

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 Brunftschwienlen 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 'Brunftschwienlen' 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. Przibram 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. Przibram. 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 Brunftschwienlen 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. Przibram, 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

that the case rests. This photograph, said to be the work of Prof. E. D. Congdon, of Harvard, is extraordinarily bad. It represents a Batrachian lying on its back, seen from in front. Were we not told that it is *Alytes*, the fact could not have been ascertained, for all but the hands is a blur. The hands are seen from their dorsal surfaces. On the radial side of the wrist of the right hand is a lump which Dr. Kammerer claims as a Brunftschwiele. The phalanges of the thumb, as Dr. Kammerer expressly declares, are unmodified in this specimen, and no *Schwielen* are visible on the left arm or hand at all. Though on analogy with other genera *Schwielen* might well occur on the wrist or forearm, the proposition which Fig. 2 is intended to support is not that set forth in the original paper which I criticised (*cf.* especially *Arch. Entw.* 1909, xxviii. Taf. xvi., where a modified thumb is vaguely represented). In the text of the present paper we are told that the *Schwielen* are very variable in position and extent. I do not, however, find any mention of modification in digit iv. This finger is, of course, external, and could scarcely function in the embrace; nevertheless, the outer side of digit iv. is most conspicuously thickened in the right hand of the animal shown in Fig. 2. So striking is this appearance that everyone to whom I have shown the figure at first sight supposes this thickening to be the *Schwiele* illustrated. I myself, on looking at the picture before reading the details, had no doubt that this was the *Daumen* with its excrescence, the hand being thus supposed to present a palmar view. Dr. Boulenger at once pointed out to me that this interpretation was impossible, for the reason, among others, that the comparative lengths of the digits proved the hand to be shown in dorsal view, and that the modified digit is iv. It must be remembered that the photograph is so indistinct that much is left to the imagination.

The peculiarity of the right digit iv. would be still more manifest if Fig. 1, which gives a normal *Alytes*, were a genuine photograph. It has, however, been so clumsily painted up that the extremities are not like those of any animal. Each finger and toe has a painted outline, not always in the right place, and only on comparison with actual specimens can the full extent of the modification in digit iv. of Fig. 2 be appreciated. As it stands, this digit is very like the *Daumen* of the original figure. I will not yet venture on a positive interpretation, but I may remark that what the new evidence suggests is that these modifications, whatever they may be, and to whatever cause they may be due, can also appear on the outside of digit iv.

I find it difficult to understand why, if these structures are as Dr. Kammerer declares, he did not make a proper series of photomicrographs of them *in situ*, showing their several positions and forms—no very hard task for such an institution as the Versuchsanstalt. Entomologists and students of fungi make such photographs constantly. Even one good ordinary photograph or drawing would have shown more than the ambiguous pictures now offered us. If anyone wishes to see how *Alytes* looks in a good photograph, he should turn to Boulenger (*Bull. Ac. Roy. Belg.*, 1912, p. 573). The latest of Dr. Kammerer's figures dates from July, 1913. A long series of *Arch. Entw.* has been published during the years of the war, often with magnificent plates. Dr. Kammerer does not state how many modified *Alytes* he has had, but by implication they have been numerous. If, on second thoughts, he was unwilling to send one to England, could he have resisted the temptation to send one to the Berlin Museum to be shown to Prof. Baur, and so confound him and other sceptics? Three years had elapsed since we openly expressed our disbelief, but

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I know that up to January, 1914, no such specimen had been sent.

Prof. MacBride urges that sceptics should repeat experiments on the inheritance of acquired characters. We, however, are likely to leave that task to those who regard it as a promising line of inquiry. Why do workers in that field so rarely follow up the claims of their predecessors? Each starts a new hare. Scarcely has one of their observations been repeated and confirmed in such a way that we could be sure of witnessing the alleged transmission if we were to try for ourselves. Brown-Séguard's observation on guinea-pigs is an exception. That has been repeated by various observers, until at length, by the work of Graham Brown, the mystery may be regarded as explained. The observation was true, but the interpretation was faulty. As I have often remarked, acquaintance with the normal course of heredity is an indispensable preliminary, without which no one can interpret the supposed effects of disturbance. This knowledge of normal genetic physiology is being slowly acquired, and already we have enough to show that several variations formerly attributed to changed conditions should not be so interpreted. Even in this case of *Alytes*, were a male with incontrovertible Brunftschwielens before our eyes, though confidence in Dr. Kammerer's statements would be greatly strengthened, the question of interpretation would remain, pending the acquisition of a knowledge of Batrachian genetics.

W. BATESON.

June 22.

The Food of Rats.

IN NATURE of September 19, 1918 (vol. cii., p. 53) a summary is given of an article by Prof. P. Chavigny on the food of rats. Some of the statements in this article appear to me to be extraordinary, particularly the alleged necessity for rats to get cooked human food. The hordes of rats which swarm along our foreshores, and in granaries and like places, could not possibly get sufficient cooked human food to keep them alive, yet they are plump and well-fed. Any one who has kept fowls or ducks in a rat-infested place knows that rats will carry off and devour chicks and ducklings, even dragging them from under the brooding mother, eating them raw. Attacks on living and dead human beings and smaller animals are by no means rare. Along the water-front rats freely catch and eat crabs, and they will devour raw fish with avidity.

Certainly rats will eat cooked food when they can get it, but they are omnivorous feeders, and I have personally known them not merely to gnaw, but to devour pumpkin, melon, apple, and other fruits. Of pumpkin-seeds they are very fond, and an apple-core makes a good bait for a trap. They do not seem to care much for raw beef; I have noticed them attack raw potatoes and pumpkin-seeds, neglecting raw steak which was lying alongside. Under a creeper in my garden near Sydney the common snail (*H. aspera*) was very abundant, and *M. decumanus* used to devour large quantities; the apex of the shell was always bitten off so that the mollusc could be readily extracted. On the Upper Waikato River, New Zealand, the same rat dives into the water and gathers the fresh-water *Unio*. On the river-banks the shells are gnawed open and the animal eaten. The shells are always bitten through at the same spot of one valve, but I forget now whether that was the right or left one.

In Australia at certain seasons a "cutworm" moth, known as the "bogong" or "bugong" (*Agrotis infusa*), swarms in myriads in many places, and is,