



Exchange across Difference

DID BOAS GET IT RIGHT OR WRONG?

From the Editors

Franz Boas's study, "Changes in Bodily Form of Descendants of Immigrants" (*American Anthropologist* 14:530–562, 1912), has played a significant role in the history of U.S. anthropology. Recently, two sets of authors reanalyzed Boas's results and came to differing conclusions. In "A Re-assessment of Human Cranial Plasticity: Boas Revisited" (*Proceedings of the National Academy of Sciences* 99[23]:14636–14639, 2002), Corey Sparks and Richard Jantz question the validity of Boas's claim that the differences in skull shape between immigrants to the United States and

their U.S.-born children were because of environmental influences. In contrast, Clarence C. Gravlee, H. Russell Bernard, and William R. Leonard find in "Heredity, Environment, and Cranial Form: A Re-Analysis of Boas's Immigrant Data" (*American Anthropologist* 105[1]:123–136, 2003) that Boas's conclusions concerning changes in cranial form over time were largely correct. Here, both sets of authors provide a follow-up to their original study, assessing their results in light of the conclusions reached by the other.

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Boas's Changes in Bodily Form: The Immigrant Study, Cranial Plasticity, and Boas's Physical Anthropology

ABSTRACT In two recent articles, we and another set of researchers independently reanalyzed data from Franz Boas's classic study of immigrants and their descendants. Whereas we confirm Boas's overarching conclusion regarding the plasticity of cranial form, Corey Sparks and Richard Jantz argue that Boas was incorrect. Here we attempt to reconcile these apparently incompatible conclusions. We first address methodological differences between our reanalyses and suggest that (1) Sparks and Jantz posed a different set of questions than we did, and (2) their results are largely consistent with our own. We then discuss our differing understandings of Boas's original argument and of the concept of cranial plasticity. In particular, we argue that Sparks and Jantz attribute to Boas a position he explicitly rejected. When we clarify Boas's position and place the immigrant study in historical context, Sparks and Jantz's reanalysis supports our conclusion that, on the whole, Boas got it right. [Keywords: Franz Boas, plasticity, anthropometry, heritability, immigrant study]

IN 1928, WHEN FRANZ BOAS PUBLISHED 504 pages of raw, anthropometric data from his classic study of immigrants and their descendants, he expressed his hope that others would make use of this enormous data set. "It seemed necessary to make the data accessible," he ex-

plained, "because a great many questions relating to heredity and environmental influences may be treated by means of this material" (Boas 1928:viii). Given this sentiment, one has to imagine that Boas would have been pleased to know that, three generations later, two independent

sets of researchers simultaneously rediscovered his material, converted it to electronic format, reanalyzed it with modern statistical methods, began to ask new questions of the original data, and produced largely similar results—but, nevertheless, came to opposite conclusions. Whereas we corroborate Boas's overarching conclusion regarding the plasticity of cranial form (Gravlee et al. 2003), Corey Sparks and Richard Jantz (2002) maintain that Boas got it wrong.

Our differing conclusions validate Boas's sense of importance in making his data accessible, and we hope that the controversy over whether or not Boas got it right will encourage others to examine this historically important data set. In the meantime, we first take up the most vexing question of all: How can two sets of researchers look squarely at the same data and come to such incompatible conclusions?

There are two answers to this question. First, there are subtle but significant differences in the questions we ask and the methods we use. Second, Sparks and Jantz, on the one hand, and we, on the other, begin from entirely different understandings of what Boas argued on the basis of the immigrant study. In particular, Sparks and Jantz's critique of Boas rests on what we believe is an incorrect portrayal of our current understanding regarding human biological plasticity and of Boas's contribution to that understanding. When we clarify Boas's argument and place it in historical context, Sparks and Jantz's reanalysis actually supports our conclusion that, on the whole, Boas got it right.

DIFFERENT METHODS, DIFFERENT QUESTIONS

We begin with the problem of methods. Sparks and Jantz present four lines of evidence to support their conclusion that Boas got it wrong: (1) a series of univariate t tests to compare head form in U.S.- and European-born samples, (2) a regression analysis to test the relationship between length of exposure to the environment and changes in head form, (3) estimates of narrow-sense heritability of cranial measurements across Boas's entire sample, and (4) an analysis of variance (ANOVA) to test for the influence of immigrant group and birthplace on all cranial measurements. In each instance, Sparks and Jantz claim that their results contradict Boas's original findings. On closer inspection, their analytic approach either (1) is statistically inappropriate, (2) addresses questions other than those Boas claimed to answer, or (3) provides independent confirmation of Boas's overarching conclusion. In no case does their analysis warrant the conclusion that Boas got it wrong. We treat these four lines of evidence in turn.

1. Univariate t Tests

The first analysis is a series of univariate t tests, which Sparks and Jantz say “represent tests that Boas could have used if the formulations had been available for comparison of same-age American and European children” (2002:

14636). In running this analysis, Sparks and Jantz standardize the cranial measurements by sex but not by age, producing 448 possible tests of the differences between U.S.- and European-born children of the same age. However, “because of small sample sizes” (2002:14637), Sparks and Jantz report that they were able to perform only 156 of the 448 possible tests.

This strategy raises several questions. What threshold for sample size did Sparks and Jantz use to determine which tests to perform? How small were the resulting subsample sizes for the U.S.- and European-born children in each immigrant group? What fraction of Boas's total sample did they exclude by ruling out roughly two-thirds of the possible t tests? How did they decide on a significance level of $\alpha = .001$? Did they use a Bonferroni procedure or other correction for the large number of comparisons? With very small sample sizes and a high threshold for statistical significance, how likely is it that this approach was powerful enough to detect differences between U.S.- and foreign-born samples, even if such differences really existed? Given that the 156 t tests they performed involved both the cephalic index and the raw cranial measurements, what fraction of these tests involved just the cephalic index?

These questions make it difficult to assess the importance of their finding that, at a significance level of 0.001, 11 t tests involving the cephalic index showed significant differences between U.S.- and European-born descendants of immigrants. In our view, this limited approach provides evidence in *support* of Boas's fundamental insight that immigrants and their descendants differ in head form. It is also our view, however, that the approach we use in our reanalysis—a single model of cephalic index as a function of age, sex, birthplace, and immigrant group—is a more powerful and more appropriate test of the differences in head form between U.S.- and European-born samples. As we know, Boas anticipated this approach even before the methods had been worked out (Gravlee et al. 2003:135). There is no way to tell, of course, but we suspect that Boas would have used our approach, rather than a series of univariate t tests, even “if the formulations had been available” (Sparks and Jantz 2002:14636).

2. Environmental Exposure

The second analysis that is asserted to contradict Boas is Sparks and Jantz's test of the hypothesis that “there were dramatic effects on cranial form depending on the time of exposure to the American environment” (2002:14636). Sparks and Jantz approach this issue by modeling the cephalic index as a function of the *length of time each child had been exposed to the U.S. environment*. So, for European-born children, the exposure is calculated as 1910 minus the year of immigration; for U.S.-born children, it is simply age.

This variable is not the one Boas referred to in arguing for a temporal effect of environment on changes in cranial form. Boas was clear on this point: “In order to understand the causes which bring about these alterations of

type, it is necessary to know *how long a time must have elapsed since the immigration of the parents* to bring about a noticeable change of type of the offspring" (1912a:7). For Boas, then, the relevant exposure was not the length of time that each *child* had been exposed to the U.S. environment. Rather, it was the length of time that each *mother* had been exposed to the U.S. environment. Thus, his claim regarding the "strong and increasing effect of American environment" was based on comparisons of "the measurements of the foreign-born child, of the child born within ten years after the arrival of the mother, and of the child born ten years or more after the arrival of the mother" (1912a:57). In other words, Sparks and Jantz pose a different question than did Boas, making their results not directly comparable to his. Moreover, as Boas recognized, cranial growth is a developmental process, with much of the development occurring in utero (Bogin 1999: 64). Defining environmental exposure as a child's age or number of years since immigration is unlikely to capture differences in exposure to the new environment at critical developmental thresholds. Boas's focus on maternal exposure to the U.S. environment, which we replicate in our reanalysis, is a more relevant and biologically meaningful focus of inquiry.

3. Heritability

The third line of evidence that Sparks and Jantz present consists of narrow sense heritability estimates (h^2) for the three raw cranial measurements: head length ($h^2 = 0.55$), head breadth ($h^2 = 0.61$), and face breadth ($h^2 = 0.49$). Sparks and Jantz interpret the estimates for head length and head breadth as evidence that "most of phenotypic variation in these traits can be attributed to genetic factors," while they regard the estimate for face breadth as an indication of "a slightly higher environmental variance component" (Sparks and Jantz 2002:14637). In our view, Sparks and Jantz's use of heritability is not directly relevant to the issue of cranial plasticity, and it perpetuates confusion about the technical meaning of the heritability concept in quantitative genetics.

The root of the problem, as Jonathan Marks puts it, is that heritability "*sounds like a property of the feature itself, when in fact it is merely a description of the population in which the trait appears*" (2002:146). Contrary to popular misconception, heritability, as a technical concept in quantitative genetics, does not tell us the extent to which a trait is genetically determined, nor does it bear on the extent to which a trait is sensitive to environmental change. Instead, it refers only to the relative degree of genetic and environmental variance for a particular trait in a given population.

Consider, for example, an estimate of heritability for human height. If the specific population we are looking at has little variation in diet, disease, and other environmental factors that can affect height, then the environmental variation will be low. As a result, the heritability will be high. If, however, the environmental variation

changes, resulting in greater differences within the population in terms of diet and other factors, then the environmental variation increases and heritability will be lower. Heritability, then, is a relative measure that can vary from one population to the next. It is not a measure of the extent to which genetics controls a trait; it is only a relative measure of variation. [Relethford 2003:52–53]

One consequence of the fact that heritability is a relative measure of variation is that estimating the heritability of cranial measurements across Boas's entire sample has no bearing on whether Boas was correct in arguing for plasticity in cranial form. As Lewontin points out, "a trait can have a heritability of 1.0 in a population at some time, yet could be completely altered in the future by a simple environmental change" (1974:400). At a minimum, then, we should want to know how Sparks and Jantz's estimates of heritability differ between U.S.- and European-born descendants of immigrants.

Sparks and Jantz do not provide relevant results, but the parent–offspring regression analysis that we report provides a first approximation (Gravlee et al. 2003:135).¹ Our analysis demonstrates that, in terms of the cephalic index, U.S.-born descendants of immigrants are less similar to their own parents than foreign-born descendants are to theirs. This pattern implies that, when h^2 is calculated separately for each group, estimated heritability is lower for the U.S.-born descendants of immigrants than for their European-born counterparts, *because of the change in environment*. Relatively high environmental variation among the U.S.-born sample corresponds to lower estimated heritability, while relatively low environmental variation among the European-born sample corresponds to higher estimated heritability.

4. Analysis of Variance

Finally, Sparks and Jantz present an analysis of variance (ANOVA) for the three raw cranial measurements and the cephalic index. Although Sparks and Jantz assert that the results "show evidence that the propositions of Boas are in error" (2002:14637), in fact they provide independent confirmation both of our reanalysis and of Boas's original findings. Sparks and Jantz apparently misinterpret their results because they begin with the assumption that the purpose of the ANOVA is "to test for ethnic group (genetic effect) and birthplace (environmental effect) on all cranial variables and the cranial index" (2002:14636). They state that, "if Boas's conclusions are correct, then birthplace should account for most of the variance, whereas the contrary should be true if the effect of the American environment is small" (2002:14636). There are two problems with this assertion.

First, Sparks and Jantz are unjustified in equating *ethnic group* with *genetic effect* and *birthplace* with *environmental effect*. Some (perhaps much) of the differences in cranial measurements among the seven European immigrant groups reflect the geographical patterning of genetic variation among local populations within regions of the

world (Relethford 2002). Yet it is also likely that Boas's "nationality" categories, which Sparks and Jantz translate as "ancestry," capture important socioeconomic and environmental differences related to cranial development. Thus, the differential effect of the U.S. environment across immigrant groups could in part reflect differences in the extent to which New York differed from the developmental environment of their home countries. In short, Sparks and Jantz reach beyond the scope of Boas's data by inferring a "genetic effect" (2002:14636) on the basis of three cranial measurements. As Marks puts it, "to establish a genetic basis for an observed difference between two populations . . . requires more than just observing the difference to be consistent. It requires presumably genetic data" (2002:91).

Second, it is incorrect to invoke ANOVA to determine whether cranial development is shaped *either* by genes *or* by the environment. In a systematic critique of this analytic approach, Lewontin (1974) clarified the distinction between two very different problems—the analysis of *variance* and the analysis of *causes*. Although the analysis of variance "appears to isolate distinct causes of variation into separate elements," he maintained, "the appearance of the separation of causes is a pure illusion" (1974:406). The basic problem is that of "analyzing into separate components the interaction between environment and genotype in the determination of phenotype" (1974:401):

The analysis of interacting causes is fundamentally a different concept from the discrimination of alternative causes. The difficulties in the early history of genetics embodied in the pseudo-question of "nature versus nurture" arose precisely because of the confusion between these two problems in causation. It was supposed that the phenotype of an individual could be the result of *either* environment *or* genotype, whereas we understand the phenotype to be the result of both. [Lewontin 1974:401]

Indeed, the interaction of genotype and environment is precisely the meaning of biological plasticity, and recognition of this phenomenon is one of Boas's enduring contributions to biological anthropology. It is, therefore, incorrect to argue that an ethnic difference in the response of cephalic index to a new environment undermines Boas's original findings. Such patterning is to be expected for a plastic phenotype that is the product of interactions between genotype and the environment during development. By framing the problem as one of genotype *versus* environment, Sparks and Jantz overlook the striking convergence of their reanalysis with Boas's initial findings. Consider the following:

- Whereas Boas suggested that U.S.-born descendants of various immigrant groups "approach a uniform type, as far as the roundness of the head is concerned" (1912a:5), Sparks and Jantz observe "a slight decrease in variation in the American-born subgroups" (2002:14637).
- Whereas for Boas and his contemporaries it was astonishing to find that "there is a difference in ap-

pearance between the immigrants and their descendants" (Boas 1940:76), Sparks and Jantz say "it is evident that there are differences between American- and European-born samples" (2002:14638).

- Whereas Boas made clear that the cephalic index, "during the period of growth of the individual undergoes *only slight modifications*" (1912a:7, emphasis added), Sparks and Jantz observe that "the change in developmental environment produced a relatively minor effect on cranial dimensions relative to familial and ancestral effects" (2002:14637–14638).
- Whereas Boas argued that "the changes which occur among the various European types are not all in the same direction" (1912b:530) and that, for example, "the Hebrews show changes peculiar to themselves" (1912a:56), Sparks and Jantz note that "differences between means and the relative magnitude of the variation between the American- and European-born samples depend on the ethnic group" (2002:14637).

For each of these findings, Sparks and Jantz conclude that their analysis "fails to support the propositions of Boas" (2002:14637). Our view is that Sparks and Jantz have in fact reproduced Boas's landmark discovery that cranial form is sensitive to environmental influences. That they did so using methods different than our own only bolsters the conclusion that, on the whole, Boas got it right.

BOAS AND THE MEANING OF CRANIAL PLASTICITY

We turn now to the more fundamental discrepancy between our reanalysis and that of Sparks and Jantz—namely, that we begin with incompatible understandings of what Boas actually said about environmental influences on cranial form. From our perspective, it appears that Sparks and Jantz arrive at their conclusion by attributing to Boas a position that he explicitly rejected.

Sparks and Jantz argue that Boas got it wrong because the differences in head form between families and between immigrant groups are generally larger than are the differences between U.S.- and European-born children of immigrants. This finding, they argue, means that there is a large genetic component to head shape and a relatively small environmental one. In general, we do not dispute their claim that there is a large genetic component to head shape, and we doubt that any human biologist would seriously contest this point. More importantly, Boas himself never suggested, as Sparks and Jantz maintain, that "the cranium can be shaped primarily by environmental forces" (2002:14636). Indeed, he repeatedly cautioned against such a view and explicitly recognized the hereditary basis of cranial form.

First, Boas was clear that the changes he observed in head shape due to a change in environment were small but real. As Ralph Holloway notes in his companion commentary on Sparks and Jantz's reanalysis, "Boas's many tables of changes of indices between foreign- and American-born

show differences of roughly 1–2 points” (2002: 14623). In the opening chapter of Boas’s final report, for example, he summarizes the change in cephalic index in two of the immigrant groups in his comparison:

The width of the head expressed in per cents of the length of the head is about 78 per cent among Sicilians born in Sicily and about 83 per cent among Hebrews born in eastern Europe. Among Sicilians born in America this number rises to more than 80 per cent, while among east European Hebrews born in America it sinks to 81 per cent. [Boas 1912a:5]

For Holloway, a difference of this magnitude “surely suggests a really significant change, but where are the statistics to show whether it truly is?” (2002:14622). Our reanalysis answers Holloway’s query. Not only do we observe figures almost identical to those quoted above (Gravlee et al. 2003:133, see table 3 and figure 3), but we also demonstrate that these results are not mere artifacts of random sampling error. For that matter, Sparks and Jantz’s own results corroborate Boas’s fundamental insight that “a number of definite, *although slight*, changes are taking place” (Boas 1940:26, emphasis added; cf. Sparks and Jantz 2002:14638).

Second, contrary to Sparks and Jantz’s depiction of him, Boas repeatedly cautioned against the conclusion that “cranial plasticity is a primary source of cranial variation” (Sparks and Jantz 2002:14637). As Holloway notes, “Boas clearly states that his studies never claimed that there were no genetic components to head shape. . . . The myth that the cephalic index was totally plastic and shaped by the environment was not something that Boas himself believed” (Holloway 2002:14622). This assessment of Boas’s position is borne out by his summary of the immigrant study in *The Mind of Primitive Man*:

The traits of descendants of immigrants examined were head-measurements, stature, weight and hair-color. Among these, only stature and weight are closely related to the rate of growth, while head-measurements and hair-color are *only slightly subjected to these influences*. Differences in hair-color and head-development do not belong to the group of measurements which depend in their final values upon the physiological conditions during the period of growth. From all we know, *they are primarily dependent upon heredity*. [Boas 1938:93–94, emphasis added]

Third, Boas explicitly cautioned against the view that plasticity in cranial form implies infinite malleability under changing environmental conditions. Sparks and Jantz misrepresent both the significance of the immigrant study and anthropologists’ current understanding of human biological plasticity by implying that cranial plasticity knows no bounds: “In America, both Blacks and Whites have experienced significant change in cranial morphology over the past 150 years but have not converged to a common morphology as might be expected if environmental plasticity plays a major role” (2002:14638). Even a casual reading of Boas’s own writings should have warned Sparks and Jantz to avoid such a claim:

It would be saying too much to claim that all the distinct European types become the same in America, without mixture, solely by the action of the new environment. . . . Although the long-headed Sicilian becomes more round-headed in New York and the round-headed Bohemian and Hebrew more long-headed, the approach to a uniform general type can not be established, because we do not know yet how long the changes continue and whether they would all lead to the same result. *I confess I do not consider such a result as likely, because the proof of the plasticity of types does not imply that the plasticity is unlimited*. The history of the British types in America, of the Dutch in the East Indies, and of the Spaniards in South America favors the assumption of a *strictly limited plasticity*. Certainly our discussion should be based on this more conservative basis until an unexpectedly wide range of variability of types can be proved. [Boas 1912a:76, emphasis added]

Indeed, for any trait with even a modest genetic basis, we should expect that genetic differences will shape responses to the environment, not that the environment will produce a single common phenotype.

Boas’s students who replicated and extended the immigrant study were equally clear on this point. Harry Shapiro, for example, concluded that “the available evidence suggests that a given type is characterized by only a limited plasticity, and that the patterns of change are fixed by the nature of its fundamental structure” (Shapiro 1939:199). Thus:

I do . . . believe that these changes when they occur, move in accordance with the fundamental structure of the organism and only to a limited degree. I emphatically do not believe that the Japanese will ever become identical with Hawaiians as a result of enjoying an identical environment and I do not expect to find that the Japanese in Hawaii will eventually lose all similarity to the stock from which they came. [Shapiro 1939:202]

Marcus Goldstein likewise emphasized that “physical change in groups as a result of the impact of a new or changed environment does not mean or imply a change of race” (1943:21). “As I understand it, all that Prof. Boas claimed, and Dr. Shapiro after him, was that the human body, including the head, was a plastic organism and responsive, within limits, to its total environment” (Goldstein 1943:17).

Fourth, as these excerpts suggest, “Boas and his students” cannot be blamed for what Sparks and Jantz identify as the “key argument for many that critically view the use of cranial measurements in population studies and forensic settings” (Sparks and Jantz 2002:14636). To begin, we do not accept as given that “many” critics use Boas’s immigrant study in this way. To substantiate their claim, Sparks and Jantz cite precisely *one* critic, Alan Goodman (1995, 1997), and neither of the cited works mentions Boas’s immigrant study or cranial plasticity. Nevertheless, Sparks and Jantz’s misinterpretation of the immigrant study is not novel, and Boas laid out his rebuttal decades ago:

Finally, a few words on the opinion that has been expressed or implied, that our observations destroy the whole value of anthropometry, in particular that the

study of the cephalic index has been shown to have no importance. It seems to me, on the contrary, that our investigations, like many other previous ones, have merely demonstrated that results of great value can be obtained by anthropometrical studies, and that the anthropometric method is a most important means of elucidating the early history of mankind and the effect of social and geographical environment upon man. The problem presented by the geographical distribution of head forms—for instance, of the cephalic index—has not been solved by our inquiry. [Boas 1940:73–74]

CONCLUSION

Judging by headlines alone, it would be hard to imagine that there is much common ground between Sparks and Jantz and us. The story about our reanalyses in a recent issue of *Science*, for example, ran with the clever but provocative title, “Going head-to-head over Boas's data” (Holden 2002). It is true, of course, that we reach incompatible conclusions. Yet the evidence to support those conclusions deserves a closer look. We maintain that, despite Sparks and Jantz's assertion that their results “contradict Boas's original findings” (2002:14636), their reanalysis is largely consistent with our own, and it reaffirms Boas's most significant discovery.

As we argue in our earlier article, the significance of the immigrant study must be understood in historical context. At the time Boas conceived the study, the prevailing view among physical anthropologists was that humankind consisted of a few, unchanging races or types—“permanent forms” which have lasted without variation from the beginning of our modern geological period up to the present time” (Boas 1940:35). Boas's immigrant study is significant because it treated this assumption as an empirical matter. The most important result was that the cephalic index, which had “always been considered one of the most stable and permanent characteristics of human races” (Boas 1912a:5), was sensitive to the environment. Given the prevailing faith in the absolute permanence of cranial form, Boas's demonstration of change—*any* change—in the cephalic index within a single generation was nothing short of revolutionary.

Sparks and Jantz do not refute this fundamental finding. Indeed, the fact that they reproduce it using methods different than our own only strengthens the conclusion that, on the whole, Boas got it right. In a sense, we have only Boas to thank for the fact that Sparks and Jantz dismiss this overlap and chastise Boas for claiming to have uncovered such “dramatic changes in head form” (Sparks and Jantz 2002:14638). If Sparks and Jantz do not see the changes as dramatic, then it is because Boas successfully undermined his contemporaries' blind faith in the absolute fixity of traits. If they do not recognize the small but real differences in head form between U.S.- and foreign-born descendants of immigrants as significant, then it is because we have come to take for granted Boas's revolutionary proof of human biological plasticity—proof that

stands reevaluation, whether by Sparks and Jantz's reanalysis or by our own.

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NOTES

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1. One method of estimating heritability is to calculate regression coefficients that predict a child's value on some trait from the average of the child's parents on that trait (Falconer and Mackay 1996:163–166). As Sparks and Jantz (2002:14636) point out, this method is no longer state-of-the-art. However, because the estimates using this method ($h^2 = .412$ for U.S. born, .648 for European born) are not substantially different from what Sparks and Jantz report, the regression-based estimates suffice for present purposes.

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