I am indebted to M. Perrotin, of the Nice Observatory, for some useful information. He tells me that "the first shock lasted *nearly one minute;* it began by being very slight at first, and then became *very intense:* this latter phase lasted from 20 to 30 seconds."

We were all accused of great exaggeration in our accounts of the earthquake (I put the first shock at half a minute), but this more than confirms our estimates. A gentleman from South America, accustomed to earthquakes every week, told us that it was "a pretty good shake up."

According to Lyell, Mallet, and others, we at Nice being on alluvial deposits—gravel, clay, &c.,—felt not only the original shock, but also the rebounds from the rocks on either side; this would account for the very violent shaking that we had. I have compared notes with dozens of people, and feel sure that it was quite different from the sort of shock they felt at Cannes, Monte Carlo, and other places on rock, even the east bay at Mentone (the west bay suffered more than Nice). What saved us from being knocked down was, I suppose, that the amplitude of the vibrations was small, probably only a few inches. In the Italian Riviera (Diano Marina, &c.) they must have been more severe. Most people think that one more shock of the same strength would have brought half the houses down. A railway carriage going at 60 miles an hour gives the best idea of what our rooms were like during the first shock; it was impossible to stand on the floor without holding on to something, like a landsman on board a ship in a storm.

It would appear from the times given in the table that the velocity of this earthquake was high: 76 miles a minute to Paris, and 81 to London—a curious case of velocity increasing with distance.

The second shock seems to have gone faster than the first. The ordinary rate of earthquake-shock velocity is (according to Prestwich's "Geology"):—

1857. Neapolitan earthquake ...	...	9	miles per	minute.
1843. United States ,,	... ...	32	,,	,,
1869. Cachar (India) ,,	... ...	83	,,	,,

The centre of the shock was somewhere in the Gulf of Genoa, near Savona. The second shock was slight. The third was strong, but short.

The noise before the first shock was very loud, like a large steam blast. There were more than half a dozen other shocks in the two following days, but they were slighter, and chiefly oscillatory; curiously enough, we did not mind them so much as the vibrations, though I believe they are much more dangerous, if severe.

The night before the earthquake some horses were nervous and refused food, and dogs howled, but I naturally supposed that it had something to do with the Carnival which was being celebrated at the time. J. E. H. PEYTON.

108 Marina, St. Leonards-on-Sea, May 19.

The Shadow of Adam's Peak.

I HAVE recently seen a paper, read before the Physical Society by the Hon. Ralph Abercromby, which apparently shatters an explanation proposed by me to the same Society of the phenomena of the shadow of Adam's Peak in Ceylon. Whilst not anxious to support my own theory, if one more consistent with the phenomena has been discovered, I venture to think that there are certain considerations which militate against the new theory, and render it incomplete; and, with your permission, I will enumerate them.

(1) Mr. Abercromby says that it is the intervention of near and moving mist which produces the apparent uprising of the shadow. Is it possible that such a simple explanation could have escaped the notice of the hundreds of observers who have witnessed the phenomenon, and returned with the impression that there was something inexplicable about the shadow? It is difficult to imagine observing and reasoning faculties so rudimentary as not to be able to observe that a shadow was on mist, and reason from that to an explanation of the apparent approach and uprising of the shadow.

(2) Mr. Abercromby's theory depends on the intervention of near and moving mist rising from the Maskeliya Valley. This valley stretches away behind the observer in a south-east direction as well as to the north-west, and mist rising from it would be quite as likely to intercept the sun's rays behind as to form a curtain in front for the shadow to be projected on, and it would be only on very rare occasions, such as Mr. Abercromby describes, that the mist would keep entirely to the north-west of the Peak. Why it should do so is not explained. Therefore the uprising of the shadow could only be seen on such very rare occasions.

(3) Mr. Abercromby says: "Our fortune was in the unsettled weather, which made the mist so coarse and close that the unequivocal bow left no doubt as to the true nature of the cause;" "the sky was covered with a confused mass of nearly every variety of cloud;" "below and around us cumulus and mist;" "a pale moon with an ill-defined *corona;*" "sometimes masses of mist coming up from the valley enveloped us with condensed vapour;" "driving condensed vapour was floating about, and a fragment of rainbow-tinted mist appeared near the top of the shadow." Under such conditions, what else could Mr. Abercromby have seen than what he describes, the shadow on the mist, a circular rainbow, spectral figures like those of the Brocken, the rising and falling of the shadow as the mist intervened or passed away? Instead of "fortunate" in his conditions, I think Mr. Abercromby was the very reverse. To be "fortunate" he should also have seen the shadow in a clear atmosphere, and noted the *absence* of any appearance of uprising. Mr. Whymper, in his famous descent of the Matterhorn after the accident, saw in the evening a fog-bow very similar to that described by Mr. Abercromby, and the presence of mist was noticed in that case. But I ask, and I am willing to rest my theory on the answer, Is not the phenomenon of the apparent uprising of the shadow, witnessed when no mist is visible, and the atmosphere to the north-west is clear? This furnishes a simple *crux* of the two theories; for any observer can notice whether mist is visible or not, and if not, whether there is any appearance of the uprising of the shadow or not. Until corrected by future observers, I maintain that the phenomenon is seen when there are not "around us cumulus and mist" and "masses of mist coming up from the valley;" in fact, when the air is so calm and clear that the coast-line can be traced at a distance of seventy miles or more. If I am proved to be correct in this opinion, the new theory has not advanced the explanation by a single step. My theory of total internal reflection depends on the difference of temperature between the air in the low country and on the Peak, which is most marked in clear calm weather, ice forming at such times on the Peak, while a fall of the thermometer to 70° F. in the low country is commented on as noteworthy by the newspapers. The conditions described by Mr. Abercromby render the idea of mirage absurd; but they also suggest, if the new theory be correct, the absurdity of there ever having been any mystery about the phenomena of the Shadow of the Peak.

May 30. R. ABBAY.

Upper Wind Currents near the Equator and the Diffusion of Krakatão Dust.

I REGRET that Mr. Abercromby, before writing his interesting and suggestive article under the above heading, had no opportunity of making himself acquainted with the conclusions arrived at by the Krakatão Committee regarding the rate at which the finest ejecta were carried round the world. The velocity he ascribes to the material, viz. 120 miles an hour, deduced apparently from the few observations he quotes, is quite 40 miles an hour in excess of that deduced from the numerous cases treated by Mr. Russell and myself. In one or two cases in the Indian Ocean the velocity does apparently approach to that given by Mr. Abercromby, but these are both exceptional and doubtful, since they were probably due to minor outbursts antecedent to that which gave rise to the grand stream which encircled the globe at an average pace of 80 miles an hour.

Mr. Abercromby has thus accidentally made the problem appear far more formidable than it really is. A constant velocity of 120 miles an hour right round the world, though not outrageous to anyone who reflects on the great mobility of the atmosphere at the height of 100,000 feet or more, certainly makes a considerable demand upon our powers of scientific imagination, while a velocity of only 80 miles an hour, even though constant over the entire equatorial belt, does not appear, at such a height, to be opposed to what is already known of the motions of the atmosphere at the far inferior elevation of the cirrus clouds.

The height of the stratum is certainly a factor which cannot be overlooked, for if we find the average velocity of the wind continually increase as we ascend to the cirrus, it is reasonable to conclude that it rises beyond this limit, and if so a constant