as the first series, only it is of a slightly more advanced character. The idea throughout is to place objects before the children, by means of which they may be able to recognize the general properties relating to them. Thus, in the first few lessons certain substances are exhibited, from which the general idea of solids, liquids, and gases can be gathered. The general characters of iron and steel, and those of a variety of other metals, are then illustrated, the metallic surfaces of which suggest the principles of the reflection of light, which are consequently treated of. The remaining lessons deal with sunlight, colour, motion, and the forces that produce it. The appliances for the experiments are of the most simple kind, and there are notes for the use of the teacher, from which the necessary information can be gathered.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Darwin on the Unity of the Human Race.

HAVING had occasion last year to allude as a fact to the circumstance that Charles Darwin assumed mankind to have arisen at one place, and therefore in a single pair, I was surprised to find that this fact was doubted, or at least very doubtfully accepted, by some of my scientific friends; and I was asked for a reference to his works in confirmation of it. My principal reliance, however, was in the recollection of a private letter to myself from the illustrious naturalist, which I had unfortunately mislaid. Having now recovered this letter, I send a copy of it to NATURE for publication, simply explaining that this letter was in reply to a letter from me in which I put the direct question, why it was that he did assume the unity of mankind as descended from a single pair? It will be observed that in his reply he does not repudiate this interpretation of his theory, but simply proceeds to explain and to defend the doctrine.

ARGYLL.

"*Down, Beckenham, September* 23, 1878.

"DEAR DUKE OF ARGYLL,—The problem which you state so clearly is a very interesting one, on which I have often speculated. As far as I can judge, the improbability is extreme that the same *well-characterized* species should be produced in two distinct countries, or at two distinct times. It is certain that the same variation may arise in two distinct places, as with albinism or with the nectarine on peach-trees. But the evidence seems to me overwhelming that a well-marked species is the product, not of a single or of a few variations, but of a long series of modifications, each modification resulting chiefly from adaptation to infinitely complex conditions (including the inhabitants of the same country) with more or less inheritance of all the preceding modifications. Moreover, as variability depends more on the nature of the organism than on that of the environment, the variations will tend to differ at each successive stage of descent. Now it seems to me improbable in the highest degree that a species should ever have been exposed in two places to infinitely complex relations of exactly the same nature during a long series of modifications. An illustration will perhaps make what I have said clearer, though it applies only to the less important factors of inheritance and variability, and not to adaptation—viz. the improbability of two men being born in two countries identical in body and mind. If, however, it be assumed that a species at each successive stage of its modification was surrounded in two distinct countries or times by exactly the same assemblage of plants and animals, and by the same physical conditions, then I can see no theoretical difficulty to such a species giving birth to the new form in the two countries. If you will look to the sixth edition of my 'Origin,' at p. 100, you will find a somewhat analogous discussion perhaps more intelligible than this letter. "Yours faithfully,

"CHARLES DARWIN."

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

THERE are many, no doubt, who will be pleased to have the English translation of some of the papers of Prof. Van der Waals which has recently been published by the Physical Society. There are those at any rate who will be glad to satisfy themselves, without overmuch labour, as to how much there is of real importance in these much-discussed memoirs, published originally in a language too little studied in this country. I do not propose to criticize the papers, though I do not think that either the thermodynamics or the conclusions will bear examination; but I cannot avoid the task, however ungracious, of pointing out that they do not show a proper appreciation of the work of Andrews.

I will make but two or three quotations, and they shall be as brief as possible.

On the first page of the author's preface appears this sentence: "These latter [theoretical considerations] led me to establish the connection between the gaseous and liquid condition, the existence of which, as I afterwards learned, had already been suspected by others." The author's preface concludes as follows: "That my conception has shown itself to be a fruitful one cannot be denied, and it may be the incentive to further inquiry and experimental investigation."

The claim put forward in these sentences appears to me absolutely untenable. This connection, or relation as it might better be called, was not only "suspected" by Andrews, but was clearly and explicitly stated by him in the Bakerian Lecture for 1869; a paper published under the very title which Van der Waals, in 1873, has taken (without a word of acknowledgment) as the title of his essay.

On p. 430 a description is given of the mode of altering the gaseous condition of a substance (carbonic acid) into the liquid condition, and *vice versâ*, by a continuous process devoid of any abrupt change. At the end of the description come the two sentences: "Now, we cannot but call this substance a gas, though formerly we called it a liquid. I have borrowed this remark from Maxwell." The whole description was given by Andrews in the Bakerian Lecture (read June 17, 1869); and was referred to and accentuated by its author in the Royal Society Proceedings abstract of his complete paper.

The curves, Plate v., Fig. 3, are taken, says Prof. Van der Waals (p. 416), from Maxwell (Maxwell's "Theory of Heat" I understand from a reference a few lines higher on the same p. 416). This is to me unintelligible. The curves seem certainly not taken from Maxwell, but are somehow obtained from the original curves of Andrews (after a transformation, which Maxwell also makes, of turning Andrews's curves right for left); and they contain the peculiarity (purposely omitted by Maxwell, for simplicity) of a bend instead of a sharp corner at the bottom of the low temperature curves. In any case Maxwell credits Andrews with the construction of these remarkable curves, which contain, indeed, the germs of the whole *discovery of continuity* made by Andrews and James Thomson. As to the curves themselves, it is utterly unintelligible that anyone with a true perception of their physical meaning should allow the isothermal marked 25°·5 to stand as part of the diagram. The translators ought to have corrected or cancelled this on the ground which led them to object, in the footnote, p. 416, to an ill-judged remark in the text.

Throughout this essay on a subject which, by patient labour and consummate experimental skill, crowned with a rich harvest of results, Andrews made incontestably his own, there is not a single reference to the title or date, or existence even, of the Bakerian Lecture; nor, with the solitary exception of a very questionable reference on p. 421, is there a hint given that Andrews ever gave any attention to the question of continuity; and no uninformed reader would guess from this essay that Andrews had done anything more than supply a quantity of numbers which afterwards turned out to be convenient for the purpose of affording such confirmation as numbers can to the "discoveries" and "laws" of Prof. Van der Waals.

Whatever weight may be given to Van der Waals' investigation, no one who knows the subject as it was known in 1869 can fail to see that neither the idea nor the proof of continuity is in any sense whatever due to him. In their ultimate form they are due to Andrews and James Thomson; though of course it must never be forgotten that the whole subject was opened up by the investigations of Faraday and Cagniard de la Tour; and