Passing the Field, or Passing it Up?[1]

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Darwin inferred Africa to be the cradle of the human species and of man's intelligence. Raymond Dart supplied the first hard evidence supporting this (initially unpopular) view. The Leakeys, their colleagues, and their successors, over more than half a century of sedulous field work, have assembled a formidible body of empirical evidence for our African origin, one that few paleoanthropological pundits seriously gainsay. Laetoli, a monument to the science of Mary Leakey, is about no ordinary fossil locality and has contributed as much to our understanding of early hominids as has any single other site. It is also much more. Discovered in 1935, almost forgotten, and rediscovered in 1959 (Leakey, 1984), Laetoli was worked by Mary Leakey and colleagues for several years following 1975. The volume at hand is a worthy tribute to this exceptional paleontologist, and to the skills of the editors, the authors, and the publisher. The widely publicized footprint evidence of early hominid bipedalism is not only asseverated, but dramatically so (the remote possibility of the footprints having been produced by bears notwithstanding, surely it is illative to ascribe human attributes to near humans). But footprints and fossil hominids aside, Laetoli remains unusual in its wealth of other fossil primates, nonprimate mammals, reptiles, birds, and molluscs. Atypical of any site study (and a joy to see here) are three outstanding contributions on the Laetoli ichnocoenoses (those of termites, bees and wasps, and a variety of animal prints and trails).

There are few difficulties with this work. A geologic history is lacking, an omission perplexing in a large monograph for which (presumably) such luxuries are yet possible. Only the public currency of Laetoli prevents asking what the point was. Moreover, the summary is inordinately brief, 7 of 561 pages. The want of either a geologic history or a substantial compendium changes the monographic stature of the book to one of a treatise lacking synthesis. The writing style throughout, though highly professional (and as laconic as so much systematics admit) communicates little of the enthusiasm that led many of the authors to campaign in the hot sun for nearly a decade. Information is imparted to us as though from a great distance; a lost opportunity, if ever there was one, for paleontology with a human face. There is a middle ground between the ennui of descriptive science and the lay readership.

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[1] Laetoli: A Pliocene Site in Northern Tanzania. M.D. Leakey and J.M. Harris (eds.). Clarendon Press; Oxford. 1987. $150.00.

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A few chapters are likewise unnecessarily dry and descriptive. Traditionally part of the stuff of which our science is made, description should be limited to concise embodiment of percepts from which to flesh out concepts. Percepts are abundant here; concepts are wanting. But no minutiae, however tedious, conceal that nearly every particular concerning Laetoli is interesting, and most are spectacular. The volume is a highly competent documentation of what is known about an extraordinary locality. Precise description and competent interpretation of the spectacular enable us to better understand the smaller bits and poorer pieces of a mundane world. How easily we forget that a fossil locality was once much more than fragments in museum drawers.

Laetoli affords us a nonpareil opportunity to walk in the mud, feel the rain, and brush off the bugs of a former epoch--a safari worth the $150.00 fare. I have never been to Laetoli; now I know that I want to visit there. This work presents no stack of evolutionary records. No new principles are espoused, no old ones defended. We learn little of adaptations and nothing of major extinctions. We do learn most all of what is known (and currently knowable) about an extraordinary locality.

Another and most important particular is that _Laetoli_ resulted first and foremost from a field study. The operative word is field--one apparently foreign to a growing and influential body of younger geologists (including paleobiologists). From reviewing the last decade of literature and research proposals that have crossed my desk, it seems to me that field work has suffered a retreat approaching a rout. Part of this decline is clearly attributable to bad press from within. For example, as early as 1963 an AGI study of undergraduate geologic education termed field work ". . an escape mechanism used by geologists to avoid serious scholarship" (Hambleton, 1963:12). One might well ask from where this "serious scholarship" is ultimately derived. Ignoring the obvious answer, it is instructive to observe what is happening in the geological sciences today, to understand where we stand and how we got there, and to chart a course toward the most useful and lasting of what we can contribute.

In my field, paleobiology, there has likewise been a rather rapid decline in the percentage of evolutionary studies and theory gleaned and conceived from empirical data (field or otherwise) to the gain of conceptual "innovations." I am old-fashioned enough to believe that concept should follow data, and I am therefore somewhat baffled by this scissors crisis. Though most theories contain merit, some time in the last 20 years they have come to enjoy equivalent stature with whatever empirical evidence supports them, and more amazing, have even been used as a test of the veracity of that evidence. Moreover, paleontologists who do not go to the field and lack the first-hand data to pursue more traditional avenues of paleobiological enquiry have seen fit to either devise or embrace one or more of several relatively new theoretical approaches which minimize the contribution of field data. This state of affairs has an historical causality lying in the interaction of at least four factors: (1) the demise of rigorous undergraduate training in the fundamentals of geology; (2) the explosive birth of popular new theories with which to associate oneself; (3) competition and increased emphasis on accomplishment in the tactics (as opposed to the substance) of scientific achievement; and (4) the trends toward orthodoxy in science.

In a keynote address to the Geological Society of America, Dave Love (1988) observed that ". . in spite of fancy time- and effort-saving equipment, we have not significantly improved on the quality of field observations made by the giants of geology 100 and more years ago" (my emphasis). Love attributed this standstill in observational competence in the face of technological innovation to a lack of rigor in basic undergraduate field, laboratory, and lecture-hall curricula. Gone at many universities are the traditional basics: physical and historical geology, geomorphology, mineralogy, structural geology, stratigraphy--even sedimentology and a required summer field course. Gone also are many hands-on-fossils courses in paleontology; the standard Invertebrate Paleontology is replaced by ("basic") Invertebrate Paleoecology and Evolution, or Invertebrate Biosystems. It is difficult to appreciate invertebrate paleoecology and evolution, much less their "biosystems", without at least a semester introduction in what invertebrates are.

We have read our packaging, found that it was good, and have decided to believe it. If our starter course is (or sounds) more advanced, so will our thinking be when coursework is completed. Wrong. "The product has been 'geologists' with very limited understanding of their discipline--geologists with tunnel vision, unable to relate what they know to the rest of their science." (Pettijohn, 1984:13). It is disturbing to see graduate students trained basically in theory contributing yet more theory with little or no experience in empirical data gathering. Though unconsciously relieved of the baggage of a lot of work and a miasma of problems associated with the tedium of empiricism, they are also freed of a more desirable universe of new data, contextual information, and (most importantly) scientific restraint. It is not simply a curiosity that the longest monographic works often contain the least speculation. As conclusions must be supported (and are, at times, lengthened) by observations, those same observations offer severe constraints on speculation.

If this were all there was to it, we could put our feet up and recommend remedial course work. Unfortunately, trends in paleontologic curricula have evolved concomitantly and deterministically with the other three factors noted above. The 1960's and 1970's were relatively good years economically and produced a glut of paleontologists, all competing for funds. Later, generally through design or lack of funding, students unable to pursue field studies adapted, there being plenty to do in the laboratory. These students were successful, they became professors, got jobs, produced more students who, under their tutelage, found little use for field studies.

Many students look for direction at some point in their careers. The more apparently rigorous and ordered the direction given and the more confidently the method is taught, the more likely it will be adhered to by its acolytes. In paleobiology in the last two decades, such direction has been most admirably provided by the approximately coeval expansion of three basic theories and/or philosophies--all are now enjoying a popular currency. These are: (1) the cladistic approach to phylogeny reconstruction (Hennig, 1966); (2) the theory of objective knowledge, or "Popperism" (e.g., Popper, 1972); and (3) the theory of punctuated equilibria in organic evolution (Eldredge and Gould, 1972). Though conceived quite independently, these polemic ideologies derive sustenance from (and have been conflated with) one another; their harmonic congruence seemingly implies that we at last have begun to organize a unified theory of methodology and synthesis in evolutionary studies.

As their influence has spread, some devotees have become more authoritarian, and it appears to me, less inclined to weigh opposing evidence objectively. They have also, perhaps unconsciously at first but with increasing overt vigor, contributed not only to the decline of field-derived empiricism but to loss in perception of the need for it.

Lest I offend my many cladist, popperist, punk-E friends and colleagues, I am pleased to applaud the obvious merits of these pabula, not the least of which has been a total revitalization of thought in paleobiology. I am also obliged to voice more general concerns, especially as they relate to other stimulating and cherished scientific companions--empiricism, objectivity, and 24 years of field work.

Everyone loves an exciting new theory; who has not been fascinated by hot-blooded dinosaurs, chaos, string theory and plasma cosmology, plate tectonics, big bangs, biomeres and bolides? Now we have putative cold fusion. However, we do not need new lamps for old just because new lamps are available and the old ones have problems. They must shed light as well as the old, and preferably better. Paleobiology, like cosmology, is now seemingly plagued by the necessity of a grand new theory every time there is a novel observation (Lerner, 1988). Though clothed in fine raiment of respectability and erudition, many a novel concept is composed of whole cloth, and from an inchoate fabric of evidence insufficient for a modest breechclout. It ". . departs on its travels, is received everywhere with admiring acceptance, and not only as a piece of rare and acute observation, but as being exhaustively true and profoundly wise; and so presently takes its place in the world's list of recognized and established wisdoms, and after that no one thinks of examining it to see whether it is really entitled to its high honors or not." (Twain, 1962:53).

Theory is cheap. The only test of a new concept is its capability to consistently unify new and old data. In paleontology, as in geology in general, the source of those data is ultimately specimens and observations in the field. "If the appropriate observations are not at hand, ideas are not testable and so" [must] "fail to become part of science. . . whether the idea catches on immediately, however, depends upon the openness of the science at that time" (Oliver, 1988:157). We must examine our motivations in accepting new concepts, investigate how and why they were brought about, and explore why our science is now so ready to receive them. Our relationship to them must be insulable, not empassioned. Significant constraints are added to perception and creativity by their ready acceptance. We must recognize this effect and defend our science from being overwhelmed by any appeals or pressures toward orthodoxy of thought.

Cladism and Popperism are basically negativist philosophies. My admittedly querulous view of them results simply from their emphasis on what we cannot do. Cladism avers that we cannot know ancestors and, by implication, that we need not look for them. Popperism informs us that we cannot prove anything in science, only offer disproof. Even truth cannot be proved, only falsified. And yet, because there is falsehood, does not scientific truth exist? (see Hsü, 1988). In an infinity of possible falsehoods lies the truth and confutation is proof of falsehood, which is scientific truth in a way; it's just that we may have to wade through this infinity to reach it. Cladism offers us an infinity of systematic arrangements, but advances no solution as to which arrangement in this gallimaufry is the closest approximation to the true one, except perhaps our faith that we have made all the proper assumptions of polarity and selected the most worthy outgroups. Adding Popper to Hennig, we choose among the myriad phylogenetic options only by disproving them one by one--instant job security.

We can abandon slavish adherence to orthodoxy by an appeal to reason and by recognizing that most of science--the universe in fact--comes to us through our definitions. A definition is scientific fact (=truth) by fiat. A sample of unreworked fossils may be considered to be evidence proving that an outcrop of rock is of Paleocene age. We defined the Paleocene and it does not exist independent of our thinking about it. Therefore, we should be capable of constructing this or any other definition to be capable of proof--proof again by our definition. A Popperian might construe these fossils to instead disprove that the rock is Cretaceous, late Oligocene, or Middle Cambrian. Obviously, the number of possible disproofs is infinite; if the fossils suggest Paleocene age (by our definition), few but the most paranoid are left with nagging uncertainties that the same fossils might lurk in a putative Ordovician limestone in Wisconsin.

But definitions are tricky things; by understanding them we are encouraged to believe that we comprehend what is being defined, and this usually is not the case (see also Gleick, 1987:281). Their advantage is that, if derived from observation, they are as close to statements of fact/truth as we can approximate. Popperism is correct in instructing us that there are things capable only of disproof, but these things are theories--not observations. Observations are our only contact with reality. Yes, there may be things we cannot know, but it is clear to me that one thing we do not yet know is what they are.

Cladism takes us yet a step farther and, like punctuated equilibria, tempts us with the hope of solving more problems better with fewer data. Methodological approach and philosophical orthodoxy, respectively, add to the appeal but detract from the utility of both. Originally put forth as a mechanism by which to sort out phylogenetic systematics of embrangled groups with little or no fossil record, cladism is currently espoused as the best way to resolve the systematics of any group--good fossil record or no.

I take issue with cladism not as a useful means of ordering organisms with poor records (which it is), but in the way it has come to be used. Stratigraphic context is no longer important to the cladist; phylogenies are sorted out exclusively by assuming polarities, examining characters, and making comparisons with the appropriate outgroups. More for less, and doubtlessly comforting to museum scientists who do not care for the field or who have immense collections made by the venerable ancients but lacking precise locality and stratigraphic data.

But don't the relations of the fossils to one another in the field mean anything? Hennig himself (1966) stated that, where available, stratigraphic information can be quite useful in establishing character polarities. My cladist colleagues insist that stratigraphic data are important as a check on phylogenetic/temporal compatibility, but their writings embody their views--I found not a whit of useful geologic information in the last 50 or so cladistic contributions received. The theoretical tail wags the empirical dog when fossils are ignored if they do not conform with theory (e.g., Schaeffer, _et_ _al_., 1972; Rosen, _et_ _al_., 1981; Patterson, 1981)--_lucus_ _a_ _non_ _lucendo_.

Addressing an audience of evolutionary theorists, it is a curious feeling to feel obliged to defend the utility of fossils. No more did Mommsen require the evidence of monuments, epigraphy, coins, ruins, Polybius or Tacitus to reconstruct his Roman history, but that history would have been the sorrier for their loss. Quality of research data may be used to select methodology; it should never dictate theory.

Hawking (1988:92) observed that "Black holes are one of only a fairly small number of cases in the history of science in which a theory was developed in great detail as a mathematical model before there was any evidence from observations." I have the highest respect for the padres of punctuated equilibria; however, I am aware of no concept in the natural sciences that has from its outset been embraced by such a propitious audience, and yet was (and is) based on so little empirical evidence. I feel it is a synecdoche for the putatively moribund theory of Darwinian gradualism. Again we are instructed that less is more--no longer need we labor slavishly in the field like drones, collecting more fossils to fill in evolutionary gaps. The gaps, we are informed, are evidence in support of punctuations. But gaps are not evidence at all (Hecht, 1983; Gingerich, 1984; Bown and Rose, 1987); rather, they are a whole field of enquiry in themselves, and (by definition) one characterized by an absence of evidence. I have even heard there are those who believe that absence of evidence is evidence of absence. This is simply an excursion in fustian reification. Unless I am missing something, the significance of an absence of evidence is palpable; it is nothing--zero.

Punctuated equilibria has certainly not been without its recent critics (e.g., Dzik, 1983; Gingerich, 1984; Dawkins, 1986; Sheldon, 1987; Kuo-Yen, 1987; Brown, 1987; Bown and Rose, 1987; Levinton, 1988), but few would contest that there is abundant evidence for stasis, one of the theory's central tenets. Though stasis can be construed as evidence of punctuation (in that a punctuation could be defined as any change in evolutionary tempo), it is not evidence of puntuated equilibria. That ideology contends that what appear in the geologic record as punctuations are actually intervals of extremely rapid and profound genetic reorganization--so rapid that morphological evidence documenting them is unlikely to be discovered, and so profound that new taxa subsequently appear to have arisen almost instantaneously. Though it is clear that stasis is evidence for itself (at least in some characters, if not in a taxon; see Bown and Rose, 1987), it says nothing about evolutionary tempo in the generation of new taxa.

Regarding anagenetic change, Gould (1988:328) stated: "Valid cases tend to add a rib, a bump, or a millimeter over millions of years--and such changes simply do not extrapolate to the evolutionary patterns that historians of life are charged to explain (they are also too slow to ascribe to conventional directional selection). . .". Actually, they add several bumps and millimeters in less time than "millions of years", and this is only in what few parts of the anatomy are left for us to examine in number and detail. Evolution <u>is</u> bumps and millimeters, and enough of them will even change the eyes of trilobites and produce new taxa. The contest between Darwinian gradualism and punctuated equilibria is concerned with how fast new bumps and millimeters arise, whether they arise all at once, and whether they arise following <u>total</u> character stasis in a taxon (not only in one or a few characters). If some of us remain skeptical, it is not through resisting the advance of science toward new marchlands by shoring up an abatis of anachronistic viewpoints; rather, the new theory has not yet successfully replaced the old because it conflicts with a lot of evidence. In this conflict, it is theory that must bow for the time being.

In my own work, I was pleased to welcome both cladism and punctuated equilibria as tool and theory, respectively, with which to pursue investigations. But the cladograms could not portray detail otherwise available through conventional stratophenetics; detail so dense that several valid, well-established clades were sister groups of themselves, and in which characters did not appear all at once like nodes on a cladogram. Groups of organisms **had** ancestral groups. If we choose to believe, **a priori**, that these groups cannot be recognized in the fossil record, we must assume that older specimens from the same geographic area and a few meters down, and which resemble them in nearly every fine detail, are not from ancestral populations. Venturing farther, if this is true, we encounter staggering problems of theoretical parsimony--we are forced to postulate innumerable immigrations and extinctions of animals almost exactly like their stratigraphic precursors and successors; these exhibiting minute, but successive and cumulative character changes in an order suspiciously alike that of our old nostrum, Darwinian gradualism. Retrieving Popper from his temporary retirement, punctuated equilibria (even the current palimpsest) as the only viable embodiment of tempo and mode in organic evolution is already disproven; I suggest that whether or not it is even a significant factor has yet to be demonstrated. Resolution of this question will require not more theory, panegyrics, or mellifluous mafficking, but exhaustive field work on all possible organisms from the most temporally complete and abundantly fossiliferous sections.

The significant gap which I now perceive is not paleontological but philosophical. Cladism and punctuated equilibria are born of our frustration in dealing with an incomplete and at times importunate and unyielding fossil record; a record generally wanting in both stratigraphic and geographic detail. All of us are exasperated with it and hanker for more, and for more faster. But let us not forget: "No scientific activity teeters more precariously on the precipice between bravery and foolishness than descriptions of unobserved objects justified only by their necessity in theory" (Gould, 1985:438).

I have at hand two surveys soliciting my opinions anent the veracity of two current geologic concepts. Have we stooped to this--is acclamation the wave of the future for pure as well as applied science? I am admittedly torn between the pride of having my opinions sought and the knowledge that, having done no original work on the hypotheses, little I can contribute concerning their health is worth recording, and nothing I can do will affect whatever truth is in them. I cannot imagine the purpose of canvassing scientific opinion in this manner, except that these polls are symptomatic of what I sense in publication and granting trends. To wit, a self-reinforcing undercurrent of belief that there are now approved ways to do science. The very "necessity" of polling indicates that a clear victor is not yet at hand and a poll serves no purpose except as a nugatory barometer of our own éclat. By endorsing such flummery, we stand to gain a bandwagoneering profession in which are lost all intellectual objectivity and creativity, and eventually all scientists who do not follow a career interlarded with fleeting buzzwords. In our strive to remain current must any of us be content to trade creativity for acceptance? "If scientific issues were always decided by Gallup polls and not by scientific arguments science will very soon be petrified forever" (Alfvén, 1988:251).

Publicity trends are an excellent example of what Gleick (1987) termed the **tactics** of scientific achivement. Whereas in the past a paleontologist's tack included his pick, specimens, and laboratory, to these we must now add the press agent. No successful paleobiologist is compleat without one. Conflicts of scientific opinion, once waged almost exclusively for an amphictyony of paleontologists, are now aired in the public press. Used at first to announce important new discoveries of interest to the lay public, the press agent is now a vehicle to disseminate new ideas, or even shopworn old ones couched in new terminology. It now appears that its most important function to our community is to preserve the currency of the ideas of his or her maker. This is accomplished by leaking information that, although you haven't heard from Dr. so-and-so lately, rest assured he is thinking about it and when a few other matters are settled he will see his way clear to once again set science on its ear.

It may be true that the essence of power and influence is that creators of categories persuade the world to view things their way (see Boxer, 1987); however, it is instructive to keep in mind that commonly (and true to their creative powers) innovators of successful bandwagons quickly move on to something else, thereby sidestepping the fray and eluding entrapment in the growing orthodoxy of their own creations. Root-Bernstein (1988) suggested that creativity can be taught, but I doubt it. I share with that writer the perception that the most creative people I have known are typified by their unwillingness to conform to much of anything in life. Their success derives from good basic training, curiosity, keen observational powers, abhorrance of orthodoxy, and a lot of hard work. Dogmatic instruction, even in creativity, carries an inherent subscription to orthodoxy--if you don't do this you won't be that. Creativity is its antithesis.

What are the lessons of Laetoli? First and foremost it is a field study. No amount of conceptualization could foresee what would be found there or do better to aid us in understanding its paleocommunity relationships. Theory could predict that very early hominids were fully bipedal, but disproving the Popperian proposition that this could not be proven required the field efforts of Dr. Leakey and her associates. Field work with fossils and the rocks that contain them is our only source of new evidence, evidence which not only builds theories but affords a means to test them. That sustained field efforts are valuable is also evident; insignificant at first inspection, Laetoli was returned to only after many years.

It is alarming that now bruited about is the idea that theoreticians in paleontology have and had little need for field work and research collections. This fallacious view has already forced both the discarding of important research collections acquired over many years in the field (Gingerich, 1986, 1987) and the wholesale appointment of theoreticians to university chairs held in the past by field empiricists. Is it not at the level of specimens and their field contexts that paleontology embraces geology, paleoecology, and biology? We must improve our dialectic if we are to achieve a balance between empiricism and speculation; and above all, we need to resist dogma if we are to defend the pure science proportion of our profession in a formerly generous society increasingly unable (or unwilling) to support the mendicants of pure science.

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