transmission of the scattered radiation through the radiating substance and through the absorbers.

Prof. Compton apparently did not read between the lines of a communication to the *Philosophical Magazine* (Barkla and Mrs. Sale, April 1923); but as the results do not suit his formula he makes a suggestion of how such results might have been obtained by very incompetent experimenters. May I now suggest to Prof. Compton that, in addition to taking other very obvious precautions, he might also use soft radiations, very thin radiators, and very thin absorbers. I do not think he will then have much difficulty in obtaining scattered radiation very like the primary and very different from what would be given by his formula.

Regarding the transformations of radiations of shorter wave-length, I will only say that it is much more difficult to obtain anything like equality of penetrating power between primary and scattered as usually detected; but an explanation of this can be given,—not the ultimate explanation, but again in terms of the J transformations.

Further, let us examine the theory of the recoiling electrons. Giving the Compton formula the best chance of success, consider what would happen to the electrons in hydrogen which require little energy for their extraction. These electrons scatter as much as a similar number in other substances (Barkla and Crowther). A simple calculation shows that when the K radiation of tin is employed, the recoil electron should produce an ionisation of the order of 1/100th part of that produced in air by the same X-radiations through the ordinary long-range electrons. Now Shearer in this laboratory observed in hydrogen an ionisation as low as 0·0016 of the ionisation in air; and remarked on the strong probability of this being an over-estimate. This would be of the right order of magnitude for the effect of long-range electrons alone. Where then is the effect of ionisation by Compton's scattering electrons? It apparently does not exist.

The evidence Compton used and obtained from the study of γ-rays is necessarily much less trustworthy; the experimenters have probably never—indeed cannot have—realised the many possibilities of error. Any transformation to a softer type—or at any rate something equivalent to that—would entirely vitiate the results obtained both in absorption and scattering experiments. Without wishing to detract from the merit of the work, one may justifiably point out the difficulties of exact measurement in this region. One is led to ask: Are experiments on the diminution of scattering really trustworthy? Accurate they cannot be; they may be entirely misleading. Thus in cases we have investigated, Compton's formula holds neither for the apparent change of wave-length, nor for the energy of the recoil electrons. But we can quite easily get many of the effects of the kind Compton considers.

It is possible that the J transformation which we have observed will be explained by a theory bearing some resemblance to that of Compton for so-called scattering. This would be supported by the evidence of C. T. R. Wilson's "fish-tracks." It seems unfortunate that Prof. Compton should have applied the term scattering to a hypothetical process which is so essentially different from the scattering of X-rays as ordinarily known. The important conclusion is this—the results of experiments on scattering and the Thomson theory explaining these are absolutely untouched.

Many of the experiments upon which these conclusions have been based were obtained in collaboration with Mr. Khastgir and Mr. Stevens, in addition to those already mentioned. C. G. BARKLA.

University of Edinburgh.
November 10, 1923.

Scientific Names of Greek Derivation.

I AM glad that Sir Clifford Allbutt, in NATURE for October 20, p. 590, supports the spelling "deinosaur," although Owen wrote Dinosauria. Only a week ago I heard a university student pronounce the word as "dinnosaur." Wherever pronunciation can be helped by correcting current forms the correction is obviously of service. From this point of view we may pardon, even if we regret, Miocene and Pliocene. No one, however, has attempted to write "Plistocene." We have for some centuries converted the Latin forms *ae* and *oe* (for the Greek *ai* and *oi*) into the forms *æ* and *œ* in manuscript and in print; but this has no classical authority and can be abandoned with much advantage, as has been done in modern Latin texts. The Greek diphthong or semi-diphthong *ei* could not well be shortened into one letter in our script, and this fact provides an inconsistency for those who join *a* and *o* to *e* in transliterations from Greek or Latin. Where the word has become anglicised in form, as cœnosarc, or where, like cœnenchyma, it is not a generic or specific name, the diphthong no doubt will remain compounded; but we may, I think with wisdom, write Coeloptychium and Taenia. Moeritherium is a case that needs attention. The British Museum, which has an honourable vested interest in the mortal remains of this fascinating creature, writes the *o* and the *e* separately. The Americans, and now the Japanese, adopt the compounded form.

Dr. L. C. Purser, to whom I confide all my classical troubles, tells me that Herodotus (Book II., 148) gives the lake in the Fayûm as ἡ Λίμνη ἡ Μοίριος, named from a king who would appear in Latin as Moeris. C. Stephanus ("Dictionarium historicum," A.D. 1633) prints "Mœridis stagnum"; but here again the separation of the *o* and *e* would seem advisable. I confess that I always write Cainozoic in preference to Caenozoic or Cænozoic, though the *æ* conforms best with our general usage. This term, however, never had a Latin form, and may now be regarded as an English word.

As I remarked in my note in NATURE for July 7 (p. 10), it is now difficult to be logical. The Encyclopædia Britannica gives us an article on Deinotherium, but makes us look under *di* for deinosaurs. Following Sir Clifford Allbutt, let us help pronouncers—and printers—where we can.

GRENVILLE A. J. COLE.

Carrickmines, Co. Dublin, October 28.

Is the Pentose of the Nucleotides formed under the Action of Insulin?

IN a letter to NATURE for June 16, p. 810, Messrs. Winter and Smith directed attention to their observation that the blood and certain other tissues of the rabbit contain, after injection of insulin, a substance which reacts as a carbohydrate towards the α-naphthol test, but has no reducing action on copper salts even after acid hydrolysis. Commenting on this they say: "It seems possible that the carbohydrate content of the animal body may be not appreciably diminished after large doses of insulin. The above facts would suggest that the sugar stored in the body as glycogen is converted into this peculiar form."

If I understand the suggestion correctly, it is that this unidentified carbohydrate substance is formed from glucose under the influence of insulin. If this is so, it should be present in normal blood and other tissues, but absent from those of diabetics.

Jackson has recently shown (*J. Biol. Chem.*, 1923, lvii. 121) that adenine nucleotide occurs in normal human blood. I have myself recorded its occurrence, together with other nucleotides, in the pancreas of