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Reports

Franz Boas's Physical Anthropology: The Critique of Racial Formalism Revisited[1]

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In 1968, the historian of anthropology George Stocking published an article on the physical anthropology of Franz Boas (1858–1942) called "The Critique of Racial Formalism." Stocking's title was inspired by a letter Boas wrote to the *American Anthropologist* in 1936 recalling his initial reactions to physical anthropology: "When I turned to the consideration of racial problems I was shocked by the formalism of the work. Nobody had tried to answer the questions why certain measurements were taken, why they were considered significant, whether they were subject to outer influences; and my interest has since remained centered on these problems which must be solved before the data of physical anthropology can be used for the elucidation of historical problems" (p. 140). Boas's physical anthropology has been characterized as "experimentally-minded" (Montagu 1944:115), with a concept of population structure "denied to his race-ridden, cephalic-index-loving contemporaries" (Tanner 1959:106). He has been credited with ushering in the "dynamic science of human biology" (Lesser 1968:107) and with being one of the first to challenge the "virtually unquestioned assumption of stability of hereditary characteristics under any and all environmental conditions" (Goldstein 1981:492). Boas's physical anthropology can be divided into three major parts: (1) growth and development (reviewed by Tanner 1959), (2) the critique of racial psychology (reviewed by Cravens [1978] and exemplified by Boas's *The Mind of Primitive Man* [1911, revised in 1938]), and (3) head form and heredity. This paper is primarily concerned with the last of these and its relation to his "critique of racial formalism."

"Formalism" is a term not often used in biology. According to Boas and Stocking, the formalism of turn-of-the-century physical anthropology was evidenced by an exaggerated concern with racial types, morphology, and classification. Boas's critique of racial formalism was, however, an essentially dualistic endeavor, encompassing both psychology and biometry. He and his students were ultimately tremendously successful in their battle against real scientific racism (Gossett 1963); the success of their program rested more on the study of culture than of morphology. Nonetheless, the reputation of Boas's work on the biology (morphology) of race was enhanced by its association with his work on race psychology. A realistic appraisal of Boas's physical anthropology requires the perhaps artificial but necessary separation of these two aspects of his work on race. In addition, his work on heredity and head form must be considered in both its anthropological (Stocking 1968) and its genetic (Tanner 1959) contexts.

Boas's views on physical anthropology developed gradually over some 20 years, beginning with his first publication on the subject in 1890 and culminating with his best-known one, "Changes in the Bodily Form of Descendants of Immigrants" (1912). The most fundamental early influence on his work must have come from Rudolf Virchow, under whom he worked at the Berlin Ethnological Museum from 1883 to 1886. Virchow, the father of modern cellular pathology, was interested in all aspects of anthropology (Boas 1902, Ackerknecht 1953). As a German of Slavic descent, he took a personal interest in the definition of population subtypes within national populations. His approach to this problem, with its heavy reliance on measurement of vast numbers of subjects, and his interpretations of his results, which allowed for a relatively large degree of racial plasticity, are reflected in Boas's work.

The period 1890–1918 was an exciting and unsettled one in the history of biology. With the rediscovery of Mendel as its centerpiece (in 1900), it saw biologists grappling with the fundamental problems of evolutionary process and the nature of heredity (Nordenskiöld 1928, Provine 1971, Mayr and Provine 1980). There were two primary and competing schools of thought in the evolutionary genetics of this time: the "Biometricians," led by Pearson and Weldon, who believed that evolution was a continuous process working on small variations, and the "Mendelians," represented by Bateson, Punnett, and de Vries, who thought that evolution proceeded by saltational jerks. The dichotomy antedated the rediscovery of Mendel but was polarized along Mendelian lines when the saltational school seized upon particulate, Mendelian heredity as the ideal mechanism for its process of evolution (Provine 1971). The modern synthesis would commence around 1918 with the work of Ronald

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Fisher, Sewall Wright, and J. B. S. Haldane. The "pre-synthesis" period is distinguished by a plurality of theories relating to the mechanisms of evolution and heredity. Boas was aware of the events and controversies of this time and familiar with the biometrical methods of Pearson and with Mendelian theory. As his physical anthopology matured during this heady period, he was profoundly influenced by a Danish botanist, Wilhelm Johannsen, whose work enjoyed an intense but short-lived popularity before the advent of the modern synthesis.

Wilhelm Johannsen (1857–1927), who was born in Copenhagen and in 1905 became professor of botany at the university there, was one of the principal architects of the "argument against selection" (Johannsen 1911, Provine 1971, Churchill 1974). His theoretical orientation was based on his experimental breeding work on Princess beans (*Phaseolus vulgaris*). In 1903, Johannsen published his "pure-line" or "genotype" theory. He viewed his self-fertilizing Princess bean populations as being composed of "pure lines," defined as "the descendants from one single homozygotic organism, exclusively propagating by self-fertilization" (1911:135). Having failed to demonstrate selection in his pure lines, he concluded that "the personal character of the mother-bean has no influence, that of the grandmother, etc., also none; but the type of the line determines the average character of the offspring" (1903, quoted in Provine 1971:94). Johannsen saw that great variability could arise from a single pure line, and this led to his conceptualization of the "genotype" and "phenotype" of an organism (1911:132–34; Churchill 1974). Mayr (1980) has pointed out that many workers during this period suffered from "genotype-phenotype confusion" and that Johannsen, despite having invented the terms, was no exception.

For many workers, Johannsen's pure-line theory, coupled with de Vries's *Mutationstheorie,* meant that Darwinian evolution by the natural selection of continuous variants need no longer be considered seriously. But there were dissenters. Harris (1911) published a rather devastating critique of the pure-line theory, claiming (1) that the pure-line advocates had not demonstrated heritability for the traits in question; (2) that any single character was limited in its selection potential and that the inability to extend a population beyond some phenotypic limit was not evidence that a pure line had been isolated from an originally heterogeneous population; and (3) that organisms are complex wholes, such that selection for any single trait is limited by and has implications for many other traits. With regard to the argument that the pure-line results proved that selection was ineffectual, Harris held that the logic of the pure-line camp was both circular and "slippery" (p. 362). He even criticized Johannsen's introduction of the word "gene," as a way of referring to the genetic "something," as being the 13th thus proposed and completely unnecessary. Johannsen's supporters included Raymond Pearl, who worked on domestic fowl, Herbert Jennings, who worked on *Paramecium,* and Franz Boas. As Provine (1971:108) points out, the majority of biologists in 1910 probably accepted the pure-line theory, but within ten years they had almost all rejected it in favor of experimental evidence that supported the Darwinian view of evolution. While Johannsen's terms "genotype," "phenotype," and "gene" survived, his genotype (pure-line) theory did not.

Boas's physical anthropology was of course shaped not only by positive forces; he saw it as a reaction to the "formalistic" anthropology of his day. This "race-ridden, cephalic-index-loving" discipline has received much treatment elsewhere (e.g., Gould 1981), but if its products are perused for hints of modernity with the enthusiasm that Boas's have sometimes been examined, some light emerges from the darkness. For example, in his *Races of Man* (1915 [1900]), Joseph Deniker argues for a multitrait racial classification system and makes explicit the distinction between race and ethnic group. Paul Topinard's *Anthropology* (1890) is almost wholly devoted to human biology and physical anthropology (with a substantial portion on the cranium), exemplifying the "multifield" approach advocated by Topinard's teacher Broca and persisting in most departments of anthropology today. Even William Ripley's *Races of Europe* (1910 [1899]), the quintessential race/cephalic-index book, contains a plea for synthesis akin to the holistic movements found in many modern scientific disciplines; it should be appreciated that Ripley's melange of race and sociology did not proceed directly from a "race determines all" perspective.

Boas's first paper in physical anthropology was called "A Modification of Broca's Stereograph" (Boas 1890). He published a number of descriptive craniometic papers during the 1890s, concerned mostly with North American Indians, the most important of which was "The Half-Blood Indian" (Boas 1894). This was one of the few "numbers" papers that Boas chose to include in his volume of collected works *Race, Language, and Culture* (1982 [1940]). In it he showed that head and face form in Indian-white hybrids tended towards one or the other parental type and not towards some intermediate value. As many have pointed out, these were results—which Boas described as "laws of heredity"—that must have prepared him for the rediscovery of Mendel.

In 1899, Boas published a pair of papers that on first glance appear somewhat contradictory. The first, "Some Recent Criticisms of Physical Anthropology," has Boas (1899*a*) defending physical anthropology against critics who would deny (1) the possibility of racial classification and (2) the practicability of description by measurement. Boas answered the first criticism by noting the importance of skeletal material (it is transportable), by recognizing that the problem of classification is a statistical one based not on single individuals but on populations of individuals, and by accepting a strongly hereditarian stance with regard to racial features while simultaneously introducing the heredity/environment dichotomy. In response to the second criticism, Boas said that measurement was important where words could not describe local varieties, but he also pointed out that single

metrics are often insufficient for accurately distinguishing populations. As an example, he noted that Eskimos, Southern California Indians, and Negroes have similar cephalic indexes. In his conclusion, Boas hinted at some of the things he would discuss in greater detail in *The Mind of Primitive Man:* "It will be seen that that part of human history which manifests itself in the phenomena that are the subject of physical anthropology is by no means identical with that part of history which manifests itself in the phenomena of ethnology and of language" (1899*a*:106).

In "The Cephalic Index," Boas (1899*b*) went from defending physical anthropology to attacking one of its most cherished tools. Although he had earlier claimed that the cephalic index was not alone sufficient for racial classification, he was now ready to conclude that "while the cephalic index is a convenient practical expression of the form of the head, it does not express any important anatomic relation" (p. 461). This was a result he found "rather surprising." He based his conclusion on correlational studies of the length and breadth of the head. Using his own data and data gleaned from the literature, he found that in many populations the correlation between the length and breadth was quite low and that there was considerable variation in the value of correlation among different races. He did not see this as a devastating criticism of craniometry, however; in fact, he suggested that the circumference should instead be included in all series of anthropological measurements, as it represented a relation of "fundamental importance."

Like many other biologists in the immediate post-Mendelian period, Boas looked for "laws" of heredity in his organism of interest. In "Heredity in Head Form," Boas (1903) reminded readers of his 1894 paper on American/European hybrids and pointed out that the result there conformed to Mendel's law. In the 1903 paper he discovered another law: "One-half of the children of a couple belonging to a certain race have a type the average of which is equal to the average of twice the father's type and once the racial type, while the other half have an average equal to twice the mother's type and twice the racial type" (p. 538). Boas believed that similar pseudo-Mendelian laws would be found for other traits.

The story of Boas's involvement in the United States Immigration Commission project that led to his "Changes in the Bodily Form of Descendants of Immigrants" (1912) and the public reception of his results is well told in Stocking (1968; see also Tanner 1959:99–103). Boas and a number of assistants measured head form and stature of parents and offspring in four American immigrant groups: Central Europeans (Bohemians, Poles, Hungarians, Slavs), Hebrews, Sicilians, and Neapolitans. The massive amounts of data collected were ultimately published as *Materials for the Study of Inheritance in Man* (Boas 1928). Boas (1912:530–32) reported ten major findings, three of which are important here: (1) American-born descendants of immigrants differ in type from their foreign-born parents; the changes that occur among various European groups are not all in the same direction. (2) The influence of the American environment makes itself felt with increasing intensity according to the time elapsed between arrival of the mother and the birth of the child. (3) The observations on intraracial heredity show an increased variability of children of dissimilar parents, which proves a regression of the children to either parental type, not a regression to the mid-parental type. Boas said that these findings could not be explained by differences in ancestry, infant-cradling practices, or uncertain paternity. He speculated that "the breaking of the more or less inbred lines of small European villages after arrival of the people in America and the consequent changes in the line of descent of urban populations may be a cause producing changes in type" (p. 555). "Plasticity" was the only thing he felt confident in claiming as demonstrated by the immigrant study (p. 557); at this point, he was an agnostic with reference to the causes of the plasticity. In his conclusion, he argued that his results did not mean the end of anthropometry and craniometry; on the contrary, they demonstrated the "great value" of studies of this type.

The results of the immigrant study made Boas quite receptive to the genotype/phenotype/pure-line theories of Wilhelm Johannsen. Boas cited Johannsen in both editions of *The Mind of Primitive Man.* In the 1911 version, which he prepared before he was fully aware of the results of the immigrant study, he used Johannsen's work on pure lines to support his own findings that local, "primitive" races were often identifiable as very distinct types (pp. 89–90) and argued that they represented the "characteristic development of a stable type." In 1938, after more than 20 years of assimilating the results of the immigrant study, he found Johannsen's work useful in a very different way. Now he emphasized the plasticity of Johannsen's beans—that even though they were self-fertilizing, much variability could be found in the descendants of any single bean (1938[1911]:39): "There are so many uncontrollable conditions that influence the development of the organism that even with identical ancestry the same form and size cannot be expected. . . . We are dealing with the fundamental difference between a constant and a variable phenomenon which must be clearly held in mind if we want to understand the meaning of the term 'race.' " In 1911, Boas used Johannsen's work to support the idea of racial stability; in 1938, he used it to justify the notion of racial plasticity. It is a change in perspective that would not have occurred had he not done the immigrant study, and it clearly reflects the change in his thinking with regard to race that the study brought about—this despite the fact that he "fully realized the smallness of the changes observed" (Tanner 1959:102) in the immigrant study.

Two papers written in 1916 clearly show Johannsen's influence on Boas. In "New Evidence in Regard to the Instability of Human Types" (1916*a*), Boas attempted to distinguish between "genetic types" (those determined by heredity alone) and "ecotypes." It is a good example of the genotype-phenotype confusion that was prevalent during this period. Today we have a hierarchical view of genotype and phenotype; Boas's was a mutually exclu-

sive one (1916*a*:714). He did offer the term "physiological types" to account for those in whose expression both heredity and environment play a role. This article, which was originally an address to a professional but nonanthropological audience, is by no means naive, but in dealing only with morphological features Boas lacked the hierarchical perspective that genotype and phenotype require. In "On the Variety of Lines of Descent Represented in a Population" (1916*b*), Boas made explicit use of Johannsen's pure lines, except that he called them "lines of identical descent." Herskovits (1953:44) compared this paper to Mendel's in the sense that it was ahead of its time, although he acknowledged that it was not exactly inspiring the then-current workers in the field. Boas attempted to discover the heterogeneity of the lines of descent in a population by calculating the variability of the average of the fraternities (families) within the population. He stated: "It is easy to see that the correlations between parents and children will be lower, the more uniform the lines composing the population. This point has been made by Johannsen" (p. 9). He attempted to refine this method in a series of papers over the next 20 years, but, lacking the technique of analysis of variance, he made little progress (Tanner 1959:104–6).

As Tanner points out, Boas's intellectual growth with regard to the problems of heredity ended with his embrace and subsequent reworking of Johannsen's pure-line theory. He offers two explanations for this stagnation (Tanner 1959:106). First, he says that Boas lacked sufficient training in mathematics and in Mendelian genetics. The former is only partly true; Boas certainly had more mathematical training than the vast majority of biologists of his time, and, as Provine (1986) points out, *accepting* the new population genetics was certainly possible without really *understanding* it. With regard to Mendelian genetics, Tanner is probably correct. Boas apparently never internalized the notion of the gene; his fundamental unit of inheritance was the trait. This made him receptive of Johannsen's pure lines but put him at odds with the theoretical population genetics of the 1920s and '30s that would revolutionize biology. Tanner's second argument is that we should just accept the fact that Boas was of Pearson's generation and not of the generation of Fisher and Wright. Interestingly enough, Kluckhohn and Prufer (1959) make the case that Boas's generation (which included Freud, Durkheim, Weber, and Bergson) was extremely influential in 20th-century social science. Churchill (1974) points out that Johannsen's generation was an in-between one in the history of genetics and that Johannsen himself has suffered from historical neglect. Boas and Johannsen were born one year apart.

Boas picked the wrong organism and the wrong traits for elucidating the laws of heredity. The most spectacular advances in genetics would be made by those studying fruit flies and guinea pigs, not Neapolitans and Sicilians. In addition, although Boas did not "love" race and the cephalic index, they certainly shaped his empirical interests in physical anthropology. With regard to the idea of plasticity, Boas could not tie the loose ends of this problem together without the analysis-of-variance technique, which would have provided the mathematical justification he sought, or without a hierarchical conception of gene and morphology. He was perhaps ahead of his fellow workers in recognizing this problem, but he did not resolve it. In choosing pure lines as his guide in analyzing populations, he led himself into an intellectual dead end. Pure lines are impossible to isolate in real, sexually reproducing species (and in fact do not exist in them). Although Boas recognized that complex populations were made up of impenetrably complex webs of pure lines, he persisted with them in the same way that Deniker persisted in naming more and more races even though he realized that as refinement techniques improved one would be compelled to name even more. Finally, pure-line thinking is a kind of typological thinking. Many claim that Boas had an almost modern conception of population. He didn't; what he had was a typological concept of population intermediate between the simple racial typology of the 19th century and the gene-based/breeding-isolate concept of the neo-Darwinian synthesis. To say that it is intermediate is not to denigrate his achievement but to put it in its proper historical perspective. All the points discussed above are offered in the hope of "explaining" Boas's work in heredity. Another relevant factor, of course, is that he had other things on his mind, such as cultural anthropology, ethnological linguistics, and folklore.

Boas has been called a highly "experimental" scientist with regard to questions of race (Montagu 1944). This implies a deductive approach, considered by some to be the hallmark of modern science. Boas was in fact a strongly inductive scientist. He was "surprised" by the results of his study of the cephalic index and of the changes in the bodily form of the descendants of immigrants. His work did result in *de facto* falsifications of hypotheses long accepted as facts, and there can be no doubt that some of his work certainly conveys a deductive impression. But Boas was far more likely to present *findings* or *results* than *evidence* in his empirical papers. It should also be remembered that although he "falsified" certain ideas, such as the utility of the cephalic index in classification and the stability of some morphological traits in human populations, he himself proposed or adhered to others that have proven equally untenable (e.g., circumferences for cephalic indexes and the pure-line theory).

Boas was one of the last great craniometrists whose work can be placed in the context of a strongly racial tradition. He did not kill craniometry or anthropometry, but he certainly inadvertently pushed it away from the racialist position. Later workers, such as Shapiro (1939), who acknowledged Boas's influence, admitted that they were studying morphological plasticity and not the dynamics of race. Kroeber, in the 1948 version of his text, brought up the immigrant study in the context of plasticity and stated that it did not really represent a challenge to the hereditarian concept of race (pp. 167–68). Since he had not mentioned the immigrant study at all

in his 1923 text, this indicates that Boas's work found a home in physical anthropology, after a number of years, only in the essentially nonracial context of plasticity. Hirsch (1927) provides one of the more interesting confirmations and interpretations of the immigrant study. With Boas, he found that there were substantial differences in head form between parents and offspring in his populations of Boston-area immigrants; he speculated that these differences were brought about by hormonal surges stimulated by the American environment, which was different in different ways from the original environments of different immigrant groups. In reality, craniometry as an important race science was destined to die out with or without Boas's help. It was, after all, *static* race science and therefore could not compete with the *dynamic* race science of eugenics that came to prominence during the 1910s and '20s.

In 1937, Fisher and Gray applied modern statistical methods to the raw data from the immigrant study that Boas had published in 1928 and in general found that the data were not significantly abnormal. Differences in the regression in stature between mother and offspring and father and offspring, though large, were not statistically significant, and this led them to resurrect the argument of uncertain paternity (Fisher and Gray 1937:92–93). More damaging was their finding that "the variability found within fraternities in these [head] measurements, unlike stature, is much less than that found between fraternities having the same parental measurements. No biological explanation of so great a difference presents itself, and the possibility must be considered that systematic discrepancies, either between different observers or between the measurements taken by the same observer when visiting different families, have contributed largely to the total variance observed." To avoid this problem, Shapiro (1939) employed only one observer, Frederick Hulse, in his extensive anthropometric study of Japanese and Japanese immigrants.

Boas's critique of racial formalism was successful, but it was more successful and generated a more viable intellectual tradition in the cultural than in the biological realm. Perhaps this was partly Boas's own doing: during the period 1900–1925, the Columbia anthropology department failed to produce a single Ph.D. in physical anthropology (Spencer 1981). There can be no doubt that Boas's physical anthropology (in the strict sense) was in some ways "modern," but to plunder his work for kernels of modernity is to strip it of its real character. His reluctance to count races and his attention to growth and development, which gave him an entirely different (from race) perspective of human variability, certainly separated him from his anthropological contemporaries. In other ways, he was of his time. Stocking (1968:194) ends his discussion of Boas's physical anthropology by stating:

> Insofar as late nineteenth-century scientific physical anthropology was heir to polygenism and parent to the obscurantism of the type concept, the authority of "science" was all on one side. Offered by the most authoritative spokesman of physical anthropology in the United States and cutting through that obscurantism, Boas' critique of racial formalism began to shift the balance in the opposite direction. Protagonists of environment and of racial equality could now quote science on their side.

In a sense, this situation is reminiscent of the situation in physical anthropology in the mid- and late 19th century, when physical anthropologists, working for the acceptance of their worthy but struggling field, offered the cephalic index as a sign of the empirical nature of their science.

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The Evolution of Chiefdoms[1]

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The principal goal of the seminar on chiefdoms held at the School of American Research January 18–22, 1988, was to understand the dynamics of chiefdoms. A chiefdom was rather loosely defined as a centralized polity that organizes a regional population in the thousands (Carneiro 1981, Earle 1987). Some degree of heritable social ranking and economic stratification was considered characteristic. The focus of discussion was on the origin of these polities, their development, and their eventual collapse, stasis, or transformation into states.

 This paper summarizes the discussions and conclusions of an advanced seminar whose participants were as follows: Richard Bradley (Archaeology, Reading), Robert Drennan (Anthropology, Pittsburgh), Timothy Earle (Anthropology, UCLA), Gary Feinman (Anthropology, Wisconsin–Madison), Yale Ferguson (Political Science, Rutgers), Antonio Gilman (Anthropology, California State–Northridge), Jonathan Haas (*ex officio,* School of American Research), Patrick Kirch (Burke Museum, Washington), Kristian Kristiansen (Center for Research in the Humanities, Copenhagen), Candelario Saenz (Anthropology, Texas–Austin), and Vincas Steponaitis (Anthropology, North Carolina–Chapel Hill).

The seminar participants accepted two important positions to guide their consideration of the evolution of chiefdoms: that research must focus on sequences of long-term change documented archaeologically and historically (Kirch 1984, Kristiansen 1982) and that chiefdoms vary in complexity/scale of development (simple vs. complex [Steponaitis 1978]), mode of financing (staple vs. wealth [D'Altroy and Earle 1985]), structure (group-oriented vs. individualizing [Renfrew 1974]), and specific history. With this accord, participants concentrated on understanding the dynamics of chiefdoms as political institutions. This required outlining the various strategies by which rulers tried to extend and maintain political control and the conditions that affected the success of these strategies. The unstable and cyclical character of most chiefdoms was apparent in the cases discussed.

Discussions of power relationships frequently returned to followers' evaluation of the cost of compliance with a leader's demands relative to the cost of refusal (Haas 1982). Constructing a complex polity requires a leader to bind a following to himself. Simply, he must control people's labor (Feinman and Nicholas 1987). What keeps them from "voting with their feet"—moving away from the centers of power and extraction? Larger groups do not form naturally; technological and social adjustments are necessary to concentrate and coordinate increasing numbers of people (Johnson 1982). The traditional answer to this question has been to point to the management functions that leaders perform. Much of neo-evolutionary thought since the 1950s (see Steward 1955, Service 1962) has emphasized the function of leaders in maintaining their groups. To understand the evolution of chiefdoms is thus simply to identify the new conditions created by technology or population growth that require central management for their effective and efficient operation.

Population growth has received considerable attention since Boserup's (1965) work and serves as a motor in the most recent general synthesis of cultural evolution (Johnson and Earle 1987). In the seminar discussions, however, it received little support as a prime mover. Drennan, Feinman, and Steponaitis emphasized the very low population densities that have been documented by intensive surveys for the chiefdoms in the Oaxaca Valley of highland Mesoamerica, for the Black Warrior Valley of Alabama, and for the Valle de la Plata in Colombia. Population density appears also to have been low for the early chiefdoms of southern England (Bradley). Population increase was certainly associated, however, with the evolution of political systems in the Marquesas, Greece, and medieval Italy. On the Marquesas, population growth and resulting environmental deterioration created a susceptibility to drought that bound a local population to its leader and his breadfruit stores (Kirch). In Greece, population growth accompanied Mycenean state formation and, following the precipitous "Dark Age" decline, contributed to the emergence of the polis (Ferguson).

Generally seminar participants were willing to accept