Letters

Mendeliana

I should like to comment on the news paragraph headed "Mendeliana" [*Science* **127**, 77 (1958)]. The facts are that the monastery in Brno, Czechoslovakia, where Mendel lived and where the museum was, got a direct hit in the Allied bombing in 1945 and a good many Mendeliana were destroyed. However, when I visited Brno in 1947, they were rebuilding the monastery and had a temporary exhibition of Mendeliana. The implication that the Czechs were not interested in preserving records of Mendel is quite false, even though in 1947 (not now) Mendel-Morganism was definitely frowned upon.

When I was in Brno I was on my own and was fortunate in finding an English-speaking curator at the folk museum who took me to the Mendel museum. I quote what I actually wrote in my diary at the time: "An Augustine monk who spoke no English met us and shewed us round. I was terribly disappointed to find that Mendel's experimental plot was now a rather unkempt flower garden, with a monument commemorating his birth centenary, 1822–1922 (inscribed in English as well as other languages). The priest shewed us some beans growing on a rubbish dump which he *said* were direct descendents of Mendel's beans! . . . I was intrigued [in the museum] by Mendel's bed (he died on his settee while sitting up)—a lovely walnut one, with side pieces like Norwegian beds. Hank [the folk museum curator] had never seen a bed with side pieces before. It did not look the kind of hard bed you would expect of a monk. He [Mendel] was not a very good plant presser" (this referred to the very poorly pressed herbarium of Mendel's which was on display with, so far as I can remember now—I haven't recorded it—some magnifying lens or simple microscope).

It is 11 years already since I was there; the whole of Brno still had a very bombed look, and I do not think Mendeliana were any more neglected than anything else at that time. It must be remembered that Brno was a very German town and was going through a difficult period. Another English biologist, who visited Brno in 1954, tells me that when she went, she found the monastery now closed (I believe that there were only eight monks when I was there) and the Mendeliana housed in a special museum. I hope that someone from the University of Illinois will find the opportunity to make contact with whosoever is in charge of the Mendel Museum in Brno.

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Sex Determination

A recent paper by M. J. Gordon [*Proc. Natl. Acad. Sci. U.S.* **43**, 913 (1957)], mentioned in "News of Science" [*Science* **126**, 1059 (1957)] puts forward a claim of success in separating the two kinds of sperm, reporting data for 31 litters of rabbits. These data contain a peculiar heterogeneity which should be noticed. In all cases, sexing involved examination of the gonads; for the last 13 litters the accessory organs also were examined. The results for these last 13 litters differ from the first 18, with respect to the *difference* between sex ratios when males were expected and when females were expected, being statistically significant beyond the level of 1 in 1000. Among the last 13 litters there is obviously no significant effect of electrophoresis, the sex ratios being 17 males to 22 females when males were expected and 16 males to 29 females when females were expected (nor is this changed by excluding the three litters for which "incorrect technique was suspected. . . .").

No explanation of this difference which is compatible with all the reported circumstances suggests itself. For the time being, therefore, three possibilities appear equally admissible: (i) electrophoresis is ineffective, but accidents of sampling or unknown factors produced a strong appearance of effect in the first 18 litters; (ii) electrophoresis is effective, but accidents of sampling almost totally obscured the effect in the last 13 litters; or (iii) electrophoresis is effective in some circumstances not yet defined. Obviously it is premature to select one of these three rather than another.

Any a posteriori analysis of data will raise in some minds the question of whether the tests of significance performed, and, more important, those reported, were suggested by the data themselves and, hence, whether the significance levels are misleading. My approach was really a priori in that I sought heterogeneity at each change of technique, and I mention in passing that changing after 8 litters to "blind" sexing for the next 10 litters did not alter the difference observed between the two sex ratios.

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Founder of Hydrographic Office

It is not correct to state, as Hugh Odishaw does [*Science* **127**, 124 (1958)] that Matthew F. Maury was "the founder of the U.S. Navy Hydrographic Office." Strictly speaking, the Hydrographic Office was established by Act of Congress of 21 June 1866 (14 Stat. L. 69), and its