Chapter 1
Neanderthals and Emergent Palaeoanthropology 50 Years Ago[1]

Opening address to the congress, 150 Years of Neanderthal Discoveries: Early Europeans – Continuity & Discontinuity, Rheinisches LandesMuseum, Bonn, Germany, July 21, 2006

F. Clark Howell

I know why you're here, the question is why am I here and I'm here because the organizers, let us say unanimously, said "would you please come to Bonn for the 150 years of Neanderthal?", and it took me a few minutes, maybe 6 or 7 seconds to say okay, and then they said what they would like me to do, and I said well, yeah, I really can do what I want, which is mostly what I do anyway, so in any case I came for several reasons. Not only because I happened to be here as Gabriele Uelsberg, the LandesMuseum Director, said for the first time in 1956, the first time I was in Europe was 1953, the same year that I received a doctorate, although I never stayed for the ceremony and they had to send it to my parents, because I was overseas, in London at Chris Stringer's museum, and Chris had just arrived I think recently, and you'll hear about that in some of the things I'm going to say to you. I'm going to talk to you about the 1950s, which is 50 years ago, so if you're under fifty there are some things that you might benefit from, from what I say. If you're over that, there are some things you might say, "well that's interesting, I really didn't know about that "or "that's not the way I heard it" or something like that. All that's possible. I really came because of something else. I was in Bonn in 1956. I was in Europe, all over with my new wife of 1 year, all over Western Europe, beginning in London and ending back in London, and all over Western Europe, Southern Europe, Central Europe and so on in a great big swoop over 3 months, looking at Neanderthal folks that I hadn't seen already in 1953. In 1953, they were all seen either in the British Museum (Natural History), now The Natural History Museum, let us not forget, and then in Paris.

And in Paris I was able to see things at the end of August; now you know if you go to Paris and you want to see something in a museum and it's not a public museum and it's the end of August, you can forget it. But I had connections, and these connections worked, I have had connections most of my life and mostly they work. I don't know why that is, serendipity, whatever it is, it's fantastic, so far so good, and in any case I was allowed to see things at the Institut de Paléontologie Humaine, including unpublished things, and including things that later were made into a fantastic dissertation and lots of other things by my colleague Bernard Vandermeersch, who's right here tonight, and that was because of Henri-Victor Vallois. I had connections to him, and he was extremely kind to me. I think one of the reasons is he never had any children, and I was a young man, you know, he thought young investigator, I don't think he had many students, frankly, anyway, and I was a young anatomist and he was an anatomist originally at Toulouse and so on and he thought, hmm why not. He was also the director of the Musée de l'Homme, at the same time as being the director of the IPH and the editor of *L'Anthropologie* along with Raoul Vaufrey, and he said "you can see anything you want in the Musée de l'Homme." Now Silvana Condemi is here and she can tell you, it's really tough to see anything, anywhere at the Musée de l'Homme sometimes, and I won't go into that any further, but it is, and everything was carte blanche for me. I cannot believe it, and I couldn't believe it then, I just took it for granted. Subsequently I learned that it wasn't so simple. But every time when I came to Europe later, and especially this big 1956 tour that I did, no obstacles were placed in my way. Now I didn't say anything about this in what I'm going to read to you, but I'm saying it sort of off the record, because it shouldn't be that way, it should not be that way. If a painting is done, dammit, you ought to be able to see it. And if you build an Aston Martin, they ought to be for sale if you have the money to buy one. And I feel that fossils are our heritage, if they're Hominidae, and everybody who qualifies should minimally be able to examine them, if they know what it means to examine somebody. You do not want an orthopedist examining you if he's an internist. I can tell you, it's not gonna work very well. You have to be equipped to do things and I insist that that's an important thing.

[1]F.C. Howell died of cancer on March 10, 2007, just 8 months after presenting this lecture. A recording made by W. von Koenigswald was transcribed by Ms. Evelyn Katz and edited by Eric Delson (both of the Department of Anthropology, Lehman College/CUNY, Bronx, NY 10468, USA), with editorial assistance from Susan C. Antón (Department of Anthropology, New York University).

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The real reason I'm here is because I happen to appreciate science in Germany. As a youngster after World War II and in college and so on the first language I learned, because I was illiterate in terms of my background and so on, was German, followed by a summer of French, followed by a year of Italian so I could cope with writings by Sergio Sergi and his father Giuseppe, and all these other people. Now they all write in English, so what use are these languages for me? Well they are useful, you know, when I want to order something or find the toilette, it works. Now I really came because of that, because I appreciate science in Germany, and what Germany did in science, in the history of science. You know the medical schools, those of you that don't know, in the United States, are based on the German system, and the teaching of anatomy and other things like that profited vastly from the German system. We didn't go anywhere else and many other parts of Europe, especially Central Europe, not to talk about Western Europe exactly, are based on systems that are essentially Germanic. So I'm here for that reason. I'm here for another reason, and I said this last night, so those who heard me say it could put their fingers in their ears, and that is because you have a Chancellor that I happen to admire greatly here. Now I haven't met her, and I haven't given her a backrub like somebody else in my country did the other day, he also does weird things all the time. But anyway she has the lead editorial page in Science that came out last Thursday, a week ago today. Now that's never been done before by any "politician". I call her a scientist because that's where she comes from, and she happens to be your Chancellor, so I would say chancellor. I would never use the word politician although I suppose to survive she has to occasionally be political. But I came because of that, and because of what she said in her editorial, which has to do with the people, and the future, and what is really important in life is education, and training of the young, and I would say training of the old. My God, they don't understand many things because they were poorly educated in my country, I insist it's my country, it's not my government, but it's my country, and I feel it's vital, and I feel that understanding of science is vital to everybody. You learn how to walk, you learn how to talk, you better learn some science, I don't care what you do with your life, you better know some science. And it's a lot more fun if you know science.

So I'm here for those reasons and also to tell you something about the 1950s. And I've written this because some people, including Eric Delson who's here tonight, will say Clark, why don't you ever write these things down and so on. And the answer is I write too much already, but I've written it down and there will be a book to come out of this and whatever I say tonight with alterations and emendations will probably be in there. So I'm going to tell you this is the way I thought when I wrote this.

We're gathered here tonight on a sesquicentennial occasion of a significant fossil discovery, the implications of which lie in a way at the very roots of human paleontology as a scientific endeavor. Due to serendipitous circumstances, I was present as well at that ceremonial occasion held in Düsseldorf and at the eponymous Neanderthal quarry locality which we had a gathering at, at a meeting memorable for its time no less than for its place with reference to the reinvigoration of basic science in the consequent decade after in excess of 6 years of Eurasian warfare. The invitation to speak here in an inaugural position affords me an opportunity to offer a personal perspective, hopefully insightful, on circumstances, events, situations, and personalities that reflect the emergence of paleoanthropolgical science within the sixth decade of the twentieth century. It's appropriate and worthy of a few reflections in respect to this first decade of the second half of the twentieth century. This was an interval of massive reconstruction in Europe via the Truman Doctrine and the Marshall Plan. Each of those individuals was absolutely unpolitical. Truman was utterly unpolitical, although he became a politician. He was a haberdasher. George Marshall, who was trained as a military man, he was really an intellectual. It was also a time of political reorientation and of attendant geographic and political polarization, of the progressive collapse or transformation of colonialism in Asia and in Africa, the development of nuclear power and weaponry, the initiation of space science and exploration, which was affected by rocketry and satellites, and it was the time of the very roots of computational science. Now all of those things are taken for granted, just ordinary, run of the mill, day to day things. Uprisings were common then as now, but in a different form in, for example, Algeria, Hungary, Kenya, Vietnam. There were local wars that continued in Korea, Palestine, Suez, Yugoslavia, and there was complete overturning of the government in China, by what became the PRC, and in Cuba, even the construction of a Berlin Wall in the case of that city, and the emergence of what was called an Iron Curtain and of the Warsaw Pact as a consequence of, a reaction to the establishment of NATO. Now all of you live with NATO although you probably don't see it or feel it, but NATO is all around you. You now live with something different and you also feel that and see it a little better and that's the European Union, and the one person who occasionally stands up and says hurrah is Winston Churchill, in that regard. Significant new representatives or heads of states then included Anthony Eden and Clement Atlee, Charles de Gaulle, Konrad Adenauer, the Shah of Iran for a moment, Nikita Khrushchev after Stalin's death in 1953, and he disowned Stalin in 1956, Mao Tse Tung, Ho Chi Minh, Dwight Eisenhower, and John Kennedy was entering the stage. We witnessed the first space flight in 1957. I was in Tanzania and watched the satellite go by. The first satellites were in 1957–1958, the first lunar rocket in 1959, the first commercial jet aircraft was in 1958, 4 years after I flew to Europe and to Africa on Boeing Stratocruisers and so on, four engine piston planes developed largely in World War II, by the British and by the Americans on the Allied side. The best part of that trip aside from Africa (my blood was infused with Africa forever after that), was that I flew first class because I had so much weight, and you could afford first class in those days, and when I got in my seat, I didn't pay attention to the person who was to the left of me. I took my gear and I put it up, I turned to the left and Marlene Dietrich was sitting next to me. Now I happened to look up at her, not down at her, but had I looked down I would have known it was Marlene Dietrich as well.

The first nuclear power plant was 1955, the first atomic submarine under the Arctic sea was 1957, the emergence of palaeo-oceanography occurred then, the first polio vaccine was in 1953, the first container shipping system appeared then, the first all purpose credit card appeared, and finally recognition of the helical structure of DNA was in 1953. Actually I always thought 1953 was the most important because that's when I got my doctorate, the first time anybody related to me anywhere, had gone to college much less got a doctorate. I was wrong. You know, I didn't know anything about Watson and Crick, I have to tell you, that spring I didn't know anything about anything like that, and I'd had courses that had to do with some of those things, way back, but I was off in another universe, getting out of here, studying real fossils, going to the field, those kinds of things. I missed the boat, often we miss the boat, we don't recognize what is in front of us, we perceive, but we do not understand what we perceive. Geochronology was enhanced then by the development of radiocarbon dating, so-called, and the applicability of the potassium argon methodology, known previously, to volcanic products of Cenozoic age, was affected through the development of mass spectrometer refinement. Now I was in universities in which each of those things occurred: Chicago, where I knew Williard Libby and people who worked with him, a man named Miller; and subsequently involved with people who were at University of California at Berkeley where I am now emeritus, because I knew Jack Evernden and Garniss Curtis. I went to one of their first major presentations in 1958 at an AAAS meeting in Philadelphia, the day after Christmas. I had been married 3 years, if you don't think my wife loved my leaving 1 day after Christmas, for a meeting in Philadelphia to hear somebody talk about dating volcanic rocks. It didn't go very well. Later she fell in love with Garniss Curtis, so it was all right, I guess, but it was hell. It'd been easier if we had had children, didn't have any children yet. Each occurred at institutions with which I was affiliated, and if I've not mentioned significant developments taken by all of you for granted, they probably were not yet envisioned or manifest, including those beepers that you have in your pocket, those cell phones, and all kinds of other things.

This perspective, however personal, reflects these manifold and varied experiences that I encountered and in which I participated within those years, at times noteworthy for the emergent formulation of our science and, of course, my incipient professional career began in those years. The initial 1950s constituted for me the completion of a University of Chicago education, a doctorate in anthropology and natural sciences in 1953, as I said. This was exactly 10 years after my secondary or high school (*Gymnasium*) graduation in 1943, after which, a few months later, I entered the U.S. Navy, trained, and they sent me overseas in the Pacific war theatre for nearly 3 years. And at the end of the war, when we were waiting to invade Japan and so on, we made a horrible mistake, but they still say it's a good idea, that we dropped two atom bombs on Japan just to tell them we were really there, and we didn't want them to stand up to us. I'm not happy about that, I'm not happy about war anymore. I was very warlike as a young person, I loved the military, I loved battles, most of what I remember in history, I wanted to be a civil war historian first of all, for the South, strange; that's cause I'm from a border state. Those 7 pre-professional university years afforded me a hitherto unperceived and hence unexpected and ever expanding intellectual, particularly scientific, immersion. My earliest Midwestern years were initially of a farm life in Kansas and subsequently in small cities, or in towns in Nebraska, then Indiana, then Wisconsin when I went into service. And I was scarcely prepared for advanced university life and as experienced by others of much more advantaged intellectual background and of course of economic status. I felt very much as a foreigner. But the amazing thing was I was much older than people of my same level in college, because we were all returning G.I.'s, and a number of us had never been to college before, we'd been in the military, and if it hadn't been for the G.I. bill we wouldn't have been in the military [sic – should be "in college"], we would have been doing whatever, working in hardware, I don't know what we would have done. With the financial support afforded to all military veterans, I was empowered to pursue an educational experience otherwise foreign to our family history. I'd already determined to seek a professional career in science, particularly in human evolutionary studies, as I thought about them then, even prior to entering military service, and these war years offered me some opportunity and time to read and to reflect and solidify such interests and potential goals. A postwar visit in August 1946, after my discharge, to New York City and its American Museum of Natural History, strongly reified my concerns when I personally met and visited with Franz Weidenreich and unexpectedly as well when we went to lunch with Ralph von Koenigswald, the latter had only arrived a few weeks previously from overseas wartime internment in Java. Weidenreich I'd first began to correspond with in my final year of high school, I was very audacious I just said, he's like anybody else, I wrote a letter to him. Unquestionably these interactions were of substantial significance to my future quest for a career in science. Luckily I made this visit as Weidenreich was to die only 2 years later at age 75. Weidenreich's own writings were very likely a major stimulus to my subsequent concerns with Neanderthals and their time and place in human evolution. Through attendance at professional meetings, travel to other universities, meeting those visiting scientists who often appeared at my own university, I soon became aware of and familiar with and eventually inducted within the sphere of scientists within evolutionary biology, in anatomy, in paleontology, biological anthropology, and prehistory and even Quaternary studies. So I did a degree that's anthropology but I did literally all my work after passing those horrendous day long exams in four anthropological fields, I did all my work in the natural sciences, aside from those languages that I had to pick up because otherwise I was a linguistic cripple. In what then was still [the] limited size and focus scope of a particular scientific endeavor, this was both possible and invaluable for a fledgling professional person like myself. I found encouragement and support not only within faculty, but also from scientists at other institutions, museums, foundations with whom I came in contact and interacted. So my own monitoring experience was then substantial, broad, and diverse. Such experiences greatly influenced my own attitudes, my own actions, subsequently, with reference to colleagues, associates, and students over the course of what became a long professional life, and ultimately globally, which I certainly never expected.

Consequent to the doctorate, I was very fortunate to be overseas for at least the summer months, or even longer on two occasions, for 6 out of 7 years of that decade. And I should mention that excludes 1955 when I married Betty Ann Tomsen of Danish descent and also resettled at the University of Chicago faculty from teaching anatomy previously at Washington University medical school in St. Louis. I never knew if St. Louis was sad or glad when I left. I have a feeling it was a mix because later the chairman made some remarks to me once in an elevator in New York. He said "my God Clark is it you?", I said yes. He said "it's too bad you left St. Louis"; I said where are you now, he said "I'm at Columbia". I said I guess we had reasons to leave, he said "I know we did". So, there you go. The years of 1953, 1956 and 1960 were years exclusively concentrated in Europe, 1959 I was in both Europe and the Levant and in Africa, and in 1954, and again in 1957, there were long stays. In Africa in 1954 I was there for over 7 months, and throughout most of sub-Saharan Africa and involved in both field and museum studies; and then subsequently in 1957, my wife and two students and I were in East Africa for over 8 months excavating at a large Paleolithic open air site in southern Tanganyika, now Tanzania. I have to tell you, you won't like it, it was better Tanganyika than Tanzania. Colonialism had something to say for itself, not everywhere but it did have something to say. You could walk down the street, you could leave the car unlocked, you could do all kinds of things. I know that people were in different straits but there was something to be said for it and I don't mind saying it.

Those four instances that involved Europe specifically, or even partially, are relevant to our concerns here today, as they mostly relate to issues of human evolution and prehistory within the Late Quaternary. Each afforded major opportunities to study hominid fossils, examine important artifact collections, often make visits to prehistoric sites widely over Western and Central Europe, not to mention the United Kingdom. And in 1953 there was an opportunity even to participate in the summer field season under the direction of Professor Hallam L. Movius at the Abri Pataud in the Dordogne, very close to the grotte or shelter Abri de Cro Magnon, and we stayed in the Cro Magnon Hotel. I took it for granted that one should stay in there if you were going to excavate, you know, just a moment away at Abri Pataud, which he had bought through the Peabody Museum in order to excavate. And I learned a lot then about doing Upper Paleolithic typology because that's what he said, "you're not gonna dig or anything, you're not even gonna see the site hardly, you're gonna stay in the basement of this farmhouse and you're gonna sort all the artifacts that come in and this is how you'll do that" and so on. And I said yes sir, yes sir, the way I'd been taught in the military; he was a colonel, of course, in the air force, but he was also my friend, and he was like a mentor and almost like a father to me, very stern but very understanding. And I did what I was told, and I learned a lot from him, and I also developed that summer a long and lasting friendship with François and Denise Bordes and many other people. I met Camille Arambourg that summer for the first time. He had a house in the Dordogne, and little did I know that later we'd work in the field together in Africa.

In several instances there were small conferences or symposia in which I was involved, and these included, in 1953, a gathering in London based at the Natural History Museum, but also held at a hotel nearby; at that meeting there was planning for future steps by the Wenner-Gren Foundation to support African paleoanthropology. And it was actually that moment when I, and others too, examined and experienced the demise of the Piltdown hoax at London's Natural History Museum at the hands of British colleagues. That means Weiner and LeGros Clark and so on, and Sherwood Washburn was there, and Charles Reed (a zoologist), myself, several other people. Augusto Azzaroli was there at the same time studying cervids from the [Cromer] Forest Bed. I didn't fully appreciate the moment as much as I might have, but I certainly knew, I never had any faith whatsoever for a minute about Piltdown ever. When I learned to read German, and I read Weidenreich's student Freidrich's long paper about that, then I read Gerritt Miller and some other things before I'd ever seen it, I said, there's something really weird about this,

this cannot possibly be, and so it wasn't. By the way, the person who was responsible for it was surely not Woodward, surely not Sir Arthur Keith, surely not ABCD, but somebody else. If you want to know, I'll tell you later, I'm almost sure who did it. And Chris Stringer will tell us whether I'm right or wrong. Sir Arthur Keith was still alive then and at the end of the summer, after I got back from France, I journeyed down to Down House in Kent to meet him and have tea with him. Later that summer, I'd sent him some of my papers and so on and he came down, he was a very tall man, very bent and so on, but I suppose about 6 ft 4 in. or so, very slender and long faced and so on, and he came forward like this – you could see all the veins and arteries. And he said, "Dr. Howell", very formal; I said, Clark Howell. He said, "I thought surely you were much older." I said, I will be. [Laughter] Sweet man, we talked for an hour and a half. He'd just finished his nap, and he was writing a book about Thomas Huxley.

In 1959–1961, I served as a participant, or a principal organizer, of Burg Wartenstein (Austria) Symposia of the Wenner-Gren Foundation. And on two occasions I attended larger and international congresses. In 1956 of course, the Neanderthal Centenary celebration in Düsseldorf, and in 1959 the Fourth Pan African Congress on Prehistory held in Leopoldville, now Kinshasa, and in the last instance prior to the Congress, I was among the first scientists to see, in Nairobi, Olduvai Hominid 5, or Zinj, after its discovery at Olduvai Gorge while I was in Ethiopia on a survey trip to the Lower Omo Basin. It took us 7 years to get permission to work in the Omo for 10 years. It was worth waiting for, it was painful to have to wait. Louis Leakey was wonderful, we had a nice dinner at their home in Karen, and Mary had a little kind of twinkle in her eye, and we never knew if it was the Scotch or whatever, and after dinner and so on they didn't always have dessert, but he said "maybe we'll have some cookies, you call them cookies don't you?" I said, you call them biscuits, he said that's right, so he brought out this metal box which had Danish biscuits or cookies in them, and he said "here open it up, it's a new one". And he opened it, and there was Zinj lying in the box, disassembled so the face was separate from most of the braincase. I couldn't believe it, I couldn't believe it. Later we all went to Kinshasa to the Pan African Congress, and at that time he offered Phillip Tobias (PVT) the chance of describing it, and he made the right choice, super guy to do it and the same with *Homo habilis*. Now some of these things we look at a little differently after they've been described, but the descriptions if they're very well done, the descriptions will always last, they'll stay there forever.

In 1950 the first major effort to bring human evolutionary studies into the framework of the modern evolutionary synthesis was a very major symposium called The Origin and Evolution of Man. It was held at Cold Spring Harbor, Long Island, New York with nearly 40 participants and over 100 registrants. I was fortunate to attend this as a student, and it played a very central role towards the redirection of the field and then crucial in my own future orientation towards studying human evolutionary biology. I'd met Ernst Mayr before, but this really, I mean you're together for a week, and you're walking around and drinking coffee, and everybody's equal, a graduate student is equal to the professor and so on, it was wonderful … and I got to know Dobzhansky very well. George Simpson was never easy to know by anybody, but he acknowledged "Mr. Howell", never "Clark". Anyway lots of people, very interesting meeting.

Another major and certainly a singular event was that of the Darwin Centennial, the largest gathering of which occurred at the University of Chicago in November 1959, with a plethora of participants and listeners, among a galaxy of scientists of international repute, mostly but not only in the Natural Sciences. It was fully and quickly published as *Evolution after Darwin* by the Chicago Press in three volumes in 1960 and then in 1962. Over nearly 3 months of that autumn, Sir Julian Huxley, I called him Julian, and I also called him Sir Julian, was an office neighbor of mine with whom regularly, morning, afternoon, lunch, whenever he felt like it, we discussed matters of common concern in regard to evolutionary biology. I'd read his volume Evolution, a Modern Synthesis, published in 1942, which got to the West Coast and I found it in 1944 and took it with me overseas, and so I'd read it, and I was very happy and honored to have a chance to get to know him. This was a man whose mind never went to sleep. He was always into something and so on. I believe that he was not given enough credit for the modern synthesis because, not only did he coin the word, but also he was too much of a synthesizer, and people often forgot all the basic science that he did, this man did a whopping amount of basic science, including unbelievable work in allometry. And he deserved everything that he ultimately got, including a very fine honorary doctorate at the University of Chicago on this very occasion, as did several other people, of course. But Julian Huxley, people thought he was sort of, you know, uppity British upper class and so on, I didn't think he was that way at all and sometimes people you know are frankly misread for whatever reason, and I won't go into details of that, but I'll just make the admonition, don't always believe what you see; I would say don't always believe what you think. Think twice and if it still goes that way, alright, maybe, but don't be so all fired sure, as my father used to say, don't be so all fired sure.

Many of the principal and major contributors to the modern evolutionary synthesis were present or represented depending on their health. Among many others of diverse fields among the 50 central participants at this centennial, this was the first such all inclusive symposium on evolution since the very seminal post war symposium in Princeton, 1947, (published in 1949) which was focused on genetics, paleontology, and evolution. That meeting built on major antecedent books by Ronald Fisher in 1929, J. B. S. Haldane in 1932, neither of whom I knew. Haldane, I would have love to have known, Fisher I think I could have done without probably. Theodosius Dobzhansky, 1937, who was a very dear man; Ernst Mayr, 1942, another mind never stopped running; and George Gaylord Simpson, 1944; all those books are customarily considered to reflect the consolidation and crystallization of the synthesis. I assume many, even most of you know this extensive reformulation within the life sciences, and perhaps even best through the volume edited by Ernst Mayr and William Provine called *The Evolutionary Synthesis, Perspectives on the Unification of Biology*, published by Harvard in 1980. Although that volume contains two informative though brief chapters devoted to embryology, among its broad ranging coverage of fields and research traditions, the developmental aspect was notably ignored or even absent within the traditional formulation of the modern synthesis. This was patently evident during the Darwin Centennial and was brought out there explicitly in that respect by Conrad (Hal) Waddington, with whom I managed to have several significant and long conversations on the subject. Waddington had a major influence on me, more than I knew until subsequently. Having studied embryology and development in the University, I considered this among other issues, including this significant role likely played by other natural scientists towards elaboration of the synthesis, as unfortunate and even unwarranted omissions. You can of course read about this and much else in Stephen Jay Gould's remarkable tome, the Structure of Evolutionary Theory, published shortly before his death.

In university, after acquiring foreign language capabilities, I'd actually read *Der Evolution der Organismes* edited by Gerhard Heberer, published in 1943, but which got to our university towards the end of the war, a volume that clearly revealed roots of the synthesis among some German and other natural scientists of the previous decades. And if, those of you who read German, useful insights into the role and participation of others are exemplified in the volume *Die Enstehung der Synthetische Theorie*, edited by Tom Junker and E. M. Engels, published in 1999 in this country, in Berlin. The critical role of development, now evo-devo, and developmental genetics is, of course, absolutely powerfully established within evolutionary biology now. It was not always so. I've always considered that developmental studies must constitute a central focus in human evolutionary biology as well, and I've said this over and over again. I consider, as have others, that the Neanderthal Centenary in 1956, published 1958, and the Burg Wartenstein1960 symposium, *Early Man and Pleistocene Stratigraphy in the Circum-Mediterranean Regions*, published 1962 in *Quaternaria*, constitute fundamental contributions to infrastructure of an emergent paleoanthropological science. This latter meeting, the one in *Quaternaria*, was organized by Alberto Carlo Blanc and myself, mostly during his second visit and residence at the University of Chicago in early 1959 as a visiting professor of paleoanthropology. His sudden death, which I learned of only 2 days before the symposium, when I visited Zurich and met Adolph Schultz, who said "I have very bad news for you", was a horrible and painful blow to me; however all the participants rose to the occasion and the symposium was an unbounded success. Blanc was another of the many people that I met in the course of my time in this world who was open, candid, translucent, generous beyond belief, etc. And there have been many such people. Each of these events facilitated, and the latter particularly emphasized, extensive discussion centered around precirculated drafts of papers – that was the Wenner-Gren plan.

An examination of each of these respective volumes is revelatory of focus and status of particular areas of interest, of fields of scientific endeavor, and of the nature and prevalence of theoretical frameworks, and it should be emphasized the extent to which congruence and even concilience was manifest as a consequence. I've broken down, and somebody can read it someday, the breakdown of people and their papers at the Neanderthal Centenary and at the workshop conference that I mentioned in parallel, which occurred later at Burg Wartenstein, and there were geologists at both but at the Burg Wartenstein Conference there were 14 instead of 4 geologists, we really had a slug of geologists because that's what we're trying to do. We had two paleontologists at Wartenstein, and there were four at the Neanderthal Congress. There were seven archaeologists at the Neanderthal Congress, and we had three archaeologists at Burg Wartenstein. And the paleoanthropologists, there were 12 at the Neanderthal Centenary and 1 at Burg Wartenstein. You can readily gain, even from a summary like this, something about the focus, the goals and the emphasis of these kinds of meetings. It should be mentioned that my passing participation (en route to Israel and East Africa) in an earlier Burg Wartenstein Conference in 1959 called *Social Life of Early Man* (published in 1961), enabled me to meet there Professor Francois Bourlière of Paris. This eventuated in our organization of a 1961 Burg Wartenstein Symposium called *African Ecology and Human Evolution*, published in 1963, that came to have a very major impact on naturalistic as well as paleoanthropological scientific studies in Africa. It's often thought to constitute a real turning point towards cross-disciplinary researches. I think that, and the preceding circum-Mediterranean one, certainly demonstrated that people of different disciplines can live and work and talk and associate together comfortably and freely with no incertitude or anything like that. Absolutely sure, and these were extremely important. The significance of this decade with particular regard to human evolutionary studies is exemplified by the very contributions that are assembled in the volume entitled *Ideas on*

Human Evolution: Selected Essays 1949–1961 edited by William Howells (with an s, no relation; [published by] Harvard, 1962), which some of you may be familiar with; if not, it's worthwhile reading those essays, if they were [originally] in German, they're translated into English. As a consequence of the aforesaid overseas travels, and museum and field researches coupled with participation in the aforementioned and other professional meetings, I gained an uncommon experience and a recognition of problems and the value to science of interdisciplinary researches. This occurred within a decade after my doctorate. So I considered myself not only unexpectedly fortunate but repeatedly and invaluably so. In these years, the most useful contribution to human paleontology were several editions of *Les Hommes Fossiles* by Boule and Vallois, including an English language edition in 1957. And volume 7 (on primate and human paleontology) of the *Traité de Paléontologie*, by Jean Piveteau; I eventually met him in Paris. Also the first such synthetic volume, *Les Néanderthaliens*, was published by Etienne Patte in 1955, a year before the Neanderthal Centennial, and he followed it 2 years later by a very useful monograph devoted to the Pech de l'Aze infant Neanderthal skull. I should add that some other major serials of interest to us had their genesis in the 1950s. We've done this [meeting] in association with DEUQUA; *Eiszeitalter und Gegenwart* saw its appearance in 1951, and I've been a DEUQUA member since 2 years before my doctorate, I thought it was a unique organization, and it was, but I've never been to a single meeting. What a shame. It's always at the wrong time of year. *Quaternaria* also started in 1954, *Vertebrata PalAsiatica* in 1956, *Radiocarbon* started in 1959, and *Current Anthropology* dawned in 1960, thanks to the hard efforts of Sol Tax. In the decade between 1951 and 1962, I contributed towards definition and critical evaluation of the so-called Neanderthal problem in ten published contributions. These were published variously in scientific journals, largely anthropological (five), and biological (one), a learned society, an encyclopedia, the International Geological Congress Commission, and in the Neanderthal Centenary Volume, *Hundert Jahre Neanderthaler*, which von Koenigswald edited. And I was to return repeatedly to the same or closely related topics in future decades as well. Overall these contributions reflect markedly the influence on me of the predominant framework of the modern evolutionary synthesis as I learned, experienced, and employed it in those times. Collectively their overall contents span much of the available and pertinent empirical data relevant towards efforts to evaluate and to seek to comprehend the role and relative place in hominid biological and behavioral evolution of those extinct antecedents of modern humankind. Many of the roots of numerous subsequent, more extensive and intensive scientific studies of specifically human paleontogical or even more broad-based paleoanthropological investigation may be similarly traced there. However, certainly there are major concerns now, unenvisioned half a century ago, especially in regards to technological developments and their elaboration in newly recognized or defined fields of research, concern and investigation; and development and applications of various methodological procedures that are innovative; and, last but not least, the enhancement of relevant theoretical frameworks including those based on hypothetico-deductive reasoning, going back of course to people involved with history of science. I think it's absolutely vital that if you do science, don't be a technician only. Have an appreciation of what you do in a historical perspective. It may not necessarily help you, it will not hurt you, it will make you happy, you will smile, it is fun, and it is important and it gets better all the time. History of science is a widely flourishing enterprise. An appreciation of past efforts and understandings of our scientific forerunners is an essential and requisite part of a scientific endeavor. Too often such history is ill known, it's ill appreciated, and it's poorly reported. In the coming days here in Bonn, we're assured of much that's hopefully new and even unexpected in our perpetual pursuit towards fuller understanding of the distant human past, and you've been very patient; thank you very much.

[Applause] Thank you. Thank you.

FCH: If you want to ask a question, please stand up, and state your name.

Q: [How about Piltdown?]

FCH: I'll tell you, if you want to know about Piltdown, you have a person who has been in charge of it for some years at the Natural History Museum, and if there is something specific you want to address, I am sure he would be happy to do that. That's my longtime friend, and I offer now my personal congratulations to a new FRS, [Chris Stringer]. What would you like him to answer?

Q: [Does he agree about who was the forger?]

FCH: …You want me to say who I think it was … There was, not now, there was a substantial *badinage* about this … various people took it up in different ways. For example, the lamented Frank Spencer, a very fine historian of physical anthropology, G. A. Harrison, all sorts of people have written about this in different ways, and the Weiner book is still an outstanding book on the subject. Phillip Tobias is the one who really pursued the Arthur Keith association. He was unconvincing to me, in my opinion, you could ask why, but I won't go into it here – I felt that it was farfetched. My feeling was then, and for various reasons strongly, that it was a man named Martin Hinton; he was a worker in the museum and later sort of worked his way up. He's famous to people who know about murid rodents and so on, wrote a fantastic book for its time about arvicolids. I never met him, although he was still alive when I was there; he was retired by then. He was a great teaser, a taunter, a jokester; they found things associated in his *equipage* subsequent to his death and so on. I never talked to Chris about this. We talked about "business", and that's not "business". That's an event, in history. We can ask Chris, what he thinks about this … Are you there? …

CBS: I've actually written a [small] part of the story, because we had a 50th anniversary event

FCH: I remember you did, but I never saw it …

CBS: It was a 50th anniversary of the exposure, and so we had an exhibition, lectures, and I did a bit of extra research, and a new edition of Weiner's book was published. So I think you are right, that Hinton's behavior was certainly suspicious. My colleague Andy Currant unpacked a trunk from the attic above the old Keeper's office, with Hinton's initials on it, and in there were bones that had been cut and stained in a very similar manner to the Piltdown remains, and I think probably Kenneth Oakley secretly suspected that Hinton was involved. But my own work, and not just my own, Joe Weiner's work long ago, points to the fact that Dawson is still a very strong candidate. He found the first remains that we know of from the site, he found the last remains that we know of, at Piltdown 2, and I think it's a complex story. There are two sites, Piltdown 1 and 2, and Dawson is the only one who we can associate with the separate sites

FCH: with both of them …

CBS: and the fact is it's almost certain that the jawbone that was found at Piltdown 1, a tooth from that then turned up at Piltdown 2, and Dawson is the only link between those finds. So I think he's a very strong suspect; one can argue that he didn't have access to all the material or the knowledge to do it himself; with someone to help him, Hinton might come into the frame. But I think Hinton also comes into the frame because of the very weird object found in the last days of digging at Piltdown 1. They found, apparently under a hedge at the site, a large chunk of elephant bone that had been carved, and even at the time, some jokingly said "what's more appropriate for the earliest Englishman than something that looks like a cricket bat?"… [laughter] … It really *does* look like it, made of elephant bone, and of course that was faked too, it was carved on fossil bone with a steel knife, probably, and the fact is that I think one can look at the stuff that was found in Hinton's trunk and you can see that perhaps Hinton for whatever reason, maybe jokingly, maybe he just wanted to bring the thing to an end, he might well have planted the bat there, because it is so outrageous, and then to his horror, he saw it published as the oldest bone artifact in the world. [laughter] … And then strangely, straight after that of course, Dawson seems to start to lose interest in Piltdown 1, and he starts to go off and develop another site. So you can put 2 and 2 together and say, yes, Dawson did most of the stuff at Piltdown 1, Hinton planted that elephant bone, which is why he is then very evasive whenever Piltdown is mentioned. He really seems to be hiding something, but I think Dawson's still the main candidate.

FCH: There you go. OK, what else?

Q: Clark, you pointed to a lot of interesting developments in the '50s, you obviously were around for interesting developments in the 60's, 70's and subsequently. Did you experience similar things in subsequent decades? Or perhaps even more spectacular developments?

FCH: … A few years ago (in my first such appearance), Gerhard Bosinski (who like you was a student at the time of the last Neanderthal meeting), anyway at a certain point Bosinski asked me would I come and give a lecture to this group of people in Neuwied who were like friends, and I said OK. So I arrived and fortunately I had something written out, because often I just talk. He said well, we have to put it into German, and I almost fell over, and he said oh we'll translate it together. He said, is that [good], the way I rephrased. And I said well it's pretty much, but it takes so arduous, can't you shorten the distance between the verbs? So I gave the talk before this group of people, and Germans are pretty tolerant people, [with] very good manners, and they sat through this, gave me a hand, and I talked about some other things that would be of interest

Now, this is like a footnote, most people, many people more or less focus on one line of endeavor, they venture just at the edges, but they never sort of break the edges and go out. I haven't been like that. I was like that as a student, aside from trying to sort all kinds of things so I could find my way in the world, and so on but I never sort of said this what I'm gonna do for the next 125 years. I've done a lot of different things and some people here would say "I'm sure glad you did those things, but why don't you ever finish them?", and my answer is there'll be people behind me who will finish it, and they'll probably do a lot better than I did. But anyway I did the best I could, and I obviously have a short attention span in some regards. I mean, I'll pursue something like a birddog, you know, I just can't let loose of it, and then they'll be a point where it begins to relax, and then the next thing you know I'm over here, and that's because I really discovered there's something about something else that I didn't recognize before or I would have been more over there already. I don't know if you know exactly what I'm saying, but this really has to do with things that catch your interest, things that brute force you, pull you, twist you in a certain intellectual direction. I believe that composers have this kind of thing happen to them. I know nothing about music except adoration, but I believe that probably [they], maybe painters who shift their gears in the way they paint, like Van Gogh from the "Potato Eaters" to the [unintelligible]. I know in my own instance that I have been perpetually, everlastingly rejuvenated by this shift that transpires, and I think it will be judged in the future, not now, but it's certainly been important in terms of what little bit I've had to do with this thing that's become a science in my lifetime.

You might call it something else, but you certainly could not call it paleoanthropological sciences way back in the past, even in the 1930s by any means. You cannot call a Pre- and Protohistoric Science Congress a paleoanthropological congress, it's Pre- and Protohistoric Sciences, and the meetings of the American Association of Physical Anthropologists or the German Association of Human *Biologie* is not paleoanthropology. So when you talk about paleoanthropology it's something that is more inclusive, it's something that encircles a series of things within it, alright? And that didn't happen until subsequently, which is what I tried to indicate tonight without overstepping. And I believe that's true, and I believe you can see it. We still do not have the people who are doing history of [our] science and so on, the way there ought to be, I won't say should, the way there ought to be people, even yet. Richard Delisle did a recent book (he's a Canadian), and it's a good book, but it ain't the right book. And two people, for utterly different reasons, said so: one at the beginning, Milford Wolpoff, with whom I often disagree but sometimes think "that's an interesting idea," or Bernard Wood, with whom I equally disagree about certain things, but I have known him a long time. And they both were unhappy about this kind of thing. There was also another small book published that has to do with australopithecines, mostly *Australopithecus* and its coming into [favor] and so on. The best book that had to do with human evolutionary studies in the broad sense, but without enough of paleoanthropology as a whole in it, was Peter Bowler's book. He's a wonderful historian of science, there isn't anything he has written that is not worth reading.

I feel that people come into the world to do this. I think they could come from Germany, they could come from France, they could come from anywhere now. You have to have several languages, that's not a problem anymore and so on. There is plenty to say about all these things. And it's a happy, busy, creative world that we're all a part of, and I wish that Angela Merkel could talk to George W. Bush, and say "George there's really something important here, would you be willing to get off your bike and listen? …" [laughter] It's not going to happen … [applause].