him, while away on my holiday, and in a private letter, Dr. Pearl's paper. He has now seen fit, although I twice asked him to wait for a full answer until my return to Cambridge, to challenge me to show in the pages of NATURE how my advice was applicable to that paper. I must leave your readers to judge how far I have succeeded in so doing.

The task has been far from an agreeable one. I should never have thought of singling Dr. Pearl's paper out for public criticism in this manner had I not been challenged to do so. I can only say that if he feels himself aggrieved at the result, he can be in no doubt whom he has to thank.

J. J. LISTER.

St. John's College, Cambridge, October 1.

Radium and Geology.

IN the Proceedings of the Royal Society for May and August there appeared important papers by the Hon. R. J. Strutt upon radium in the earth's crust and the earth's internal heat. Taking known values of the heat production of radium, per gram per second, assuming Lord Kelvin's estimate of the conductivity of rocks *in situ* and Prestwich's estimate of the temperature gradient at the surface, Mr. Strutt shows that, if the gradient expresses the outflow of heat due to radium in the earth, the radium must be confined to a comparatively thin crust, because his laboratory experiments prove that the smallest radium content existing in the rocks examined would give a much higher gradient than the one observed if the radium were distributed throughout the entire earth.

In the present connection the crust must be defined by the depth beyond which no heat is caused by radium. In these circumstances, if we adopt a certain temperature gradient at the surface, there is only one value of the radium content which will correspond to any assumed thickness of the crust, and there will also be one corresponding temperature at the bottom of the crust and throughout the interior. I have calculated these at intervals of five miles, both for Prestwich's estimate of the gradient, viz. 1° F. for 42·2 feet descent, and also for the more commonly accepted one of 1° F. for 60 feet.

Gradient 1° F. *in* 42·2 *Feet.*

Thickness of the crust in miles	Radium content per cubic centimetre	Temperature at bottom of crust, Cent.	Temperature at bottom of crust, Fah.
		°	°
15	$15{\cdot}39 \times 10^{-12}$	519	966
20	$11{\cdot}55 \times 10^{-12}$	692	1277
25	$9{\cdot}13 \times 10^{-12}$	865	1589
30	$7{\cdot}70 \times 10^{-12}$	1038	1900
35	$6{\cdot}60 \times 10^{-12}$	1211	2211
40	$5{\cdot}77 \times 10^{-12}$	1384	2464
45	$5{\cdot}13 \times 10^{-12}$	1557	2834

Gradient 1° F. *in* 60 *Feet.*

Thickness of the crust in miles	Radium content per cubic centimetre	Temperature at bottom of crust, Cent.	Temperature at bottom of crust, Fah.
		°	°
15	$10{\cdot}27 \times 10^{-12}$	363	676
20	$8{\cdot}08 \times 10^{-12}$	484	894
25	$6{\cdot}39 \times 10^{-12}$	606	1112
30	$5{\cdot}09 \times 10^{-12}$	727	1330
35	$4{\cdot}62 \times 10^{-12}$	848	1547
40	$3{\cdot}84 \times 10^{-12}$	969	1725
45	$3{\cdot}59 \times 10^{-12}$	1090	1984

From the above tables it appears that the radium contents corresponding to such values as are usually assigned to the thickness of the earth's crust by geologists and seismologists are well within the amounts contained in the rocks examined by Mr. Strutt, and that consequently the surface gradient can be fairly accounted for by the theory. But we have also some indication of internal temperature from volcanic products. Prof. Bartoli found the temperature of lava issuing from Etna to be 1060° C. If this came up from beneath the crust it would correspond to a thickness of from thirty to forty miles, according to the rate of increase which we attribute to the gradient. So far all seems favourable to the theory.

Since any reasonable assumption for the mean radium content of the crust would supply sufficient heat to maintain the observed gradient, it follows that no heat can pass up from the interior, because, if it did, the gradient would be higher than it is. The conclusion would be that the earth is not a cooling body, and it is consequently reduced to a state of thermal stability.

Thus a fundamental belief of geologists is shattered at a blow. Sir A. Geikie writes in his chapter on dynamical geology that "it is useful to carry in mind the conception of a globe still intensely hot within, radiating heat into space, and consequently contracting in bulk." . . ." Wide geographical areas are upraised or depressed." These changes of level are constantly going on, such as have been described by Prof. Hull and Dr. Spencer, and the recency of these movements shows that, if they are due to a cooling globe, that process is still in progress, and the primeval heat not yet exhausted. Although there may be differences of view as to the exact mode of its operation, yet it is not too much to assert that there is a consensus of opinion among geologists that the movements of the crust are chiefly attributable to the ultimate cause so concisely expressed by Sir A. Geikie.

It seems clear that one or other of these views concerning the internal heat of the earth must yield. They cannot both be correct; and if the radium theory is to hold the field, how are the movements of the earth's crust to be accounted for?

O. FISHER.

Graveley, Huntingdon, September 28.

IF the internal heat of the earth is mainly due to the radium present therein, must we not assume that the same is the case with the moon? If such were the case, then the internal heat of the latter would be far greater than we have hitherto supposed, and it would be difficult to explain the lack of volcanic activity there.

The age of our satellite is not sufficient for us to assume that all the radium is dead or that none is being produced.

B. J. PALMER.

Technical Schools, Southend, October 4.

Vectors, &c., at the British Association.

IN the report (August 30) of the discussion on the use and notation of vector analysis at the British Association it is stated that I "deplored the substitution of vectors for quaternions." The statement is misleading, for was it not Hamilton more than any other single man who taught us how to use vectors in product and quotient combinations? What I did and do deplore is the substitution of non-quaternionic vector algebras in all their variety of notation for the Hamiltonian or quaternionic vector algebra —a very different thing.

I should like to add that (notation excepted) I was thoroughly in sympathy with all that Prof. Henrici said in opening the discussion. He showed admirably the conciseness of vector methods in attacking both geometrical and physical problems, and so far as he went in the limited time at his disposal there was absolutely nothing to choose between his mode of presentation and that which Hamilton himself might have adopted in the same situation. In his reply at the end of the discussion he pointed out that the quaternion, *as a quantity*, could be got quite easily from his system by taking the difference of his vector and scalar products. That, of course, is self-evident, but it does not seem to me to touch the real issue. It leaves his system still non-associative in vector products, and in higher applications, especially with the differential operator ∇, this introduces difficulties which