mental systems over time?

D. Little, *New Contributions to the Philosophy of History*, Methodos Series 6,
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Here I will make use of the "microfoundational" approach to historical analysis that is discussed in Chapters 3 and 5 in connection with such things as historical causation, causal mechanisms, and everyday social action (Little, 1994, 1989, 1998). And I will attempt to formulate a series of questions and assumptions that may permit us to attempt to begin to analyze social identities and mentalités in these terms. I will also connect this set of issues to the interpretivist approach to the human sciences: the idea that understanding human actions requires interpretation of the meanings that actors assign to their doings.

9.1 Mentalités in Historical Inquiry

Most fundamentally, a mentalité is thought to be a *shared way of looking at the world and reacting to happenings and actions by others, distinctive from other groups and reasonably similar across a specific group*. A mentalité is a socially shared mental system. This characterization folds together a number of things: cognitive frames for understanding the world, values and norms around which one organizes one's actions, and a repertoire of reactions and responses to scenarios in the world. And all of this comes together in the form of a signature form of consciousness and behavior. A mentalité shapes the individual's experience of the world, and it provides a specific foundation for one's choices and actions as events in one's world unfold. And a mentalité is thought to be shared across a social group, so it is not simply a set of individual and idiosyncratic mental attitudes. Finally, a mentalité is historically significant in two related ways: it is an important fact about the people of the past; and it is a causal factor that influences actions and can therefore enter into historical explanations.

Historians of the *Annales* school gave special attention to the task of reconstructing the mentalité of people and groups of the past. Emile Durkheim's ideas about the social world lie in the background in the emphasis offered by Marc Bloch or Jacques Le Goff on this aspect of history's tapestry. Durkheim regarded the morality of a people as a social fact about them that is causally relevant to other collective facts (Durkheim, 1938, 1966; Lukes, 1972). The founders of the *Annales* school—Bloch and Febvre in particular—were deeply influenced by Durkheim, and their basic terms of analysis of historical reality included the ensemble of historically specific ideas, values, and modes of thinking that characterized a given population at a given time (Friedman, 1996). In this historical ontology, the mentalité of a population was an operative historical factor, and it was amenable to historical investigation. Emmanuel Le Roy Ladurie, for example, sought to capture the mentalité of the peasants of Montaillou in his book of that title, offering substantial commentary on their attitudes towards death, sex, and religion (Le Roy Ladurie, 1979b). Lawrence Stone writes of Le Roy Ladurie's "sheer brilliance in the use of a unique document to reconstruct in fascinating detail a previously totally unknown world, the mental, emotional, sexual, and religious life of late thirteenth-century peasants in a remote Pyrennean village" (Stone, 1979). And the sorts of features of "worldview" that are often invoked in describing a mentalité include superstition and magical beliefs as well as more ordinary modes of thinking and understanding. (André Burguière's *Annales School* is an important current source on this subject; Burguière, 2009.)

Several questions are pressing when we consider this concept. First, is the governing idea of underlying variation of worldviews across cultures and times valid in any non-superficial sense? Trivially, of course, we recognize that tastes and morés vary across places and cultures. This was one of Montesquieu's insights (Montesquieu, 1989). But is there a more fundamental way in which Scots experience the world differently from Basques or Yoruba? Or are the differences associated with tastes and manners simply an overlay that sits on top of a more fundamental human similarity? This question pushes us towards the debate between advocates of "human nature" against the "historicists," according to whom the most basic features of human cognition and action are contingent and historically shaped.

It seems credible that even fairly deep aspects of cognition and behavior are historically and culturally variable. Deep aspects of "human nature" are plastic and subject to historical construction. It is evident that much of an adult's mental makeup is the result of his/her history and the enveloping culture within which the individual has developed. Learning is a fundamental aspect of human life, and it occurs at virtually every level. Modes of reasoning, self-control, willingness to cooperate with others, and definition of the appropriate distance of separation between two people in a conversation are all human performances that are culturally and individually variable. They are the outcome of social learning. Further, human culture fundamentally influences human behavior—and culture is only transmitted through lived experience. This leaves it open that there may be elements of common human mental life, but it also leaves the field clear for historians and ethnographers to attempt to tease out very specific and distinguishing features of mental life and action. (Clifford Geertz argues strongly for this plasticity in many places; for example, in his careful study of Balinese identity; Geertz, 1983.) So this supports the idea that the notion of a mentalité is consistent with current understandings of social psychology and mental life.

Second, we need to reflect upon the ways in which possession of a mentalité should be expected to vary across individuals, places, and cohorts. Heterogeneity and variation are fundamental aspects of the social world (Chapter 3), and this surely applies to mentalités no less than social structures. We should expect variation, since every human attribute comes in a range across a population—and even more so for learned traits. So if we think that a mentalité comprises a cognitive framework, a value system, and a set of expectations about behavior, we should also expect that there will be a range of ways in which these items are instantiated in different people within the same group.

Third, we need to attempt to trace out some of the mechanisms through which a mentalité is reproduced and maintained across generations and places. We need an account of the microfoundations of mentalité. We have already sketched some of these mechanisms in earlier chapters. But the fundamental idea is that there is a range of institutions through which children and young people acquire mental skills and content, both formal and informal—schooling, religious education, family practices, and local traditions, for example. This is a fundamental axiom of the approach described above as “methodological localism” (Chapter 3). So for there to be a persistent mentalité for a population, there must be a reasonably consistent social and cultural system across the population that transmits this ensemble of items. And sociologists and historians need to be able to uncover some of the specifics of these institutions.

9.2 Components of a Mentalité

Let us look more closely at the presumed composition of a mentalité. A group’s mentalité has several dimensions. It is thought that a mentalité is a thick fact about the individual: it colors his interpretation of the world around him (*cognitive*), it affects his behavior (*behavioral*), and it constructs the stories that he tells about his people (*narrative*). It involves the ways in which one characterizes oneself, the affinities one has with other people, the ways one has learned to behave in stereotyped social settings, the things one values in oneself and in the world, and the norms that one recognizes or accepts governing everyday behavior. And it profoundly affects the ways we behave and respond to the world.

So a mentalité invokes a number of different areas of psychological competence: knowledge, motivation, perception, memory, personality, and emotion, to name a few. And yet one’s mentalité seems to stand apart from any of these psychological concepts singly. Cognitive psychology focuses on some aspects of this mix; social psychology and personality psychology focuses on other aspects; but there is no area of psychology that attempts to capture all of “social identity” as a psychological real process or structure.

Moreover, a mentalité is embodied in an individual; and yet it is produced by the experiences we have in relations to other individuals and groups. A mentalité can be said to be a feature of a group or a community as much as it is a feature of particular individuals within a given community. And this fact is causally important: we cannot explain the individual’s mentalité without reference to the sustained and fairly consistent features of the group with respect to its mentalité. So a mentalité has an aspect of “social-ness” that cautions us against a narrowly psychological interpretation of the concept.

Let us begin with a few assumptions about what we mean by a person’s mentalité. We might single out a number of aspects of a mentalité as a psychologically real construct, embodied in a particular person through a particular body of experience and a specific location within a community:

- an epistemic frame in terms of which I understand the social world
- an element of my psycho-cognitive-emotional apparatus
- a model of how to behave in certain common social settings
- a self-ascription defining the features of action and comportment that are most defining of “me” in the world

- a self-valorization of the things that are most worthwhile to me
- an account of who I am related to and similar to; who my affinity groups are
- a map expressing my location within a particular extended community

A mentalité is thus a composite of the individual's mental state. It is a *concrete psychological reality*: moral framework, social ideology, affinities and allegiances, worldview, emotions, norms and values. Each of these can be investigated in substantial detail. Second, one's mentalité has much to do with *narrative*: the stories we tell to say "who we are," the stories we tell about who "our" people are. These narratives are flexible and influential for our actions and choices, and the actions in turn fold into the continuing narrative.

Third, a mentalité is to some extent motivational or behavioral: persons sharing a mentalité have some common motives; some level of preparation for cohesive action; and a common set of assumptions about the world that encourages similar behavior. This is what makes mobilization around affinity groups a feasible strategy for political activity. These complexes of values, beliefs, and traditions influence action and behavior (e.g., traditions of solidarity among miners), so mentalités can have significant historical effects. The role of mentalité in creating qualities of sociality—altruism and other-concern, loyalty, solidarity and fairness—is crucial for social behavior. These qualities differ consistently across communities and across time. These social action features derive from both theories of how things work and from norm and value assumptions.

So far we have analyzed a mentalité as an individual characteristic. But there are important similarities in these sets of mentalité features in individuals in a time and place, because of common experiences, common institutions, and common historical settings. This is what makes mentalités social rather than purely idiosyncratic and individual. Putting the point over-simply: individuals develop through the experiences they have with people and institutions; commonalities in these experiences should give rise to common features of mentalité. Some of these similarities correspond to common experiences of oppression (race, gender); others are durable but arbitrary traditions of taste and practice (Alsatian, Breton).

Finally, many observers have noted that there is a performative aspect to a mentalité. By ascribing to oneself a particular mentalité, one is also disposed to behave in accordance with this identity. The self-referential aspect of mentalité is important for the explanation of behavior and agency. If I am a Welsh miner and I learn that "miners stick together," my own character may take on this feature—even if I also have the capacity for timidity. Thus the identity I come to possess in turn affects the development of my individual personality, which in turn influences my dispositions to behavior.

9.2.1 Interpretation of Historical Actors and Behaviors

It is apparent that the idea of a mentalité invokes many of the issues that have been central to the *verstehen* approach to the human sciences (Outhwaite, 1975; Ringer, 1997). History consists of human actions. And human actions require interpretation. We might say most generally that an "action" is an event of individual behavior that derives from a person's mentalité and purposiveness. And the facts about an individual's mentalité that can underlie an action are diverse: purposes, goals, allegiances, passions, features of identity, a sense of history, and aspects of role self-ascription, for example.

If we take the view that social outcomes are ultimately the result of the actions of individuals, then we plainly need to have a more nuanced and satisfactory framework of analysis within which to understand "action". Rational-choice theory is one such framework; Aristotelian theory of deliberation is a somewhat broader framework. But it is plain that the origins, motives, dynamics, and meanings of individual actions are broader and more heterogeneous than these rational-intentional theories would suggest. Purposive action is an important part of the story of social action—but it is only a part.

So what is involved in interpreting historical actions? The wide range of possible mental contexts of behavior means that the task of interpretation is a challenging one. The intellectual task of interpretation is to arrive at an understanding of the agent's behavior as action. This means arriving at a theory or construction attributing mental states to the actor that come together in such a way as to produce the action that was performed. Perhaps we might interpret former President Richard Nixon's final year in office as the resultant of several distinct mental activities and states: self-deception, rational calculation, an emotional unwillingness to be defeated, and a degree of weakness of the will. Or consider Emmanuel Le Roy Ladurie's interpretation of the actions of dozens of people in a massacre in Romans in 1580 (Le Roy Ladurie, 1979a). The actions seem *prima facie* incomprehensible; so the historian's task is to arrive at an interpretation of the beliefs, impulses, group dynamics, and practices that existed at the time in the context of which the actions "make sense" (Ricœur and Thompson, 1981).

Making sense of the human world has always been a part of the continental tradition in philosophy. History and meaning are subjects that have played central roles in continental writings relevant to the project of the human sciences for three centuries, and dozens of philosophers have focused on these and related topics in deeply fertile ways—Kant, Rousseau, Hegel, Montesquieu, Vico, Herder, Schleiermacher, Fichte, Feuerbach, Marx, Nietzsche, and Dilthey, to name an important dozen. So continental philosophy of social science has much to draw upon as a resource for the philosophy of history (Sherratt, 2006).

Several strands of thinking have been particularly important in the continental tradition of the philosophy of the human sciences. First is the idea that the human world is a world of meanings and relationships. Human action is meaningful for the agent, and it is meaningful for the other humans who are affected or observe it. So an important part of understanding the social world is interpretation of the created meanings of actions, expressions, and artifacts. This line of thought brings us into the hermeneutic tradition, from Dilthey to Ricoeur, and the range of efforts in philosophy, theology, criticism, and psychology to provide a basis for interpretation.

A second pillar of thinking in this tradition is the crucial role of history in human affairs. History matters; it is through history that humanity makes itself, and central social creations are the product of long historical evolution—the state, language, religion. Vico and Herder offer good examples of this approach, and Hegel offers another. The philosophy of history is core to Hegel's thinking—not only in his lecture notes on the philosophy of history but the *Philosophy of Right* and the *Phenomenology of Spirit* as well.

On the historicist approach, all social action is framed by a meaningful social world. To understand, explain, or predict patterns of human behavior, we must first penetrate the social world of the individual in historical concreteness: the meanings he/she attributes to her environment (social and natural); the values and goals she possesses; the choices she perceives; and the way she interprets other individuals' social action. Only then will we be able to analyze, interpret, and explain her behavior. But now the individual's action is thickly described in terms of the meanings, values, assumptions, and interpretive principles she employs in her own understanding of her world.

And hermeneutically minded philosophers emphasize a crucial point: human actions reflect purposes, beliefs, emotions, meanings, and solidarities that cannot be directly observed. Human actions and practices are composed of the actions and thoughts of individual human actors—with exactly this range of hermeneutic possibilities and indeterminacies. So the explanation of human action and practice presupposes some level of interpretation. There is no formula, no universal key to human agency, that permits us to "code" human behavior without the trouble of interpretation. This is the meaning of the "cultural turn" in the historical social sciences: the idea that it is necessary to look for more nuanced interpretations of historical actors and their mental lives, if we are to have adequate explanations of their behavior (Sewell, 2005, Chapter 1).

This said, it is incorrect to imagine that the interpretive approach is inconsistent with the causal or rational-intentional approach. Rather, these approaches are compatible and complementary. It is a fact that human action is meaningful and intentional, and all social science must take account of this fact. But it is also true that actions aggregate to larger causes and they have effects on social outcomes. Meaningful, deliberate action is often the mechanism through which a given set of institutional arrangements (a property system, say) cause a social outcome (slow investment in new technologies, say). So meanings are themselves causes and causal mechanisms (a point that Donald Davidson makes in the case of individual action; Davidson, 1963). There is nothing incompatible between an understanding of history that gives focused attention to the workings of institutions and structures, and the behavior of actors within these forces; and one that focuses more closely on the meanings and mental frameworks that guide the actions of the actors.

Another important strand of thought that is relevant to the interpretations of socially specific systems of knowledge and behavior is the work of the ethnomethodologists, and Erving Goffman in particular (Goffman, 1980, 1974). Goffman begins with the intuition that there are patterns in the ordinary social interactions between individuals in various societies. Whether and how to greet an acquaintance or a stranger, how close people stand together, how loudly people speak, what subjects they turn to in idle social conversation, how conflict is handled—all of these topics and more seem to have specific and nuanced answers in various specific social environments. And it seems likely enough that there are persistent differences at this level of social behavior across cities, gender, race, and class. Here is one of Goffman's descriptions of his goals, couched in terms of local rules of conduct:

> By and large, the psychiatric study of situational improprieties has led to studying the offender rather than the rules and social circles that are offended. Through such studies, however, psychiatrists have inadvertently made us more aware of an important area of social life—that of behavior in public and semi-public places. Although this has not been recognized as a special domain for sociological inquiry, it perhaps should be, for rules of conduct in streets, parks, restaurants, theaters, shops, dance floors, meeting halls, and other gathering places of any community tell us a great deal about its most diffuse forms of social organization. . . . Sociology does not provide a ready framework that can order these data, let alone show comparisons and continuities with behavior in private gathering places such as offices, factory floors, living rooms, and kitchens. To be sure, one part of "collective behavior"—riots, crowds, panics—has been established as something to study. But the remaining part of the area, the study of ordinary human traffic and the patterning of ordinary social contacts, has been little considered. . . . It is the object of this report to try to develop such a framework. (Goffman, 1980, pp. 3–4)

The school of ethnomethodology attempts to provide this kind of detailed observation and description. This approach is illustrated, for example, by Harold Garfinkel's descriptions of the procedures embodied in the practices of professional accountants or lawyers (Garfinkel, 1967). A major objective of the method is to arrive at an interpretation of the rules that underlie everyday activity and thus constitute part of the normative basis of a given social order. Research from this perspective generally focuses on mundane forms of social activity—e.g. psychiatrists evaluating patients' files, jurors deliberating on defendants' culpability, or coroners judging cause of death. The investigator then attempts to reconstruct an underlying set of rules and ad hoc procedures that may be taken to have guided the observed activity. The approach emphasizes the contextuality of social practice—the richness of unspoken shared understandings that guide and orient participants' actions in a given practice or activity.

One feature that stands out in the work of Goffman or Garfinkel is the commitment to careful, detailed observation and description of social behavior. They are interested in capturing the nuances of ordinary behavior, and their research gives a great deal of emphasis to the importance of providing detailed descriptions of ordinary social interactions.

But we can also discern a second scientific objective at work in these kinds of writings—the goal of arriving at explanations of the patterns of behavior that are uncovered through this micro-descriptive work. Any body of phenomena that demonstrates consistent patterns over time is potentially of scientific interest, because the observable patterns imply an underlying causal order that ought to be discoverable. And this is the more true if there are stable differences in the patterns across contexts. If there are very specific patterns of behavior in these mundane situations of social encounter, how are we to explain that fact? What sort of structure or fact could count as a cause of these patterns of behavior?

One particularly appealing approach to explanation in these circumstances is to make an inference from behavior to rules that is familiar from Noam Chomsky's view of generative linguistics (Chomsky, 1965)—from patterned behavior (performance) to the underlying "grammar" or system of rules and mental paradigms that produces it (competence). So we might go a bit beyond Goffman's own description of his work, and say that his detailed descriptions of social behavior invite him to reconstruct the underlying and psychologically real set of rules that "generate" the behavior. Here we are invited to consider the social actor as possessing a "grammar" of ordinary behavior that guides the production of actions in specified circumstances. This interpretation of the intellectual project of this work seems consistent with Garfinkel's approaches. And this brings us back to the idea of a mentalité—an underlying and real feature of the mind that mediates the person's relationship to the social world.

9.2.2 Darnton, The Great Cat Massacre

There is a fundamental problem for historiography that is raised by the idea of a mentalité: how would we investigate these "subjective" facts about a long-vanished people? How can we confirm the notion that a population possesses a mentalité? How would we support a claim like this: "medieval villagers of the *Vosges* possessed a mentalité that distinguished them from their modern counterparts and their contemporaries in other regions"? There are several answers we might give. For example, we might we might imagine a contemporary sociologist using some of the many-country surveys of values such as the World Values Survey (Inglehart et al., 2008) as a basis for judging that French and Italian people in 1960 possessed significantly different moral frameworks with respect to certain subjects. Or we might turn to some of the tools of ethnography to get at the thoughts of actors in the past. On this approach we might use some of the tools of ethnography, semiotics, and hermeneutics in attempting to interpret the artifacts, ceremonies, and behavior of people of the past. Robert Darnton's work illustrates the second possibility.

Twenty-five years ago, Darnton offered a highly original perspective on historical understanding in his book, *The Great Cat Massacre: And Other Episodes in French Cultural History* (Darnton, 1984), and the book still warrants close attention. He proposes to bring an ethnographic perspective to bear on historical research, attempting to arrive at nuanced interpretation of the mentalités and worldviews of ordinary folk in early modern France. (Significantly, Darnton collaborated with Clifford Geertz at Princeton, and the influences seem to have run in both directions.)

Darnton attempts to tease out some of the distinguishing elements of French rural and urban culture—through folklore, through documented collective behavior, or through obscure documents authored by police inspectors and bourgeois observers. He is "realist" about mentalités; and he recognizes as well the plasticity and variability of mentalités over time, space, and group. ("I do not believe there is such a thing as a typical peasant or a representative bourgeois"; Darnton, 1984, p. 6.) And he is more interested in the singular revealing incident than in the large structural narrative of change; he demonstrates that careful historical interpretation of a single puzzling event can result in greater illumination about a historical period than from more sweeping descriptions and narratives. (This view parallels that attributed to Simon Schama in Chapter 3).

Darnton does not accept the notion that "good" social history must be quantitative or highly "objective"—that is, neutral with respect to perspective. Rather, he sees the task of a cultural social historian as one of uncovering the threads of voice and action that permit us to reconstruct some dimensions of "French peasant worldview" and to see how startlingly different that worldview is from the modern view. Our distance from the French peasant is great—conceptually as well as materially. So the challenges of uncovering these features of agency and mentalité based on very limited historical data are great.

In the title essay of the volume Darnton goes into a single incident in detail: the autobiographical account of Nicolas Contat, a printer's apprentice (later journeyman), in which Contat describes an episode of cat killing by the apprentices and journeymen in the shop. Darnton relates the incident to its cultural and social context—the symbolic role that cats had in festivals in the countryside, contemporary attitudes towards violence to animals, the sexual innuendo represented by killing the mistress's cat, the changing material relations between master and worker in the eighteenth-century trades. Darnton offers a "thick description" of this incident, allowing the reader to come to a relatively full interpretation of the significance of the various elements of the story. (This concept draws on Clifford Geertz's celebrated essay; Geertz, 1971.) At the same time, he sheds light on the background mentalité and social practices of workers and masters. So the essay is a paradigm of interpretative cultural history. Darnton describes his work in these terms:

> It might simply be called cultural history; for it treats our own civilization in the same way that anthropologists study alien cultures. It is history in the ethnographic grain.... This book investigates ways of thinking in eighteenth-century France. It attempts to show not merely what people thought but how they thought—how they construed the world, invested it with meaning, and infused it with emotion. (Darnton, 1984, p. 3)

Darnton implicitly considers whether this incident should be considered an instance of class resistance—that is, whether we can see the germs of class struggle in this complex moment. And his general perspective is that such a reading would be reductionist and anachronistic. There is resistance in this incident; there is sharp hostility between shop workers and the master and his family; but the resistance and the resentment are thematized around more specific grievances and patterns than the class struggle story would suggest. (It may be that we could better relate Darnton's reading of the incident to Scott's "everyday forms of peasant resistance," emphasizing as it does the role of humor and undetectable violence; Scott, 1985, 1990.) The workers' conduct in this incident is not aimed at overthrowing the master, but at imposing an episode of pain and celebrating a moment of riot.

The notion of "reading" runs through all the chapters, for Darnton suggests that one can read a ritual or a city just as one can read a folktale or a philosophic text. "The modes of exegisis may vary, but in each case one reads for meaning—the meaning inscribed by contemporaries in whatever survives of their vision of the world" (Darnton, 1984, p. 5).

The analysis of folk tales is just as rewarding. Darnton offers a content analysis of the folk tales collected by several generations of folklorists. He disputes the psychological interpretations offered by Fromm, Bettelheim, and others, most convincingly on the grounds that they fail to pay close enough attention to the narrative content and known historical context of the stories. Instead, Darnton offers an interpretation of the world and worldview of the peasant storytellers who invented and repeated these tales: the omnipresence of hunger, the hazards of life on the road, the burden of children in poor household. He shows that there is great consistency in the narratives of these stories over many generations—and also there are national differences across German, French, and English versions of the stories.

Darnton's work in this book is valuable for the philosophy of history in several ways. First, it exemplifies a different model of historical knowledge: not a series of events, not a cliometric analysis of society and class, but an interpretation of moments and mentalités in a fashion designed to shed light on the larger historical moment. It is an effort to make historical understanding "ethnographic." Second, it possesses its own form of rigor and objectivity. The facts matter to the interpretations that Darnton offers—the facts of the multiple versions of folk stories, the facts of what we know about the changing circumstances in the printing trades, the facts of peasant hunger at several periods in the seventeenth and eighteenth centuries. And third, it has the potential for shedding deeper light on French popular action than we are likely to gain from a traditional "rational actor" or class-conflict approaches. The motives that Darnton discerns among the printers are sometimes goal-directed; but sometimes emotional, and sometimes related to the simple recklessness of young men in constraining circumstances.

Finally, Darnton's work here provides some specific insights into questions about the historical study of "mentalités". Darnton shows that it is possible to make significant headway in the project of figuring out how distant and illiterate people thought about the world around them, the social relations in which they found themselves, the natural world, and much else. The documents available to us in the archives have a richness that speaks to these ways of thinking the world; it is therefore a valuable task for the historian to engage in piecing together the details of daily life and experience that the documents reveal and conceal.[1]

9.3 How Is a Social Identity Created and Reproduced?

If mentalités are historical realities, then we need to be able to answer some very concrete questions about them: What are the causal foundations that reproduce and sustain this cluster of items in social consciousness? What are some of the normative and coercive elements that gain consent around the behaviors associated with the mentalité? What is involved in the "making" of a group mentalité? What is involved

[1] Stephen Greenblatt brings his own expertise as a literary theorist and critic into a similar effort to understand historical sensibilities of Elizabethan writers and audiences in *Will in the World* (Greenblatt, 2004).

in sustaining it? Are there corrective mechanisms that constrain random drift of elements across time, space, and groups? A person's expression of a group mentalité is the result of a personal series of experiences, emerging from concrete and historically specific institutions and circumstances. So it should be possible to arrive at empirical and historical theories of how interactions and institutions combine to create embodied "mentalités" in young people.

In other words, we need an analysis of the mechanisms of transmission and maintenance that serve to proliferate a mentalité through a population and across generations. What are the processes through which individuals and groups acquire their identities? There are some obvious mechanisms—personality development within the family, exposure to values and identities in schools and faith institutions, exposure to identity commitments through the media and the Internet. But it would be very useful to have more focused and detailed studies of the ways in which identities are transmitted at the level of the developing individual.

Where do these elements come from for the individual? Through learning and lived experience. One's rich and intimate experience of living with others—family, friends, neighbors, co-workers—who possess values and who tell stories about "who we are" is a thick form of personal development. And one's own experience of the values and emotions of others—the experience of racism and discrimination if one is black and gay—is a powerful catalyst for shaping one's view of the world. This experience shapes one's values, sense of justice, and key memories.

An important existential feature of mentalité formation has to do with the relationship between the individual and a social network of interaction among people bearing this identity. The individual is offered examples of good behavior and thought by others within his/her social network, and the individual is quietly rewarded and punished by others within the social network on the basis of the degree of fit between behavior and group expectations. Thus there is an aspect of "ascriptiveness" to many identities. Rachel has her specific identity as a Russian Jew, in part, because others attribute the identity to her and create a regulative scheme that affects her behavior through example, incentive, and punishment. When she acts "out of role," it is likely enough that she will be sanctioned in one way or another.

These points make it clear that a mentalité is *socially constructed*: it is informed and shaped by the actions of others, and it is partially constituted by regulative categories expressed by others (Hacking, 1999). Specific institutions contribute to the socialization described here; education, socialization, and maturation are concrete social processes. And an important insight of Marxism and feminism arises here: those institutions are "biased" by the social, economic, and gendered interests and assumptions of those who lead and embody them. So it is reasonable to expect that the identity and mentalité elements that emerge will themselves possess some degree of cognitive and normative bias.

These assumptions presuppose a set of processes of socialization and acculturation: during childhood development through which the person absorbs values, cognitive frameworks, worldviews, and dispositions. Each individual arrives at a durable set of values, cognitive frameworks, narratives, and assumptions of commonsense through routine processes of socialization. A normal human raised in typical social settings will internalize values, worldview, assumptions, through routine processes of cultivation, socialization, and language learning. But this process is not deterministic or mechanistic. Different individuals, exposed to the same social, cultural, and political environments, can achieve different configurations of identity elements.

Institutions shape and propel the development of the social psychology of the individuals—young and mature—who pass through them. Important instances include family, schooling, religious institutions, youth networks, military, and media. Schematically, how do these institutions work to shape and influence individual beliefs, frameworks, motives, traditions, etc.? The transmission of values and worldview within the family appears to be a relatively straightforward process, and in traditional societies it is an especially crucial part of socialization. Children acquire a fund of knowledge, practical skills, moral ideas, and dispositions of character from their parents—through example, through discipline and correction, and through routine social interaction. Schools and religious institutions provide the young with another axis of social example and instruction that shapes their knowledge and value sets. Interaction with peers, teachers, and religious authorities provide more opportunities for the young to develop their social grammars, their stocks of commonsense, their values and norms, etc. Pervasive communications technologies—broadcast media, for example—have the capacity to model values, stories, fads, and examples of behavior—which the young "learning machines" incorporate into their ongoing representations of the world and their position within it. Michael Mann emphasizes the importance of paramilitary organizations in creating and maintaining a characteristically fascist mentalité among young men in the societies he studies (Mann, 2004).

Another central empirical fact is that individuals often develop their elements of mentalité in highly common circumstances—work, race, geography, urban landscape, rural circumstances, language. And they do so in interaction with each other in ways that reinforce identity elements. So it is understandable that important and distinctive identity elements emerge—resulting in credible commonalities of "group identity". This helps explain some important macro-identities—gender, Chinese-American, unionist. It also helps explain less obvious facts—the solidarity of miners, the sneaker preferences of Brooklyn teenagers, the insularity of Appalachians.

Important examples of this feature of identity formation include gender, race, and work. Marxist sociologists have given a good deal of attention to the ways in which work environments—factories, mines, or workshops—influence the development of a shared social psychology among the people who work in these environments; (Braverman, 1975; Gutman, 1976). The regulation of the work environment; the alienation created by the work process; the everyday forms of resistance developed by workers (jokes, working slow, petty theft) all have effects on the social psychology of workers.

Gender and sexuality likewise structure the experiences of women and people of a variety of sexual orientations. Visible and invisible codes of conduct encourage and sanction a variety of forms of behavior for people in various of these groupings—with effects both on external behavior and internalized norms and expectations. The common experience of discrimination, coercion, and domination creates a social psychology for members of these groups that influences worldview, norms, and self-expectations.

The fact of racialized treatment of people based on racial attributes creates another set of identity-forming elements of social psychology for members of visible racial groups. The fact of discrimination and unequal treatment, the fact of the threat of racial violence for non-conforming members of racial groups—has evident effects on the social psychology of members of groups defined in these terms. But it is also possible to track the workings of "within-group" socialization—the proliferation of positively marked modes of speech and behavior within the group.

9.3.1 E. P. Thompson, The Making of the English Working Class

E. P. Thompson's *Making of the English Working Class* (Thompson, 1966) represents a tour de force in the concrete, meso-level investigation of the formation of a class. Thompson analyzes "class", but he offers trenchant criticism of the structuralist definition of this concept—the notion that a class is a group of people defined by their shared position within the relations of production.

Whereas other Marxist historians focused particularly on the large structures of capitalism, Thompson's eye was turned to the specific and often surprising details of artisanal and working culture in pre-industrial England, the many ways in which the working people at the bottom of English society conceived of themselves and created their own organizations for education and politics in the last half of the eighteenth century. Neither peasant nor middle class, the many segments of working people in England were socially organized by trade and skill, and with remarkably distinct cultural traditions, songs, and political repertoires. They were not, in fact, a "class". And yet, they became a class—this is the "making" that Thompson's title refers to. (Harvey Kaye's *British Marxist Historians* offers an excellent survey of the major British Marxist historians—Hobsbawm, Hilton, Dobbs, Thompson, and others; Kaye, 1995.)

Several questions are fundamental for our purposes: How does Thompson identify or define "working class identity"? What mechanisms does he identify in the "making"? What institutions or practices stabilize these identities over decades?

Commentators often describe Thompson's central contribution as being the provision of a detailed understanding of "class consciousness" in counterpart to Marx's conception of a "class in itself"—a group of people defined in terms of their relation to the system of property relations. On this line of interpretation, Thompson provided one of the missing links within Marxist theory, by demonstrating how the transition from "class in itself" to "class for itself" was accomplished. This is too simplistic a reading of Thompson, however. For one thing, Thompson's book demonstrates the very great degree of contingency that attached to the historical construction of the English working class when we consider this process in cultural detail. But to find that the process is contingent, is also to negate the Marxist idea that there is a necessary and direct connection between a group's structural position in the property system and its social consciousness. For another and related reason, Thompson's story goes well beyond Marx's in its emphasis on the independent agency of English working people. Their organizations, their ideas, and their political strategies were not simply derivative of the structural situation of "labor and capital", but rather were the result of specific acts of leadership, creativity, and popular mobilization.

So let us consider the main elements of Thompson's historiography. What was his goal as a historian of this period of England's social history? In writing the book, Thompson took a huge step forward in creating the field of social history, and he established a paradigm of historical writing that guided a generation of historians. His goal is almost ethnographic: he wants to discover the many threads of thought and culture that passed through the many segments of English working people. He takes ideas and ideology very seriously—and recognizes that the ideas of English Methodism and the rhetoric of liberty were profoundly important in these segments of English society. In particular, the ideas and the modes of organization that were associated with Methodism, were deeply formative for the laborers' and artisans' consciousness that was being forged.

Just as important as these elements of "high" culture, Thompson articulates his concept of the "moral economy" of the crowd—the idea that there is a shared set of norms in popular culture that underlie social behavior (Thompson, 1971). He identifies popular disturbance—riots, strikes, and expressions of grievances of various kinds—as a crucial indicator of political behavior and popular consciousness. And he tries to demonstrate that the popular disturbances of the eighteenth and nineteenth centuries were governed by a set of norms that were popularly observed and enforced—about price, about social obligation, and about justice. The "bread riot" was not a chaotic or impulsive affair. And this becomes an important theme in the consciousness of the working class that Thompson describes: a consciousness that denounces political oppression as deeply as it decries exploitation.

In other words, Thompson's version of working class consciousness invokes liberty and justice as much as it does deprivation and material factors. "In the end, it is the political context as much as the steam-engine, which had most influence on the shaping consciousness and institutions of the working class" (Thompson, 1966, p. 197). "The people were subjected simultaneously to an intensification of two intolerable forms of relationship: those of economic exploitation and of political oppression" (p. 198).

> The culmination of this retelling of the multi-threaded histories of English working people is indeed "a working class consciousness"—a more or less coherent social and political philosophy that supported a political program and a morality of equality and solidarity. "Thus working men formed a picture of the organization of society, out of their own experience and with the help of their hard-won and erratic education, which was above all a political picture. They learned to see their own lives as part of a general history of conflict between the loosely defined 'industrial classes' on the one hand, and the unreformed House

of Commons on the other. From 1830 onwards a more clearly defined class consciousness, in the customary Marxist sense, was maturing, in which working people were aware of continuing both old and new battles on their own" (Thompson, 1966, p. 712).

Thompson formulates his understanding of "class" in these terms:

By class I understand an historical phenomenon, unifying a number of disparate and seemingly unconnected events, both in the raw material of experience and in consciousness. I emphasise that it is an historical phenomenon. I do not see class as a "structure", nor even as a "category", but as something which in fact happens (and can be shown to have happened) in human relationships. (Thompson, 1966, p. 9)

The outstanding fact of the period between 1790 and 1830 is the formation of "the working class". This is revealed, first, in the growth of class-consciousness: the consciousness of an identity of interests as between all these diverse groups of working people and as against the interests of other classes. And, second, in the growth of corresponding forms of political and industrial organisation. (Thompson, 1966, p. 194)

The book represents Thompson's effort to identify the concrete historical processes and institutions through which the consciousness of English men and women crystallized around a class identity. There was nothing inevitable about this process, and we can imagine that different historical circumstances could have resulted in a very different outcome (people identifying themselves regionally, nationally, or religiously, for example).

Thompson emphasizes the "agency" aspects of the processes he describes: members and leaders of this group actively shaped the identity of class towards which they moved through the first half of the nineteenth century in England. "The working class made itself as much as it was made" (Thompson, 1966, p. 194). Leaders play an important role in the story that Thompson advances, and their leadership is embodied in the ideas, doctrines, and institutions that they articulated and promulgated. Paine, Thelwall, Hardy, Spence—all these thinkers and leaders play a crucial role in the process of transformation and creation that Thompson describes.

Political debates, facilitated by organizations, corresponding societies, and pamphlets, played a critical role in the emergence of the class consciousness that Thompson describes. The London Correspondence Society (LCS) is a key protagonist in Thompson's story; it was an organization whose leaders articulated political positions, mobilized followers, and communicated publicly and privately with followers and other organizations in other places. The main dimensions of these debates served to frame the politics of the century: constitution; liberty; inequality; property; participation in government; freedom of consciousness. And Thompson believes that the practical intellectual engagement that leaders and ordinary working people had in these debates played a very important role in the fashioning of English class identity. "In the end, it is the political context as much as the steam-engine, which had most influence upon the shaping consciousness and institutions of the working class" (Thompson, 1966, p. 197).

Here again it appears that contingency is a critical element of the story; if different currents of thought had been most prominent—if more attention to economic and social equality had been the rule in place of the constitutionalism of many of the debates Thompson describes—it is possible to imagine that English working class consciousness would have developed into a more revolutionary key.

Traditions of popular protest—the grain riot, the moral economy of the crowd—both represent the manifestation of an embodied group identity, and a central mechanism through which these strands of identity are conveyed from generation to generation. In "The Moral Economy of the English Crowd in the Eighteenth Century" Thompson provides a highly focused and detailed interpretation of the values and paradigms of the crowd (Thompson, 1971, 1991). Rejecting the view that riot and collective disturbance results as a reflex to grain shortage or price surge, Thompson shows that disturbances were highly structured and disciplined, and that they were provoked against a background of very specific values and expectations on the part of the under-class. His treatment centers on the role of grain and bread in the domestic economy of the poor. And he argues that there is clear evidence of a specific set of expectations about markets, the availability of grain, and the constraints on grain prices that governed popular behavior in times of dearth. It was a community-based ethic, a paternalistic set of values that placed obligations on farmers, traders, and officials—or else threatened to produce a daunting range of expressions of popular unrest. He finds that both ends of this range of social action find their origin in the sixteenth century *Book of Orders*. Two hundred years later the crowd is found to be exacting sales of grains from farmers and transporters—just as prescribed by the *Book of Orders* (Thompson, 1991, pp. 224ff).

The state plays an important role in Thompson's story. He documents alternating periods of repression and benign neglect of working people's organizations. Here again the important message of contingency percolates through the account. Another important element of Thompson's story of the emergence of "class" in England is the activity of Protestant churches throughout the period. Churches were places for debate; for principled discussion and disagreement; for organization around shared tasks—in short, they represented salient points of mobilization and cultivation of values and political attitudes.

So through a number of different avenues—church, political societies, conditions of urban and rural work, and collective action—English working people came to have (and came to fashion for themselves) a distinctive complex of values, narratives, and aspirations—an identity.

9.4 Are Mentalités Stable Over Time?

So far we have examined the making of a mentalité. But what about the equally important problem of sustaining and maintaining mentalités? We would also like to know more about the mechanisms of change for mentalités over time—the social processes through which an identity may "drift" or mutate over time. We would expect there to be a range of degrees of occurrence of the various elements of the "identity" and different levels of attachment to the various elements and norms. So historical and contemporary research needs to be deliberately anti-essentialist. We would like to assess the degree to which identities and mentalités are relatively stable historical constructions.

It is important to recognize that the various components of one's mentalité are culturally variable; so mentalités can be diverse and historically plastic. It is clear that mentalités are not uniform across a population; there is no single profile of "American Baptist" that fits all Baptists. This fact of human cognitive and moral plasticity has large implications. Individuals and communities can rewrite the code. There are significant variations in each of these ensembles of identity elements across individuals, across time, across culture, and across group. For example, among "millenarian White Lotus adherents" in Qing North China there are important differences in the mix of values, the relative strength of some of the values, and the presence or absence of other cultural features. Thus there is no "essential White Lotus mentalité," but rather a cluster of similarities among rural Buddhist people in the region (Naquin, 1976). So we need to have some way of conceptualizing how a given identity is instantiated in different ways across individuals within a given group.

It is clear that a mentalité shaped along these lines sketched above will show significant variation across individuals. Each individual's experiences are somewhat different. And each person will process those experiences somewhat differently. What makes a mentalité a socially shared form of consciousness is the fact that some groups have a high degree of commonality of experience—both through exposure to the prior generation and through one's own experiences in everyday life. But at the same time it is apparent that there will be substantial variation in values, memories, narratives, and styles of thought within the identity group. So the social identity of being "Latino", "Polish", or "disabled" should not be expected to be a uniform and homogeneous feature of consciousness. The metaphors of "flavor" and "patchwork" serve us better.

The stability question comes down to this: do the central features of a given social identity show a reasonable degree of continuity and stability across the population bearing the identity, and across time for this people through generational change? Or are the features we have identified here so plastic that it is more reasonable to assume that they change too quickly to allow them to function as historical causes? Are there feedback mechanisms within a population that work to contain drift and diffusion within a group identity? Or do processes of Brownian motion introduce unavoidable entropy into group mentalités?

Something like this process of drift shows up in some of E. P. Thompson's writings about the "moral economy" of the crowd. As market institutions and market prices begin to crowd out earlier assumptions about "just price" and community-based justice, there is a clash of mentalités across generations; and eventually a more modern mentalité prevails (Thompson, 1971).

However, there is a surprising degree of stability in the main features of a social mentalité; so it is important to identify some of the social mechanisms that might underlie that stability. Some of the factors identified above serve as well to provide some degree of stability across generational change. The "tuning" that results across persons as they interact has the effect of stabilizing a set of identity elements. My beliefs and norms are adjusted as I interact with others in daily life. And as I interact with others who share antecedent identity elements, my idiosyncratic modifications are pruned.

The common circumstances discussed above have a stabilizing effect on at least some of the forms of identity that human beings experience. If domination and discrimination are important determinants of female or racial identity, and if those patterns of social relationship are embedded in enduring social structures—then we should expect the elements of female or racial identity to persist with some stability over time.

9.5 Are Mentalités Historical Causes?

How might the features we have identified here as contributing to a mentalité function within historical explanation? Here are several ways in which a socially embodied mentalité might have historical effects:

- explanations of collective behavior (Naquin, 1976; Tilly, 1986)
- explanations of the diffusion of ideas and innovation (Rowe, 1984)
- explanations of differences in response to types of social institutions; draft, tax regimes, corruption control (Levi, 1988; Tarrow, 1996b)
- moral systems, widely shared, influence social behavior (Moore, 1978)

The concept of a mentalité fits easily into the conception of historical explanation outlined in Chapter 5. That conception is "agent-centered"; it invites the social scientist or historian to construct explanations of social and historical outcomes by analyzing how actors, pursuing their goals and values within existing social institutions, have brought about the outcomes in question. But if actors are more distinguished in their thinking and representing than economics or political science represents them to be; if actors inhabit a historically specific mentalité as they confront history; then these mentalités play a crucial role in historical interpretation and explanation.

Now we are ready to come back to the question of microfoundations. The topic of microfoundations is an important theoretical question in this story. It invites us to ask several foundational questions: What are the social processes within which the complex social practice is embodied in human behavior and knowledge at a certain time? What are the social processes through which this body of knowledge is transmitted relatively intact from one generation to the next? What are the social mechanisms of transmission through which these clusters of human knowledge and their variations are conveyed across space and across social groups (from village to village)?

There are several aspects of this story that require microfoundations, corresponding to origins, persistence, and change: the social mechanisms of reproduction of the mentalité from generation to generation (persistence) and the social mechanisms of transmission of innovation across space and time (change). If we committed the intellectual sin of reification and imagined that there is one unique and extended social mentalité that is "Alsatian life experience wherever it occurs," we would have missed a crucial part of the story just told; there is no essential social practice of Alsace. Rather, there are bundles of bits of knowledge, assumptions, judgments, and behaviors that are embodied in the thoughts and actions of individuals at a certain time; and there are social mechanisms through which these bundles of knowledge are transmitted across generations and across space and time.

9.5.1 Charles Tilly and Contentious Politics

Charles Tilly's lifelong work on contentious politics provides another fertile example of the role of mentalités and practices in history. And Tilly lays the basis for significant progress on the task of reconstructing the "microfoundations" through which mentalité, group identity, and political practice emerge through identifiable material processes. Marxist theorists of revolution have made revolution too easy, in several ways. First, they have often made the "making of a class" too mechanical and straightforward. Once a class recognizes its interests, it will have a collective purpose in acting on behalf of these class interests. But collective action is never that simple; shared interests intersect with, and often conflict with, personal, familial, local, regional, national, racial, workplace, religious and other forms of interest.[2] So there are many hard questions—How and when do material interests become salient for collective action? And through what means do leaders and groups attempt to act on the interests that have been mounted as salient?

This is the set of questions to which Tilly's long career has offered deep and nuanced answers. In *The Vendée* (Tilly, 1964) he follows one line of analysis—the complex and historically specific material circumstances through which different groupings in the Vendée region came to identify their interests in different ways, and to affiliate with each other in different ways leading to counter-revolution. In *The Contentious French* (Tilly, 1986) he traces a different thread: the evolving set of practices through which the "contentious French" have chosen to make their voices and deeds heard: riot, seizure, crop burning, public demonstration. It is Tilly's central achievement to have identified popular unrest as itself a family of practices, maintained in popular memory and reaffirmed through future actions. "As people's grievances, hopes, interests, and opportunities for acting on them change, so do their ways of acting collectively" (Tilly, 1986, p. 3). And Tilly maintains that groups establish traditions and repertoires of popular unrest that are historically distinctive—not generalized solutions to exploitation and tyranny, but historically conditioned sets of stylized responses that are available for choice in new circumstances.

> With regard to any particular group, we can think of the whole set of means it has for making claims of different kinds on different individuals or groups as its repertoire of contention. Because similar groups generally have similar repertoires, we can speak more loosely of a general repertoire that is available for contention to the population of a time and place. That includes a time, place, and population as broad as seventeenth-century France. The repertoire actually constrains people's action; people generally turn to familiar routines and innovate within them, even when in principle some unfamiliar form of action would serve their interests much better. (Tilly, 1986, p. 3)

He offers these examples of actions within the French repertoire through the mid-nineteenth century: grain seizure, invasion of fields, destruction of tollgates, attacks on machines, serenades, expulsion of tax officials, tendentious holiday parades,

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[2] See my "Marxism and Popular Politics: The Microfoundations of Class Struggle" (Little, 1998) for further discussion of this issue.

intervillage battles, pulling down of private houses, forced illuminations, acting out of popular judicial proceedings, turnouts (p. 392). By the late nineteenth and twentieth centuries the repertoire had altered: strikes, demonstrations, electoral rallies, public meetings, petition marches, planned insurrections, invasions of official assemblies, social movements, and electoral campaigns have replaced these more traditional forms of collective action (p. 393).

Tilly takes it as axiomatic that large structures influence politics. His question is, how do the particulars of mentalité and repertoire shape the responses that individuals and groups take to these large structures—state, market, economy? "Our problem is to trace how the big changes affected the interests, opportunities, and organization of different groups of ordinary people during the centuries since 1598, then to see how these alterations of interests, opportunities and organization reshaped the contention of those people" (p. 5).

Among the factors that Tilly identifies as central to collective action are: "the population's daily routines and internal organization;... prevailing standards of rights and justice;... the population's accumulated experience with collective action;... current patterns of repression" (p. 10). "The dominant question will remain: *How did statemaking and capitalism alter the ways in which ordinary French people acted together—or, for that matter, failed to act together—on their shared interests*?" (p. 11).

Tilly emphasizes the interplay between structural and material factors, on the one hand—the instruments of exploitation and repression, chiefly—and the historically variable and contingent means that peoples have developed to permit them to—sometimes—resist these factors as they attempt to order their lives as well as possible. What is chiefly of interest in the present context is his emphasis on the subjective factor in this dynamic—the contingent, historically variable development of a toolkit of collective action. Successful tools for collective action are just as difficult to discover as successful schemes of crop rotation; and once imagined, they become the (again, variable) content of popular memory and the basis for the next collective effort to defend collective interests.

Just as Marc Bloch can fruitfully trace the movement of specific agricultural techniques across the map of France, so Tilly can attempt to discern the diffusion of the field invasion or the ceremonial burning of tax records. And in both cases we can ask the microfoundational questions: how are these forms of local knowledge conveyed, diffused, and adapted to new circumstance?

9.6 Assessment

This chapter moves us forward in several ways in attempting to formulate a place in historical ontology for mentalités. First, it lays out the several dimensions of inquiry that are needed: psychological (how are mentalités embodied in the individual); sociological (how are mentalités transmitted and sustained); empirical (how much stability or plasticity is there in a socially embodied mentalité?); and historical-causal (how do these complexes of institutions and mentalité elements influence historical change?).

Second, there are fertile strands of research in historical social science that permit us to broaden and deepen our understanding of these issues and topics, and that give us a deeper understanding of the ways in which meaning elements interact with structural or material elements. People and peoples act—and they do so in the context of both structural conditions and habits of mind. These habits of mind are historically durable (to some extent), and they influence the frame of action and the outcomes and strategies that historically situated individuals take.

Third, it is entirely reasonable for historians to treat mentalités, identities, and practices as historical factors that can be invoked in historical explanations. These historical categories are intangible, to be sure; but as the historians of the *Annales* school and others have demonstrated, it is possible to conduct rigorous historical research into the details of the mentalités and identities of a time and place.

There is also a point of convergence in our discussions of mentalités with the topic of popular politics and social movements. Thompson (1971), Bianco (2001), and Tilly (1986) have all shown that popular protest—bread riot, tax uprising, or revolutionary demonstration—have fundamental and distinctive elements of social "practice" embedded within them. There are stylized patterns of protest that recur throughout a given tradition—French rural people, Chinese villagers, Italian industrial workers—that represent historically developed palettes of protest. These are not instances of "generally optimal tools of protest", but rather highly specific traditions of popular action that could have evolved very differently. Tilly documents the continuity of patterns of protest through French contention, and Thompson demonstrates that bread riots had a distinctive moral economy that prevailed at a time and changed over time. Individuals have learned how to express their protest and how to come together in stylized forms of collective action—"this is what we do when the landlords ignore subsistence crisis."

So a careful re-reading of historians such as Bloch, Tilly, Thompson, or Darnton can take us a long ways towards a better understanding of the causes, variety, and trajectories of social mentalités and practices. And—as is characteristic of the very best social science and historical research—these authors take us to sometimes surprising insights into how these factors work in real historical contexts.

Conclusion

In the preceding chapters we have discussed dozens of historians and historical problems that span Eurasia. Fundamentally, we have unfolded a conception of historical explanation that derives from the central idea of situated human action; the idea, as Marx put the point in 1850, that "men make their own history, but not in circumstances of their own choosing." In other words, historical explanation fundamentally involves identifying the features of agency and structure (and natural constraints) in the presence of which the great and minor events of history have transpired. Fundamentally historians are faced with the challenge of making sense of the choices that actors have made in bringing about the historical processes that interest us, given the circumstances in which they find themselves.

Formulating ideas about agency is therefore key for historians, and we have seen a wide variety of theories of agency in the preceding chapters: Robert Darnton's ethnographic study of the book-makers' apprentices; interpretations of historically specific mentalités; attributions of rational or materialist motivations to participants in riots and rebellions; interpretations of religious commitments; and so forth, throughout the almost endless variety of forms that human agency takes. This is the aspect of historical imagination that corresponds roughly to the "hermeneutic" or interpretive strand of historical thinking: what did these historical actors want? How did they think about the world? The topic of mentalités came last in our discussion, but in another sense it comes first: to explain historical outcomes, we must have a theory of the states of mind of the actors who make history and endure it.

Arriving at better understandings of the ontology of social structures has been a second key focus of the philosophy of history unfolded here. How do structures influence and constrain human action? How are structures embodied in the actions and thoughts of individuals? What are the microfoundations of social structures? We have seen that it is crucial to avoid reifying social structures—attributing to "the state" or "the proletariat" a causal and ontological presence that transcends the individuals who constitute it. But we have seen as well that there are coherent and defensible ways of formulating conceptions of extended social structures that do not reify them, and that nonetheless provide them with an important and potent source of historical causal force. Once embedded in barracks, police stations, businesses, social networks, and command structures, the military structure

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of the Burmese junta creates a highly coercive set of social constraints within which Burmese citizens must act. So the fact that each structure is necessarily embodied in the actions, thoughts, and motives of a population of people does not imply that the structure lacks "autonomous" causal effectiveness to influence agents' behavior.

We have also seen that it is very important for historians to arrive at deeper understandings of the metaphysics of social causation. This means, first, understanding the complete inadequacy of traditional positivist interpretations of causation: "causation is no more than regularity". This Humean view does not serve the natural sciences well, and it certainly does not help us when it comes to social causation. So it is necessary to explore a different model of causation that fits better with what we know about the actual workings of social processes. The model developed above is "causal realism"; it holds that the task of arriving at a causal explanation comes down to discovering one or more causal mechanisms linking antecedent to outcome. This approach conforms well to the actual practice of historians constructing narratives. And it is supported by a careful analysis of the metaphysics of social causation as well. The microfoundational approach argued above holds that social causation proceeds through the behavior of individuals making choices within structures. But whether or not one accepts the microfoundational approach, it is necessary for historians to have a better idea of what they mean when they judge that "X caused Y."

A fourth important idea that recurs throughout this book is the fact of historical contingency. Historical events are the result of the conjunction of separate strands of causation, each of which contains its own inherent contingency. And coincidence, accident, and unanticipated actions by participants and bystanders all lead to a deepening of the contingency of historical outcomes. We have seen, however, that the fact that social outcomes have a high degree of contingency is entirely consistent with the idea that the idea that a social order embodies a broad collection of causal processes and mechanisms. These causal mechanisms are a valid subject of study—even though they do not contribute to a deterministic causal order.

Further, we have seen repeatedly that social phenomena are heterogeneous and plastic. Institutions change over time in response to the actions and intentions of participants (plasticity); and generally similar institutions are nonetheless significantly different in their mid-level characteristics and dynamics (heterogeneity). Cities illustrate both characteristics. The institutional regimes through which a given city manages an important urban problem—handling the provisioning of clean water, let us say—change over time; this illustrates plasticity. And different cities have very different internal functional organizations, all serving to fulfill roughly the same set of urban functions but in very different ways (heterogeneity). It is important for historians and historical social scientists to keep these fundamental ontological facts about the social world in mind as they attempt to conceptualize the past. Otherwise we are likely to produce stylized and repetitive interpretations of the institutions and actions of the past, overlooking the important ways in which those institutions differed from each other and from contemporary equivalents.

The related ideas of meso-history and comparative history conform well to all these recommendations. By paying attention to the mid-level processes and institutions of a given time period, the historian is drawn into the distinguishing features as well as the common features of these institutions. (How did French absolutism really work, when it came to collecting taxes, raising armies, and managing a bureaucracy?) And by engaging in careful comparison across complex cases, the historian is brought to recognize the facts of institutional variation and, sometimes, commonality. (How did proto-industrial handicraft production work in Amsterdam and Suzhou; what were the similarities as well as differences of these pre-modern economic institutions?) Likewise, several discussions above have illustrated the explanatory value that derives from the study of meso-level social institutions and organizations—for example, the transportation system that exists in a given region. Further, meso-history and comparative history lead the historian to have a more practical recognition of the contingency and path dependency of mid-level economic, political, or social institutions. It is difficult to maintain that there is only one way of managing a fiscal system or growing a pre-modern industrial economy, when one's research lays bare the similarities and differences that existed in different settings in France or Japan.

Many of the examples explored here come from Chinese history. That fact is deliberate; along with R. Bin Wong and other comparativists, I believe that China's historical experience provides rich and substantive examples of social processes that are momentous and different from their counterparts in the west. So studying such varied topics as economic growth, technology change, bureaucratic organization, peasant mobilization, ideology, or revolution in China affords an opportunity to see the blindspots of traditional modes of interpretation and expectation on the part of European or American historians. (Similar points could be made about the study of India, Ghana, Burma, or Argentina.)

Turning to questions about evidence and objectivity, our discussion offers support for the idea that historical inquiry is an empirically rigorous endeavor. We have seen many instances of historical research that refute easy statements such as "the past is unknowable," "historical interpretations are inherently subjective," and "all historical statements are the result of the historian's bias." On the contrary, we have seem ample examples through which historians engage in detailed historical research involving different kinds of historical evidence and theories from the social sciences, and arrive at well-grounded hypotheses about circumstances in the past. Questions like these turn out to have answers in which we can have a fair degree of confidence: "What was the typical annual food budget for an agricultural worker in England's midlands in 1700?", "Why did Parisian artisans support mobilization against the monarchy in 1848?", "Did the Chinese Cultural Revolution involve deliberate strategies of mass killing?" It will sometimes turn out that there just is not enough historical evidence available to answer a given question, but this is surprisingly uncommon. Debates exist over the interpretation of the facts; but often enough, further research suffices to narrow the range of debate for the next generation. So we have found many reasons to be optimistic about the objectivity and truthfulness of historical knowledge.

This said, of course, there are important pragmatic differences between historians and historical social scientists. Fundamentally, the historian is interested in discovering and explaining the particular, rather than in formulating theories that apply with broad generality. History is not simply the unfolding of theoretical premises, and good historical knowledge does not result from deducing consequences from general social science theories. Rather, the historian's task is to discover the particularity and granularity of the materials in front of him or her. That being conceded, it is clear that historical inquiry can be importantly assisted by social science theory; and this is especially valuable when it comes to trying to identify particular causal mechanisms within a given historical process.

We have not considered every problem that arises in the doing of history. We have focused on the knowledge enterprise: what is involved in knowing (some of) the facts about the past? And what is involved in arriving at satisfactory explanations of these facts? There are other goals that historians have in doing their work, from illustrating a moral point, to entertaining a reading audience with surprising stories about those who came before. But many of the most interesting historical writings fall squarely within the "cognitivist" approach, and their examples support an interpretation of historical knowledge that is evidence-based, rigorous, and post-positivist. On this interpretation, history is a kind of social science, sharing commitments to evidence, rigorous reasoning, and critical use of theory in arriving at true statements about the world. And this is a lofty aspiration for historians and philosophers.

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