demonstrative, because the comparisons on which it rested were of too sketchy and remote a character and the gaps bridged by hypothesis were too wide, and so morphology was led in many cases to wild and ridiculous conclusions. But the opinion of an expert in systematics is not to be dismissed as mere "verba". It is really a deduction from thousands of relevant facts which the critic has neither the time nor experience to be able to consider.

Prof. Haldane concludes with a disquisition on the subject of mutations, into which I will not follow him. As I said in my previous letter, it would have been an easy matter to have gone over the whole of his discourse and applied destructive criticism to every part of it, but I determined to confine myself to meeting his objections to the evidence adduced by me. But I shall conclude by placing on record my view of the nature of mutations and trusting to the future to vindicate it.

Johannsen, who invented the term 'gene', later publicly expressed his regret that he had ever done so, and defined mutations (or genes) as "superficial disturbances of the chromosomes". They have nothing to do with the characters of the natural races of animals (or plants). With this opinion I fully agree. Mutations begin differently and are inherited differently from true racial characters. This is the opinion of the best systematists whom I know, and surely in the long run the opinion of those who really understand what species, sub-species, and biological races are will ultimately prevail.

E. W. MACBRIDE.

43 Elm Park Gardens,
Chelsea, S.W.10,
July 2.

PROF. J. B. S. HALDANE, in his reply (NATURE, July 2) to Prof. E. W. MacBride's letter, once more returns to the subject of my experiments with the sawfly *Pontania salicis* and its transference from the foodplant *Salix Andersoniana* to *S. rubra*. As this portion of his letter is misleading, and might appear convincing to anyone not acquainted with the facts, it seems that a statement from me is necessary.

Actually, owing to Prof. Haldane's lack of knowledge of the geographical distribution of the two willows in question, not one of his remarks is relevant to the subject. It still remains a fact that *S. rubra* is a rare hybrid between *S. purpurea* and *S. viminalis*, and to challenge this on the ground that Druce records it from sixty-nine vice-counties shows a lack of appreciation of the basis of such a list, and of the numbers of individuals which represent such a hybrid in Nature. Moreover, the introduction of a reference to the value of *S. rubra* as an osier only makes matters worse; osiers are not cultivated here.

Again, when Prof. Haldane quotes Druce as recording *Salix Andersoniana* from thirty-four vice-counties, scientific accuracy should have caused him to state that these thirty-four (except for an outlier in Glamorgan) lie north of a line drawn across the country from N.E. Yorks to Lancashire. The probability of *S. Andersoniana* coming into contact with osier beds is thus very remote. Further, his statements imply that he does not realise that, in those northern and Scottish counties where both plants occur, the favoured habitats of *S. Andersoniana* differ widely from those of *S. viminalis*, *S. purpurea*, and, consequently, of their hybrid. In Durham, for example, *S. Andersoniana* is a plant which grows in profusion on the sea banks on the magnesian limestone and then jumps to subalpine areas well inland, whilst *S. viminalis*, *S. purpurea*, and *S. rubra* occupy the intervening zone. Very rarely indeed do *S. Andersoniana* and *S. rubra* overlap; in fact, in spite of very careful exploration to settle this very point, I cannot point to one locality which they possess in common!

Prof. Haldane ought to have realised that before I commenced the experiments I should take the elementary precautions of making sure (1) that the small patch of *S. rubra* selected for the work was free from *Pontania* galls, (2) that no other species of willows near carried the same species, and (3) that the colony of *S. Andersoniana* from which the transference was made was in a district remote from contact with *S. rubra*.

J. W. HESLOP HARRISON.

Armstrong College,
Newcastle-upon-Tyne,
July 4.

Filtration of Plant Viruses

THE preparation of graded collodion membranes has been greatly improved of recent years by W. J. Elford, who has developed a technique with which he can produce membranes of highly uniform structure and easily determinable average pore size. These he has used in an investigation of the probable sizes of bacteriophage and various animal viruses. We have examined a number of plant viruses with membranes prepared according to Elford's methods and with his generous help and advice; and a short statement of some of our experiences and results may be of interest in themselves and of value to others.

First as to the method of preparation of the membranes. In our hands it has not proved easy to obtain consistent results. The eventual pore size is dependent on the rate of evaporation from the surface of the liquid and is also enormously affected by the presence of traces of water; and very small local or general differences in atmospheric humidity, slight currents of air, and the like affect the final product to a surprising extent. There may be marked difference of pore size between the central area of a membrane and the portions lying nearer the rim. Such difficulties are not insuperable, but the most painstaking attention to detail is essential, and at present we find it advisable to standardise every membrane individually before use. Standardisation leaves room for some degree of latitude in the data, and, leaving aside theoretical considerations as to the applicability of the formula used to membranes of this structure, in our hands repeated standardisation of the same individual membrane has shown a progressive diminution in average pore size. These difficulties are gradually disappearing, but we mention them as a warning of the necessity of checking one's results with the greatest care.

The virus material we have used consists of juice extracted from diseased plants. This juice is very complex and may contain tannins, resins, and other readily precipitated materials which do not occur in animal tissues. As a consequence there is a rapid clogging of the pores, especially of the finer membranes, in spite of very thorough preliminary clarification by passage first through paper pulp and then through a coarse ($0{\cdot}6\mu$ or $0{\cdot}7\mu$) membrane. With some plants, for example, tomatoes, especially if more than a very few weeks old, this plugging is so thorough as to make the results quite useless as a guide to the size of the particles. With tobacco and certain other plants it is much less serious, but it is always present to some extent. To this is perhaps to be attributed the fact that we do not get a sharp endpoint. We do not find that the virus passes undiminished in quantity through the series of membranes down to a definite pore size, at which it no longer passes: there is a progressive reduction in amount all the way down. To take one example (where the quantity of virus