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Anatomy of an Anomaly

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I. INTRODUCTION

Since the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* (1970), a great many people in the sciences and elsewhere have used his distinction between paradigms in normal science and anomalies both in normal science and in scientific revolutions to explain developments in contemporary science. Not all appeals to Kuhn have been equally illuminating. It has sometimes seemed that those on the fringes of established science cry "paradigm bias" to explain why their work doesn't get any attention when it is in fact the work itself that is to blame. Presumably, some evidence that conflicts with received views is ignored for good reason, and some without good reason. When an apparent anomaly is dismissed for no good reason, then the scientists in question are behaving badly. But are they behaving "unscientifically"?

In this study, we examine in detail a particular case of anomalous evidence meeting received view. In this case, the received view is a theory about human origins in the Americas, and the anomaly is a site in Mexico, the age of which is apparently in conflict with that received theory. Without trying to decide whether the received view is correct, or whether the anomalous evidence is worth considering (which is, after all, a job for specialists), we will follow the story of what happened to the scientists involved, and draw conclusions about what can and cannot be expected from science as a real human institution. In particular, we will argue that, in periods of instability in science ("revolution," if you like), it is in the very nature of science to treat anomalous evidence with hostility and suspicion, even when there is little evidential reason to suspect it.

II. THE RECEIVED VIEW

The received view, accepted by a majority of anthropologists and archaeologists, is that humanity did not evolve independently in the Americas, and so must have migrated there from elsewhere. For various genetic reasons, it seems that all aboriginal Americans are more closely related to one another than they are to any other populations, and are more closely related to the peoples of Asia than those of other parts of the world. The reasonable conclusion to draw from this evidence is that the first Americans migrated from Asia, either across the Bering Strait or across a land bridge. Large-scale migration by boat is unlikely, even across so narrow a body of water as the Bering Strait, so a hypothesized Bering Land Bridge is the best hypothesis for a migratory route.

This sequence of deductions entails a limited number of opportunities for migration. A land route was fully available only when there was sufficient glaciation for sea level to drop by about a hundred and fifty feet; such a drop in sea-level is necessary for the Bering Land Bridge (or, perhaps more properly, the land mass now called Beringia) to appear. On the other hand, if there was so much glaciation that land routes across North America were impassable, no migration could take place. These two constraints severely limit the number of opportunities for migration to special periods during ice ages.

The best candidate for a time for that migration is generally taken to be a period during the Late Pleistocene, about twelve thousand years ago. Although claims of earlier migrations are occasionally pressed on the strength of archaeological finds, the view that humans arrived relatively recently seems to be fairly well-established. So confidently was this view held that in 1962, writing for *Scientific American*, William Haag could say, “Man’s occupation of the New World may date back several tens of thousands of years, but no one rationally argues that he has been here for even 100,000 years.”

There is an impressive array of evidence for the recent-migration view, and comparatively little for any earlier human presence in the Americas. What seemed to be evidence of earlier occupation has usually turned out to be misleading. David Meltzer (1993) describes the situation this way:

> By the early 1950’s there were already indications of a much earlier human presence in America. Those hints would become broader as the years went by, until today scores of purportedly ancient sites have appeared, some with estimated ages upwards of 200,000 years. Each new candidate for great antiquity brings with it fresh claims, but the outcome remains the same. Skeptics ask hard questions. Debate ensues. The claim is accepted by some, rejected by others, while the rest wait and see. So far at least, the Clovis barrier remains intact. A pre-11,500 B.P. human presence in America does not now exist.

There are at least three impressive kinds of evidence for a Late Pleistocene migration (or set of migrations): evidence from Native American languages, evidence from dentochronology, and evidence from mitochondrial DNA.[1] All three kinds of evidence point to three waves of migration, the earliest in the Late Pleistocene, as hypothesized. The earliest clearly datable sites so far

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[1] We get this classification of evidence from Meltzer 1993, pp. 84-94.

are those at Clovis and Folsom, and they are no earlier than 11,500 BP. Add to these pieces of evidence the absence of clear evidence for anything earlier, and you have a powerful argument for the recent-migration view, which gives strong reason to be skeptical of finds that purport to be older. Consider the kinds of evidence in turn.

Native American Linguistics

The hundreds of thousands of languages that have been spoken on the American continents form a bewildering variety, but many linguists now think that they fall into three families: Amerind, Na-Dene, and Eskimo-Aleut. The Amerind languages show the most variety, and are geographically the most widespread, being spoken in areas from Canada to Tierra del Fuego. These two facts argue for the relative antiquity of the common language from which they all derive. The Eskimo-Aleut languages are fewer in number and more similar to one another. They are also spoken in a smaller area, around the northern coastal regions. The Na-Dene group is intermediate in variety and extent. Those languages also are spoken in areas to the south of the greatest southern extent of the Eskimo-Aleut languages, but not so far south as the Amerind languages. Moreover, the language groups can be arranged in order of similarity to Old World languages, with Eskimo-Aleut being most like, and Amerind least like, the languages spoken in Asia. This arrangement of languages points to three separate waves of migration, with the ancestors of Amerind speakers arriving first. However, this relative ranking gives us little in the way of absolute dating for the migrations.[2]

Dentochronology

In the 1920's Hrdlicka noted a trait that all Native American teeth possess, which is also characteristic of the teeth of the people of Northern Asia. On the basis of this characteristic, a particular shovel-like shape to the incisors called sinodonty, he concluded that Native Americans divided into three genetically distinct groups: Eskimos, Athabaskans, and South Americans. Christy Turner (1986) made a statistical analysis of American teeth to check this classification. Looking at other, similarly heritable characteristics of teeth, and cataloging similarities and differences from nine thousand different prehistoric Americans, he also concluded that Native Americans divided into three genetically distinct groups, but he identified the three groups more directly with Greenberg's three linguistic groups.

In addition to supporting the three-migrations view, the dental evidence can give us an absolute chronology. The dental characteristics that are identified in Turner's study are genetically determined, environment having

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[2] A full elaboration of this evidence and what it implies is to be found in Greenberg 1987.

little or no impact. In this way, the evidence provided by teeth, like that provided by blood groups, can give us a clear picture of the genetic relations among populations. Since mutations occur in a regular way, we can also tell how long ago two populations diverged by how many genes they share and in how many they differ. When a gene expressed itself in a visible and easily-preserved part of an animal, such as a tooth, then we can use the variations in that part to date the genetic history of the animal. In the case of humans in North America, we can tell by distributions of kinds of sinodonty that the North American population split from the North Asian population about twelve thousand years ago — which confirms the late-Pleistocene migration view.

The mtDNA Clock

Similarities in gross anatomical characteristics, and even to some extent in the genetic code underlying them, can sometimes arise due to similar environmental pressures, even when the two populations are not closely related. There are parts of the genetic code, however, that do not get expressed at all, or are expressed only in neutral characteristics. In those genes, the regular rate of mutation is not affected by environmental pressures. In particular, mitochondrial DNA (mtDNA) is not subjected to the mixing forces of fertilization, as all a creature's mtDNA comes from its mother. So given a reasonable estimate of how quickly and how regularly mutations occur in mitochondrial DNA, we can fairly accurately date when populations diverged. By that measure, Americans split from North Asians about 20,000 years ago. This is earlier than the other methods gave us for a first migration, but may be accounted for by the estimate of the rate of mutation.

The Response to Anomalies

Given this impressive array of evidence, it seems eminently reasonable to think of the Late Pleistocene migration as established. Even though there are occasional finds that seem to be datable to much earlier, it is more reasonable to think there must be something wrong with the dates for those sites than to accept them at the cost of overturning so well-grounded a theory. The inability to explain why a site seems to be earlier than the late Pleistocene is no bar to accepting the late migration theory, especially if the alternative is accepting an earlier migration while being unable to explain the linguistic, dental, and genetic evidence. Meltzer (1993, p. 21) characterizes the archaeologist's position this way:

> [T]his problem is compounded by too many false alarms. Scores of sites have been advertised as possessing great antiquity. But on closer inspection, each has failed to live up to its advance billing. Caveat Emptor. Archaeologists have long memories — it's part of our business, after all — so it is hardly surprising that under such

> circumstances any and all new claims for great antiquity in the Americas are met with skepticism bordering on cynicism. The response may not be commendable, but it is understandable.

Most of the archaeologists who give this understandable response are considerably less conciliatory than Meltzer. In fact, Haag's response cited earlier, which dismisses claims of extreme antiquity for human presence in the Americas as irrational, is the norm rather than the exception. The oldest sites that have stood up to careful scrutiny, and whose evidence is completely unambiguous, are Clovis and Folsom, both datable to after 12,000 BCE, and so completely consistent with the Late Pleistocene migration.

III. THE ANOMALY

Occasionally an archaeological find seems to challenge this received view. The specific archaeological project that is central to this work was located at Hueyatlaco, Valsequillo, which is a few kilometers south of Puebla, Mexico. The area had become very well known among archaeologists due to the varied extinct animal forms. The initial excavation began in 1962. During the continued process of excavation five sites were discovered and stratigraphic sections sequenced (Irwin-Williams 1967a). The final excavation at Hueyatlaco was concluded in 1973. Field work continued throughout the excavational process by the members of the team, including Dr. Cynthia Irwin-Williams and Dr. Virginia Steen-McIntyre.[3] Later consultants affiliated with the project were Ronald Fryxell, B. J. Szabo, and C. W. Naeser in continued efforts to resolve the dating controversy surrounding the evidence accumulated during the excavation process at Valsequillo, Mexico (Malde and Steen-McIntyre 1981). There were no irregularities in the team's methods, and the site was guarded to prevent tampering or accidental destruction of evidence (Irwin-Williams 1967a).

The principal investigator on this project, Cynthia Irwin-Williams (1978), characterized the archaeological site as an area that contained a "kill-site" and activities indicative of butchering and camping activities of "Early Man." The artifacts discovered clearly establish that they are of nonlocal origin, ranging from a crude unifacial percussion-flaked lanceolate object (projectile point) manufactured by a less sophisticated group, to bifacial cutting tools, scrapers, and cutting edges, well-made tools of an advanced nature. In her article published in 1978, Irwin-Williams states that the abundance of now-extinct fauna in the Valsequillo area attracted early hunters. There were locations in the area suitable for camping, and nearby were sites suitable for

[3] The complete team consisted of Dr. Cynthia Irwin-Williams, archaeology, Principal Investigator, Professor Juan Armenta Camacho, archaeology, Dr. Virginia Steen-McIntyre, tephrochronology, Dr. Harold E. Malde, geology, Dr. Clayton E. Ray, palaeontology, Dr. Dwight, malacology, R. B. Taylor, Dr. Gordon Goles, neutron activation analysis, Mr. Mario Pichardo del Barrio, palaeontology.

slaughtering activities and sites that were appropriate for butchering procedures because of the close proximity of small streams. Irwin-Williams acknowledges that modern estimates regarding the presence of man in this locale ranges from 11,000 years to more than 30,000 years.

Controversy began in 1967, before the digs were completed. Despite the thorough efforts and the competence of the archaeological team members at Hueyatlaco, Jose L. Lorenzo, Director of Prehistory at the *Instituto Nacional de Antropologia e Historia*, launched several allegations regarding the integrity of the project at Hueyatlaco, El Horno, and Tecacaxco (commonly referred to as Valsequillo). The most significant allegation was directed to the authenticity of the artifacts retrieved from the Hueyatlaco site. Lorenzo (1967) alleged that some of the artifacts had been planted by laborers working at the site and then commingled with other artifacts in a way that made it impossible to separate and identify the planted artifacts. The intentional commingling of evidence, if it occurred, would raise substantial doubts about the age of the site, as well as the integrity of the principal investigator and other members of the archaeological team. The allegations were addressed by Cynthia Irwin-Williams (1967b) in the *Paleo-Indian Institute Miscellaneous Publications* stating that the "allegations are utterly without basis in truth" and that Lorenzo was motivated "by distorted personal animosity and irrational inability to change an opinion." During 1969, Cynthia Irwin-Williams further refuted Lorenzo's allegations with written statements from three reputable professionals in the field of anthropology and archaeology (Irwin-Williams 1969).

By June of 1969, Barney J. Szabo and Harold E. Malde had completed their attempts to date artifacts retrieved from Valsequillo, and joined by Cynthia Irwin-Williams, published the results (Szabo, Malde, and Irwin-Williams 1969). One important means of dating the stone tools recovered at Valsequillo was to date the strata in which they were found by dating fossils and other animal remains from the same site. Radiocarbon dating on molluscan fossils (shellfish) which showed an age greater than 35,000 years. The uranium method gave a result of 260,000 ± 60,000 years. A mastodon tooth retrieved from El Horno was dated using the uranium method and was calculated to be older than 280,000 years. Likewise, a camel pelvis recovered from the Hueyatlaco site was dated using the uranium method closed system at greater than 180,000 years, and using the open system as 245,000 ± 40,000. A horse metapodial recovered from the Atepitzingo site in the Valsequillo area was dated using the uranium method open system date at 260,000 ± 60,000 years. In the concluding remarks of the article (Szabo, Malde, and Irwin-Williams 1969, p. 243) the authors noted, rather mildly, that some of these were perhaps too old stating that "we cannot explain why some of these dates are much older than expected from archaeological evidence." In the same article, Malde commented that one of the difficulties in evaluating the samples was possibly due to a lack of stratigraphic markers from the field for correlation with the various sample localities. Later (the results were published in 1981), he and Virginia Steen-McIntyre would collect samples of the stratigraphic layers including samples of pumice and ash to resolve just this point.[4] Additional stratigraphic information would help determine whether the artifacts were located in an erosional trough such as a stream channel, which would indicate that the beds bearing artifacts were of a younger age. This possibility raised doubts that could not be ignored. Drs. Steen-McIntyre, Malde, and Roald Fryxell, a specialist in mapping sediment layers at archaeological sites, returned to Hueyatlaco for the additional excavation. The work to determine the stratigraphic sequence was undertaken in 1973. This final excavation established a sequence of age for the first time, showing that the artifacts did not lie within a stream channel and thus, were not younger than the ash deposits that covered them.

With a more complete stratigraphic picture of the site developed by the 1973 trench, it now became apparent that Dr. Steen-McIntyre faced the problem of matching the ash and pumice deposits with a known volcanic source for purposes of dating. More samples were taken and examined, but none of them proved helpful in source identification. Pumiceous glass (volcanic ash blown into the air on eruption) contains glass shards which contain a large number of bubble cavities, known as vesicles. As the volcanic glass weathers, moisture moves through the exposed surfaces. In temperate climates this process may be completed in 20,000 years. As the pumiceous glass becomes hydrated, the vesicles also begin to collect water. The total filling of the vesicles may require ten million years or so. Thus, evaluating the fill within the vesicles assists in age determination.

Using a petrographic microscope and special light-masking techniques, Dr. Steen-McIntyre began the task of examining the samples of the volcanic ash layers from Hueyatlaco containing volcanic glass and mineral phenocrysts. Phenocrysts are mineral crystals that were growing within the liquid magma at the time of eruption. The examination process requires approximately eight hours of microscope time for each sample. During the microscopic examination of the phenocrysts, Dr. Steen- McIntyre detected a phenomenon she described as resembling a picket fence. The samples, instead of having fresh-looking crystal surfaces, looked rather shaggy, having a “picket fence” appearance. The volcanic glass fragments were also weathered and had absorbed water from the soil in which they lay until excavated. Some of the vesicles had puddles of water in them, indicating they were of considerable age. In previous research, Dr. Steen-McIntyre had performed dating procedures on ash layers at Yellowstone National Park (Steen-

[4] Identifying the source of the volcanic pumice and ash proved to be difficult. Additional samples were later collected by Steen-McIntyre and Fryxell, some of which were later compared to fragmentary samples taken from a volcano, La Malinche, near the site. None of the samples proved to be identical to the La Malinche samples. Two layer samples did look similar, but not identical as expected. The glass and crystals in the pumice lumps produced from the Tetela Brown Mud looked very different. See Steen-McIntyre, Fryxell, and Malde, 1981, pp. 1-17.

McIntyre 1980). The samples from Hueyatlaco bore a striking resemblance to those from Yellowstone dated at 251,000 years.

Some zircon crystals from two of the volcanic layers, the Hueyatlaco Ash and the Tetela Brown Mud, were given by Dr. Steen-McIntyre to another geochemist, C. W. Naeser, to process for dating. Naeser used the zircon fission-track dating method, which relies on radioactive properties of certain elements. The results from this process demonstrated the Tetela Brown Mud to be 600,000 ± 340,000 years BP, and the Hueyatlaco ash was determined to be 370,000 ± 200,000 years BP. The minimum age ranged from 170,000 years to 260,000 years BP (Steen-McIntyre, personal communication with Suzanne Clark). Szabo's results, using the uranium-series method, ranged in age from 180,000 to 260,000 years BP. Naeser's zircon fission-track method showed ages ranging from 170,000 years to 260,000 years BP. Both sets of dates agreed with Dr. Steen-McIntyre's observations of 251,000 years. Three separate methods, calculated by three separate geologists, yielded similar results, yet the results met with skepticism and hostility.

As members of the team began to complete their respective dating methods and the results were presented to her, Irwin-Williams became critical of the results and indicated her dissatisfaction in all of the publications regarding the Valsequillo project by various team members. Irwin-Williams was clearly distressed that date estimates place human presence at Valsequillo long before 30,000 BP, the earliest date she could accept. It is not improbable that Irwin-Williams feared her career was in jeopardy in light of such dates. She certainly feared (or at least was wary of) what might happen if she was associated with fringe elements. When, at a meeting of the Geological Society of America, Malde and Fryxell announced their early dates for the Valsequillo site — which dates were established by three independent dating methods — the announcement was reported on the UPI wire for November 14, 1973. Irwin-Williams reacted with anger. In a letter dated 3 November 1974 to Alan L. Bryan, a colleague in Alberta, she said

> My capsule comment on the situation (expletives deleted) is that this is one of the most irresponsible public announcements with which it has ever been my misfortune to become involved. Of the three dating methods used by Malde on the materials, two are so new that we have essentially no information on their validity. The third (fission-track dating) gave an anomalous result of about 300,000 ± 300,000 (in other words, no date at all).

This sounds eminently reasonable. If two of the dating methods are experimental, and one gives an essentially worthless result, then the dates are surely suspect. Compare the charge of irresponsibility with the text of their announcement, as reported in *Quaternary Research* (Steen-McIntyre, Fryxell, and Malde 1981):

> The evidence outlined here consistently indicates that the Hueyatlaco site is about 250,000 yr. old. We who have worked on geological aspects of the Valsequillo area

> are painfully aware that so great an age poses an archeological dilemma (Szabo et. al., 1969). . . . In our view, the results reported here widen the window of time in which serious investigation of the age of Man in the New World would be warranted. We continue to cast a critical eye on all the data, including our own.

This statement seems eminently cautious. Moreover, the UPI reports that Fryxell said, at the same meeting, “It’s not fashionable to come into a meeting and say ‘I don’t know,’ but that’s essentially where we are right now.” This declaration of ignorance hardly sounds rash and irresponsible. Moreover, Irwin-Williams seems to be getting the fission track date wrong. Steen-McIntyre, in a letter to J.L. Bada, cites the date given by that method as 370,000 $\pm$ 200,000; a wide range of error, but hardly meaningless. The experimental methods (Tephrahydration and Uranium series) have since been found to be reasonably reliable.

Dr. Steen-McIntyre was a graduate student at the time the Valsequillo project began. She was working on her Ph.D. at the University of Idaho and the Valsequillo project was to be her research project. It became clear to her after three years of hard work, that the subject of her dissertation would have to be changed due to the controversial nature of the Valsequillo findings regarding the age of the site as published by Szabo and Malde in 1969. Eventually, Steen-McIntyre was forced to choose a less controversial dissertation subject, how to examine volcanic ash samples. Steen-McIntyre finally obtained her degree in 1977. Between 1969 and 1973, frictions within the Valsequillo archeological team with regard to the date of the site were building. Malde was enthusiastically promoting an early date (ca. 200,000 years BP), while Irwin-Williams was promoting a more conservative, but still controversially early, date (ca. 20,000 years BP).[5] Steen-McIntyre’s allegiance was with Malde, but her subordinate position on the team and in the profession of archeology led her to be more cautious. Her caution, along with her thorough scholarship, made it possible for her to continue to find employment. In early 1973, Virginia Steen-McIntyre had achieved international recognition from several organizations, including the National Academy of Science, from whom she also received funding for foreign meetings and speaking engagements. She worked part-time in her area of expertise for a government laboratory, and even became an adjunct professor of Archaeology at Colorado State University.

Correspondence between Irwin-Williams and Steen-McIntyre during the late 1970’s shows that both were becoming increasingly frustrated with the impasse. The Valsequillo material, mostly hard artifacts, points to an early date, but the mass of other evidence, much of it inferential in nature, points to a much later date. After the dating process of the Valsequillo project reached

[5] Irwin-Williams’s championing of this date is particularly puzzling, since it is both too late for the physical evidence at Valsequillo (which points to an age an order of magnitude higher) and too early for the larger body of indirect evidence (which points to a date 10,000 years later).

completion and she had obtained her degree, Dr. Steen-McIntyre attempted to publish her article on the Valsequillo site. She encountered serious difficulties in that regard. Delays were often explained by excuses such as: “the article has been misplaced or lost;” she finally managed to get her article on Valsequillo published in November, 1981. Soon thereafter, Dr. Steen-McIntyre met with scorn and ridicule from her peers and was once even accused of ruining Cynthia Irwin-Williams’s career (Steen-McIntyre, personal communication with Suzanne Clark).

Barney Szabo encountered similar difficulties after the article he and Harold Malde published. Even though Szabo, fearing negative reactions from the findings, attempted to distance himself from the Valsequillo project, his attempts were insufficient to escape the disapproval of the scientific community. He encountered that censure head-on while he was seeking a research grant for another project. The reviewing scientist recommended the grant be denied on the basis of Szabo’s involvement with the Valsequillo project. Szabo had been labeled an incompetent scientist and lacked credibility (Steen-McIntyre, personal communication with Suzanne Clark).

The process of publication is clearly a highly charged political phenomenon. Editors of scientific journals are influential individuals in an authoritative capacity. It is a process very similar to the method a scientist encounters when seeking research grants. Both are contingent upon the credentials of the individual seeking publication or funding as well as the criteria to which the editor adheres. The processes are equally prone to subjectivity and bias.

Steen-McIntyre is not the only student of American archaeology to be treated badly on the basis of her views. E. James Dixon (1993, p. 128) reports similar responses to his writings when he merely suggested a mechanism for migration other than the Bering land bridge:

> In the early 1980s I had published a popular article on the peopling of the Americas in which I merely hinted that humans may have colonized the Americas via the Pacific. I was sharply and swiftly criticized by several of my colleagues. One senior associate suggested that I not pursue this further for fear of losing my credibility within the profession.

It was not just Dixon’s colleagues that found his views dangerous; editors of journals criticized his professional writings, not because they failed to meet the journal’s scholarly standards, but because they argued against the received view. Dixon had done a series of studies in which he and a colleague had grown hemoglobin crystals from material recovered from spear points. They matched the hemoglobin from those points with that found in living species, and also with specimens recovered from extinct species. The result was that some of those points could be dated to well before the Clovis and Folsom barrier, as the animals whose blood was on them were extinct before 12,000 BP. So either humans were in America before the late Pleistocene, or these animals survived longer than is currently supposed. Dixon sent these results to *Science*, with the following result (Dixon 1993, pp. 111-112):

> After Loy [Dixon's partner in this research] left, I went through the laborious task of editing our article to meet the requirements of the journal, and soon it was in the mail. About two weeks later I received word that our manuscript had passed the first level of screening by the board of reviewing editors and that it had been sent on to specialists within the field for technical review. After two months had passed, we had received no further word from the journal, so I decided to call the editorial office. The following week I received a letter from the editor stating that although the reviewers had unanimously recommended publication, they would not publish the article.

In other words, there was no complaint about the article on either stylistic or technical grounds, but only about the conclusions for which he argued.

IV. CONCLUSIONS

This kind of reaction to anomalous evidence is, as Meltzer says, understandable, but it also sounds quite contrary to the spirit of science. And yet it is a common response to anomalies. Not only in archaeology, but in every other science as well, challenges to the received view are treated with exaggerated suspicion. It is entirely reasonable to treat anomalies with suspicion. After all, if a piece of evidence comes to light that is inconsistent with a well-grounded theory, it is not always clear which of the two has to yield. Frequently, apparent anomalies evaporate on further examination. There is some incentive for scientists to try to overturn received theories, and so they may overstate what their evidence shows. If a received theory is backed by lots of evidence, it would be irrational to abandon it at the first anomalous finding, even if there is no alternate explanation available for the anomaly.

But what has happened to dissenters in the archaeology of the Americas — especially those who dealt with the Hueyatlaco evidence — goes beyond mere suspicion. Their data are treated with contempt, their results (even when they are modestly stated) are treated as crackpottery, and they are sometimes accused of incompetence or dishonesty. Why these extreme reactions? In every science, anomalies are met with this same hostility. It seems to be standard practice in science, and yet it sounds paradigmatically unscientific. The reason this is hard to square with our notions of science is that we are failing to see science as the socially embedded practice that it is.

Science seems to be an abstract method of theory choice, which is immune to abuse. At the same time, science seems to be a social practice, subject to all the abuses any human institution is subject to. These claims cannot both be true, and yet both seem plausible. It does seem that the scientific method (insofar as there is a single method) is designed precisely to root out error and tend toward truer and truer pictures of the world. On the other hand, scientists are people, and scientific investigation is done by people in societies, and it would be amazing if they didn't bring their biases into the laboratory with them. We have three choices: we can endorse the first view and reject the second, we can endorse the second view and reject the first, or we can find a way to reconcile the two views. In fact, the two view are indeed compatible. When the proponents of the self-correcting nature of science say “Science is unbiased” and the proponents of science as an ideologically driven enterprise say “Science is biased,” they are not disagreeing, because they are talking at cross-purposes; they mean different things by the word ‘science’. The former are talking about a method employed in theory choice, abstractly conceived; the latter are talking about a socially instantiated practice that has theory choice as a component. Consequently, it is possible for the abstractly characterized method of theory selection to be self-correcting, and yet be embedded in a larger practice which to some extent undermines, or even defeats, self-correction.

This distinction between science as theory-choice procedure and science as social practice is easily confused with another, related distinction, the distinction between good and bad science. For example, many scientists would admit that particular scientists may have let bias creep into their work, but that when they were doing so, they were doing bad science. In other words, it is ideal science, or good science, that corrects itself. But both sides to the debate can agree that there is good and bad science. The believers in science may admit that some scientists are biased, but they want to assert that it is not merely in the ideal that science corrects itself, but also in real practice. They want to claim that science as we actually do it has a tendency toward truth, which would be unwarranted if it were only science as ideally practiced that has that feature. Also, many of the political critics of science want to claim that even when science approaches the ideal of objectivity, it still serves political power. So the distinction between real and ideal science does not illuminate the problem.

The defenders of the objectivity and self-correcting nature of science think of science as a method, structurally designed to weed out error. In particular, it is meant to weed out error due to the personal perspectives of scientists. The scientific method, as described in innumerable science textbooks, is something like this: a hypothesis is conceived, it doesn’t matter how; logical consequences of that hypothesis are deduced; experiments are designed to see if those consequences are true; if not, the hypothesis is proven wrong, and the process returns to the beginning, with a revised or completely new hypothesis. If the consequences are correctly deduced, and the experiments are well-designed and well-performed, then the original hypothesis is refuted, even if it was the pet hypothesis of a well-beloved and authoritative scientist. Richard Feynman (1990, p. 156) describes the method this way:

> In general we look for a new law [of physics] by the following process. First we guess it. Then we compute the consequences of that guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your

> guess is. It does not make any difference how smart you are, who made the guess, or what his name is — if it disagrees with experiment it is wrong. That is all there is to it. It is true that one has to check a little to make sure that it is wrong, because whoever did the experiment may have reported incorrectly, or there may have been some feature in the experiment that was not noticed, some dirt or something; or the man who computed the consequences, even though it may have been the one who made the guesses, could have made some mistake in the analysis.

Feynman goes on to say that this picture is a bit oversimplified, but his further remarks only serve to add details to the three-part structure: hypothesis, deduction, experiment. The results of the experiment then have an effect on what hypotheses get proposed, so the process is a self-correcting spiral, homing in on accurate representation of the world. It is easy to see how this understanding of science would lead one to think that it could not possibly be biased. If a biased scientist presents a faulty hypothesis, it will not be borne out by experiment; and so bias is rooted out, at least in the long run, by the structure of science itself.

The critics of science as we now practice it do not see science as this idealized and highly abstract method of theory choice. The classical “scientific method” is a component of science, but it is not the whole thing. They are thinking of science as a social practice that starts well before hypothesis with background information, distribution of resources and opportunities, and ends with publication and discussion of theories. What theories are accepted, published, and discussed forms the new background information out of which new hypotheses arise, so on this picture, too, science spirals, but the spiral is guided by more than just observation and experiment. It is because of these additional forces on scientific inquiry that science (in the “practice” sense) can be biased, even if science (in the “method” sense) is immune to bias.

Science as a social practice can be broken down into three stages: hypothesis selection, theory choice, and theory uptake. Theory choice has been the focus of much discussion of science, and so has become science itself for so many people, because it is amenable to abstract treatment. In particular, it is amenable to a normative understanding; understanding science as theory selection allows us to develop logics of science, and interpret particular cases of theory selection in terms of how well they achieve the goals of science, including an accurate picture of the world. But obviously there is more to how science gets done, and more to what scientific theories we accept, than the logic of theory choice alone. The scientific practice, as actually undertaken by real, working scientists, is better represented as a three-stage structure, with theory choice taking place in a context of hypothesis selection and public uptake.

At the stage of hypothesis selection, science gets its direction. To begin with, what science gets done is partly a function of what previous scientists have already done and what presently employed scientists would like to see done. Scientists are partly hired, promoted, and otherwise evaluated on the strength of how interesting the problems are that they are pursuing, so what we find out about the world is in part a function of what presently employed scientists find interesting. Proponents of theories that postulate a pre-Clovis human presence in the Americas will (as Steen-McIntyre's case shows) have trouble finding employment. Hypotheses that no one respects will have trouble finding funding and support; hypotheses that are very radical will be difficult even to formulate, for lack of a history. So, what theories we accept is constrained by what hypotheses get tested. At the theory-uptake stage there are similar constraints. If no scientific society or journal finds your work important or interesting, it won't get published, and so other scientists will not try to replicate the results, and the general public will never find out about it. A lot of evidence against the standard view gets weeded out at this stage (as Dixon's case shows). Evolutionary biology had to wait decades for Gregor Mendel's groundbreaking work because it languished in a second-rate journal that nobody was reading. Even if a paper on a problem considered marginal by the majority makes it to publication, if the scientific community doesn't pick up on it, discuss it, and expand on it, it vanishes into obscurity. So while we confine ourselves to consideration of the scientific method, it is true that any hypothesis, no matter what it is or who brings it up, is treated equally, when we turn to the social practice of science, we see that only hypotheses that can attract enough interest to get resources, publication, and discussion really have a chance to be accepted.

These two ways of looking at science give us another way to draw Kuhn's distinction between normal science and scientific revolution, without his flirtations with anti-realism. When there is an accepted theory in place (a "paradigm", if you like), there are well-structured alternative research projects, developing different aspects of the received view. Scientists who undertake different research projects see each other as all doing respectable work, even if they are mutually inconsistent. Scientists who undertake projects outside the well-structured set of alternatives (like flat-earthers or creation scientists) are dismissed as crackpots. Scientific work that is within the pale of respectable work is then evaluated solely on the grounds of how well it meets the canons of science in the "method" sense. Anything respectable as determined by the received view will be accepted as worth doing, and will have a chance at publication and funding. The middle stage of theory choice looms large, and the forces that operate on problem selection and theory uptake have little work to do. In a time when evidence is turning up that calls a received view into question, the line between crackpottery and respectable science is temporarily blurred. As a result, the first and third stages of the scientific enterprise take on a larger role. If it is no longer clear (except in extreme cases) who the crackpots are and who the good scientists are, the question of who gets hired, who gets funded, and who gets published will have a correspondingly larger effect on the resulting science. Also, without clear criteria for distinguishing between good science and bad, the criteria actually applied will be more prone to subjective bias. Unfounded charges of incompetence or fraud will be much more common, and more injustices will be done.[6]

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