

tion as to equities in water appears in the pronouncement that under common ownership "the general government should collect as a tax" on all users of water amounts which may be defined broadly as corresponding to the railway standard of "what the traffic will bear"; he ignores the fundamental economic principle that while common ownership implies the right to impose conditions of distribution and use, it involves primarily the obligation to minimize taxes or other costs of distribution in the common interest.

In pointing his views as to the finality of legal relations already developed in the west, Professor Aldrich declares, "Every western state has voluminous laws on the subject, and ten times more voluminous legal decisions on those laws." Were his familiarity and sympathy with the west still greater than he professes, he would realize that the Idaho water law is better than that of Wyoming after which it was modeled, that the later Oregon law is still better, and that the California water law enacted a few months ago is the best of all, since with each passing year growing knowledge as to physical facts and relations, increasing population and industries, and concurrently advancing standards of equity fall into closer accord—indeed he would realize that the very principles he criticizes are the outcome of experience in the west, where the natural water supply is so meager that it is necessarily measured and apportioned and utilized more carefully than in any humid land, and might even learn that the proposition *All the water belongs to all the People* was first crystallized and expressed through the National Irrigation Congress (an essentially western organization, made up of western men, dominated by western ideas) at a meeting in Spokane wherein the preponderating representation was from Washington, Idaho, Montana and Oregon.

To those unfamiliar with the situation it may be of interest to know that two opposing views concerning the administration of water and other resources have come up in the western states; the formerly prevalent but now minority view is that the resources shall

be exploited for the private profit of those who acquired possession before their value was realized; the later view, already held by the great majority, is that the resources shall be developed, conserved against needless destruction, and utilized in the common interest, under customs and laws established primarily by communities, secondarily, by states, and finally, as need arises, by the federal government. On these opposing views Professor Aldrich contributes polemics, which may be needful pending more specific knowledge; but it is to be regretted that he does not contribute a fact, a figure, a principle, or any other iota of that definite foundation on which alone scientific discussion may fitly rest, and on which sound legislation may eventually be erected—unless, indeed, he is right in his remarkable main contention that the western states, commonly considered the most actively-growing part of the country, are already so bound by statutes and decisions that further progress is impossible.

W J MCGEE

DR. RADOSAVLJEVICH'S "CRITIQUE" OF PROFESSOR BOAS

TO THE EDITOR OF SCIENCE: The number of the *American Anthropologist* just issued from the press (Vol. 13, 1911, No. 3) contains an article by Dr. Paul R. Radosavljevich entitled "Professor Boas's New Theory of the Form of the Head—a Critical Contribution to School Anthropology" (pp. 394-436). The admission of such an article into a respectable scientific journal seems to have resulted from a misapplication of the praiseworthy editorial principle that no student, however high his professional standing, shall be exempt from the most rigorous criticism on the part of the least of his fellow-workers. In the present instance, however, we have to deal not with a critique, but with a lampoon. The extraordinary character of Dr. Radosavljevich's paper requires an immediate reply, especially in view of Professor Boas's protracted stay in Mexico. Without desiring to forestall a fuller rejoinder by those more

competent to speak on the subjects under discussion, I regard it as necessary to illuminate in a preliminary way the nature of the "contribution" that seeks to brand one of the most critical scientific thinkers of the age as an irresponsible sensation-monger incapable of the most obvious precautions against errors of observation and interpretation. I will confine my attention to two points: (1) Dr. Radosavljevich's representation of Professor Boas's theory; (2) Radosavljevich's refutation of "Boas's first conclusion."

1. Dr. Radosavljevich writes (p. 405):

On page 32 of his [Boas's] report¹ he says that no evidence has been collected which would show an actual change in type due to the direct influence of environment, because the type of immigrants changes from year to year, owing to a selection which is dependent upon the economic conditions of our country, "far-reaching" changes in "type" which "can not be ascribed to selection or mixture." According to Boas the racial characteristics do not survive under the new social and climatic environment of America. We may therefore call Boas's theory the *environmental-economic* theory, the first theory of its kind; *environmental*, because it claims that the descendants of the European immigrants change their type "even in the first generation almost entirely." . . .

The theory may be called *economic*, because it claims that the panics of 1893 and 1907 caused a "sudden decrease in the *general* physical development of immigrants" and a "sudden" increase in the cephalic index.

A comparison of page 32 of Boas's report with the exposition just quoted at once absolves Dr. Radosavljevich from the charge of morbid devotion to accuracy of statement. On page 32 Professor Boas has nothing to say of the economic conditions of our country; nor is there a word on the causal connection between changes due to environment and the change in type of immigrants due to economic selection. In the first paragraph of page 32 Professor Boas states that the form of the body seems to be the most stable characteristic of any given race, but that Gould and Baxter,

¹"Changes in Bodily Form of Descendants of Immigrants," Washington, 1910.

Bowditch and others have found indications that under more favorable environment the physical development of a race may improve. Then follows the statement:

No evidence, however, has been collected which would show an actual change in type due to the influence of environment. Where changes of this kind seem to occur—as, for instance, in a comparison of the types of city population and country population in southern Germany and in Italy—the inclination of observers has been rather to attribute the difference either to the selective elimination of the weaker type or to the immigration of different types.

After a brief transitional sentence Boas then, in the final paragraph of the page, proceeds to characterize the contribution to knowledge made through his own investigation as compared with the contributions of his predecessors as previously summarized: while *they* had collected no evidence showing an actual change in type due to environment, but were inclined to ascribe changes to selection or mixture, *Boas* has demonstrated "a far-reaching change in the type—a change which can not be ascribed to selection or mixture, but which can only be explained as due directly to the influence of environment."

Our comparison makes it impossible to characterize Dr. Radosavljevich's exposition of page 32 in parliamentary language.

What, then, of Boas's "economic" theory? This, it may be well to mention, is a favorite butt of Radosavljevichian sarcasm (pp. 415, 420, 426). The fact of the matter is that, on page 28* of his "Report," Boas states that after the panic of 1893 a sudden decrease in the general development of immigrants occurred and persisted for several years; that a similar change *seems* to have occurred after the panic of 1907, and that his observations on this point have been confined to East European Hebrews. On page 30 we learn that among the questions not yet studied by Boas, is "the important problem of the selection which takes place during the period of immigration, and which is indicated by the change of type of immigrants after the panics of 1893 and 1907." On page 39 Boas again calls at-

tention to the drop of all absolute measurements of foreign-born Hebrews arriving in America after 1894.

An attempt to combine all the material, adult and children, for these years, brings out the sudden drop after 1893 even more clearly; and a similar phenomenon is repeated between the years 1907 and 1909. For this reason I am inclined to believe that the type of immigrants is directly affected by financial panics. This can be due only to a selection which takes place in Europe, and which may also be affected by the return emigration from the United States to Europe. The material, so far as it has been discussed, does not give a definite answer to this important question, the solution of which would require a series of parallel measurements taken in Europe.

Were there any possibility of misunderstanding the foregoing statements, it would be eliminated by the legend of Boas's Fig. 17:

The most striking feature of the diagram is the general decrease in all measurements in the period following the year 1894, which indicates that the arrivals during the period following the panic of 1893 were underdeveloped in every direction.

When Radosavljevich reproaches Boas for not studying the effect of American soil and financial panics on the *same* individuals during a period of time representing the age of his subjects (p. 420 f.), he shows that he has not the faintest notion of what Boas is discussing in connection with financial panics.

Unfortunately those who attended a meeting of the American Ethnological Society on March 27, 1911, will be unable to accept this relatively favorable view of Dr. Radosavljevich's attitude towards the "economic" theory. For at that meeting Dr. Radosavljevich, in the course of a lengthy paper, presented a temperate, and formally quite unexceptionable, criticism of what he conceived to be Boas's economic theory. In a discussion Professor Boas pointed out that the criticism rested on a misunderstanding, and Dr. Radosavljevich then—in the presence of Professor Farrand, Drs. Spinden, Goldenweiser and Fishberg, the present writer, and other anthropologists—publicly apologized for his misinterpretation with much profusion of courtesy.

This circumstance seems to have escaped Dr. Radosavljevich's memory.

2. Dr. Radosavljevich asserts that Boas's own observations do not support his theory of a change of type (pp. 406-411, 429). For, says he,

All Hebrews (born in America and in different countries of Europe) are of the same sub-brachycephalic type. Not one age, not one sex, not one individual of Hebrew nativity is represented either by dolichocephalic or by mesocephalic type. The same is true in its way of the Sicilians measured. They are of a high mesocephalic type, both in America and in Europe.

It would be difficult to find in the whole range of scientific literature a more naïve instance of conceptual realism. For Radosavljevich the conventional classificatory divisions of head forms obviously have an absolute biological value. Boas states that foreign-born Hebrews have an index of 84, American-born Hebrews an index of 81. That this difference is a *real* difference results, of course, not from a mere inspection of these two figures, but from the customary statistical treatment of the series involved. But Radosavljevich's mind is undebauched by statistical method. What matters a change of 3 units in the cephalic index so long as "the bulk of both American-born and foreign-born Hebrew boys and girls belong to the same brachycephalic (or rather to the sub-brachycephalic) type"? Sub-brachycephaly ranges from 80 to 86.9. Had Boas found that American-born Hebrews had an index of 79.9, then, we may presume, Dr. Radosavljevich would have hailed the result as the discovery of an actual change of type, provided only that foreign-born Hebrews had an index of 80.001!

As the subject of head forms has an esoteric tang, it may be well to illustrate the logical point at issue by statures. Suppose that we class men below 5 feet 8 in. as short, above 6 feet as tall, and between these limits as middle-sized. Assume further that a race which in Europe has an average height of 5 feet 3 in. adds three inches to its stature in America. Then, according to the classifica-

tory realism of Dr. Radosavljevich, we should not be justified in asserting that a difference in type had taken place, for both statures, 5 feet 3 in. and 5 feet 6 in. fall within our category of shortness. As a matter of fact, it depends entirely on the statistical comparison of foreign-born and American-born individuals (involving the numbers of cases and standard deviations) whether the difference is a real one. A difference of 1 cm. may constitute an actual difference of type in the statistical sense.² Dr. Radosavljevich's inability to grasp what Boas means by "a change of type" and in his insistence on dragging in the irrelevant conventional classification of head forms must be regarded as pathetic.

I believe I have sufficiently elucidated the nature of Dr. Radosavljevich's contribution. I should gladly have refrained from any comment, were it not for the regrettable fact that there are so few anthropologists in America who are able to form an intelligent opinion on the issues involved, and that the uninitiated are liable to be deceived by Dr. Radosavljevich's imposing array of bibliographic references.

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REPORT OF THE SEAL COMMISSION

TO THE EDITOR OF SCIENCE: In a recent debate in the House of Representatives on the fur seal bill, Congressman N. E. Randall, of Iowa, made the following statement, as reported in the *Record*, p. 2158:

Dr. David Starr Jordan wrote a book. He appropriated this chart of Professor Elliott's and incorporated it into his book as his own. Dr. William T. Hornaday, of New York, subsequently wrote a book. He saw Dr. Jordan's book containing this Elliott chart, and Dr. Hornaday reproduced that chart in his book crediting it to Dr. Jordan. About that time Professor Elliott became alive to the situation and wrote a letter of protest to David Starr Jordan. Dr. Jordan was compelled to admit the plagiarism of the chart and that Dr. Elliott was the author of it and the discoverer of the facts it exhibits.

² Westergaard, "Die Grundzüge der Theorie der Statistik" (Jena, 1890), p. 187.

This charge of plagiarism is so specific and circumstantial that it demands an answer. The chart referred to is doubtless the one printed in the back of the first preliminary report of the Seal Commission of 1896-7, of which Dr. Jordan was chairman. As secretary of the commission I am familiar with the circumstances. When the manuscript of the report was submitted to the Treasury Department, Mr. Charles S. Hamlin, then assistant secretary, suggested the advisability of a chart or map giving the relative location of the Seal Islands and showing the general migration route of the herd. The commission had in process of preparation, under the direction of Mr. Charles H. Townsend, for its final report, an authentic chart of the herd's migration route to be platted from the log records of numerous sealing vessels, but this could not be got ready in time for the preliminary report, and Dr. Jordan was averse to publishing a merely conjectural chart. The assistant secretary held that such a chart, even if not strictly accurate, would be useful to the general reader and indicated how it could be readily compiled from the records of the department. Dr. Jordan acquiesced in this arrangement and left the entire matter of the preparation and printing of the map to the department. He did not see the chart until it appeared in the printed report. That the chart appears without legend as to authorship or source was explained by the fact that it was prepared by the department's own draftsmen and from its own data. Furthermore, the chart contains no information which was not at that time the common property of any one having a general knowledge of the subject. It consisted of a series of curved lines emanating from the passes of the Aleutian Islands, sweeping down through the North Pacific Ocean, veering toward the American coast and returning to the Aleutian passes and thence to the Pribilof Islands, the winter months being printed in the series of lines at points supposed to mark the progress of the herd. How completely this chart was superseded by Mr. Townsend's records platted from some 120 sealing logs will be seen by reference to his