none of the three is dead-beat. Thus the apparent amplitudes of the records will depend on the proximity of the natural period to that of the seismic wave. This may account for the fact that whilst the seismograph and the horizontal-force magnet indicate a maximum of disturbance at from 4.31 to 4.33, the declination magnet indicated more disturbance at 4.36. A movement of 17 mm. on the seismic trace answers to a tilt of fully 9″, but it may be produced in a variety of ways, and no immediate deduction is possible as to either the character or the amplitude of the disturbing motion. C. CHREE.

January 1.

[Added January 4.—An examination of the glass scale used with the magnetic curves shows that a correction of about −0·5 minute is required to the times deduced by it. This brings the above times from the magnetic and seismic curves into even more perfect agreement.]

The Commercial Products of India.

IT is not customary for an author to reply to his reviewers, but I trust you will permit me to depart from that usage. Captain A. T. Gage, superintendent of the Royal Botanic Gardens, Calcutta, stands, to my recent work "The Commercial Products of India," in an entirely different position from an anonymous reviewer, and his opinion, as expressed in certain passages of the notice published in NATURE of December 17, 1908, therefore seems to me to call for a reply.

Captain Gage accuses me of having "unnecessarily spun out" certain articles by a "failure to discriminate between essential and superfluous information and between proved facts and mere opinions not worth recording." He then proceeds to exemplify that contention by quoting one sentence regarding tea. Removed from its context, that particular passage might fall under the condemnation he has passed upon it, but when read in connection with the sentences immediately preceding and following, its meaning and value are, I venture to think, abundantly brought out. The contention, it will be seen, is advanced that even in China tea appears to have been first used as a vegetable or medicine, and that it was not until the fourth century that its modern usage as a beverage began to attract attention. If I am justified in assuming that many of my readers may find interesting what had proved such to myself, it seems likely that the fact that the habit of tea drinking is not very ancient, even in China, will not be regarded as superfluous information.

Then, again, Captain Gage apparently objects to my method of exemplifying the failure, so far, with rhea cultivation in Kangra. I have given prominence (so he affirms) to the fascinating effect on myself personally of the undying faith of a very old lady. Now anyone at all familiar with the recurrent interest in rhea and China grass—aware, in fact, of the extent of capital even now at stake—would hesitate to pronounce rhea, as Captain Gage has done, "a distinctly doubtful crop." The fibre, at all events, is in itself immensely valuable, hence, in reviewing India's position in the controversy of future production, I felt myself compelled to give actual results in preference to dogmatic pronouncements. My position regarding India's future participation is briefly that, while we have the "undying faith" of some of the pioneers, the results so far attained have not been exactly favourable; but I have urged that there is distinctly a future for the crop when certain misleading statements and misconceptions have been effectively removed. In other words, I by no means concur with Captain Gage that rhea is "distinctly a doubtful crop."

But my reviewer has fallen foul of me because my abridged articles on tea and rhea (as he thinks) are longer than the originals. Perhaps I may be permitted to explain that the chief difficulty I experienced in writing the work in question was the necessity, imposed on me, to restrict and restrain my efforts on every hand by calculations or ratios of space to articles, and by the final accomplishment of the entire task within one volume. Captain Gage's criticisms on the science of circumscription are, in fact, examples of that very difficulty, only that he fails in the all-important detail of accuracy. If he will consult again the original work he will perhaps discover that it often happened that a subject was there dealt with under two or more positions. In the new work each had to be disposed of once and for all. Hence *Boehmeria nivea*—Rhea—does not have fifteen pages in the old and sixteen pages in the new work, as Captain Gage affirms, but sixty and sixteen pages respectively. So also *Camellia thea*—Tea—does not have fourteen pages in the old and thirty-five pages in the new work, as Captain Gage also affirms, but eighty-two and thirty-five respectively. The articles on these two subjects thus occupy, as near as possible, the exact spaces reserved for them in the scheme of the new publication. GEORGE WATT.

Richmond, December 19, 1908.

The Isothermal Layer of the Atmosphere.

I HAVE read with much interest the letters on this subject that appeared in NATURE during last February and March, and also the account of the discussion at the British Association (NATURE, October 1, 1908), and my only excuse for re-opening the question at this late date is that a point seems to have been overlooked which appears capable of explaining the phenomena without any appeal to an isothermal layer. Both in the correspondence and in the discussion several physicists cast doubt on the accuracy of the thermograms, but, so far as I have seen, only Mr. A. L. Rotch, at the British Association, mentioned that his instruments were verified for low temperatures and pressures. The following physical effect on the barographs does not appear to have been mentioned, and I should be glad to know what precautions are taken to eliminate it in practice. Pressures are necessarily registered by aneroids, and it appears to be assumed throughout all these discussions that a lower pressure on an aneroid means a higher altitude, but this is not so. In 1892, when I was a temporary observer in Ben Nevis Observatory, Mr. Edward Whymper visited the district to have some fourteen or fifteen aneroids of various sizes compared with the mercurial barometers at the low-level station, and as soon as possible afterwards at the top of the hill. It was invariably found that the indexes kept on falling after the aneroids had been brought to rest in the observatory. The rate of fall was at first fast, but became slower as time went on, and it depended upon the difference of pressures between the two stations and also upon the time taken in transit from one to the other, being greater for greater differences of pressure and less for longer times of transit. The aneroid would tend to give the true pressure immediately on arrival or after some hours, according as the standardisation had been rapid or slow. The effect is due to a kind of elastic fatigue, and was reversed on returning the aneroids to sea-level.

Mr. A. Mallock, F.R.S. (Proc. Roy. Soc., vol. lxxx., p. 530), has shown that up to the altitudes corresponding to pressures of about 100 millimetres of mercury the velocity of the balloons increases slightly, but at these altitudes it decreases so suddenly that the hypothetical balloons with which he deals must there have ceased rising. It is clear that at such altitudes the conditions are most favourable to the operation of elastic fatigue. The change of pressure to which the aneroid has been subjected is considerable; the time of ascent is fairly rapid, and the velocity is suddenly destroyed; but although the balloon may cease to rise, the apparent pressure does not cease to fall. Consequently, when the barogram is deciphered, if the effect of elastic fatigue is ignored, an increased height will be inferred at the same epoch as a constant temperature. There may even be an increase of temperature if the balloon should leak slightly or if the gas should be sluggish in acquiring the low temperature of the air into which it has risen, and, cooling somewhat, causes the balloon to descend slightly. It should be remembered that at these altitudes a small change of pressure corresponds to a very considerable change of altitude, so that this effect of fatigue would be greatly exaggerated. The great differences of altitude at which the isothermal layer