struck several times during my residence here, and one of the steel and concrete domes was struck once. In the early history of the Observatory one of the astronomers was killed in his room in the assistants' house.

Altogether I have observed many hundreds of brilliant flashes, as many close ones perhaps as distant ones. I have many times observed these 'multiple' flashes, usually two or three succeeding each other. They are strikingly clear and sharp and sometimes appear to vibrate. They certainly give the impression of there being distinct flashes, but I cannot conclude that this is a fact, because they are invariably seen only when the flashes are distant, usually too distant for there to be any sound. Now we have every reason to expect that just as many of this type of flash should occur near by as at a great distance if there are really several flashes instead of one. But in all my experience I have never seen a case near by; they are always at a great distance. One of the strongly marked characteristics of the near flashes, in appearance as in sound, is their sharpness and shortness—the nearer the shorter. Flashes within a hundred yards are sensibly instantaneous to both eye and ear, and *always single*.

From these facts I conclude that the 'multiple flashes are due in some way to erratic refraction in the atmosphere.

It would not be without value perhaps for two observers, some twenty or thirty miles apart and in telephonic connexion, to test this matter by observing whether the flashes which appear multiple are observed to be so at both stations in cases where the flash is very close to one station and distant from the other.

C. D. PERRINE.

Observatorio Astronomico de la
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Córdoba, Dec. 19.

I HAVE to thank Dr. Perrine for his observations on my article on "Progressive Lightning." They are interesting as indicating some difference in the appearance of lightning in the Argentine, where the strokes are exceptionally strong, and that of lightning in Great Britain. Here without any question the appearance of the multiple flash is found when the distance is as little as a kilometre, and I certainly believe much less. In Cordoba Dr. Perrine only observes this when the flashes are so distant that thunder is barely heard. Such a distance here is from sixteen to twenty kilometres. I suppose here, with a flash near enough, even if it were multiple, the eye would be so dazzled by the primitive flash as not to be able to see those that follow, but it is difficult to account in this way for the great distance implied by the faint audibility of the thunder. I have seen a large number of photographs taken with an ordinary camera held in the hand, and not intentionally moved as Dr. Hoffert's was, but nevertheless not really fixed, which show the multiple flash, and the size of the flashes on the plates indicates that they must have been fairly near. At any distance such as sixteen kilometres the flash would occupy but a small portion of the plate.

I could have wished that Dr. Perrine had supplemented his eye observations with photographic records which would be easily obtained in so favourable a locality. It would appear that it is a suitable place for my proposed rocket experiment, that is, if the descent of the empty case and stick is not a danger as it is with me. I might add to my previous observations that the rocket, preferably of about three centimetres calibre, should be without a head but with one calibre extra of solid composition over the hollow left by the spindle, and that the usual proportion of coarse charcoal be retained, so that the whole length of the trail should contain live sparks to improve its conductivity. Such a rocket would attain perhaps double the usual height and move with immense speed.

If any question should arise as to the conductivity of the trail, this could be assured by allowing the rocket to carry up a hundred metres or more of fine copper wire arranged like a life-line so as not to kink. Owing, however, to the inconvenience of laying this in thunder rain I would first rely on the trail.

C. V. BOYS.

The Polishing of Surfaces.

IN NATURE of Sept. 4, 1926, Mr. J. M. Macaulay suggested that, in the process of polishing, surface layers were actually melted—the energy supplied being ample to produce melting, and the difficulty with regard to temperature disappearing if the heat, due to friction, was produced at points of contact rather than over large areas (the temperature at a *mathematical* point-source of heat being infinite). In NATURE of Jan. 29, Mr. N. K. Adam contends that "it does not seem necessary to suppose that actual liquefaction occurs," since "the polisher will tear away the surface particles of the glass" and "some of these particles will naturally be redeposited elsewhere at random, thus forming the amorphous layer." Now it is difficult to believe that particles of glass spread at random will have a polished surface, even although these particles be of molecular dimensions, since each particle will be covered by a surface layer of gas, or other substance, which will prevent cohesion at ordinary temperatures. It might be contended that in the polishing process the particles torn from the surface had no time to assume a surface layer, but that is surely a highly improbable assumption. It seems to me that Mr. Macaulay's letter gives the key to the rational explanation of surface polish.

JAMES MUIR.

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Glasgow, C.1, Jan. 31.

PERHAPS my letter of Jan. 29 requires some further explanation. I did not suppose that the particles were redeposited as a dust on the surface, expecting them to coalesce spontaneously to a polished layer; this would of course be impossible. If, however, we examine what would happen to the *molecules* on the liquefaction hypothesis and on mine, it appears that mine is simpler and more probable. On the liquefaction hypothesis, the forces of adhesion between the moving polisher and the surface (constituting friction) are supposed to produce heat first; this heat then liquefies the surface layer. That means that the friction sets up irregular thermal vibrations in the surface molecules; it is then necessary to suppose that the energy of these vibrations is dissipated less rapidly than it accumulates, owing to the small thermal conductivity of the material; finally, the vibrations become so intense that the molecules can no longer stay in their positions in the solid, and 'melting' occurs. It must further be supposed that the surface layers remain liquid sufficiently long to flow to a plane surface, with whatever assistance may be given by the motion of the polisher.

My hypothesis states that the polisher adheres to the surface, dragging away small particles. In the same manner, at other localities, the adhesion of the surface to the polisher drags back some of the particles adhering to the polisher. Thus the polisher acts as