St. Moritz lies on the northern slope of a valley running from south-west to north-east. At the beginning of the observations the opposite slope was buried in snow, but the northern slope both above and below the point of observation was almost free from snow. Thus the most brightly illuminated part of the ground surface was of a dull brown or gray colour. Under these circumstances, the reading was about 50° in the middle of the day, being a little higher earlier and later, viz. about 52° at 10 a.m. (date October 21 and 22). These readings, as well as those mentioned below, refer to the highest point of the sky, which is distant 90° from the sun, and were taken when the whole sky was free from cloud. On October 26, after a five-inch fall of snow, the reading was 41° at 10.15 a.m.

By October 29 most of the fresh snow had gone, and I found at 11.40 a.m. the reading as high as 48° . After this we had several feet of snow, and at 12.50 p.m. on November 13, the reading was again 41° . Each of these readings is the mean of four, and I find two readings of the same thing seldom differ more than 2° . Hitherto I have not been able properly to evaluate the readings of my instrument in absolute measure, though I hope to do so later. But to gain an approximate idea of their meaning, I have calculated the polarizing power of the two piles on the assumptions—first that Fresnel's laws of the reflection of polarized light are accurate, and secondly that the index of refraction of my plates is 1.52. We may consider the light from the sky as consisting of two parts completely polarized, one in the plane of the sun, and the other perpendicular thereto. The ratio of these parts is .376 for the reading 40° , and .271 for the reading 50° . Again we may divide the light into a part unpolarized and a part completely polarized in the plane of the sun. The ratio of these parts is .546 for 40° and .428 for 50° . So it seems fair to conclude that the light reflected from the fresh snow was sufficient to increase the unpolarized part of the sky light by more than a quarter.

JAMES C. MCCONNELL.

St. Moritz, Switzerland, December 10.

The Ffynnon Beuno and Cae Gwyn Caves.

I WILL answer Dr. Hicks's question in as few words as possible. Nothing is to be gained by terming me a "highly prejudiced" observer, or by saying my views are of "no consequence" and "not worth anything." Your readers can form their own conclusions on these points. I am not "highly prejudiced" against, neither have I any "bias against," the existence of pre-Glacial man or of his "migrations"; on the contrary, I favour these subjects.

I did see the section of drift exposed at the Cae Gwyn Cave, and I can hardly describe it (from my own point of view) without giving offence. My view is this: the section showed nothing but rain-wash derived from the closely-adjointing non-Glacial drift. The section showed a re-made deposit, horizontally stratified, and with stones resting on their flat sides. No doubt there were Glacial stones in the rain-wash, derived from the ever-shifting post-Glacial marine drift close by; the latter being merely a re-laid Glacial drift. Stones with Glacial scratches may be found in the lower gravels of the Thames.

To me, the caves and their surroundings are in the highest degree suspicious, and in size insignificant, and not comparable with large and typical caves. They are small and painfully narrow tortuous passages only, on a hill-side, and close to the surface. The lower cave is furnished with a very large hole, opening up to the surface just above; and the upper cave had at one time a similar opening. The post-Glacial drift above is always on the move, and every shower of rain brings it down with its derived stones.

Since writing to NATURE, in November 3, I have referred to some of the papers published on these caves. I turned first to the list of mammalian remains, only however to find that the animals (like the implements) are entirely characteristic of the most recent post-Glacial deposits. Even near London we get in gravels of no great comparative antiquity the bones of *Elephas antiquus*, but in the caves merely *E. primigenius* is found. As regards antiquity, the animals no doubt overlap at both ends of the scale, but their meaning, as found in these caves, points in one direction only, and that is to the most recent and not to the most remote of Palæolithic times. None of the cave mammals are characteristic of pre-Glacial deposits.

It would seem that Dr. Hicks does not realize the nature of Dr. John Evans's criticism. La Madelaine is the newest of caves, and represents the most recent of Palæolithic times: it is

a kind of connecting link between Palæolithic and Neolithic times. Therefore, if Dr. Evans's criticism is taken with mine, the two clearly prove that there is a distinct chronological value in the classification, not that there is "no chronological value" as concluded by Dr. Hicks. Dr. Hicks also appears not to realize the fact that river-drift and cave implements do not only differ in roughness and abrasion but in style. The cave men used different implements from the river-drift men, they were changing from savagery to barbarism. If Dr. Hicks produces implements made by pre-Glacial men, he must show us something obviously older than the oldest river-drift tools, not fall back upon refined tools which are, to re-quote Dr. Evans, "precisely like many from the French caves of the reindeer period, such for instance as La Madelaine." If Dr. Hicks abandons his scraper, he is still in no better position, for his finely re-trimmed knife and the implement in the British Museum are identical in age and character with it. So are the flakes: the one with long narrow facets is characteristic of the latest, not of the earliest work. So is the pointed and drilled bone. No drilled bones have been found in moderately old river-gravels, and what is more, no instrument suitable for boring a small hole through bone has ever been found in such a gravel. Drilled bones and small flint drills belong to the very latest of Palæolithic times. In the remains of my own collection of Palæolithic implements I have here over a thousand examples of the major class, and an equal number of minor forms illustrative of the development of knife and scraper forms, but they give no support whatever to Dr. Hicks's conclusions; they all, in fact, point in a diametrically different direction. I am acquainted with Prof. Prestwich's views, and I believe I was the first person to find implements in the highest terraces of the Thames Valley; but I do not see that Prof. Prestwich's conclusions have any direct bearing on the Ffynnon Beuno and Cae Gwyn caves.

I do not suppose that any opinion of mine will influence Dr. Hicks, and I have no wish to influence him or any other observer. I merely wish to put on record the fact that, after many years' experience amongst drifts, and implements, and fossil bones, my conclusions are entirely opposed to Dr. Hicks's. Dunstable.

WORTHINGTON G. SMITH.

P.S.—Since the above has been in type, I have seen the report in last week's NATURE (p. 166), but I prefer to let my letter stand just as written before the report was seen by me. Prof. Hughes has cut away the geological and palæontological supports; I shall be content to resist the idea of the pre Glacial age of these caves on purely archaeological grounds.—W. G. S.

The Planet Mercury.

THE planet observed on the mornings of December 7 and 9 by your correspondent "G. F. P." (NATURE, December 15, p. 151), was probably not Mercury but Jupiter, as these bodies were near together at the time, and the latter was by far the brightest and most conspicuous. The circumstances, described by "G. F. P.," under which the object was noticed render it certain that it could not have been Mercury, for the latter was decidedly small, and might have been easily overlooked on the several mornings I saw it early in the present month. Jupiter, on the other hand, was very bright and plain, and might easily attract attention in the way stated by your correspondent. On the 9th instant the two planets were about 3° apart, Jupiter being situated to the west of Mercury.

Had "G. F. P." really observed the latter planet, he would have instantly remarked its half-moon phase in his $3\frac{1}{4}$ -inch telescope, and must have mentioned Jupiter, as well as Venus, as visible at the same time.

There is no difficulty in observing Mercury with the naked eye if the planet is carefully looked for in the proper spot, at the times of his eastern elongations in the first half of the year and at the western elongations in the last half. I have seen the planet on certainly more than fifty occasions. In May 1876 I noticed Mercury on thirteen different evenings. Sometimes the planet is quite conspicuous in the twilight as a naked-eye object.

W. F. DENNING.

Bristol, December 16.

Meteor of November 15.

IN NATURE of December 1 (p. 105) Mr. B. Truscott writes of a wonderfully fine meteor seen at Falmouth on the night of Tuesday, the 15th ult., and asks in effect if it was seen by other

eyes than his: so perhaps it may be permitted to be said that it was seen in the parish of Llanefydd, Denbighshire, by a correspondent of mine, who writes:—"On Tuesday night, November 15, while returning homewards on foot, happening to look eastwards I saw a long train of brilliant light suddenly flash out of the sky. At first I thought it was lightning. But instead of vanishing it descended with great rapidity, the light increasing in brilliancy as it neared the earth. The night was rather dark, although the sky was thickly studded with stars, but in a few seconds so intensely brilliant had the light become that a pin might have been picked up from the road with the greatest ease. While I was looking, the object that accompanied the flash burst, and displayed a magnificent mauve and red fringe of light. I say fringe, as it would be impossible for me to describe otherwise the shape, for it appeared to me to project shafts of light, some long and some short, like what would be the rays of a great star. There was in the direction in which I was looking a thick wood, and the effect on the trees of the silvery light I first noticed was richly beautiful. But the effect of the mauve and red light was magnificently grand, and to me in no little degree awful. The whole wood was enveloped in a red lurid glare, which lasted as near as I can calculate some six or eight seconds. The effect altogether was like a brilliant transformation scene, and the meteor having passed away, the darkness of the night seemed to be in the last degree intense."

J. LLOYD BOZWARD.

A Correction.

IN the footnote, p. 152, second column, there is an unfortunate transposition for which myself more than the printers are to blame, which it is important to correct.

After the words "by Aristotle, Probl. I E 3 and Metaph. A 5" occur the words "which he attributes to Pythagoras . . . on the nature of the Beast."

These words should have come at the end of the subsequent paragraph where I say that "Muhamad-al-Sharastani assigns reasons for regarding all the numbers up to 10 inclusive as perfect numbers."

It is these reasons which I speak of as being by him attributed to Pythagoras, &c.

I may take this opportunity of giving, as another example of the use of the New Nomenclature, the well-known extended Theorem of Fermat, which may be expressed by saying:

"Every number must divide the Fermation of which the index is its totient, and the base any one of its totitives."

Athenæum Club, December 15.

J. J. SYLVESTER.

ISOLATION OF FLUORINE.

ONE of the most difficult problems of modern chemistry has at last been satisfactorily solved. After three years of incessant labour, occasionally interrupted by temporary feelings akin to despair, M. Henri Moissan has at length isolated in considerable quantities that most baffling of elements—fluorine, and has been enabled to determine its principal properties. The experiments themselves are among the most interesting ever performed, and their details, as described by M. Moissan in the December number of the *Annales de Chimie et de Physique*, form the most fascinating reading. They must of necessity have been extremely costly, for by far the greater portion of the apparatus employed was constructed of platinum, and it is not often that one hears of a platinum tube 80 centimetres long and of $1\frac{1}{2}$ centimetre diameter being destroyed in each experiment, as happened in the earlier stages of these researches.

The isolation of fluorine has formed a worthy object of the attention of chemists ever since the first remarkable experiments of Sir Humphry Davy, who was rendered dangerously ill by being exposed to the corrosive fumes of hydrofluoric acid. Although Davy was not successful in obtaining free fluorine, yet he brought clearly to light the nature of hydrofluoric acid, and proved it to consist of hydrogen combined with an unknown but extremely active element—fluorine. The history of all the attempts which have since been made to effect the preparation of

free fluorine might occupy a volume, and it will therefore only be necessary to refer to the later work of our countryman, Gore, who, in 1869, published his researches upon the electrolysis of hydrofluoric acid, and of certain fluorides, and left our knowledge of the acid itself in a most complete state. M. Moissan, working in the laboratory of M. Debray, now steps in and achieves the result so ardently sought after during the last eighty years—another example of the irresistible power of human perseverance.

In the light of the experience gained by former experimenters, it appeared that the action of a powerful electric current upon the compounds of fluorine with the non-metallic elements, such as hydrogen, phosphorus, and arsenic, would be most likely to yield the desired result; knowing also that fluorine must be an extremely energetic substance, it was absolutely essential to work at very low temperatures. Hence M. Moissan's first attack was made upon the fluorides of phosphorus and arsenic, but finding these to be practically impregnable, he diverted his attack, guided by certain indications afforded during his first attempt, upon hydrofluoric acid itself. Finding, however, that pure hydrofluoric acid is an exceptionally bad conductor of electricity, as has been stated by other workers—that even a current from fifty Bunsen cells would not pass through the liquid—he eventually, after several essays, succeeded in converting it into a conductor by dissolving in it a quantity of the double fluoride of potassium and hydrogen. On passing the current from twenty Bunsen cells through the now conducting medium, hydrogen immediately commenced to be evolved at the negative terminal, while fluorine was with similar rapidity evolved at the positive pole, and exhibited its tremendous activity upon everything that came near it: burning up hard crystalline silicon like tinder, setting fire to organic matter, and forming fluorides with incandescence with many other elements.

Having thus indicated the general course of these researches, it will no doubt be interesting to follow M. Moissan during the carrying out of his principal experiments.

The first series consisted in examining the action of electric induction sparks upon the gaseous fluorides of silicon, phosphorus, and arsenic. The gases were introduced into glass eudiometer tubes standing over mercury, and the spark was passed between two platinum wires connected with an induction-coil actuated by a few Grenet or Bunsen cells. On introducing dry silicon tetrafluoride, SiF_4 , and passing sparks for an hour, no decomposition was effected, the result being discouragingly *nil*. Dry phosphorus trifluoride, PF_3 , however, behaved quite differently, phosphorus being deposited upon the inner wall of the tube; but the fluorine liberated at once combined with the residual trifluoride to form the more stable pentafluoride, PF_5 . Some time ago this pentafluoride of phosphorus was prepared by Prof. Thorpe, who also submitted it to the action of the induction-spark, unfortunately without effecting any decomposition. Precisely the same result has been arrived at by M. Moissan, using a 0.04m. spark; but on obtaining sparks 0.2m. long, a rapid etching of the walls of the glass tube occurred, and the meniscus of mercury entirely lost its brilliancy. After an hour's duration the experiment was concluded, and the apparatus allowed to cool, when it was noticed that the volume had diminished; moreover, the gas was found to have changed its properties, yielding a precipitate of silica in contact with water, while the residual gas consisted of the trifluoride of phosphorus. Hence $\text{PF}_5 = \text{PF}_3 + \text{F}_2$, which latter forms, with the glass, silicon tetrafluoride, and, with the mercury, fluoride of mercury. So here again the experiment was disappointing, and although fluorine was for the moment liberated, this method was certainly not suitable for the preparation of free fluorine.