would be no particular difficulty in maintaining any thesis if results could be treated in this way.

Mr. Fletcher's work has been partly on the above lines. He obtained a "solution of excreta" by growing plants in water culture, and then used this solution as a medium for plant growth. It proved to be toxic, and the conclusion is drawn that the plant first used excreted some poisonous body. The experiment, however, is not a very good one. It is well known by those who have worked with water cultures that bacterial decompositions are liable to take place in the solution, producing substances injurious to plants; precautions always have to be taken to prevent development of bacteria. It does not appear that any such precautions were taken by Mr. Fletcher, indeed, the conditions under which he worked seem to have been favourable to bacterial development; well water was used, and the "solution of excreta" was allowed to evaporate at ordinary temperature until sufficiently concentrated for the second part of the experiment. There is no evidence that the toxic substance was excreted by the plant; it might equally well have been a bacterial product.

In another set of experiments crops were grown in rows side by side, and three lots of measurements were taken:—(1) the yield in the outside row, bordering on the bare ground; (2) the yield in the middle row; (3) the yield in a row bordering on another crop. The first is the highest, the second shows the effect of the plant on others of the same kind, and the third shows the effect on others of a different kind. The falling off in yield in the second and third cases is regarded by Mr. Fletcher as proof of a toxic excretion; it is generally explained as due to lack of water or food, and no satisfactory evidence is adduced against this view; indeed, Mr. Fletcher states that the reductions in crop are less marked under a more evenly distributed rainfall. We cannot consider that the question of root excretion has been materially advanced in any of these publications. E. J. R.

ACID-RESISTING ALLOYS.

A PAPER was read at a recent meeting of the Faraday Society by Mr. Ad. Jouve describing the remarkable resistive character of ferro-silicon and other silicon alloys. Attention was directed to the fact well known to analysts that no methods of analysis for this substance, based upon the use of acids, with the exception of hydrofluoric acid, are employed for ferro-silicons, because ferro-silicon containing more than 20 per cent. of silicon is insoluble in acids. This protective property of metalloid is being made use of in producing acid-resisting vessels. Ferro-silicons, however, are not the only substances which possess this property; almost any alloy of a metal with this metalloid will behave in the same way to a greater or lesser degree, according to the nature of the metal. Calcium-silicide is, for example, unaffected by acid, whereas calcium itself acts vigorously upon water.

As showing the resistance of these alloys, which are called "Métillures," to acids, the following example is interesting:—Nitric acid, even as a vapour such as is obtained at the exit of a bisulphate retort or when mixed with nitrous acid, does not affect them at all. A striking example of this is given by a pipe which has been submitted for nearly five years to the daily passage of 660 lb. of nitric acid vapour at temperatures varying from 150° to 200° C. without its loss in weight exceeding a few decigrams in a total weight of a score of kilograms. This loss occurred quite at the beginning of the period, and was probably due to a few impurities remaining on the inner surface of the pipe after fusion.

Sulphuric and hydrochloric acid appear to have still less effect, and pipes of ferro-silicon have been used for carrying and condensing gaseous hydrochloric acid. Acetic acid and the mixture produced by treating calcium acetate are also without action. Seeing the extremely high price of platinum, which is the most stable of all industrial metals, it would appear probable that the advent of these new resisting alloys will become of very considerable importance. The chief drawback to their use is in the brittleness and weight of the alloy, the vessels made of it being generally rather thick.

CERTAIN ASPECTS OF THE WORK OF LORD KELVIN.[1]

WHEN a man of the first magnitude works continually at a single group of subjects from an age preceding twenty to an age exceeding eighty, the circumstance is so exceptional and the output so enormous that no ordinary summary or criticism can do it justice.

I shall not aim at any chronological sequence, and, in fact, propose to begin with those later physico-philosophic views which seemed to determine the direction of his thoughts and the attitude of his mind to nascent and contemporary discoveries in recent years.

For this aspect, even if difficult to treat of, is one which a biographer is bound in some fashion or another not to shirk; and, although myself unable to regard it with full sympathy, I am confident that my point of view is neither presumptuous nor disrespectful.

KINETIC THEORY OF SOLIDITY.

Now, I confess that for some years before his death Lord Kelvin's attitude to fundamental physical or philosophical questions was somewhat of a puzzle to me. He seemed to be abandoning ground which he himself had opened up to explorers, and discouraging others from advancing in directions where he himself had pioneered. As a matter of fact, I was uncertain whether his position was even consistent and logically tenable or not; and at the British Association meeting at Leicester, during a discussion on the constitution of the atom in Section A, I had an opportunity of respectfully and deferentially challenging him on this subject. He responded, as always, in the kindest manner, and with great and almost exceptional lucidity indicated what had now become his position. I would not be understood as implying that he carried conviction, or led me to regard that position as a desirable one to occupy; but he showed it to be a consistent and logical one, which he had every right to occupy if he chose, and on which, therefore, it must be left for posterity, or at least for effluxion of time and progress of discovery, to pass anything in the nature of ultimate judgment.

I was much interested in this pronouncement, and before leaving Leicester jotted down a few notes concerning it, with the view of publishing them in his lifetime, in order that he might, if he chose, add to, or subtract from, or modify the statement. Other things prevented rapid publication, however, and accordingly it is too late for one of the objects in view, but still the notes are worth publication as suggesting genuine antithetical or alternative views of the universe. (They have now appeared in NATURE for July 2, 1908.)

It may seem as if the real antithesis was between the postulates of a connecting medium, on the one hand, and of action at a distance across empty space, on the other, and as if Lord Kelvin were in favour of the latter view. I do not, however, think it would be fair to attach to him that responsibility. I think it was more a matter of practical politics, with him, than a philosophical conception. I think he would have liked to see an explanation in terms of a connecting medium, if it could have been managed; but, after spending some years in the attempt, he abandoned it either as too difficult or as hopeless, and constrained himself to be satisfied with unexplained forces between masses of matter acting according to specified laws; the question of the medium or mechanism through which they acted being left out of account as unnecessary from the point of view of practical dynamical calculation and consistent reasoning.

He did speak at times, however, as if immediate action across empty space would be logically satisfactory to him, and quite good enough as an explanation; the only question being, was it the true one? To me I confess that any such philosophic scheme must necessarily be a cold and merely descriptive account of material activity—that it must necessarily fail to go to the heart of the matter or to constitute what may more reasonably be called "explanation." The conception of forces acting according to a specified law of distance is capable of yielding dynamical results truly, but not of explaining them. Ex-

[1] Abridged from the presidential address to the Faraday Society, delivered by Sir Oliver Lodge, F.R.S., on May 26.