of this intensity difference. Accordingly the use of his reduction curves would lead to a decrease in the measured absolute magnitude with advancing type, an effect actually shown by the B stars.

The spectral types of the stars used at the Norman Lockyer Observatory were determined from the intensity of the $1\pi - m\delta$ lines of helium. The $1P - mD$ lines, which were used for estimating the absolute magnitudes of the stars between Bo and B8, treated

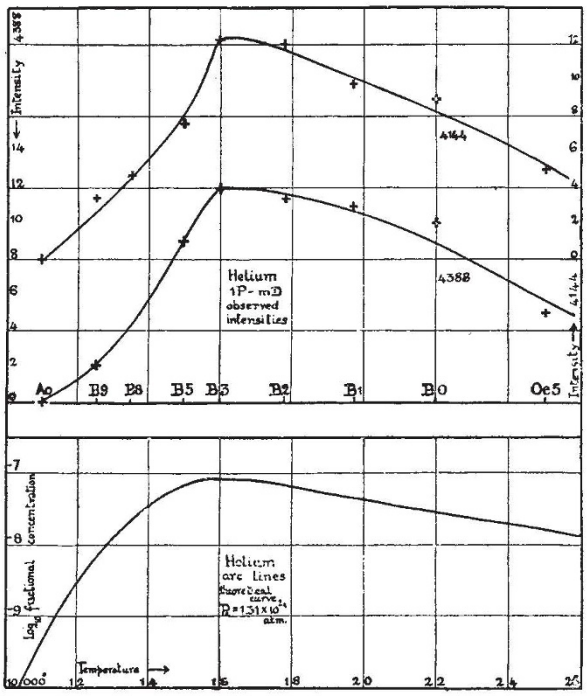

FIG. 1.

as a single group, show intensity changes identical with those of the $1\pi - m\delta$ lines. It therefore appears that measures made on these lines give the spectral type, and that the general decrease of absolute magnitude with advancing type, as shown by Edwards's measures, is the result of the well-known statistical correlation of type and luminosity in the B stars.

The parallaxes used by Edwards in forming the reduction curves do not appear, in all cases, to have the best values usually adopted for the stars in question. For stars belonging to the Pleiades, for example, $\pi = 0{\cdot}009'' \pm 0{\cdot}002''$, a value considerably smaller than the various parallaxes which Edwards assigns to individual stars belonging to this group. Such uncertainties, and the fact that the helium lines give only a measure of type, tend to cast doubt on the value of this method for the determination of luminosities within a given spectral class.

CECILIA H. PAYNE.

Harvard College Observatory,
Cambridge, Massachusetts,
April 9.

Physics and Relativity.

THE preoccupation of philosophers with relativity, to the exclusion of all other branches of science, is very puzzling to a physicist; but most puzzling of all is their continual assertion that relativity has had a profound influence on physics. May I place on record certain facts, well known to all physicists, which appear to me to dispose of this delusion.

"Science Abstracts" A (Physics) for 1923 contains 2542 abstracts. Of these 43 are indexed under "Relativity," but 30 more should have been so indexed. Of these 73, 51 deal with relativity and nothing else; they would be wholly meaningless to one who rejected its conceptions. Thus there are left 22 papers, or 1 per cent. of the total, in which the effect of relativity on the rest of physics may be manifested. The remaining 99 per cent. are exactly what they would be if relativity had never been heard of.

The 22 papers include all those on the experimental evidence for relativity, but they do not include all those which involve facts explained by relativity. Thus, papers on the scattering of charged rays and on the "relativity doublet" are not really concerned with relativity. They are concerned with the fact that the mass of a charged body varies with its velocity, but not with the explanation of that fact. The fact was actually known before any theory of relativity was propounded, and would of course remain a fact if relativity were abandoned.

If it is urged that these 22 papers are peculiarly important, I can only offer a denial. I had not noticed many of them until I looked them up in "Science Abstracts," and I am sure there are very many papers on other branches much more widely read.

Perhaps an even more convincing fact can be cited. Many writers on relativity confine their attention to that subject. But there are two outstanding exceptions. Both Prof. Einstein and Prof. Eddington, during the period of their work on relativity, have published other work of the highest value, lying on the main track of physics (or astrophysics) and forming the starting-points of researches well known and interesting to all students. But this work is wholly independent of relativity; there is nothing in it that might not have been published by an opponent of that doctrine.

The gulf between relativity and physics will probably be bridged in the future, possibly by a wide extension of relativist conceptions. But prophecies of the course that scientific inquiry will take are seldom fulfilled; and philosophers might do well to consider whether it is worth while to discuss at such length the consequences of a development which has not yet occurred.

NORMAN R. CAMPBELL.

Sunshine and Health in Different Lands.

THE statement, that from the health point of view we cannot have too much sunshine, is very commonly made, but I agree with Mr. Bonacina that it should not be accepted as axiomatic without investigation. It seems to me to rest on a very slender foundation and to be opposed to many well-known statistical facts.

In European countries the death-rate is highest in the south and lowest in the north, the British Isles and the Scandinavian countries being especially favoured. Though the time of possible sunshine is nearly the same in both parts, the intensity is far greater in the south.

I happen to have by me records of the summer sunshine in England and the death-rate of the summer quarter during the period 1881 to 1912, and they show the following facts. The summers of 1888, 1894, 1910, 1912 were the four most sunless summers of the group, and all four had an exceptionally low death-rate. The sunniest summers were 1887, 1899, and 1911. The summer of 1887 had a death-rate slightly above the average, 1899 had the highest death-rate in the group, and 1911 the third highest (the death-rates have all been corrected to allow for the secular decrease).

These statistics do not support the supposition we are discussing, but very much the reverse. The fact that ultra-violet rays are beneficial in certain diseases does not, in my opinion, prove that an excess of