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Scientific Change as Political Action: Franz Boas and the Anthropology of Race

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1. Introduction: Weber and Boas

Franz Boas was a social activist who campaigned against racism and nationalism throughout his life. While his writing on anthropological theory and method barely hints at this political interest, he was part of a paradigm shift in anthropology that had far-reaching effects on the way we think of race and human diversity. In 1904, when Boas was in the midst of work that would become *The Mind of Primitive Man*, Max Weber wrote a seminal essay on value-freedom in the social sciences. Weber argued that “it can never be the task of an empirical science to provide binding norms and ideals from which directives for immediate practical activity can be derived” (Weber 1949: 52). This kind of value-neutrality was necessary for scientific objectivity, Weber thought, because empirical evidence could provide no support for political or moral evaluations. And yet, *The Mind of Primitive Man* appears to do just that. It is an extended argument against the nineteenth century conception of higher and lower races. Since this conception has a clear relation to slavery, colonialism, and segregation, its rejection entails “directives for immediate practical activity.” The revolution in anthropology of which Boas was a part thus fails Weber’s test of value-freedom; at the same time it seems like a salutary episode of scientific change. What might this episode in the history of science tell us about scientific value-freedom and objectivity?

Before this question can be addressed, the ground needs to be cleared in two fields. Scientific value-freedom has been well worked, and Section 2 will articulate the concepts necessary for our later analysis. The second area, the tangle of Boas' writings, presents a greater challenge. While there is a substantial amount of excellent Boas scholarship, none of it bears directly on the question above. Treatments of Boas' political activism have been largely, if not entirely, biographical (Cole 1999; Hyatt 1990; Stocking 1968, 1992). The interesting philosophical questions concern the relation of these commitments to the substance of his work, and that requires a close analysis of the logic of his arguments and the content of his claims. Boas' writing, however, is notoriously diverse, and where there has been close reading of his texts, Boas' contributions have been treated in a fragmentary manner. Sections 3, 4 and 5 will attempt to remedy these lacunae—or rather, make a start on such a project—by presenting a close reading of key Boas essays between 1890 and 1911.¹ I will try to show that Boas' essays on methodology and on race are parts of a unified assault on key presuppositions of nineteenth century anthropology. His arguments were not, and could not have been, value-free. The final section will turn, at last, to the question of what the revolution in anthropology might teach us about value-freedom and scientific objectivity.

2. Impartiality, Neutrality, and Objectivity

Weber had a sophisticated understanding of value-freedom. In saying that scientific theories should entail no evaluative or prescriptive statements, he expresses a form of *value-*

¹ *The Mind of Primitive Man*, incorporated large parts of these essays. The original essays, however, are more integrated into the context of Boas' methodological critique of nineteenth century anthropology. Looking closely at the essays thus helps show how Boas' work on race and his methodological ideas were unified.

neutrality. Another form that value-freedom might take, however, is *value-impartiality*².

Scientific inquiry would be completely impartial when values play no role in the generation or confirmation of a scientific theory. While Weber insisted on value-neutrality, he argued for a limited form of partiality³ in social science. Social scientists attend to particular problems because of their interests and concerns. More substantively, social problems help delimit the ideal type concepts that are used to describe and explain social phenomena. Social or political concerns thus help generate the concepts of social theory. Once the explanatory work began, however, the values were to disappear. Weber thus preserved objectivity by limiting the role of values to what was later called the context of discovery. When it came to justifying social scientific claims, Weber took social science to be impartial.

The debate has been refined since Weber made his contribution. A fixture of the late twentieth century debate has been the distinction between contextual and constitutive values. As I have argued elsewhere (Risjord 2000), this terminology hides a pair of distinctions.

Constitutive values are intrinsic, essential, or required by a particular form of scientific inquiry, while contextual values are part of the background commitments of the scientists or larger society. This is a difference in the roles that values may play in science, not among kinds of values. Among kinds of values that might influence science, it is useful to distinguish between epistemic (sometimes called “cognitive”) values and non-epistemic (sometimes called “non-

² While the difference between impartiality and neutrality has been a part of the debate throughout the twentieth century, Hugh Lacey deserves credit for making the distinction perspicuous (Lacey 1999).

³ Henceforward, I will use “partial” to describe scientific inquiry that fails to be impartial. “Partial” in this sense does not suggest incompleteness. Following Root, discussed below, I will use “partisan” as the contrary of “value-neutral.”

cognitive” or “social”). Virtually all contributors to the debate now agree that epistemic values are constitutive of scientific inquiry. The questions of value-freedom have concerned the role that non-epistemic values might play. One way to preserve objectivity would be to insist that only epistemic values are properly constitutive of inquiry (Hempel 1965; Lacey 1999). There are a number of lines of argument, however, strongly suggesting that non-epistemic values cannot and should not be excluded from a constitutive role in objective inquiry.

Helen Longino has argued that non-epistemic values can have an important function in theory confirmation. Her argument begins with the fact that no state of affairs is evidence for or against any hypothesis all alone. What makes something evidence for or against a particular hypothesis is a set of background assumptions or beliefs. Hence, given suitable adjustments in background beliefs, a single state of affairs might be evidence for different, even contrary, hypotheses. Since background assumptions and beliefs can embody non-epistemic values, it is possible for values to influence the confirmation of scientific theories (Longino 1990: Chapter 3). This argument against impartiality raises the problem of objectivity anew. Why not regard such intrusions of moral or political commitment as bad science? Longino’s solution is to demand social processes that screen out idiosyncratic values and open the evaluations to critical discussion. Objectivity does not require impartiality, but it does require that bias be eliminated, and her criteria are designed to eliminate biases. Longino thus shows one way in which impartiality might be rejected without threatening objectivity.

While Longino’s model aims at limiting bias to protect objectivity, some feminist philosophers of science have looked for ways in which political criticism might enhance objectivity. Alison Wylie has argued that politically motivated critique can sometimes make science empirically better. She has analyzed cases where theories or interpretations (particularly

those in archeology, but other disciplines as well) were interrogated from a feminist standpoint. In the cases she discusses, the feminist scientists illuminated the influence of androcentric values on existing theory. By taking a feminist standpoint, the scientists were able to see how the theory was limited in its explanatory power, or to expose new evidence that bore on the problem. Thus in some cases, an explicitly political concern seems to generate better science. But once impartiality fails, what makes one theory empirically better than another? In other words, how can evidence serve to confirm or disconfirm theories, given that evidence is theory laden and theory is value laden? Wylie's solution is to look at the full breadth of the ways in which a theory is supported:

[M]y thesis is that archaeological evidence derives its stability and autonomy from two sources: the *security* of the background knowledge invoked to establish a link between the surviving record and the past events or conditions that produced it and the epistemic *independence* of the evidence thus constituted. (Wylie 2002: 192)

Security of the background is partly a matter of its credibility, but also the degree to which there is an understanding of the processes by which the evidence was produced. Independence has two dimensions as well: independence of the test hypotheses from background assumptions, and independence of the hypotheses from each other, where multiple sources of evidence are brought to bear. In the episodes of feminist science that Wylie analyzes, the evidence was independent of the feminist values and the successor theories inspired by them. Objectivity is thus preserved by criteria easily recognized as epistemically sound, even though the inquiry was not impartial.

So far, "objectivity" has been left undefined in the discussion. Work on this idea has shown that it is a multifaceted concept, with a complexity that is difficult, if not impossible, to reduce (Janack 2002; Lloyd 1995). Whether or not there is a univocal analysis, the literature has made it clear that objectivity cannot be simply identified with value-freedom or a disinterested perspective. There are at least three senses of "objectivity" that are relevant to the discussion of

scientific value-freedom: (1) objectivity as freedom from bias, (2) objectivity as intersubjectivity, and (3) objectivity as methods (or the result of methods) that are reliable.⁴ The most important point is that all three senses of objectivity are consistent with a constitutive role for non-epistemic values. Longino and Wylie draw on different senses of objectivity to show how their form of value-laden inquiry can preserve objectivity. Longino's criteria are designed to eliminate bias and make scientific judgment intersubjective. Wylie, on the other hand, is more concerned with the ways in which evidential support might be robust. Wylie's and Longino's approaches are thus complementary, preserving objectivity in different ways.

Longino and Wylie both work in the tradition of feminist philosophy of science, and they aim to challenge more than the impartiality of science. Feminist critique calls the neutrality of science into question too. On Wylie's account, one of the strengths of feminist approaches in science is that it looks at existing inquiry through a political lens (Wylie 2002: Chapter 14; Wylie and Nelson Forthcoming). Politically motivated critique makes visible embedded values that were invisible to the practitioners. The feminist position is not simply that these hidden values need to be exposed and expelled. Rather, the successor theories encode their own values and political standpoints, albeit in a more critical, self-conscious way. Worries about objectivity arise again at this point. Even if inquiry could fail to be impartial, yet be objective, how can it fail to be neutral, and still be objective? Louise Antony has called this the "bias paradox:" feminist critique exposes the biases of non-neutral science, but replaces them with non-neutral biases of their own. Without a neutral criterion of objectivity, there are no grounds on which to object to biased inquiry (Antony 1993: 188-191).

⁴ This way of managing the complexity of the concept of objectivity is similar to, but not identical with Sharon Crasnow's (2006).

The bias paradox is not a special feature of feminism. Rather, it arises for any position that argues against the neutrality of science. For example, Michael Root (1993) and Charles Taylor (1973) both argue that, at least in some domains, social scientific inquiry must entail or implicate (in Grice's sense) policy recommendations; neither invokes feminist political commitments in their argument. We might ask Antony's question of Root or Taylor: what makes the political values encoded in your favored theories better than the alternatives? Rather than answering this question directly, Root and Taylor call into question one of its presuppositions. Non-neutral (or "partisan," as Root calls it) inquiry looks problematic because twentieth century philosophy has presupposed a Humean distinction between facts and values. Values cannot be reduced to facts, nor are evaluative propositions entailed by descriptions. The question thus presupposes that there is no scientific evidence that might be brought to bear on the values embedded in partisan science. Root and Taylor deflect the question by calling the fact-value dichotomy into question. They argue that any attempt to draw a line between purely descriptive and purely evaluative terminologies must fail (Root 1993: Chapter 8; Taylor 1973: Section 3), hence the idea of neutrality is unattainable.

Agreeing that facts and values should not be dichotomized does not completely mitigate the sting of Antony's question. In what sense (if any) could partisan inquiry be objective? There seem to be two possible answers, roughly corresponding to Longino's and Wylie's strategies for preserving the objectivity of partial inquiry. One might demand that there be critical, public discussion of the embedded evaluative claims. This does not look like a fruitful strategy. Even if the discussion converged on a single opinion, the most we could hope for would be consensus. And unless we engaged some further apparatus (value-realism, Peircean convergence, Brandomian double-bookkeeping, etc.), the kind of "objectivity" obtained seems too weak, cut

off from the important sense of objectivity as something real and independent of the knowers. The other strategy would be to show how value-laden theoretical claims could be supported by empirical evidence in a reliable and robust way. This strategy is partly supported by Root's and Taylor's arguments that facts and values should not be dichotomized. To articulate this strategy more completely, one must show (1) specific ways in which facts and values are intertwined and (2) how reliable methods might gather evidence that supports or undermines those fact-value complexes. Here, I suggest, the revolution in anthropology might provide some lessons. Nineteenth century anthropology was not neutral. It had clear policy implications, and it was replaced by a theory with its own political commitments. However, the debate was never explicitly about the political values, it was about method, evidence, and theory. Boas' work, therefore, might show us how inquiry that is both partial and partisan can be objective.

3. Higher and lower civilizations

Nineteenth century anthropology was characterized by classificatory systems that fit existing human groups into an evolutionary hierarchy. Edward Bennett Tylor, Herbert Spencer, Louis Henry Morgan, and others had synthesized facts of cultural, linguistic, and physical diversity into stages of the growth of civilization. Within the space of a generation, this kind of theorizing disappeared and was replaced by intensive, local investigations; what we now call ethnography and cultural anthropology. The reasons for this dramatic change were no doubt multifaceted. Moreover, the changes were manifested in slightly different ways in Britain and the United States, and the institutional pressures may have been somewhat different. By all accounts, however, Franz Boas' body of work was an important ingredient in the complex changes of early twentieth century anthropology.

The program that Boas criticized is crystallized in the opening paragraph of Tylor's *Primitive Culture* (1871). While it is often quoted because it introduces the concept of culture into Anglophone anthropology, it is more important for the way in which the concept of "culture or civilization" frames the leading questions of the project:

Culture or Civilization, taken in its wide ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society. The condition of culture among the various societies of mankind, in so far as it is capable of being investigated on general principles, is a subject apt for the study of laws of human thought and action. On the one hand, the uniformity which so largely pervades civilization may be ascribed, in great measure, to the uniform action of uniform causes; while on the other hand its various grades may be regarded as stages of development or evolution, each the outcome of previous history, and about to do its proper part in shaping the history of the future. To the investigation of these two great principles in several departments of ethnography, with especial consideration of the civilization of the lower tribes as related to the civilization of the higher nations, the present volumes are devoted. (Tylor 1871: 1)

Tylor here introduces "two great principles": "the uniform action of uniform causes" and the evolution of culture. In this paragraph, the principles are aligned with a pair of questions. Indeed, they can be seen as presuppositions of any adequate answer to the questions. The first question is about the similarities among human cultures: Why do human groups that are widely separated in time and space have similar specific cultural elements? Tylor suggests that the similarity can be explained by finding like causes for like effects. The causes and effects, for Tylor, were psychological. The concept of culture presented in the first sentence identifies culture with a person's "capacities and habits." The principle of "the uniform action of uniform causes" thus expresses a commitment to explain cultural similarities by appeal to the universal application of the "laws of human thought and action." This theoretical commitment came to be known as the "psychic unity of mankind." The second question is about human cultural difference: Why do some human groups exhibit higher forms of civilization while others exhibit lower? This question clearly presupposes that there are higher and lower grades of culture.

Taylor's answer to this question invokes the idea that cultures evolve from lower to higher grades. Taylor's evolutionary scheme systematically answers both questions. The laws of thought are the same everywhere, so all human groups go through the same developmental stages. Observed differences among contemporary cultures arise because of differences in the rate of their evolution.

On the surface, Taylor's project might seem value-neutral. There is talk of "grades" of civilization, and "higher nations," but it is quite a leap from these ideas to substantive political commitments. Metaphors are important, but not all uses of "higher" and "lower" imply that lower is inferior. Indeed, in *Primitive Culture*, Tylor tries to draw a line between his discussion of culture and others' talk of race:

For present purposes it appears both possible and desirable to eliminate considerations of hereditary varieties or races of man, and to treat mankind as homogeneous in nature, though placed in different grades of civilization. The details of the enquiry will, I think, prove that stages of culture may be compared without taking into account how far tribes who use the same implement, follow the same custom, or believe the same myth differ in their bodily configuration and the colour of their skin and hair. (Tylor 1871: 7)

The implications of this passage can be seen when it is put into the context of the nineteenth century debate about the origin of human races. Ploygenists argued that the different races had distinct origins. Monogenists contended that there was a single human origin and that the different races were a product of environment or evolution. In the above passage, Tylor is trying to set aside this issue and focus on the question of cultural evolution. He is provisionally siding with the monogenists by supposing that "mankind [is] homogenous in nature, though placed in different grades of civilization." At the same time, he is trying to distance his inquiry from the question of whether grades of civilization can be mapped onto different races. Tylor is thus here expressing his awareness of the political implications of his work: if there are grades of civilization, and civilization tracks race, then there are grades of race too.

Within his own text, Tylor may have set aside the question of whether some races were superior to others, but in the larger context of evolutionary anthropology, it was impossible. Polygenists and monogenists agreed upon the very point that Tylor tried to finesse: that psychological, social, linguistic, and biological differences among humans were correlated, and that the correlation was explained by the existence of race. Tylor can be seen as providing two premises of an argument: (1) Cultures evolve from lower to higher types and (2) cultural evolution is a psychological phenomenon. It follows that there are higher and lower *psychological* types. Given the correlation of psychological and biological differences, it follows that there are higher and lower races. In the context of nineteenth century thought, Tylor's evolutionary scheme for the development of civilization had political ramifications for race relations.

Boas understood the relationship between Tylor's evolutionary project and the nineteenth century politics of race. It is expressed most clearly in his 1904 essay, "The History of Anthropology."⁵ Deceptively named, the essay is a systematic critique of "the ambitious system of Herbert Spencer and the ingenious theories of Edward Burnett Tylor" (Boas 1904/1979: 27). In it, he distinguishes between "historical" and "classificatory" projects in anthropology. The historical project "endeavors to reconstruct the actual history of mankind" (Boas 1904/1979: 24). He traces the classificatory project to eighteenth century zoology:

⁵ "The Human Faculty as Determined by Race" expresses similar sentiments. The target of his criticism was the idea that "the white race represents a higher type than all others" (1894/1974: 222), and he makes some remarks on the justification of slavery (1894/1974: 235). The chapter on the "The Race Problem in Modern Society" in *The Mind of Primitive Man* further demonstrates his awareness of the political and moral ramifications of anthropology.

...the main efforts were directed towards a classification of the races of man and to the discovery of valid characteristics by means of which the races could be described as varieties of one species or distinct species. (Boas 1904/1979: 25)

Boas portrayed nineteenth century anthropology as a mixture of the historical and classificatory projects. The historical aspect accounted for the anthropological concern with evolution, and the classificatory aspect accounted for the way in which the evolutionary scheme categorized existing human groups into stages:

The classificatory aspect was combined with the historical one, and the leading discussion related to the discovery of mental differences between the zoological varieties or races of men, and to the question of polygenism and monogenism. The passions that were aroused by the practical and ethical aspects of the slavery question did much to concentrate attention on this phase of the anthropological problem. (Boas 1904/1979: 25)

Boas thus saw the project of evolutionary anthropology connected the psychic unity of humankind to the issue of slavery.

Boas did not think that the relationship between evolutionary anthropology and slavery was accidental or contextual. Rather, he went on to say that there is a “subjective valuation which is characteristic of most evolutionary systems” (Boas 1904/1979: 28), and this value twists the historical questions into classificatory ones:

From the very beginning there has been a strong tendency to combine with the historical aspect a subjective valuation of the various phases of development, the present serving as the standard of comparison. The oft-observed change from simple forms to more complex forms, from uniformity to diversity, was interpreted as a change from the less valuable to the more valuable, and thus the historical view assumed in many cases an ill-concealed teleological tinge. The grand picture of nature in which for the first time the universe appears as a unit of ever-changing form and color, each momentary aspect being determined by the past moment and determining the coming changes, is still obscured by a subjective element, emotional in its sources, which leads us to ascribe the highest value to that which is near and dear to us. (Boas 1904/1979: 26)

Civilizations like those of Europe are more valuable than the cultures encountered in Africa, Australia, or the Americas. Because we see ourselves as the highest form of civilization, the

historical project of discovering origins became the classificatory project of sorting out the stages that lead to us. Without the “subjective value,” anthropology would have a primarily historical orientation. Put in contemporary language, Boas was arguing that a constitutive, non-epistemic value turned nineteenth century anthropology into a partisan enterprise.

After such an explicit recognition of the role of a moral value in scientific theory, one might expect Boas to argue that a proper science should be value-free. He did not make such a Weberian move. Instead, he made a broad methodological and empirical argument against the project of nineteenth century anthropology. Empirically, he argued against both presuppositions of Tylor’s project: the “like effects, like causes” principle and the classification of groups into higher and lower civilizations. Methodologically, he argued that the “classificatory” project of anthropology required a “deductive” methodology, and that such a method was epistemically inferior to an “inductive” method. Evidence gathered by inductive methods then further supported his empirical arguments. Boas thus argued against the constitutive values of nineteenth century anthropology by undermining the theory of which they were a part. Rather than arguing that science should be purged of values, Boas argued that valuing higher over lower forms of civilization led to an *empirical* dead end.

4. Cause and method

Tylor’s project tried to explain similarities among human groups as effects of similar, underlying causes. Boas argued against the presuppositions of this explanatory framework in some of his earliest published work. In 1887, he criticized Otis T. Mason, curator of ethnology at the United States National Museum, for his arrangement of ethnological material. Like most museums of the time, the ethnological collection of the National Museum arranged similar artifacts together: baskets in one display, arrowheads in another, and so on. The idea was to

make the collection amenable to cross-cultural comparison. Pan-human developmental trends in technology, decoration, and so on could be easily identified. Boas objected, arguing against the like causes principle (Boas 1887a, 1887b; Mason 1887). Boas' critique of the like causes principle was more fully developed ten years later in "The Limitations of the Comparative Method in Anthropology" (Boas 1896/1940). While he did not put it in these terms, Boas' critique showed that Tylor's phrase "the uniform action of uniform causes" is ambiguous. When Mason articulated the principle, it took the form of the platitude that "In human culture, as in nature elsewhere, like causes produce like effects" (Mason, quoted in Boas 1887b: 485). Boas showed that the logical function of the principle required a stronger commitment: like effects must have been produced by like causes.

In "The Limitations of the Comparative Method in Anthropology," Boas pointed out that the like causes principle played two roles in evolutionary anthropology. First, according to Boas, the comparative method argued as follows: similar ideas arise in different locations, like effects have like causes, hence the human mind obeys the same laws everywhere. It is relatively easy to see that "like effects have like causes" is an unreliable principle when applied to cultural phenomena. Boas provided a series of counter-examples of cases where similar artifacts, practices, or ideas arose under different circumstances. Since the like causes principle is so unreliable, we cannot infer that the human mind follows the same laws everywhere from mere cultural similarities (Boas 1896/1940: 273-275). The second role is to generalize from specific historical relationships to general patterns of evolution:

"The comparative studies of which I am speaking here attempt to explain customs and ideas of remarkable similarity which are found here and there. But they pursue also the more ambitious scheme of discovering the laws and the history of the evolution of human

society. The fact that many fundamental features of culture are universal, or at least occur in many isolated places, interpreted by the assumption that the same features must always have developed from the same causes, leads to the conclusion that there is one grand system according to which mankind has developed everywhere; that all the occurring variations are no more than minor details in this grand uniform evolution.” (Boas 1896/1940: 275)

For instance, having proven that patriarchal family structures developed out of matriarchical ones in some instances, the comparative method concludes that it always does so. This follows only if one assumes that like effects (*e.g.* patriarchal families in different parts of the world) must have like causes (Boas 1896/1940: 275). So, the attempt to articulate a unified evolutionary system depends on the like causes principle. Since the principle is false, Boas argues, there is no explanation of the sort that the evolutionists are attempting to discover.

Boas was not skeptical of the attempt to find laws that govern cultural change. His conclusion had a methodological focus: if there are laws of culture, then the evidence for them can not be mere cultural similarities. The problem with the like causes principle is that it licensed bad inferences and weak explanations. In both “Limitations...” and the earlier exchange with Mason, Boas thus turns immediately from the criticism of the principle to a methodological discussion of “inductive” and “deductive” methods. Boas frequently contrasted “inductive” and “deductive” methods in this period of his writing (Boas 1887a, 1894/1974, 1896/1940, 1902), and it is important not to import a twentieth-century philosophical meaning to these words. He characterized the distinction in the 1887 exchange with Mason:

One method of studying [the occurrence of similar inventions in regions wide apart]—and this is Professor Mason’s method—is to compare the phenomena, and to draw

conclusions by analogy. This is the deductive method. The other method is to study phenomena arising from a common psychical cause among all tribes and as influenced by their surroundings; i.e. by tracing the full history of the single phenomenon. This is the inductive method. (Boas 1887a: 588)

Boas argued that the classificatory elements of evolutionary anthropology required a deductive methodology. Boas' idea seems to be that, in a deductive method, the classificatory scheme was logically prior to the gathering of facts that would fit into the framework. While this might seem an exaggeration, Tylor himself says something very similar:

“The quality of mankind which tends most to make the systematic study of civilization possible, is that remarkable tacit consensus or agreement which so far induces whole populations to unite in the use of the same language, to follow the same religion and customary law, to settle down to the same general level of art and knowledge.... It is this state of things which makes it so far possible to represent immense masses of details by a few typical facts, while, these once settled, new cases recorded by new observers simply fall into their places to prove the soundness of the classification. (Tylor 1871: 10-11)

The like causes principle facilitated the explanation of similarity by a classificatory scheme, and the deductive methodology ratified that scheme by supplying instances. Therefore, the rejection of the like causes principle called the deductive methodology into question.

Boas argued that the inductive method was superior to the deductive method on straightforward epistemic grounds. In “Limitations...,” he affirmed the epistemic goals of anthropology, goals that he shared with the evolutionists:

It will be well to restate at this place one of the principle aims of anthropological research. We agreed that certain laws exist which govern the growth of human culture,

and it is our endeavor to discover these laws. The object of our investigation is to find the *processes* by which certain stages of culture have developed. The customs and beliefs are not the ultimate object of research. We desire to learn the reasons why such customs and beliefs exist — in other words, we wish to discover the history of their development. The method which is at present most frequently applied in investigations of this character compares the variations under which the customs or beliefs occur and endeavors to find the common psychological cause that underlies them. I have stated that this method is open to a very fundamental objection. (Boas 1896/1940: 276)

Notice how the questions are slightly different than those in *Primitive Culture*. Tylor tried to explain the similarity and difference of customs. Boas' new question is why particular customs and beliefs exist, and he gave this question an historical spin. The questions have to change because the presupposition of the earlier questions—the like causes principle—has been rejected. Any comparison now requires a careful reconstruction of the history of the custom or belief. He calls this the “historical” or “inductive” method:

A detailed study of the customs in their relation to the total culture of the tribe practicing them in connection with an investigation of their geographical distribution among neighboring tribes, affords us almost always a means of determining with considerable accuracy the historical causes that lead us to the formation of the customs in question and to the psychological processes that were at work in their development. (Boas 1896/1940: 276)

The historical method has “much greater accuracy than the generalizations of the comparative method will permit” (Boas 1896/1940: 276). The historical method is thus superior to the comparative method, given that we want to discover laws of the development of society. Boas

has thus presented a straightforward argument for an epistemic value: given certain epistemic ends (the discovery of laws of development), the historical method is a better means of reaching those ends than the comparative method.

5. Racial Classification

In “The History of Anthropology,” Boas’ complained that the “subjective valuation” of our own culture turned the historical question about the development of human cultures into a classificatory one. Because we see other forms of life as stages on the road to ourselves and explain cultural difference in evolutionary terms, we end up imposing a classificatory scheme on the world’s cultures. A long, central passage of the essay was devoted to showing how the classificatory scheme failed on empirical grounds. At the beginning of the discussion, Boas mentioned “the Aryan question” (Boas 1904/1979: 30; *cf.* Boas 1911b: 124). This apparently incidental reference is important, I suggest, for understanding how Boas conceived of the classificatory character of nineteenth century anthropology. “Aryan” was first used to denote a class of European languages, the group we now call “indo-European” languages. Philologists took (and still take) these languages to have developed from a common root. It was a project of nineteenth century philology to reconstruct the original language. It became the question of the origins of the “white race” presently occupying northern Europe. The classification of races, on which the Aryan question depended, presupposed that somatic traits (skin color, hair texture, shape and size of the head, etc.), linguistic traits, and cultural traits all evolved together. In “The History of Anthropology” and other places (1894/1974; 1911a; 1911b), Boas argued that the linguistic, somatic, and cultural criteria of classification failed to correspond. Put in modern terminology, he was arguing that the classifications of evolutionary anthropology were not *concordant*: variation in one trait did not track variation in another. His examples showed that

groups sharing a somatic type had structurally and historically different languages, groups that shared a language failed to share a culture, and so on. The categories used by the classificatory project thus failed empirically.

The argument that racial classifications cannot be consistently identified is one of two Boasian arguments against race that have since become canonical. The second was first floated in an 1894 address to the Anthropology section of the American Association for the Advancement of Science entitled “Human Faculty as Determined by Race” (1894/1974), and later incorporated into *The Mind of Primitive Man*. In this essay, he was directly concerned with the question of whether some races have a higher “capacity for culture.” He thus looked at the evidence of differences among cultures. The upshot was that variation within racial groups is as great as variation between them:

When studying any single anatomical characteristics of races, we find the same phenomenon which was observed in the cases here quoted: the variations inside any single race are such that they overlap the variations in another race so that a number of characteristics may be common to individuals of both races. Still, the single feature does not characterize the race and the differences are sufficiently numerous to permit a satisfactory definition of the characters of races.

The overlapping of variations is significant in so far as it shows that the existing differences are not fundamental. (Boas 1894/1974: 227; *cf.* Boas 1911b: 91-94)

While he was clearly a bit tentative about it in 1894, Boas had hit upon a telling phenomenon. If the anatomical characteristics are such that “the variations inside any single race are such that they overlap the variations in another race,” then those characteristics cannot be used for racial classification. The ultimate consequence of this phenomenon is the demise of the nineteenth

century concept of race.⁶ In 1894, however, the race concept was still framing Boas' inquiry, and he was not ready to reject it completely.

Moreover, evidence available in 1894 did leave one significant inter-racial physiological variation: brain size. The evidence as he had it apparently showed that the brains of white Europeans were larger, on average, than other populations. He was also aware of Manouvrier's work that seemed to show the cranial capacity of "eminent men" to be larger than average. Boas made some important critical observations about these results. He pointed out that there is substantial intra-racial variation in brain size, and he cast some doubt on the idea that brain size and psychological capacity are correlated. Nonetheless, he felt compelled to conclude that there are differences in average brain size between races, and these make it slightly probable that there are racial differences in psychological capacity. He stopped short of concluding that there are significant mental differences, and methodological considerations were prominent in his argument. To identify hereditary differences in psychological traits or capacities, one would have to separate social and environmental influences from psychological influences. That kind of careful empirical work had not been done. Moreover, the evidence about non-western peoples was just too unreliable. He criticized the accounts of travelers, missionaries, and traders on the grounds that the writers did not know the language, were biased, and had only a superficial understanding of the culture. The cross-cultural study of psychological capacities required observers who "really entered into the inner life of a people," and these were rare. After scouting the best suggestions for differences in psychological traits among races, Boas concluded:

⁶ I have discussed the consequences of this phenomenon, and its twentieth century genetic analogues, in (Risjord forthcoming).

Based on these considerations we believe that in the more complicated psychological phenomena no specific differences between lower and higher races can be found. By this, however, we do not mean to say that no such differences exist or can be found, only that the method of investigation must be different. (Boas 1894/1974: 239)

Notice how Boas' methodological values function in both of his arguments against racial categorization. Induction is methodologically important for conclusions about racial capacity because the discovery of intra-racial variation requires a more sensitive sampling strategy than the discovery of inter-racial variation. Hence, if anthropologists held to the method that Boas called "deductive," intra-racial variation would have been much harder to discover. Boasian induction was important for the issue of classification too. The deductive method, as Boas saw it, began with anthropological categories and sought to apply them to human populations. Sampling a small number from a given human group would be sufficient to support their racial categorization. The inductive method demanded more exhaustive data collection prior to categorization. Finally, in all of his discussions of classification, Boas used his more detailed evidence to disrupt the anthropological categories that assimilated linguistic, psychological, social, and somatic variation into evolutionary stages. Boas' methodological commitments thus functioned to both illuminate the limitations of the current data and to accumulate new data that undermined the classifications of nineteenth century anthropology.

6. Objectivity, neutrality, and the scientific grounds for value critique

Boas' empirical and methodological critique was an important factor in the profound shift in anthropology that occurred at the beginning of the twentieth century. While it is beyond the scope of this essay to detail the ensuing development of ethnography and cultural

anthropology,⁷ a couple of points are in order. First, anthropologists did not adopt a successor theory of the same kind that dominated nineteenth century anthropology. In a way, the changes were more profound. Boas' "inductive method" required a thorough study of each culture as a whole. The succeeding generations of anthropologists took up this project and produced holistic ethnographies and detailed culture-historical surveys. It would be misleading, however, to say that this work had no theoretical orientation. By and large, early twentieth century ethnographers were committed to the idea that cultures were distinct, that these cultures encompassed the full range of material, psychological, and social traits, and that a culture trait's location or function in the whole cultural context gave each trait its significance. The second important point is that this orientation was not value-neutral. A constitutive value of ethnography is that human cultures are morally and politically equal; differences among cultures do not support the judgment that some cultures are morally superior to others (Harris 1968; Herskovits 1953; Hyatt 1990; Jarvie 1964; Risjord 2000; Stocking 1968). As this value was the denial of the value of some cultures or civilizations over others, its consequences were reversed too: the constitutive value of ethnography entailed that it was morally wrong to enslave or colonize other cultures.

The revolution in anthropology that occurred between the nineteenth and the twentieth centuries is thus an example of theory change from one partisan theory to another. What might this episode tell us about the objectivity of partisan theory change? Is it, as Antony suggested about feminist science, simply a matter of arbitrarily swapping one set of values for another? At the end of Section 2, this question had been sharpened to the challenge of showing (1) specific

⁷ My take on these changes is presented in (Risjord 2006) and (Risjord 2000).

ways in which facts and values are intertwined and (2) how reliable methods might gather evidence supporting or undermining those fact-value complexes. Let us consider these in turn.

The value embedded in nineteenth century anthropology was, according to Boas, to more highly esteem higher civilizations over lower. This evaluation presupposes that human groups can be divided into categories (kinds of civilization or culture). It also requires that the criteria for the categories be concordant and make a significant, non-arbitrary distinctions. As applied to racial questions, the anthropological categories depended on a concordance of somatic characteristics with social and psychological characteristics. Nineteenth century anthropology further required that the categories form a plausible evolutionary sequence. To value some stages of civilization as higher, then, is a *thick* evaluation in the sense of Root (1993: 208) and Williams (1985: 140). A concept like “higher civilization” must be thought of as having both descriptive and evaluative dimensions to its meaning. The evaluation makes sense only if significant descriptive commitments are satisfied. The descriptive dimension of “higher civilization” linked the evaluative concept to other descriptive commitments of the theory. In particular, the categories that it presupposed were elaborated in the systematic, evolutionary theories. The evaluative dimension, Boas argued, was not accidental to the role of the value in evolutionary theory. He thought it turned the historical questions about human evolution and current diversity into the comparative questions of Tylor’s *Primitive Culture*. The thick evaluative concepts of higher and lower civilizations were thus constitutive in a deep way: the descriptive commitments of the concept linked it to the larger theory, and the evaluative commitments gave the theory its classificatory character.

Now that we have seen the specific way in which facts and values were intertwined in nineteenth century anthropology, the question of how Boas’ empirical critique was able to

undermine its constitutive values becomes clear. Boas' critique had both a methodological and a substantive component. The methodological side of the argument nicely fits Wylie's model of partial inquiry. Wylie's criteria of security and independence preserve the objectivity of value-laden inquiry by providing robust evidence. Boas' methodological arguments are partial insofar as they were motivated by his political worries about the consequences of evolutionary anthropology. He saw the limitations of the comparative method because he was concerned about the political consequences of evolutionary anthropology. His critique of the deductive method was that it was unreliable. Finding positive instances does not provide adequate epistemic support of the categories, and moreover, the method did not uncover sufficient details to tell against the categories. The argument for an inductive method was that, given the goals that he shared with the evolutionists, detailed ethnographic and historical work furthered those ends. Finding the specific role of a cultural item within the whole culture and on identifying its historical relationship to other cultures provided a clearer understanding of how specific culture traits came about. It thus provided good evidence about how and why the culture traits developed as they did. He was thus arguing that his method was more credible (secure) than the alternative in use. Finally, Boas' arguments had a high degree of independence from the evaluative commitments of either evolutionary anthropology or his own theoretical orientation. Boas' methodological critique thus provides a nice example of how failures of impartiality need not result in a loss of objectivity.

Failures of neutrality are worrisome because it has not been clear how empirical evidence—the business of scientific inquiry—could provide reasons to reject values. Boas' empirical and theoretical arguments show how Wylie's strategy for preserving objectivity can be extended to non-neutral theories. What is crucial to the objectivity of partisan inquiry, however,

is not the character of the evidence, but the character of the value concepts. The constitutive values of nineteenth century evolutionary anthropology were thick. The descriptive dimension of the values connected these values to the other claims of the theory, and thus, in their theoretical context, the constitutive values had empirical consequences. We have canvassed Boas' empirical arguments against the like causes principle and the categories of evolutionary anthropology. It is clear that Boas argued against the evaluative concepts of evolutionary anthropology by falsifying their descriptive presuppositions and consequences. Confirmation holism is one of the lessons of twentieth century philosophy of science: theories meet experience as a corporate body, not piecemeal. Boas' critique illustrates how this shopworn conception of confirmation and theory change can be extended to the case of non-neutral science. The thick values are embedded in the theory, and the theory is tested as a whole. As falls the theory, so fall its thick constitutive values.

The root of Antony's bias paradox is the worry that when values become part of the content of the theory, choice of one theory over another seems arbitrary. We can now see that the bias paradox is an illusion created by forgetting that when theories are value-laden, values become theory-laden. Thick evaluative concepts have a descriptive dimension that integrates them into the rest of the theory's descriptive and methodological apparatus. When values become constitutive of a theory in this way, they participate in the testability of the whole. Those tests may be judged for objectivity using the same criteria that apply to any theory. There are positive lessons here too for feminist scientists, critical theorists, action researchers, and other practitioners of partisan inquiry. The values that inform partisan research gain empirical support insofar as they are thick, with multiplex relationships between the descriptive content of the values and the rest of the theory. It would be a mistake to think that the values implicit in a

theory need to be abstracted, shorn of their descriptive content, and defended on purely ethical or political grounds. Moreover, the example of Boas' critique shows that partisan inquiry need not adopt entirely novel methodologies or conceptions of objectivity. While methods should always be subject to critical scrutiny, perfectly ordinary scientific methods and epistemic criteria can be used to support theories with political consequences. Indeed, the main lesson to be learned from Boas is how scientific inquiry can be used to make an empirical case for political evaluations.⁸

⁸ This essay was greatly improved by conversations with Alison Wylie, Kareem Khalifa, Jim Bohman, Paul Roth, and the participants in the 2006 Philosophy of Social Science Roundtable. Many thanks to all.

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