Saturn; as it would appear that without making some such supposition, no definite limit can be fixed.

Applying this supposition to the sun, with reference to meteoric swarms, we have 2.44 times the sun's radius, taken at 433,000 miles, or 1,056,520 miles as the distance at which the sun would prevent the meteors coalescing to form a planet. In Note 3 Prof. Darwin states this at one-tenth of the earth's distance from the sun, probably by inadvertence. G. R.

The Ordnance Survey and Geological Faults.

IN view of the re-survey of the United Kingdom, it seems to me that if the officers of the Survey were directed to take special notice of the levels of the former survey on both sides of great geological faults, and to compare these levels now so as to ascertain if any appreciable relative change had taken place during the forty or fifty years since the first survey, valuable information as to the motion of these faults, if any, might be obtained.

This idea is mainly suggested to me by the fact that in this neighbourhood a great fault intersects the Old Red Sandstone close to its contact with the Highland schists, it has been traced from Stonehaven on the east coast to Loch Lomond on the west, and seems to give remarkable evidence of being, at least to a certain extent, in motion. The village of Comrie, famous for its "earthquakes," is situated on this fault, and the "earthquakes" are as lively as ever. In the valley of Strathmore farmhouses placed in the proximity of this great dislocation are, or were, celebrated for being "haunted," on account of the noises and tremors by which the inhabitants are from time to time alarmed.

Most, if not all, British "earthquakes" have been, I think, wisely attributed to similar causes.

Of course fifty years is a very minute part of the history of one of these old faults, but if the data of the Ordnance Survey be so accurate as is usually supposed, some trace of shifting might possibly be discovered if the necessary observations were made.

Newport, Fife, March 18.

JAS. DURHAM.

The Discovery of the Potential.

MR. E. J. ROUTH has lately published a most valuable "Treatise on Analytical Statics." I quote from the second volume, p. 17, the following note:—

"The earliest use of the function now called the potential, is due to Legendre in 1784, who refers to it when discussing the attraction of a solid of revolution. Legendre, however, expressly ascribes the introduction of the function to Laplace, and quotes from him the theorem connecting the components of attraction with the differential coefficients of the function. The name, Potential, was first used by Green," etc.

From this note it appears that the discovery of the potential must be attributed to Laplace. This is a wrong opinion, and some fifteen years ago Baltzer proved that the introduction of the function is due to Lagrange ("Zur Geschichte des Potentials," in *Journal für die reine und angewandte Mathematik*, vol. lxxxvi. p. 213, 1878). Some historical documents in favour of Lagrange's priority have been found, by the writer of these lines, in Todhunter's "History of the Mathematical Theories of Attraction and the Figure of the Earth," and collected in a note at the end of vol. i. of his work, "Il Problema Meccanico della Figura della Terra" (Torino, 1880), where a full account of the early history of the potential is given, with numerous bibliographical indications. OTTAVIO ZANOTTI BIANCO.

Private Docent in the University of Turin,
March 21.

THE historical note on p. 17 of my "Statics" is chiefly founded on the statements in Todhunter's "History," and in Thomson and Tait's "Natural Philosophy." The references to these two writers are given in the note. Both Dr. Todhunter and Lord Kelvin ascribe the introduction of the function for gravitation to Laplace, and assert that the name of "Potential" was first given to it by Green. My own reading, though not so extensive as theirs, had not led me to form any different opinion. In Nichol's "Cyclopædia of the Physical Sciences" the first introduction is given as due chiefly to Legendre, Lagrange, Laplace, and Poisson. In Chambers's "Cyclopædia" Laplace's name alone is mentioned. Baltzer, as cited by Mr. Bianco, mentions the use of the function by Lagrange in the *Mém. de Berlin*, 1777. This is earlier than the memoir of Legendre, but as Legendre assigns

the introduction of the function to Laplace, it is difficult to compare the dates. I am at present unable to refer either to the memoir of Lagrange or to the treatise of Mr. Bianco.

E. J. ROUTH.

Van't Hoff's "Stereochemistry."

THE review of the above by "F. R. J." in NATURE, p. 436, raises some important points in connection with this peculiarly fascinating branch of chemical science. In referring to the recent ingenious and attractive theory of P. A. Guye, that the numerical value of optical activity is dependent upon the relative masses of the four groups attached to the asymmetric carbon atom, and which carries with it the corollary that if two of these four groups are of equal mass the rotatory power will cease, your reviewer states that Guye "was unable to verify this view in all strictness." I think, however, that he hardly emphasises sufficiently that this important corollary has in every case, when put to the test of direct experiment, broken down. As far as I am aware, there is not a single instance of an asymmetric carbon atom attached to four groups *qualitatively distinct*, being found optically inactive in consequence of two of those groups being *quantitatively equal in mass*. Indeed some such substances are not merely active but powerfully so. The reviewer considers that this inadequacy of Guye's theory is palliated by the alleged fact that the amount of rotatory power of the esters of an active acid is determined by the weight of the alkyl-group. This point, which is one of the cardinal pillars of Guye's theory, I have recently put to the test of actual experiment, by measuring the rotatory power of a number of the esters of active glyceric acid, which have been prepared by Mr. J. MacGregor and myself. In this investigation we found the most extraordinary verification of Guye's theory, as far as the optical properties of the normal series of methyl, ethyl, and propyl glycerates were concerned; with the appearance of isomerism, however, this regularity ceases, thus the isopropyl glycerate has a markedly lower rotation than the normal one, whilst the normal and secondary butyl compounds have a lower rotation than the isobutyl ester. Nor are these differences consistently explicable by taking into consideration the interatomic distances, as measured by atomic volume, for the molecular volume of the normal propyl glycerate with its greater rotation is less than that of the isopropyl compound with its smaller rotation, whilst the molecular volumes of the isobutyl and secondary butyl glycerates are almost exactly equal, although the rotation of the former is much greater than that of the latter.

The reviewer, in referring to the rotation exhibited by the salts of active acids, states that in the case of tartaric acid all the salts "display in solution the same rotatory power, irrespective of the atomic weight of the metal," and is apparently satisfied that "the clue to this anomaly is furnished by the electrolytic theory of Arrhenius," according to which "it is the ion $\text{CO}_2(\text{CHOH})_2$ which is alone responsible for the rotation." The reviewer has in this endorsed the method of special pleading adopted by the advocates of this theory, in which the metallic tartrates have been summoned as witnesses, whilst only the testimony of those favourable to the theory has been admitted. Thus one of the commonest of the metallic salts of tartaric acid—tartar emetic—has a rotation which differs entirely from that of the other tartrates, and thus conclusively negatives the dogma that the rotation of the solutions of metallic salts is independent of the particular metal which has replaced the hydrogen of the acid. Fresh light has been thrown on this point in the course of an investigation, which I have recently carried out with Mr. Appleyard on the rotatory power of the metallic salts of active glyceric acid, and which has shown that the specific rotatory power of the glyceric acid has one value when deduced from the rotations of its alkaline salts (lithium, ammonium, sodium, and potassium), another value when deduced from the salts of the alkaline earths (calcium, strontium, and barium), and a third from the salts of the magnesium group of metals (magnesium, zinc, and cadmium). Now it so happens that almost the only salts of tartaric acid which have had their rotation determined are those of the alkaline metals, which also in the case of glyceric acid yield practically the same rotation. Hence if only the rotations of the alkaline glycerates had been determined, the same erroneous conclusion would have been arrived at concerning the rotation of glyceric acid. Whatever may be the ultimate interpretation put upon these new results, and I prefer for the present to ab-

stain from any generalisations, it is obvious that the notion of the rotatory power of saline solutions being independent of the particular metal present in the salt is altogether untenable.

PERCY F. FRANKLAND,

University College, Dundee, March 11.

THE notice referred to by Prof. Percy F. Frankland was written, and the proof returned to the printer, before the end of last year. Since then two researches have been published—by Purdie and Walker, and by Frankland and Appleyard—in which facts are adduced, apparently irreconcilable with Guye's theory. Had these facts been at my disposal I should doubtless have expressed myself more guardedly.

Prof. Frankland says: "As far as I am aware, there is not a single instance of an asymmetric carbon atom attached to four groups *qualitatively distinct*, being found optically inactive in consequence of two of those groups being *quantitatively equal in mass*;" and he complains that I have hardly emphasised this sufficiently. My reason was, that I was not altogether convinced of the fact, as may be seen from the following passage, which I transcribe, and which originally formed a footnote to the notice in question:—

"The present reviewer ventures to suggest that cases such as are sought by Guye are to be found in those compounds in which two of the four different groups attached to an asymmetric carbon atom are themselves asymmetric carbon atoms of equal and opposite enantiomorphism. Such compounds would exist in two distinct forms; but as the two opposite enantiomorphic groups would be of equal mass and would be situated at equal distances from the central asymmetric carbon atom to which they are attached (inasmuch as the two opposite enantiomorphic modifications of a compound always have the same molecular volume), the conditions necessary for optical inactivity according to Guye's theory would be fulfilled, and neither of the two forms ought to cause rotation of the polarised ray. Such a case has already been observed in the two inactive, non-racemic trihydroxyglutaric acids described by Emil Fischer (*Ber. der deutsch. chem. Ges.* 24, p. 4214), although it does not appear to have been hitherto interpreted from this point of view."

I afterwards suppressed this footnote, partly because it seemed to me out of place in such a notice, and partly because the optical activity of the two trihydroxyglutaric acids could be accounted for in another way; namely, by the fact that, as pointed out by E. Fischer, the mirror images of their molecules are congruent with the molecules themselves. But the passage will show why I was indisposed to enter a proved negative against Guye's theory.

As regards the charge of "endorsing special pleading" in the interests of the electrolytic theory of Arrhenius by suppressing the fact that tartar emetic has, in solution, a different rotation from the other metallic tartrates, I may say at once that I was ignorant of this fact. I am not a specialist on the subject of the optical properties of organic compounds, and I merely summarised, doubtless uncritically, the account of Oudemans' law given in van't Hoff's book. Indeed, the brief notice, as its wording everywhere indicated, was a summary rather than a criticism.

I take this opportunity of rectifying an omission. At the time of writing the notice I was not aware that Prof. Crum Brown had, independently of M. Guye, put forward, in the Proceedings of the Royal Society of Edinburgh, views on the influence of the various substituting radicles in modifying the optical rotation of organic compounds.

F. R. JAPP.

University of Aberdeen, March 18.

Standard Barometry.

THE question of absolute accuracy in barometer readings is one of great importance to meteorologists; but there has been so much uncertainty shown by the accumulated facts relating to the subject, that I think that no one who has carefully studied the matter has felt fully satisfied that strictly comparable international standards had been obtained. An uncertainty of at least 0.1 mm. was indicated by the various international comparisons of normal barometers which have been carefully made and discussed during the past ten years. I think that at last a definite conclusion has been reached, and that the very recent results published in paper No. 4, Band xvi. of the *Reperatorium für Meteorologie* will be accepted as proving that at St. Petersburg at least normal readings are obtained.

About twenty-five years ago Director Wild, of the Central Physical Observatory at St. Petersburg, established the first normal barometer of the *modern* form; and as much as twenty years ago he claimed to have obtained practically normal readings. Moreover, he urged that the transfer of these normal readings from place to place by means of portable barometers was impossible within the desired limits of accuracy, and that each country ought to have its own thoroughly investigated normal barometer. This last has been proved by the results obtained by various investigators; and now Prof. Wild offers the proof of the accuracy of his normal barometer in the paper just referred to, which bears the title "Die normal-barometer des Physikalischen Central-Observatoriums zu St. Petersburg."

This paper was presented to the Academy of Sciences on November 4, 1892, and in it Wild gives the results of the inter-comparison of three local normal barometers.

Normal barometer No. I. was mounted at St. Petersburg in 1870, and was fully described in Band iii. of the *Repert. f. Meteor.*

A second normal barometer was mounted at Pawlowsk (about twenty miles from St. Petersburg) in 1887, and a third normal was mounted at St. Petersburg in 1891, and is known as normal No. II.

In 1887 and 1888 Wild found that the St. Petersburg normal I. and the Pawlowsk normal did not differ by more than 0.01 mm.

In 1892 the St. Petersburg normals I. and II. were found to agree within the limit of error of observation (less than 0.01 mm.).

In 1892 the St. Petersburg Normal II. was dismantled, taken to Pawlowsk, and there compared with the Pawlowsk normal, and the two were found to differ by only 0.01 or 0.02 mm.; that is .004 or .008 inch. It must be added that these comparisons have all been checked by means of comparisons with portable barometers of the highest class.

The paper by Prof. Wild is accompanied by illustrations of these various normal barometers. The St. Petersburg normal has recently undergone some alterations, and these are also fully described. Altogether this is perhaps the most important contribution to the subject that has appeared since Prof. Wild's famous memoir of 1873; for we can now rest assured that farther refinement is not required by any practical demands.

It seems to me that now that we are sure of the accuracy of Wild's normal, it is more necessary than ever that we should know with greater certainty its relation to the principal standards of Europe. I desire, therefore, to propose a plan by which a series of comparisons can be carried out for a few places at a very slight expense, and with as much accuracy as portable instruments will permit. In 1883 it became my duty to transport to America, from Hamburg three of the Wild-Fuess portable barometers of the highest grade; and it was of great importance to take every possible precaution against their being injured or their condition altered in any way so as to affect their readings. I devised a mounting on shipboard which was very satisfactory, and gave me no cause for uneasiness regarding the barometers, even in stormy weather. So many barometers are sent out from England to almost every country that I strongly urge the use of a similar arrangement in all cases where it is desirable to retain an assigned barometer correction.

The accompanying sketch shows my manner of mounting the barometers. Two small strips of wood, AA, are screwed to the woodwork running lengthwise of the vessel. They are placed about two feet apart, and are inclined at an angle of perhaps 45°. Small leather straps, say 15 inches long, are fastened to these strips by single screws as shown at BB. A rather soft stuffed flat cushion or pillow is now placed against the woodwork (wall) as shown at C. The box containing the barometer is now pressed against the cushion and the two extremities are placed within the grasp of the straps BB. These last are buckled and drawn tight enough to hold the barometer box firmly against the cushion C. The barometer is thus held in such a manner that no ordinary jarring can cause any damage to it, as there is no direct contact with a rigid surface, since the pillow prevents it from touching the wooden strips, and the soft yielding straps have a spring-like effect.

The lower part of the sketch shows the barometer box DD in position, with the barometer shown within it. Of course the cistern is held uppermost. On account of the jarring motion of the ship's screw in rough weather, it is desirable to locate the barometers well amidship, and also have the cistern of the baro-