Letters to the Editor.

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The Inheritance of Acquired Characters in Alytes.

FOR those who are not concerned with the details of this debate I suppose that the critical sentence in Dr. Kammerer's letter (NATURE, August 18) is that in which he expresses himself as follows: "I willingly admit that the traditional explanation of the pads, namely, that they are produced by friction with the skin of the female, may possibly be a fable," adding references to passages in which he had already discussed alternative hypotheses. But those alternatives were ultimately rejected, and his final judgment was that until the assumption that the pads arise through functional adaptation can be replaced by a better, it remains the only acceptable account.¹ Remarking that the alleged nuptial pads may possibly be due directly to life in water and not an adaptative response, he now tells us that the relevance of his observation to the theory of heredity is in either alternative the same. But is it?

The heavy task of searching for evidence of hereditary transmission of acquired characters has clearly been undertaken in the single hope, forlorn indeed, but undying, that the difficulty created by the existence of the *adaptive* mechanisms might be removed. They constitute a very grave difficulty in all theories of evolution. Various evidence, mostly ambiguous but as a whole significant, does suggest that in special cases, by violent treatments, the germ-cells of animals may be affected, more or less injuriously, and that the consequences may persist at least for some generations; but that does not help us with the problem of adaptation. Dr. Kammerer's admission would relegate the Alytes pads to that class of phenomena. Had this been all that was claimed, I should have felt some interest in the matter, but less.

The significance of the story is now reduced. In 1909 we were told that nuptial callosities appeared on the thumbs of treated males, and that all the males of the "F₄" generation had them. The claim that this was a true adaptation was made without any qualification whatever. This led to my request (made privately in 1910, published in 1913) that a specimen should be produced. In 1919 we hear for the first time that the swellings appear in various other regions of the arms. When at length a specimen is produced, I find it mounted to display a dark thickening on the palm of the hand, a place which, unless I am mistaken, had not previously been specified. That this was the structure to which Dr. Kammerer particularly wished to direct our attention appears also from the fact that the new photograph sent to Prof. MacBride, which I have not had the privilege of inspecting, was made from it. So far as I am aware, this is the only specimen ever exhibited publicly.

Dr. Kammerer complains that I did not at the Linnean meeting produce "a single one of the many objections" alleged in my letter of June 2. His memory is at fault. My chief objection was the position of the pad on the palm. Any one who attended the meeting will know that I directed very prominent attention to this feature. To make my

objection clear and conspicuous I asked in German: "*Das Männchen umarmt sein Weibchen—so—[turning the backs of my hands inwards]—nicht?*" To which Dr. Kammerer as I thought nodded assent. No one can have forgotten that the next speaker took me to task for this, saying by a slip, induced I suppose by what he had seen of the specimen, that "of course" the common toad clasps the female with the palms towards her.

Why Dr. Kammerer should think that in writing of his diagrams I had in mind a book of Plate's (which I hear of for the first time), I cannot imagine; for I added the exact references to his own paper of 1909, Figs. 26 and 26a. The pictures which I threw on the screen, illustrating the fantastic story of Mendelian segregation in respect of the modified habits, will also be found in his paper 12 *Flugschr. d. Deut. Ges. f. Zuchtungskunde*, 1910, and again in *Natur*, Munich, December 12, 1909, papers to which all readers desiring to see the prodigious scope of the original claims should refer. A more detailed though unillustrated account appears in *Mendel Festschr.*, Brünn, 1911.

I do not propose to rebut the minor allegations made by Dr. Kammerer. Several of these would not have been made had he seen my letter in NATURE of July 3, 1919. The answers to the rest will be evident to those who have followed the discussion.

The question remains, what is the real nature of the swellings in the animal exhibited? That on the palm did not look like a nuptial pad. What there may have been on the back of the hand I do not know. I made no statement about it, though Dr. Kammerer says I did. I might no doubt have asked to see the back, but I had no reason to suppose there was anything more to see. The palmar mark was what we were shown for our conviction. This looked so unlike what I remembered of real *Brunftschwien* that I did ask in the discussion, "*Wie wissen Sie, dass sie Brunftschwien sind?*" I knew our frog and toad very well, and, of course, Lataste's drawings of sections, but it was some years since I had looked at other species. I thought that perhaps, where the development is slight, as in *Rana agilis*, the external appearances might be less unlike what I had seen in the Alytes, but they are not. When with that specimen fresh in mind I examined a series of nuptial pads in various Batrachia I realised still more vividly how widely the structure in the Alytes differed from the real thing. In my letter, therefore, I laid stress on the dissimilarity.

Dr. Kammerer writes that his specimen was examined out of the glass by Sir Sidney Harmer and Mr. E. G. Boulenger, but we are not told whether they are among the "dozens" now convinced. Mr. Perkins states that "the epidermal spines are very obvious in the intact specimen." He is the only independent witness, of those whose opinions have reached me, who claims to have seen anything so definite.

I have a strong curiosity to see this Alytes again. Dr. Kammerer challenges me to supply him with apparatus for the purpose of photographing it. I will make a different offer. For the opportunity of examining it at leisure in the British Museum, where comparative series are available, or if preferred in Prof. MacBride's laboratory, I am willing to pay 25*l.* either to the Versuchsanstalt or to other appropriate authority. Plenty of responsible people travel between Vienna and London, and there should be no difficulty in arranging for safe conveyance.

W. BATESON.

The Manor House, Merton, S.W.20,
August 26.

¹ 1919, p. 353: "Bevor also unsere Annahme, die Schwielenbildung geschehe durch funktionelle Anpassung, durch keine bessere ersetzt werden kann, bleibt sie die einzig akzeptable."