

conditions of society." This possible theory, he says, "has been made a subject of not less careful study and reflection than the system itself." But after a patient analysis and comparison of its several forms, he comes to the conclusion that it is insufficient to account for the facts.

Thus, as it seems to me, he clearly repudiates the theory of spontaneous growth.

Mr. Morgan thinks that his solution of the problem of relationships must have escaped my notice, because I did not discuss it in my paper read before the Anthropological Institute; but in that memoir I quoted from the chapter in question, and went on to say—

"Mr. Morgan admits that systems of relationships have undergone a gradual development, following that of the social condition; but he also attributes to them great value in the determination of ethnological affinities. I am not sure that I exactly understand his views as to the precise bearing of these two conclusions in relation to one another; and I have elsewhere given my reasons for dissenting from his interpretation of the facts in reference to social relations."

Thus I expressly pointed out that Mr. Morgan, while characterising the "classificatory" system, to use his own terms, as "arbitrary and artificial," nevertheless also regards it as having "undergone a gradual development following that of the social condition." Surely Mr. Morgan must have written his letter without having my book by him, for it seems to me that the above passages, taken together, represent his own theory, as given in his letter. Mr. Morgan hints that the conclusions contained in his last chapter had escaped my notice. He appears to have overlooked the fact that I quoted from that very chapter. I was not, however, reviewing his work, and differing fundamentally, as I do, from the conclusions adopted by him, while feeling deeply also the great obligations to him under which ethnologists lie, I preferred to state my own views rather than to dwell on the differences between the conclusions at which he and I have arrived.

JOHN LUBBOCK

Down, Kent, June 7

Attraction and Repulsion caused by Radiation

I DID not intend to reply to Prof. Osborne Reynolds' letter in NATURE, vol. xii. p. 6, but some persons expect me to say something about it. If the Professor would be careful not to answer me with the ideas that occur to him as he is "on the point of sending off the paper" (see Phil. Mag., Nov. 1874), he would save himself the trouble of many explanations. After my thousand experiments it is scarcely respectful to try to overcome all by his few, and, after three years of my thought, rather hasty to tell me that he explained it all so suddenly with perfect certainty, and that I am unable to comprehend him. It is also scarcely wise to lead us to infer that probably he cannot explain the whole, but that he knows somebody who will soon do it.

Prof. Reynolds seems to base his calculations on some of my experiments which dealt with a perceptible amount of gas, and has not taken notice of those where there is no amount of gas known to be present; for example, in a chemical vacuum.

Prof. Reynolds must show that there is gas or vapour remaining, and he must also show that there is enough to produce the mechanical results. He tells us that the forces will increase as the density of the gases diminishes. The speed will, but if the force does, that can only be up to a certain point, when it is equally certain that a change will take place, and the motion of the particles or molecules will be attended with less force according as they diminish in number. The opposite to this involves something not intended. I suppose he does not intend to speak of forces without matter. The analogy with sound is not quite happy, as that is so readily diminished by lower pressure; although the speed is the same, the power is small. Besides this, what will he say to the case where there is no heat and only light? I am abundantly willing to allow molecules and forces, but I see no place for such as I have been acquainted with.

I am working at the subject and shall be glad to come to a true conclusion. Scientific men need not be so very much afraid of a new law of nature, for some are wanted, and there are certainly many yet waiting to be discovered before nature becomes intelligible to us.

I by no means deny that the phenomena are connected with molecular movements, but I believe that Prof. Reynolds has neither explained this nor proved it by experiment. His explanation suits only a part of my work; and so does the saying that the "experiments stand in much the same relation to the kinetic

theory of gases that Foucault's pendulum occupied with regard to the rotation of the earth." This is an analogy showing much acuteness, viewing the matter from what I consider the unproved side.

Prof. Reynolds goes far when he says that my experiments are "the only direct proof that has ever been obtained of the kinetic theory of gases." It may be, but if so, physicists must have been too easily pleased with their theories.

I might say much more, but I prefer to wait. There is but little good done by short notes when such a large and important subject waits for elucidation.

London

WILLIAM CROOKES

American Indian Weapons

THE Pai-ute weapon, described by Mr. Mason in your last number (p. 107), although extremely interesting and quite new to me, appears scarcely sufficiently characteristic of a war weapon to form an exception to the statement of Schoolcraft, that the clubs of the North American Indians as a rule are curved. It would be interesting if it could be ascertained how such a peculiar instrument as that described by your correspondent came to be used as a weapon of war. Its form precludes the possibility of its having been designed for such a purpose. The mode of holding it suggests the idea of its having originally been used as a pounder, the thick end having perhaps been employed for pounding grain, beating out grass for cloth, or for preparing skins. It somewhat resembles the instrument used for making bark cloth in some of the Polynesian Isles, and it corresponds to the Beate (Battelle) still used by Irishwomen for beating flax, and occasionally, I have no doubt, as a weapon of war; but these are used with the flat side, not the end. The only weapon I know of that is used like the Pai-ute club is the New Zealander's Merai or Pattoo-Pattoo, the sharp end of which is thrust into the back of the head of the offender; and I have suggested elsewhere that this peculiar and awkward mode of using it arose from its having been originally what its form resembles, a stone axe blade (celt), used as the Australians now use it sometimes, in the hand without any handle. The sharp edge at the end of the Merai shows its original intention, in the same way that the flat end of the Pai-ute club could never have been designed as an offensive weapon, but would have been useful as a pounder; it may be, in fact, a "survival" converted to other uses. There exists, of course, no law of nature to prevent North American Indians from using straight clubs as well as curved ones, but my observation of their weapons confirms the statement of Schoolcraft, that as a rule they do not. Amongst races in a more primitive state of culture, as amongst the Australians, we find that nearly every form of club that is made straight is used also in a curved form, the curvature arising merely from the natural bend of the branch out of which it was constructed; when these natural curves were found useful, they appear to have been retained and systematised. But the North American weapons are of a more advanced and conventionalised description, and we cannot trace their origin and growth so clearly as amongst lower savages. The description of the Moquis boomerang by Mr. Mason is an interesting fact, which, combined with the mention of it by Bancroft amongst the Pueblo Indians of New Mexico, points to the probability of a connected area of distribution. Drawings of weapons such as those given in your journal are of the utmost value in assisting to trace the distribution of like forms.

A. LANE-FOX

Guildford, June 12

Hardened Glass

PERHAPS the following short and preliminary account of some observations on the optical and mechanical properties of De la Bastie's toughened, or, as I think more correctly, hardened glass, may interest your correspondent Mr. James H. Logan (vol. xii. p. 87).

Immediately after the publication of M. De la Bastie's specification I prepared specimens of the glass, I submitted them to careful optical examination by polarised light. Perhaps the best experiments are those made by means of short cylinders and small cubes and parallelepipeds carefully "hardened." A small cube with half-inch sides thus prepared has its sides ground plane and polished. The operation of polishing may be dispensed with if a small microscopical thin cover be cemented on the ground surface with Canada balsam. The cube is then mounted between strips of blackened cork, and examined in the