depended on the medium. Of course the same view is even more universally held to-day.

All this might, however, have been passed over as an "indiscrétion de jeunesse" if M. Mercadier had not in June last made the extraordinary claim to have proved on such a basis of argument and experiment that the electromagnetic system of units has a theoretical justification which the electrostatic system lacks.

In this recent paper the notation is changed, and k' is used for $1/\mu$. Here again the invariability of this quantity in non-magnetic materials is used as an argument to prove that it does not depend on the nature of the medium.

For the rest M. Mercadier develops certain mixed systems of dimensions, which I need not discuss.

In answer to his complaint that I omitted to notice his memoir in a paper which I wrote on the same subject in 1889, I wish to point out that I did not then enter upon the bibliography of the subject. I regarded myself as dealing with a theory well understood by experts, and as advocating a change in notation chiefly for the benefit of less advanced teachers and students. The considerations advanced were direct deductions from Maxwell's theory. That theory was more generally understood in 1889 than when the discussion in the *Philosophical Magazine* took place in 1882, and since the latter date the practice of retaining K and μ in dimensional formulæ is spreading.

As far, however, as M. Mercadier's papers of 1883 were correct, the ideas they embodied had been explicitly stated in the *Philosophical Magazine* some months before. As far as they went beyond that point, by the attempt to discriminate between the theoretical validity of the electrostatic and electromagnetic systems, the arguments adduced were quite unsound.

ARTHUR W. RÜCKER.

Royal College of Science, South Kensington, February 5.

The Cloudy Condensation of Steam.

MR. AITKEN'S letter (p. 340) shows that he has curiously misunderstood me. I never entertained the smallest "objection to" his "not countenancing the nucleus theory to explain" the action of electricity upon the steam jet. On the contrary I was rejoiced to find that so able and distinguished a physicist appeared to hold the same opinion on this point as myself. In labouring to abbreviate I must have become very obscure. Perhaps my meaning may be made clearer by an amplified and annotated paraphrase of the words in question (see *ante* p. 213).

After trying to show that dense condensation takes place only when there is an actual *discharge* of electricity, which, however, need not necessarily electrify the jet, I go on: "The inference clearly is that in some way or other the action is brought about by the *air* in which electrical discharge has taken place, and not directly by the electricity itself. Since so much has been said in the earlier part of the lecture about the influence of dust in promoting condensation the [erroneous] idea has, no doubt, occurred to many of you that in the present case also the air owes its condensing power to the fact that it has become charged with dust. [The great majority of the many scientifically educated people to whom I have at different times shown the experiment at once made this suggestion.] Minute particles are indeed torn off the electrodes by the discharge and [you may think] form nuclei upon which the steam condenses. This [mistaken] hypothesis seems at first sight to be favoured by the experiments of Liveing and Dewar, and by the well-known fact that burning touchpaper induces condensation; it also has the support of Prof. Barus, who appears inclined to think that such condensation is *in all cases* due to the action of small particles of matter. On the other hand, it is noteworthy that Mr. Aitken, who knows more about the condensing property of dust than any man living, gives no countenance to the nucleus theory as explaining the action of electrical discharge upon the steam jet. The possibility of such an explanation must necessarily have presented itself to the mind of one so familiar with the subject, and since he does not make the slightest allusion to it, I imagine that his experiments have led him to the conclusion that it is untenable. This affords me great satisfaction, inasmuch as my own experiments have led me to the same conclusion—not only as regards the action upon the steam jet of electrical discharge, but also of burning matter." [I did not intend to imply, though the words of the abstract apart from the context unfortunately seem to bear that meaning, that Mr. Aitken thought the action of *burning matter* was not due to nuclei, but that I myself thought it was not.] Then follows an account of experiments tending to show that the air does not derive its power of condensing the steam jet from dust but from dissociated atoms.

The above will, I hope, convince Mr. Aitken that, except perhaps as regards one slipshod sentence, which I regret having overlooked when correcting the proof, he has no cause to feel aggrieved. I am confident that my hearers never for a moment understood me to say that he had abandoned one iota of his conclusions regarding the action of dust, but merely that he did not consider the dust-nucleus theory applicable to the case of the electrified steam jet.

I believe that I am well acquainted with all Mr. Aitken's papers on the subject of condensation, but I do not remember the experiment with the polished ball referred to in his letter. Perhaps it is an unpublished one. The experiments which he mentions in his final paragraph, relating to the condensation caused by certain acids, were made upon water-laden air contained in closed vessels, and not upon the steam jet. The conditions in the two cases are very different, so much so that, for example, hydrochloric acid, which in the steam jet is the most active source of dense condensation that I have met with, was found by Mr. Aitken (he will pardon me for reminding him) to form no foggy condensation at all in a receiver of moist filtered air; while ordinary dusty air, which exerts such a powerful action in the closed vessel, fails to produce any sensible effect when introduced into the open steam jet.

SHELFORD BIDWELL.

Southfields, Wandsworth, February 11.

On the Cardinal Points of the Tusayan Villagers.

IN the second volume of the *Journal of American Ethnology and Archæology* I have pointed out, for the first time, that the four cardinal points among the Tusayan villagers are not the same as those of the astronomers, or that their north is approximately north-west. I also gave, in the same article, tables with the amount of the angular variations, showing that the sacred rooms, or kivas, where the mysteries of their ceremonial worship are performed, are oriented, roughly speaking, in accordance with their conception of the positions of north, west, south and east. It was shown that the amount of angular variation was constant, and later, in a description of the ruins of A-na-to-bi, the same orientation was made known.

In an article published in the December number of the *Journal of American Folk Lore*, it was stated by me that the cardinal points among these aborigines are determined by the solstitial risings and settings of the sun.

The publication of Prof. J. Norman Lockyer's work on "The Dawn of Astronomy," in which the orientation of certain of the sun-temples in the Nile valley and elsewhere in the old world is referred to solstitial points in the horizon, gives a new interest to these observations among the aboriginal housebuilders and their descendants in America.

Since the publication (1892) of my observations on the orientation of Tusayan (Moki) kivas and its relationship to solstitial points of sunrise and sunset, I have examined the scanty data which we have regarding the orientation of temples in Central American ruins, and have unearthed significant facts bearing on this question, as well as that of the kinship of the Pueblo people and those who once inhabited the "cities" of Mexico, including Yucatan. Evidences of relationship between the aboriginal housebuilders of Arizona and New Mexico, and those of Nahuatl and Maya stocks have elsewhere been presented. It seems to me that the above observations made in 1891, quite independently of the discoveries of Lockyer on the orientation of temples in the old world, in the light of his discussion, open a field of research in the archæology of the housebuilders of Central America which is sure to lead to interesting discoveries.

J. WALTER FEWKES.

Boston, Mass., U.S.A.

The Scandinavian Ice-sheet.

MANY geologists affirm that the Scandinavian ice-sheet became confluent with that of Scotland, and reached the East

Anglian coasts. Perhaps some of your readers could inform me whether the following difficulty, which has occurred to me, has been already raised, or has received a satisfactory answer. A submarine channel, some 400 fathoms deep, sweeps round the southern coast of Norway from the Cattegat to about the 62nd parallel of latitude, whence it gradually opens out into the deeper water further north. If the 100 fathom-line of soundings were to become the coast margin of north-western Europe, this channel would form a fjord, considerably broader than the straits of Dover, and for the most part 1800 feet deep. A further general upheaval, amounting in all to some 2500 feet, would convert this fjord into a wide valley, sloping gently towards the north, which was bounded on one side by the Scandinavian mountains (then commonly rising to a height of about 5000 to 9000 feet); on the other by a nearly level plateau (with a yet slighter slope, but in the main northward), elevated generally some 2000 feet above the bed of the valley. In such cases, if any trust can be placed on the evidence afforded by Greenland at the present day, the drainage of Scandinavia would obey the law of gravitation, even when in the form of ice, and would be diverted down the fjord or valley towards the northern Atlantic.

T. G. BONNEY.

The Nomenclature of Radiant Energy.

REFERRING to Prof. Simon Newcomb's letter in your issue of November 30 last (p. 100), suggesting a nomenclature for radiant energy—if no one else has already pointed it out, I would suggest that the word *irradiate* might be used in place of *illuminate*. It would be just as expressive, and would have the advantage of consistency; and its use would leave the word "illuminate" to its proper sphere.

A. N. PEARSON.

Melbourne, January 9.

THE FOUNDATIONS OF DYNAMICS.

IT is rather curious that at the present time, when applied dynamics embraces so wide a range, so much attention should be directed to its foundations. One would have thought that the basis of a department of science which is used and used successfully in the investigation of the motion of vortex rings in a fluid, and the propagation of waves of electromagnetic disturbance, had been fully understood, and that no doubt of the firmness of the logical structure on which so huge a weight is laid, was entertained by those who are most active in turning it to practical account. If, as some appear to believe, our dynamical methods are founded on a vicious circle, how is it that the same men have been so successful in applying them to the elucidation of physical phenomena? Surely the repeated attempt to do this ought only to have led, if not to confusion of contradictory results, to continual failure to obtain any explanation at all.

On the other hand the extended use of dynamics has led scientific men themselves to a more general familiarity with dynamical processes. The study of dynamics is now a recognised part of scientific education, and the exigencies of teaching the subject have rendered necessary a much more complete examination of its fundamental assumptions than was usual before, when a few gifted mathematicians, by the force of their own genius, were led, almost "by a way they knew not," to the glorious results of physical astronomy. Again the recognition, more or less clear, that the old action-at-a-distance theories are really mathematical shortcuts, each gathering up into a single formula the result of the physical actions on molar matter of a medium in which it is immersed, has directed attention to the ether, and raised many questions of extreme interest as to the localisation of energy, and the conditions of its transference from place to place. Though a whole race of subtleties has with the new views sprung into being to mock our attempts to find firm footing, we are forced to the conviction that in this action of a medium lies the best means of scientific progress at the present time. As a consequence we are led to the reconsideration of the theory of energy, and therefore also of the conceptions of force, &c., and discussions as to the foundations of dynamics have been revived and carried on with a keener interest.

No one has worked with more zeal at the task of restating the doctrine of energy on anti-action-at-a-distance principles than Dr. Oliver Lodge, and it happens that recently his views have again been brought to the front by an address on the Fundamental Hypotheses of Dynamics delivered in 1892 by Prof. J. G. MacGregor before the Royal Society of Canada, and an article by the same author in the *Philosophical Magazine* for February 1893. An instructive paper has been presented by Dr. Lodge to the Physical Society, in which he has re-stated and defended his position. The discussion which took place on that paper, and the divergence of opinion then manifested, showed how wide is the interest in this subject, and how far it is still from being completely settled.[1]

The chief points in Dr. Lodge's papers are his insistence upon contact action as the cause of all action between bodies, and his re-statement of the principle of the conservation of energy. Only incidentally and as a preliminary, in his last paper at least, are the laws of motion touched upon. On the other hand, the chief burden of Dr. MacGregor's address is the laws of motion, and an attempt so to formulate them so as to give a logical basis for the science of dynamics in its application to physics. In his *Phil. Mag.* paper, however, he deals with Dr. Lodge's views with respect to energy.

I do not propose to restate the positions of the parties to the present controversy, but to endeavour to say how the question appears to an outsider who has felt keenly the difficulty of teaching the elementary principles of dynamics without introducing confusion by unnecessarily obtruding the fundamental *cruces* of the subject; or, on the other hand, slurring over matters of really vital importance.

In the first place, it seems to me that there is in general no sufficiently clear recognition of the fact that abstract dynamics is really abstract, and depends upon certain ideal conceptions just as much as does geometry, and that its application to practical problems must be made on certain assumptions, axiomatic in the proper sense or not, which must be justified by the results of experience. Abstract dynamics is a purely ideal science, geometric in a somewhat extended sense, caused by the introduction of certain notions not ordinarily employed in purely geometrical processes. So long as we confine ourselves to the ideal as we do in geometry, there are about it only difficulties of the same kind as we have in geometrical conceptions, and these I do not here propose to discuss. It is only when we apply the science to the interpretation of nature that we meet with the difficulties that every one must admit do exist, and which there is no blinking if we want to be straightforward, as to absolute direction, uniform motion, &c.

In this application we take some standard for the measurement of time. In this we are guided by the idea derived from the first law of motion, that any body in relative motion, which there is reason to conclude is not changed by the action of other bodies, may be taken as timekeeper. In practice we have recourse to a joint result of this idea and the equality of action and reaction, and take as our standard the rotation of the earth on its axis. [Of course this standard may not agree with some other and preferable standard means of time reckoning, but this will not affect the argument.]

In abstract dynamics we can and do imagine a system of axes of reference of some kind or other, but quite ideal so far, and agree upon or assume the existence of some mode of measuring intervals of time. We then consider the velocities and accelerations of different particles rela-

[1] A rejoinder to this paper appeared in the September number of the *Philosophical Magazine*.