In order to discriminate CC from Cc plants progenies from these, which are seen to number 463 together, must have been grown on in 1864. In addition to abundant new unifactorial data the additional bifactorial data supplied by the experiments is seen to be large. 175 of the plants were heterozygous for both of the two seed characters, and, if 30 seeds from each had been classified, these would have given 5250 seeds, nearly ten times as many as the 556 reported from the bifactorial experiment. The classification of these plants as double heterozygotes must indeed have required that about half this number of seeds from each plant were examined. In the following year also nine-sixteenths of the progeny of 127 F_2 plants, or about 815 F_3 plants, must have borne seeds segregating in the 9 : 3 : 3 : 1 ratio, so that a further 24,000 seeds could have been so classified in 1864. Evidently, however, Mendel felt that the complete classification of 529 plants in the bifactorial experiment was sufficient; he does not even add, for the simultaneous segregation of A*a* and B*b*, the 639 plants completely classified in the trifactorial experiment, which suffice to raise the recombination fraction significantly higher than 46·56 per cent. (from 44·9 per cent.).

TABLE III.—*Comparison of Numbers reported with Uncorrected and Corrected Expectations.*

	Number of plants tested.	Number of non-segregating progenies observed.	Number expected.		Deviation.	
			Without correction.	Corrected.	Without correction.	Corrected.
1st group of experiments.	600	201	200·0	222·5	+1·0	−21·5
Trifactorial experiment .	473	152	157·7	175·4	−5·7	−23·4
Total	1073	353	357·7	397·9	−4·7	−44·9

In the case of the 600 plants tested for homozygosity in the first group of experiments Mendel states his practice to have been to sow ten seeds from each self-fertilized plant. In the case of the 473 plants with coloured flowers from the trifactorial cross he does not restate his procedure. It was presumably the same as before. As before, however, it leads to the difficulty that between 5 and 6 per cent. of heterozygous plants so tested would give only coloured progeny, so that the expected ratio of those showing segregation to those not showing it is really lower than 2 : 1, while Mendel's reported observations agree with the uncorrected theory.

The comparisons are shown in Table III. A total deviation of the magnitude observed, and in the right direction, is only to be expected once in 444 trials; there is therefore here a serious discrepancy.

If we could believe that Mendel changed his previous practice, and in 1863 went to the great labour of back-crossing the 473 doubtful plants, the data could be explained, for in such progenies misclassification would be only about one-fiftieth part as frequent as in progenies by self-fertilization. Equally, if we could suppose that larger progenies, say fifteen plants, were grown on this occasion, the greater part of the discrepancy would be removed. However, even using families of 10 plants the number required is more than Mendel had assigned to any previous experiment, and there is no reason for thinking that he ever grew so many as 7000 experimental plants in one year, apart from his routine tests [2]. Such explanations, moreover, could not explain the discrepancy observed in the first group of experiments, in which the procedure is specified, without the occurrence of a coincidence of considerable improbability.

An explanation of a different type is that the selection of plants for testing favoured the heterozygotes. In the first series of experiments the selection might have been made in the garden, or, if the whole crop was harvested, on the dry plants. In either case the larger plants might have been unconsciously preferred. It is also not impossible that, in some crosses at least, the heterozygotes may have been on the average larger than sister homozygotes. The difficulties to accepting such an explanation as complete are three. (i) In the tri-factorial experiment there was no selection, for all plants grown must have been tested. The results here do not, however, differ in the postulated direction from those of the first series. On the contrary, they show an even larger discrepancy. (ii) It is improbable that the supposed compensating selection of heterozygotes should have been equally effective in the case of five different factors. (iii) The total compensation for all five factors (21·5 plants) must be supposed to be greater than would be needed (16·8 plants) if families of 11 had been grown, and less than would be needed (30·0) if 9 only had been grown, though nearly exactly right for the actual number 10 of F_3 plants in each progeny (22·5).

The possibility that the data for the trifactorial experiment do not represent objective counts, but are the product of some process of sophistication, is not incapable of being tested. Fictitious data can seldom survive a careful scrutiny, and, since most men underestimate the frequency of large deviations arising by chance, such data may be

[2] The area available is given by Iltis as only 7 m. by 35 m. Dr. Rasmussen estimates that he might have grown 4000–5000 plants in this area,