would they have a tendency to turn over in falling? I rather think they would turn over, and the further they have to fall, the oftener would they turn. The same with a meteor. I suppose, in order to find this out, one should have a cinematograph camera attached to a telescope, but then the difficulty would be to set it going just when the meteor is there. Here is a suggestion for the dim future, when the Society will have got together enough funds!

Yours Sincerely,

ETHEL VOIGT.

[The above would no doubt account for the bright patches in the trail, but the shape of a meteorite would be roughly spherical as a crescent shaped body which, nobbled in the manner suggested, would not obey the laws of equilibrium in its orbit. It seems probable that the bright patches are due to the trail doubling back on itself owing to air currents. At the overlapping places a patch would appear.—H. G. T.]

Extracts from Publications.

Mr. Hollis, writing to the *English Mechanic* regarding a paper by Mr. Holmes of the British Astronomical Society, says:—

Mr. Holmes asks for what reasons do we believe, or are there good reasons for believing, that meteors are visible because they ignite by friction, and is our atmosphere sufficient to prevent them arriving with some force on the Earth? Mr. Holmes gave figures showing the equivalent of the whole atmosphere between the meteor and the surface of the Earth expressed in volume of air at surface density, which did not amount to very much, and asked with some humour whether anyone would care to stand with only that between him and the muzzle of a loaded gun? In the discussion which followed—and this paper called forth some valuable remarks—the opinion was expressed that perhaps sufficient account had not been taken of chemical action; also it was pointed out that the immense velocity of the meteors increased rather than diminished the resistance of the air, but the feeling was evidently pretty general that Mr. Holmes had touched a weak spot, and that this statement about the incandescence of meteors by friction had got into the text books without much in the way of definite proof.

[*English Mechanic.*

Mr. Thorp writes in the *English Mechanic*:—

How do we know that the atmosphere is one-millionth of sea level density at seventy miles high? We are situated somewhat as a fish would be in an ocean five miles deep, which, finding the pressure five tons on the square inch, and rising to two and a half miles found the pressure only two and a half tons, argued that the next two and a half miles would reduce this by a ton and a quarter, and so on, and that at each rise of two and a half miles the pressure would be reduced one half to any height. This would be an error, as we know, since the pressure of the water would cease at the surface; but the fish would not be able to reason thus, and to me we appear to reason fish fashion. As we cannot get up the second three and a half miles even to test the question what evidence is there that the atmosphere has not a definite surface at, say, ten miles high? There is much evidence from analogy that such definite surface exists, for, as the Sun, Jupiter, and Saturn are all gaseous—specially the Sun—and they all present sharply defined limits to their atmosphere, we are entitled to argue a smaller body, like the Earth, must also have a limited envelope. Full stronger evidence is offered by Venus. In her case we never see anything but her atmosphere, illuminated by the Sun, and her atmosphere presents a very hard, definite outline, showing that it certainly does not thin out in the manner the Earth's atmosphere has been supposed to do. Let us suppose that our atmosphere ends at ten miles. Its density will be enormously greater than Mr. Thomson supposes at seventy. In fact it would be at least half the density at sea level, and a meteor would strike it with tremendous energy, and the resistance be great enough to volatilise the whole. I submit that the calculated heights are very rough and of no value, because it is impossible to obtain distance from a single observation; and when two observers have supposed they saw the same meteor, I suggest, in fact, they saw two different ones, and thus deduced an enormously erroneous height. The observations of position are necessarily grossly inexact, and a very slight difference of displacement would account for getting seventy miles instead of ten.

[*English Mechanic.*

Mr. Hollis, writing to the *English Mechanic*, says :—

I think we must accept the computed heights of meteors. The accumulation of results, showing that the paths are fifty or sixty miles above the earth, makes it clear that this is not far from the truth, for errors of observation would not conspire to agree in that way. Secondly, determinations of the height of the atmosphere from the observed length of twilight gives it a limit of about 200 miles, so that there is probably something in the way of atmosphere at a height of 70, though it may be attenuated. Thirdly, the effect of the great velocity of the meteors in causing resistance must not be under-estimated. Resistance varies as the square of the velocity. This is a fact proved for comparatively small velocities by direct experiment with projectiles, and also by comparison of wind pressures with velocities. In some figures, referring to a great storm that I have now before me, when the velocity of the wind was 88 ft. per second, the pressure was 18 lb. to the square foot; therefore, by the above law, if the velocity had been 40 miles per second, the pressure would have been $(40 \times 5280\text{-}88)^2 \times 18$ lb. $= 103,680,000$ lb. per square foot; so that, if we reverse the operation, and suppose the pressure created by the motion of the meteor rather than by the motion of the air, the pressure caused, even in an attenuated atmosphere, must evidently be large. The question is not new. In a book on *Meteoric Astronomy* by Kirkwood, published in 1867, it is written :—"This question has been discussed by Joule, Thomson, Haidinger, and Reichenbach, and may now be regarded as definitely settled. A velocity of 30 miles a second would produce a temperature of 2,500,000°." He does not give all the data and figures for this result, or I should be glad to quote them; but I have no doubt that they were at least as trustworthy as those of Mr. Thomson, who evidently was taking extreme cases on the opposite side. Haidinger's Memoir is in the British Association Report for 1861, with a note by Mr. Greg, and in that there is a suggestion that the light emitted by fire-balls does not arise from mere incandescence, but is caused by electricity; so, though as I think we may take it, the appearance of shooting stars is caused by motion through an atmosphere, causes other than incandescence may be considered.

[*English Mechanic.*

Mr. Eddington, in explaining a paper on the Principle of Relativity as applied to Astronomy before the Royal Astronomical Society, said:—

Dr. de Sitter's paper is largely occupied with mathematical investigations, and as the subject is probably not very familiar to astronomers generally, perhaps I had better begin by explaining a little about the Principle of Relativity, why we need such a principle, and why there is reason to believe in it. It is well known that physicists now are inclined to attribute the property of matter called mass or inertia to an electrical origin. If this hypothesis is true, then the mass of a body is not strictly constant, but contains a term depending on the square of the velocity. The extra term is very small, but yet, in the case of a planet which is moving very fast, it may just become appreciable in astronomical measurements. If we take Mercury, which is moving fastest, and which also has a large eccentricity, its mass would change between aphelion and perihelion by something of the order of one part in fifty million in consequence of the change in its velocity. This is just on the verge of what might be appreciable in astronomical measurements. Now we could put this term into the equations of motion, and work out the result quite independently of Relativity, simply assuming the electrical nature of matter. But we run up against a difficulty at once. What shall we assume for the more accurate law of gravitation? The law of gravitation, as ordinarily expressed, depends upon the product of the masses; and if we begin juggling with the idea of mass in this way, it involves a reconsideration of the law of gravitation, for the phraseology of the accepted law has become ambiguous. That is where the Principle of Relativity helps us. It asserts that it is impossible in any system to detect the uniform motion of that system through æther. That is not an accurate definition, but that is what practically it amounts to. If you prefer a really scientific definition, I may quote from Dr. de Sitter's paper: it is "the postulate that the transformations with respect to which the laws of nature shall be invariant are Lorentz transformations."

Now why should we believe in this principle? You will see, of course, that it does indicate to us in a way a new law of gravitation, because it asserts that the alteration in the masses produced by the motion of the whole system through space (introducing that little extra term) is compensated by other changes in the equations of motion, including, of course, the law of gravitation. The reason why we think there is some probability in the Principle of Relativity is because it is always found by experiments, undertaken to detect the motion of our system through the æther, that there is an exact compensation. Moreover, it has been proved that for a very large class of natural phenomena this compensation occurs in the actual fundamental laws themselves—the laws of electrodynamics—and so prevails in all phenomena which depend on those laws.

[*The Observatory.*

The Harvest Moon is not generally understood by the public, but the true cause has long been known to astronomers and can be very easily explained. It arises from the fact that the ecliptic or the Sun's apparent path through the heavens is variously inclined to the horizon at different seasons of the year. The celestial equator is always at the same angle with the horizon, and hence equal portions come above the horizon in equal periods of time. If the Moon moved in the celestial equator, she would rise and set directly in the east and west points of the horizon respectively, and she would rise later each night by a nearly constant interval. But the Moon moves in a path which is constantly inclined to the ecliptic at an angle of about 5 degrees, though for the present explanation she may be regarded as moving in the ecliptic; and as the ecliptic is inclined to the celestial equator at an angle of 23½ degrees, the Moon in all parts of her orbit does not rise at equal intervals on each succeeding night.

[*Popular Astronomy.*

Notices of the Society

Election of Members.

The attention of members is invited to Bye-law No. 14, regulating the election of persons who desire to join the Society. It is hoped that those who are already members will induce others to join. Forms of application can be had from the Secretary, Mr. P. N. Mukherji.

The Library.

An opportunity will occur during the next few months, owing to one of the members of the Society going to