ment but of fact; no one who examines the actual fragments and sees how precisely the edges of these bones fit one on to the other can refuse to admit that the parieto-temporal angle of Dr. Smith Woodward's restoration is a genuine peculiarity of this skull. If this is admitted it becomes impossible to tilt the upper margin of the parietal upwards and outwards. In other words, this peculiar articulation of the temporal bone affords confirmatory evidence of the proper location of the middle line.

It is a very interesting fact that the curious conformation of the temporal region of the brain, to the reality of which Prof. Keith objects, is quite analogous to that exhibited in the remarkable cranial cast of the Gibraltar skull, of which he is the custodian, and in some of the casts of primitive crania (negro, Australian, and Tasmanian) which he kindly obtained for me.

The greater part of Prof. Keith's letter deals with the lack of symmetry in the original reconstruction, which was due to a slight error in the positions assigned to the occipital and right parietal fragments. The need for this correction was realised before the meeting of the Geological Society last December; and this was taken into consideration when I was writing my preliminary note. G. ELLIOT SMITH.

The University of Manchester.

"Aëroplanes in Gusts."

I SHALL esteem it a favour if you will spare a little space in which to refer to the unsigned review of the first edition of my book, "Aëroplanes in Gusts," printed in NATURE of October 2.

It is not at all my intention to refer to or contest an adverse opinion standing alone, but there is associated with that opinion, in a way that might appear to justify it, a misstatement of fact that I can scarcely be expected to pass without an endeavour to correct.

Your readers are informed that I "measure the effect of a gust of wind by the accelerations of the air particles relative to the aëroplane." That I certainly do not do, and your reviewer has no excuse whatever, in anything I have written, for attributing to me so simple and foolish an error as the words imply. A most casual reading of my book, even in its first pages, shows, decisively, that I quite properly measure the gust—not "the effect of a gust," whatever that may mean—by the acceleration of headway, or acceleration of the velocity relative to the air, which, independently of that due to gravity and even of that due to the propeller, is being *impressed* upon the flying machine by the air.

The confusion made possible by not maintaining or exhibiting, as I have done in my book, the distinction between an actual acceleration and an impressed acceleration, and by not excluding the gravitational acceleration, scarcely needs enlarging upon or explaining in the columns of NATURE.

S. L. WALKDEN.

Muswell Hill, N., October 4.

I HAVE just received the second edition of "Aëroplanes in Gusts," and in reply to the author's criticism of my review, I cannot do better than quote the passage on p. 2 containing the definition :—

"Using therefore the term 'headway' in place of the cumbersome 'velocity relative to the air,' it will be taken for granted that the reader knows that :—

"(1) The instantaneous *strength* of gust at any point of the air as regards a given flying-machine flying at that point is measured by the acceleration of headway which any singularity of the air at that point is impressing upon the flying-machine, and the *direction* of the gust is opposite to the direction of the impressed acceleration.

"For example :—If the air is accelerating downwards at 40 ft. p.s. p.s., it is impressing upon the flying-machine an upward acceleration of headway of 40 ft. p.s. p.s., and this is the measure of the downward gust. In other words, the gust is of strength 40 ft. p.s. p.s., downwards. Simple velocity as distinguished from rate of change of velocity is, it will be noticed, completely ignored."

(The author then goes on to point out that accelerations may be represented by straight lines. Agreed.)

On p. 4 he says :—

"The general method of finding the impressed accelerations acting at a given instant upon a flying-machine consists in first answering the question :—

"If at any given instant the flying-machine could be suddenly transformed to a small smooth concentrated mass, how would it accelerate relative to the air?

"The acceleration answering the above question is the 'resultant relative gravity' of the following discussion, and when common gravity is subtracted *in vector sense* the result is the acceleration tendency or impressed acceleration due to the gusts. When from this result the impressed acceleration due to the absolute acceleration of the air at the place of the flying-machine is also subtracted *in vector sense* there will usually be found an impressed acceleration remaining. This is due to the air having what is called "velocity structure" at the point, and to the flying-machine in crossing that structure creating for itself a rate of change of headway."

If Mr. Walkden considers that he has received any injustice through the use of the term "effect of a gust" in substitution for his reference simply to "a gust," or the measure thereof, the reference to "effect" should certainly be withdrawn. But as regards his views on "impressed accelerations," the above quotations will probably appeal to readers of NATURE far more effectively than any criticism, however adverse. Yet several journals have reviewed the book favourably, and it has run into a second edition.

THE REVIEWER.

Mass as a Measure of Inertia.

CAN any of your readers enlighten me as to the authorship of the definition, "The mass of a body is the dynamical measure of its inertia"? I am under the impression that it is due to Clerk Maxwell, but have not been able to find where it occurs. I should be grateful for information as to where to look for it.

W. C. BAKER.

School of Mining, Queen's University,
Kingston, Ont., October 13.

ENGINEERING RESEARCH AND ITS COORDINATION.

THE questions of the coordination and encouragement of research in engineering have been brought forward in various ways recently. In April Sir Frederick Donaldson, chief superintendent of Woolwich Arsenal and president of the Institution of Mechanical Engineers, referred to them in his presidential address. At the recent summer meeting of the same institution held in Cambridge, Mr. G. H. Roberts, of Woolwich, read an interesting paper entitled "A Few Notes on Engineering Research and its Coordination," while the matter was also touched upon

by the president of the Institution of Water Engineers in his presidential address.

"I have long thought," he says, "and indeed it must be obvious to all who reflect upon the subject, that a great mass of experimental work is lost to the community because the results in many cases are not properly recorded, and even when complete records are kept, the results remain with the investigator," and after referring to the advantages of combining for research the experience and opportunities of a number of people, he continues:—"It occurs to me therefore to ask whether it is possible to make this institution"—the Institution of Water Engineers—"a clearing house for the handling of some at least of the many problems to which we devote time and thought."

Again, Sir Frederick Donaldson writes:—

Research in the hands of firms and engineering undertakings has already been advocated, and no one would wish to see such efforts in any way hampered, but if it were possible to coordinate the work more than is done at present, and also to place the results at the disposal of the profession more readily than is now the case, great advantage may be expected to result. Is it not worth considering whether inquiries should not be made to see if an Engineering Research Committee, the bounds of which should be much wider than membership of this institution alone [the Institution of Mechanical Engineers] could be got together with a view to organising, coordinating, and assisting research, more especially for engineering purposes?

Mr. Roberts's paper commences with the statement that

Although engineering as an applied science has now reached a high state of development, and has in many of its branches become highly specialised, it is somewhat remarkable that no definite and generally recognised system has been formulated for making known for the benefit of the profession as a whole the results of the numerous private researches and experiments which are continually being carried on.

The paper describes a few of the researches of general interest carried on at Woolwich Arsenal

with the hope that it may induce others to come forward and add to the stock of general knowledge and it may thus form the nucleus of a clearing house of engineering information.

Sir Frederick Donaldson goes farther than the formation of such a clearing-house; he suggests, as we have seen, in addition the organisation, coordination, and assistance of research: we will return to this point later.

To many readers of NATURE interested mainly in branches of science other than engineering, the need for a clearing-house may appear strange. A man after he has carried through a research in chemistry, physics, or one of the biological sciences, is not usually averse to giving his paper to the world. He communicates it to one of the scientific societies. In due time it appears in the journal, and is abstracted into one or more of the numerous and valuable periodicals which undertake such work for the great benefit of other investigators. But it is otherwise with much engineering or other technical research. The work is carried out for a special purpose: to determine the proper material to use in some structure; to see if some alloy which it would be convenient to employ for a certain machine will retain its properties under the conditions of temperature and stress to which it will be subject; to settle the form of bolt or screw-thread which for a given diameter offers the greatest resistance to shocks or impact and the like. Mr. Roberts's paper gives us examples. He records the results of tests on many specimens of timber used in the arsenal; of an investigation into the standard shapes and dimensions of tensile specimens; of numerous experiments on aluminium alloys. He describes a special instrument for indicating the yield-point of tensile specimens, and discusses the effect of the time-factor upon results of tensile testing and the unification of methods of reporting. Any of these investigations might have been carried out in some other works, and the result, when it had been utilised for the job in hand, forgotten and left to pass into oblivion.

Investigations of the kind, though of real value, may scarcely be of sufficient importance to be worked up as a paper for communication to one of the technical societies—always a somewhat elaborate business—and, indeed, results and methods sufficient for the purpose in view, and deserving of record, would be felt not unfrequently to be unsuitable for an evening's formal discussion. Again, there is the desire, sometimes the necessity, to keep the results private, and the disinclination to spend time in working them up for publication. Possibly some of these difficulties could be met by a committee guiding a staff of men whose business it would be to keep in intimate touch with works in which investigations of general interest were going on. The knowledge of these men would enable them to suggest to the committee what researches it was important should be secured for the public: they might assist the workers in preparing these for publication, or, where complete publication was not necessary or desired, in abstracting such parts as could usefully be placed on record. The committee, or the committee's records, would in time become a storehouse of information to be searched by a would-be investigator before he commenced his own experiments. Useful knowledge would be disseminated and overlapping prevented. The difficulties of the attempt are fairly obvious. Success, if it could be achieved on a sufficient scale, would be a real advantage to engineers.

But this is distinct from Sir Frederick Donaldson's suggestion of organising and advising as to research. To attempt this for the whole field of engineering science is a heavy task, and it may be questioned whether such work is not better done by a number of special committees, each working in a more limited field. Possibly a main committee like the main committee of the engineering standards committee is wanted to start the subordinate bodies and coordinate their work. Such special committees do exist at present. Prof. Hopkinson mentioned in the discussion on Mr.

Roberts's paper the gaseous explosions committee of the British Association. The alloys research committee of the Institute of Mechanical Engineers; the newly established research committee of the electrical engineers; the reinforced concrete committee of the civil engineers; or the Government Advisory Committee for Aëronautics, are all instances. For the success of such committees three things are needed—a man or men to carry out the research, a laboratory or works with proper equipment for the experiments, and funds to defray the expenses.

Prof. Hopkinson did well in the discussion at Cambridge to direct attention to the individuality of research. Much—everything—depends on the man, and he must have freedom. The committee may specify the objects of the inquiry, and indicate in general terms the methods to be followed, but no real result will ensue unless the investigator has ideas of his own, and, after the suggestions laid before the committee are approved, is free to carry them out.

The gaseous explosions committee owes its success to Dugald Clerk and Hopkinson; the alloys research committee to Roberts-Austin, Carpenter, and Rosenhain; while the work of the Advisory Committee for Aëronautics would lose nearly all its value were it not for the energy and devotion of the staff of the National Physical Laboratory.

Engineering research—technical research, indeed, of all kinds—differs, however, from much scientific research in that it can be organised. The problems proposed are usually fairly definite. What are the properties of a certain series of alloys? How are they modified by temperature, forging, annealing, and the like? Do the results of impact tests depend on the form and dimension of the specimen? What is the exact series of changes of temperature and pressure in the cylinder of a gas-engine? How are the forces and couples on an aëroplane related to its aspect to the wind? The problems may be difficult, the answers may elude inquiry; but, given the man, the laboratory, and the funds, a committee meeting at intervals to discuss the results of the experiments may reasonably hope in time to meet with success.

Sir Frederick Donaldson and his colleagues have raised questions of great interest and importance, well worth the careful consideration of those engaged in bringing the results of scientific inquiry to bear on the problems of manufacture and construction.

HIGHER EDUCATION AND THE STATE.

LORD HALDANE had something important to say upon the subject of provision for higher education in the course of his speech at the opening of the new buildings of the department of applied science of the University of Sheffield on Saturday last. An account of his address will be found elsewhere in this issue, but we are more particularly interested in a summary of the main points, communicated by him to representatives of the Press. Lord Haldane explained that he desired it to be realised fully that he was announcing the considered decisions of the Cabinet upon the subject of university education, and was indicating the policy to be followed. The substance of his remarks was expressed as follows:—

The main features of the Board of Education's scheme are a recognition of the great strides being made in university education by the United States and Germany, and an intention to maintain closely the connection between pure science and applied science and to check any tendency on the part of any of the younger universities to cultivate the latter at the expense of the former. Theory and practice must keep together. Men of business must remember that much of what is distinctive in the inventive and industrial genius of this country comes from theoretical sources.

Unless we wake up fully about this matter of education, and particularly higher education, I am a little nervous as to what the state of things with regard to our industrial supremacy will be fifteen or twenty years hence.

The nation will have to make up its mind to give considerably more out of central funds. The plans for these advances are now fashioned. I hate any idea of increasing expenditure, whether out of local or national sources, if it can be avoided. But this cannot be avoided. It is salvage money, and unless you spend it you will go back as a nation, and your revenues by which you keep up your fleets and your armies will begin to shrink, because you will not be holding your own in that great industrial position from which your power and your wealth have come.

We have now, therefore, a definite statement of the position which university work is to take in the national scheme of education adumbrated by various Ministers since the beginning of this year. There is a clear acknowledgment of the fact that in the matter of State provision for higher education we have not kept pace with other progressive nations; that scientific work which has no industrial interest is as important as that of which the direct application can be seen; that national advancement can be secured best by increase of scientific knowledge; and that all these things involve contributions from the national exchequer greatly in excess of those hitherto given.

Readers of NATURE scarcely need reminding that the policy thus broadly outlined has been urged consistently and persistently in these columns. Ten years ago, Sir Norman Lockyer, in his presidential address to the British Association at Southport, gave the evidence from which each one of the points mentioned by Lord Haldane could be justified; and since then, year by year, particulars have been given in the reports of the British Science Guild of the progress being made in the endowment of higher education and research abroad, in comparison with the position in this country. It was shown, for instance, in the last report of the Guild, that the total receipts of universities in the United States in the year 1910–1911 amounted to nearly nineteen million pounds, and the benefactions to four and a half millions. In the same year, the total receipts of those universities and university colleges in Great Britain