

*The Peace Conference Atlas. A Series of Maps to Illustrate Boundary and Other Questions under Consideration at the Peace Conference, 1919. Maps 24. (London: Edward Stanford, Ltd., n.d.) Price 5s.*

THIS small atlas is not designed specially to illustrate the Peace Treaties, but rather the problems which faced the Peace Conference. It should prove useful in studying the vexed problems of European racial and national boundaries. The maps are black and white, with the boundaries, as in 1914, in red, and a red wash used in many cases to indicate areas of speech. Presumably the dividing line is taken at a bare majority, but this is not stated, and in any case we fear that such simplification of Eastern European problems as these clear-cut maps suggest is outside the scope of practical statesmanship. In comparing the maps showing Italian speech and the boundaries of Yugo-Slavia we note some discrepancies, but on the whole the maps are carefully prepared and well printed. The larger scale maps deal chiefly with Eastern Europe, but the late African and Pacific possessions of Germany are not omitted.

#### LETTERS TO THE EDITOR.

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#### Dr. Kammerer's Testimony to the Inheritance of Acquired Characters.

PROF. MACBRIDE'S letter in NATURE for May 22 last calls for some statement from me. When, in 1910, I was engaged in writing those chapters of my book, "Problems of Genetics" (1913), which deal with the effects of changed conditions in producing genetic variation, I endeavoured to form an opinion as to the validity of the cases usually claimed in recent years as having given positive results. I had no difficulty in showing that nearly all this evidence is unsubstantial. The copious and astonishing observations said to have been witnessed by Prof. Tower, of Chicago University, and by Dr. Kammerer, of the Vienna Versuchsanstalt, naturally called for exceptionally careful examination. The results of both these authors had been very widely accepted, and had begun to pass current in the text-books. In the case of Prof. Tower's paper, as I demonstrated in my book, close textual criticism revealed features which suggested that implicit confidence should be postponed pending confirmation—a conclusion to which I had already come when, on a visit to Chicago in 1907, I had seen illustrative specimens which Prof. Tower was good enough to show me. Prof. Tower's results are still quoted (e.g. by Babcock and Clausen in their recent text-book, 1918), but we have for some years awaited fresh light on the facts or any explanation of the difficulties to which I directed attention.

In the case of Dr. Kammerer's statements, most were plainly incapable of ready verification. The instance of *Alytes* was the most favourable for this purpose, inasmuch as the males with the horny pads, said to have been produced in response to changed

conditions, could be easily preserved. So, no doubt, might the Salamanders, of which the "*sattsam bekamte*" history, as Prof. Baur calls it, has been published in numerous German periodicals; but there was this difference: that whereas Salamanders corresponding with Dr. Kammerer's several patterns can be had from the dealers, students of the *Batrachia* are, I understand, agreed that *Alytes* with *Brunftschwien* does not exist in Nature. I therefore wrote from Cambridge (July 17, 1910) to Dr. Kammerer asking for the loan of a demonstrative specimen, promising to examine it with every care and to return it in due course. He replied in English (July 22) that he was on a holiday, continuing: "As soon as I shall be returned to my usual work—two congresses and a journey to Munich are still between—I will send to you any objects you may need for your book and have interest for, with the greatest pleasure! I hope that it will not be too late then for using them in the chapter, 'Effects of External Conditions,' of your future book.

"I am not quite sure whether I killed already specimens of *Alytes* with '*Brunftschwien*' or am possessing only living males of this ( $F_4$ ) generation.

"But I do not doubt that also other objects are well fitted to show easily the effect of conditions and their inheritance. Especially my new experiments on influence of soil, etc., upon colours (not yet published, except some preliminary notes; for instance, in the *Verh. Deut. Naturforscher u. Aerzte*, Salzburg, 1909) are much more favourable for that purpose than the instinct variations, in spite of their morphological consequences.

"I have also promised (i.e. Dr. Prziham has in my name) to Mr. Doncaster to spare him a series of tadpoles with alterations, etc., for your museum; and it is my intention to fulfil this promise, together with that given to you in my present letter during the beginning of this autumn." Nevertheless, neither I nor the Cambridge Museum (as Dr. Doncaster tells me) ever received any of the promised material.

Later in the summer of 1910 I unexpectedly was able to attend the *Mendelfeier* at Brünn, and was for some time in Vienna, having the privilege of being the guest of my old friend Dr. Prziham. I was many times at the Versuchsanstalt, and inquired in vain for the *Alytes*. On one occasion especially, about October 3 or 4, I was there in company with Profs. E. Baur, Lotsy, Nilsson-Ehle, Dr. Hagedoorn, and the late M. Ph. de Vilmorin. Those who survive of that party will remember that, on conferring together, we all shared the same feeling of doubt. After seeing what Dr. Kammerer showed us we were entirely unconvinced, and in particular it seemed to us inexplicable that, if *Alytes* had existed with *Brunftschwien* in July, one specimen of so great a curiosity should not have been preserved, if only for exhibition with the Salamanders at Dr. Kammerer's numerous lectures. I may add that I expressed my doubts categorically to Dr. Prziham, the head of the Anstalt, but I am glad to think that, though he defended Dr. Kammerer, our cordial intercourse continued unbroken up to the time of the war. Few, I imagine, will now consider that, on the evidence available, my scepticism was not justified. (For an elaborate and destructive criticism of Dr. Kammerer's statements, see Boulenger, G. A., *Ann. and Mag.*, August, 1917, p. 173).

After reading Dr. Kammerer's new paper I agree with Prof. MacBride that a fresh inquiry is desirable. The two photographs, Taf. x., Figs. 1 and 2, which he accepts as proof of Dr. Kammerer's observation, present some very curious features, and I feel much curiosity concerning them. It is, of course, on Fig. 2