

affection, kissing, tears, cradles, father and child.

The sixth chapter, 'Primitive Child-Study' or 'The Child in the Primitive Laboratory,' embraces the following headings: Licking into shape, massage, face games, primitive weighing, primitive measurements, measurements of limbs and body, tests of efficiency, sleep, heroic treatment.

I believe these two statements show that the points of view, according to which the author has coördinated his material, are based entirely on considerations foreign to it. This is particularly clear in the sixth chapter. The various customs collated there have hardly any psychological connection and can, therefore, not be held to elucidate in any way the mode of thought of primitive man. He neither thinks of studying children—as we are just beginning to do—nor does he subject them to tests. The customs recorded by the author are practiced for a variety of purposes, but, certainly, the fact that they resemble in a general way tests which we might apply does not give us a right to consider them as psychically connected.

Almost the only chapters in which we can find a connecting idea are the philological ones with which the book opens. In these the author makes a compilation of the uses to which the terms 'father' and 'mother' have been put by various people. But here another lack of the whole work becomes particularly glaring. The quotations are gleaned without any attempt at criticism, and much of the material that is offered is not a safe guide to follow, because the observations and investigations of the writers referred to were not sufficiently thorough.

The book is an illustration of the dangers with which the comparative method of anthropological investigation that has come into vogue during the last quarter of a century is beset.

The fundamental idea of this method, as outlined by Tylor and in the early writings of Bastian, is the basis of modern anthropology, and every anthropologist must acknowledge its soundness.

But with its growth have sprung up many collectors who believe that the mere accumulation of more or less similar phenomena will advance science. In every other science the

material on which induction is based is scanned and scrutinized in the most painstaking manner before it is admitted as evidence. It is absurd to believe that anthropology is entitled to disregard this rule, which is acknowledged as fundamental in all other inductive sciences. Furthermore, the object of anthropological research being to elucidate psychological laws on the one hand and to investigate the history of human culture on the other, we must consider it a primary requirement that only such phenomena are compared as are derived psychologically or historically from common causes. How this can be done has been shown by no one better than by Tylor. Only the common mistake of attributing any two phenomena that are somewhat alike to a common cause can explain the reasoning that led the author to amass and to place side by side entirely heterogeneous material.

I believe anthropologists, by silently accepting as a contribution to science a compilation like the present made on unscientific principles, will give countenance to the argument that has been brought so often against anthropology as a branch of science: namely, that it is lacking in a well defined scientific method and that, therefore, it is not equal in rank to other sciences.

FRANZ BOAS.

NEW YORK, May 1st, 1896.

THE DISCUSSION OF INSTINCT.

TO THE EDITOR OF SCIENCE: I have been much interested in the letters in your columns on the instinctive activities of young birds. Certain opinions which I hold—and others that the writers suppose that I hold—have been criticised. To explain my exact position, however, would occupy more space than I can reasonably ask you to afford me. May I be allowed, therefore, to content myself with stating that I have in preparation a work on *Habit and Instinct* which will, I hope, be published towards the close of this year. There my own observations will be described and reference will be made to the work of other observers, and there the provisional conclusions drawn from such observations will be discussed. I desire to make this statement, lest my silence should be regarded as discourteous in the coun-

try where I met with so much kindness and such uniform courtesy.

C. LLOYD MORGAN.

UNIVERSITY COLLEGE, BRISTOL, ENGLAND.

THE SUBJECT OF CONSCIOUSNESS.

EDITOR SCIENCE: Referring to the review of my 'Lehrbuch der Allgemeinen Psychologie' in your valuable magazine for September, 1895, which has but recently come to my notice, I sincerely regret that the reviewer should have fallen into so manifest an error as to suppose the 'subject of consciousness' of my 'Psychologie' to be equivalent to 'self-consciousness;' though he expresses himself with some hesitancy when he says 'it seems most nearly,' etc. As I have pointed out in my work, the misunderstanding is quite apt to arise, from the fact that the word 'subject' is often used in the sense of the 'Ego' or 'Self,' as even shown by the reviewer when he says, 'the consciousness of self or subject.' But that is just the very sense in which I do *not* use the word 'subject.' With me, the 'subject of consciousness' does not designate the 'Ego' or the 'conscious mental individual,' but only its fundamental unifying general abstract element, which always exists in the closest union with the other element, which I call *attribute* of consciousness, and *with which* it constitutes the individual unit 'consciousness' or 'conscious individual.' When this is distinctly understood it will be impossible to mistake the 'subject of consciousness,' *i. e.*, the psychological foundation of *all* mentality, for 'self-consciousness,' which is but a later development of the *individual* mind, the 'mental *individual*.' It is a source of great satisfaction to me to have been the first to call attention to this fundamental unifying element. I call it 'subject,' though I shall gladly give up the name if any one will suggest another that is not so liable to be misunderstood. In my 'Psychologie' I lay particular stress upon the fact that, if this 'subject' were not originally present in mental life as the unifying element, together with the attributes of consciousness (sensations, feelings, etc.); if, therefore, as the associationists think, mental life were possible without a subject of consciousness, it would be impossible to explain 'self-consciousness,' which makes its

appearance later; for it is precisely this self-consciousness, which is based primarily upon the existence of the 'subject' as an element of consciousness; but *for that very reason* it is far from being identical with that 'subject.'

JOHANNES REHMKE.

GREIFSWALD, April 16, 1896.

THE PREROGATIVES OF A STATE GEOLOGIST.

EDITOR SCIENCE: In connection with the communication of Dr. Keyes, published in SCIENCE, April 24th, page 365, permit me to say to any who may have a passing interest in the subject that I sent the impression paper copy of the original manuscript to the Editor of SCIENCE with a copy of the publication as it appeared, with a request that he kept the two for some months in order that any one wishing to look into the matter might have an opportunity to do so and judge for himself whether I wrongfully represented matters in my communication published in SCIENCE of April 3d last. I might also state that I sent Dr. Keyes a copy of the letter nearly three months before it was published, with a statement that I would publish the same if he did not do something to give me credit for that which was mine, but which had been published under his name.

ERASMUS HAWORTH.

A CORRECTION.

It is unfortunate that although the figure from Dr. Mügge's paper which I reproduced in SCIENCE last week (p. 698) was expressly marked 'top' on one side, it has been inserted upside down by the compositor. In its present position the figure is meaningless and even misleading.

T. A. JAGGAR, JR.

THE ABSOLUTE AND THE RELATIVE.

TO THE EDITOR OF SCIENCE: Your correspondent 'M.,' in the number of SCIENCE for April 24th, raises a new issue with me; one which has only an indirect bearing upon the subject matter of my article on the 'Illusion Concerning Rest.' In that article I attempted to demonstrate that motion cannot be created or destroyed by collision, but that the body in motion can be only deflected thereby. Now my friend abandons that demonstration and