LETTERS TO THE EDITOR.

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Number of Strokes of the Brush in a Picture.

THE number of strokes of the paint brush that go to making a picture is of some scientific interest, so I venture to record two personal experiences. Some years ago I was painted by Graef, a well known German artist, when, finding it very tedious to sit doing nothing, I amused myself by counting the number of strokes per minute that he bestowed on the portrait. He was methodical, and it was easy to calculate their average number, and as I knew only too well the hours, and therefore the number of minutes, I sat to him, the product of the two numbers gave what I wanted to learn. It was 20,000. A year and a half ago I was again painted by the late lamented artist Charles Furse, whose method was totally different from that of Graef. He looked hard at me, mixing his colours the while, then, dashing at the portrait, made his dabs so fast that I had to estimate rather than count them. Proceeding as before, the result, to my great surprise, was the same, 20,000. Large as this number is, it is less than the number of stitches in an ordinary pair of knitted socks. In mine there are 100 rows to each 7 inches of length, and 102 stitches in each row at the widest part. Two such cylinders, each 7 inches long, would require 20,000 stitches, so the socks, though they are only approximately cylinders, but much more than 7 inches long, would require more than that number.

The following point impressed me strongly. Graef had a humorous phrase for the very last stage of his portrait, which was "painting the buttons." Thus, he said, "in five days' time I shall come to the buttons." Four days passed, and the hours and minutes of the last day, when he suddenly and joyfully exclaimed, "I am come to the buttons." I watched at first with amused surprise, followed by an admiration not far from awe. He poised his brush for a moment, made three rapid twists with it, and three well painted buttons were thereby created. The rule of three seemed to show that if so much could be done with three strokes, what an enormous amount of skilled work must go to the painting of a portrait which required 20,000 of them. At the same time, it made me wonder whether painters had mastered the art of getting the maximum result from their labour. I make this remark as a confessed Philistine. Anyhow, I hope that future sitters will beguile their tedium in the same way that I did, and tell the results. F. G.

The Hydrometer as a Seismometer.

A SHORT time ago (NATURE, May 25) I directed attention to a misconception which seemed to prevail among seismologists as to the behaviour of a spirit-level. It may perhaps be useful to point out another fallacy, also of an elementary hydromechanical nature, involved in some of the unsuccessful attempts to record vertical motion.

It was first proposed by Dr. Wagener, we read,[1] to record vertical disturbance by means of a floating buoy free to rise and fall in a vessel of water. The buoy was to provide a steady point when the vessel suffered a vertical disturbance. The device was improved, we are told, by Prof. Thomas Gray, who gave the buoy the form of a hydrometer with only a slender stem projecting above the surface of the water. Prof. Milne experimented with both forms; but even with the hydrometer form, adjusted to a state of the most sluggish stability, several earthquakes left no record of vertical motion. The instrument was abandoned as not sufficiently powerful to be self-registering.

But the theory involved in these attempts is entirely fallacious. Any body, be it buoy or hydrometer, floating in liquid, suffers no displacement whatever relatively to the liquid when the containing vessel is moved vertically. The whole moves as one rigid system. More generally, it may be claimed that any system which is in statical equilibrium, and which would remain undisturbed despite a change in the value of gravity, may suffer a vertical displacement of its supports without any relative disturbance of its parts. The whole of such a system moves as if rigid when displaced vertically. Of such a kind is the hydrometer floating in the vessel filled with liquid; of the same kind, also, is a common balance with equal weights in the two scale-pans. These two systems present a true dynamical analogy, and are equally useless for detecting vertical disturbance. A spring supporting a load; on the other hand, or any form of apparatus the potential energy of which is partly elastic, is not of this class, and is available as a seismometer for vertical motion. It would seem as though a false analogy between the hydrometer and the spring balance had led to the fallacy in question.

The spirit-level (if my previous contention is conceded) is sensitive alike to each of two kinds of disturbance between which it was expected to discriminate. The hydrometer, on the other hand, is insensitive to the very disturbance which it was designed to record. The freezing of the water, indeed (contemplated as an inconvenient contingency with the proposed instrument), would, very precisely, make no difference at all in its behaviour. The instrument has, it is true, been long superseded; but the false principle involved remains as a source of grave confusion for the unwary reader of seismological writings.

It may be remarked that violent earthquakes have been known to damage the rigging of ships in a neighbouring harbour, and to jerk guns from the decks, without any visible movement of the water. Assuming the correctness of the view now urged, a sudden alteration of sea-level would completely account for this. The ship is not in any way spring-borne for such a displacement, but may be subjected to a vertical impulse of any degree of severity.

It should be added, also, that a severe shock of earthquake is credited[1] with having disturbed a hydrometer instrument to the extent of 1·1 mm. If the onus of explanation rests with me, I can only suggest that the effect (if really caused by vertical motion at all) may perhaps have been due to the elasticity of the walls of the containing vessel or of the hydrometer. G. T. BENNETT.

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[1] Milne, "Earthquakes," p. 33; Milne, "Seismology," p. 65; *Trans. Seismological Soc. of Japan*, vol. i., p. 70, vol. iii., p. 54.

The Pressure of Radiation on a Clear Glass Vane.

IN an article on "The Elimination of Gas Action in Experiments on Light Pressure," read before the American Physical Society in December, 1904, and published in the *Physical Review*, May, the writer made the following statement:—"A thin vane of clear glass, accurately vertical and mounted radially, may be used to advantage to demonstrate light pressure. If the light has been filtered through several thicknesses of glass there will be but little absorption by the thin vane and its two surfaces will be warmed nearly equally. Consequently the radiometric effect will be small. The reflection of the radiation at the two surfaces will make a difference of about 16 per cent. between the energy in front of and behind the vane. Hence the light pressure will be about one-sixth of that due to the same light beam falling upon a black surface. The throws for such a vane had only about a ten per cent. variation in a range of air pressures from about 10 mm. to 200 mm. of mercury."

Although a large number of observations had been taken on both clear glass and silvered glass vanes, the data were not published at that time. It was then felt that the elimination of gas action was the important point, and the final statement in the paragraph quoted, that the throws for such a vane had only a 10 per cent. variation in a range of air pressures from about 10 mm. to 200 mm. of mercury, was considered sufficient experimental evidence that gas action had been eliminated.

Since this paper appeared, the writer has learned that there is a difference of views among mathematical physicists concerning the pressure of radiation on a non-absorbing medium. On this account he has gathered

[1] *Trans. Seismological Soc. of Japan*, vol. iii., p. 55.