equality of intensity of the two bromine lines on all occasions combined with the certainty of the chemical value of its mean weight (79·916) justify the use of its heavier line in this connexion as a reference line of mass 81·9. Taking this value, the strontium line comes out 87·8 and hence its most probable mean atomic weight rather more than 87·7, a value higher than the chemical one but not seriously discordant.

Experiments with barium, employing long exposures, have resulted in the identification of its strongest line as 138 with a most probable mass of 137·8. There is certainly no other line comparable with this in intensity. Search for lighter isotopes suggested by the chemical atomic weight 137·37 is prevented for the time being by the penumbra of the enormously strong line of iodine, with which element parts of the present apparatus are saturated.

Although mass rays of the heavier rare earth elements are exceedingly difficult to produce, and the resolving power of the present instrument also limits the possibility of work in this field, some progress has been made. Lanthanum (138·91) gives a single line of satisfactory strength at 139 and may therefore be taken as a simple element. A commercial sample of praseodymium (140·92) showed the same line strongly but with indication of one at 141, so the experiment was repeated with a highly purified sample prepared by Auer von Welsbach. This gave only one line at 141, indicating that praseodymium is most probably simple. The results with rare earth elements of even atomic number are much less definite. Neodymium (144·27) gives an indistinct band 142 to 150, suggesting several isotopes not differing greatly in relative proportion. Erbium (167·7) shows a similar faint effect from 164 to 176, doubtless due to its isotopes and those of other rare earth elements present as impurities.

Further attempts to obtain results with zirconium, niobium, and molybdenum have again been completely unsuccessful.

F. W. ASTON.

Cavendish Laboratory,
Cambridge, June 2.

John Harrison.

I REGRET that I have been prevented by illness from expressing an opinion previously as to the nature and value of John Harrison's horological inventions, which have been recently discussed in the columns of NATURE by "R. A. S." and Mr. A. R. Hinks (April 19, p. 570). Having had more opportunities than most people of examining and analysing Harrison's work and writings, I should like, if it is not too late, to have an opportunity of placing on record a considered opinion.

Harrison's five marine timekeepers, the construction of which occupied a period of forty-two years (1728–1770), afford ample evidence of a steady progress towards mechanical efficiency and improved timekeeping. Wooden wheels appear in No. 1 only, and while certain features, such as the train, become complicated by the addition of a remontoir, others, such as the pivoting and control of the balances, etc., are notably simplified. The finished product, No. 5 (and its predecessor, No. 4) is certainly not "clumsy," although too complicated and delicate to allow of being reproduced in quantity at a paying price.

The passage which "R. A. S." quotes from my book as to the comparative merits of Harrison and Le Roy fully expresses my opinion of their *relative* merits; but surely it would be no valid detraction of, say, Barrow's work on the theory of limits to say that Newton's was based on broader principles. Barrow paved the way for Newton; and, similarly, Harrison paved the way for Berthoud, Arnold, Earnshaw, and even for Le Roy himself—in that he showed that a marine timekeeper was not, as hitherto thought, a mechanical impossibility.

It is not quite fair to Harrison to suggest that he loved complication for its own sake (although it is a valid objection against some of his successors, such as J. G. Ulrich); and it is necessary, before passing a final judgment upon his work, to remember that all his devices, gridiron pendulum, grasshopper escapement, large pendulum arcs, cycloidal cheeks, remontoirs, etc. form part of a harmonious system, which should be judged as a *whole*. Any one who has the patience to read through his last pamphlet, "A Description of such mechanism . . ." (1775), will be convinced of this fact. That this system is no longer used is proof that it was too complicated and delicate to be commercially practicable; but it does not prove that it was not efficient. Harrison was, like many pioneers, a man who combined genius with imperfect education. That professional watchmakers should have scented a chance of profit in marine time-keepers and superseded his work (except in the maintaining power, still used in all chronometers) is not surprising; but they would never have moved in the matter had it not been for Harrison's example.

I entirely concur with Mr. Hinks's view—that since Harrison accomplished, by his comparatively amateurish mechanism, what no professional watchmaker could do, it is scarcely fair to decry it because it is now obsolete. Nor is it fair to suggest that all Harrison's work was "retrograde." He tried, for example, to make a compensation balance, and that he failed is no proof that he did not appreciate its superiority over his (perfectly practicable) compensation curb. Witness this passage from the pamphlet previously referred to.

". . . and I can now boldly say, that if the Provision for Heat and Cold could properly be in the Ballance itself, as it is in my Pendulum, the Watch . . . would then perform to a few seconds in a Year. . ."

With regard to the grasshopper escapement, which "R. A. S." refers to as retrograde, I can only say that I have recently had occasion, in connexion with the repairing of Harrison's No. 2 timekeeper, to go into the theory and action of this contrivance, and that my opinion of it has been considerably enhanced. Although I would not be understood as advocating the use of so complicated and delicate an escapement in anything but a very perfect and well-attended clock, I think that its performance would then considerably surprise the supporters of either the dead-beat or gravity escapements. I should add that the "grasshopper" escapement, as generally figured in horological works, is but a faint travesty of Harrison's own design.

RUPERT T. GOULD.

A Test for Possible X-ray Phosphorescence.

IN examining photographs of the β-ray tracks produced in air, C. T. R. Wilson (Proc. Roy. Soc. A, vol. 104, p. 1, 1923) found that two tracks which were undoubtedly pairs were not alike: one was sharp and the other was a diffuse track; that is, pairs exist of which the two components have been ejected with an appreciable time interval. He roughly estimates this time interval to be of the order of 0·001 of a second. The first track he attributed to the photoelectron ejected by the primary X-ray, and the second track to an effect of the resulting fluorescent radiation. It was decided to search by a more direct method for the existence of such phosphorescence in the secondary X-rays from solid radiators.

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