Mr. Williams :

The artificial synthesis of experimental systems such as rooted leaves seems to be an experimental approach of great popularity among biochemists and plant physiologists. It is apparent to me that such systems involve biochemical and physical manipulations, which could just possibly not be representative of relationships as they exist among plants in coenocytic contact with the total environment. The justification of your methodology would be to accumulate a background of knowledge exploring the degree to which findings in the artificial system correlate with actual processes in the living plants **in vivo.**

Could you tell me whether in your own work you find it necessary to document your findings against the background of the living plant, or do you consider that your methodology is already established?

I would like to make reference to your work on CCC in relation to leaf area, and tuberisation. There are suggestions from work done at this university by Spence and Haynes that on the basis of field experiments, sweet potatoes respond to manipulation of leaf area, for example by adding higher levels of nitrogen to the soil. It responds to this sort of manipulation, by delaying — roughly in proportion to the degree to which the leaf area is increased — the time required for them to attain maturity. I think this is what Prof. Milthorpe has termed ontogenetic drift.

Dr. Humphries :

The plant physiologist is up against a very difficult problem in dealing with the whole intact plant, and anyway if we can get a simple system to work on, to give us some idea of the processes that take place in the plant, it is very welcome. This is why we introduced this rooted leaf system. It is, in fact, not such a simple system as we hoped it would be, because, as Dr. Wilson points out, the lamina itself, as well as the root system, acts as a considerable sink for the carbohydrate, and you cannot really say that this is just a simple sort of sink relation.

Dr. Wilson :

With reference to Mr. Williams' question, a major consideration of ours has been the calibration of processes in systems which we like to call phyto-models, against the same processes, as they occur in the intact plant. For example, carbohydrate accumulation is known to take place on the intact plant with nitrogen deficiency. Carbohydrate accumulation also occurred in the rooted leaf. It is possible, therefore, to study carbohydrate accumulation, using the rooted leaf, provided this process could be calibrated against the parallel process in the intact plant. We have measured the rate of leaf production in intact plants compared with that in grafted plants. In the grafted plants, in fact, leaf production was reduced, compared with the intact plants. But this did not preclude the appearance of what we felt were real effects of different scions and root stocks on tuberisation.

Dr. Sidrak :

Are there any known factors, externally or internally, which affect the kinin production in any of the plant studies?

Dr. Humphries :

I think, just to summarize our knowledge on this, all that is being done with the natural system so far, is that fruits have been taken and extracted, and the kinin content estimated. Very little is known about the development of the kinins during the development of the fruit, and we know practically nothing about its determining factors.

Dr. De Gras :

You have said that when you observed shortening of the stems, the shortening could be interpreted as a reduction of the sink but is not there also an increase in the part of the leaf which reached compensation point?

Dr. Humphries :

This is true, and we thought first of all that this was a possibility, but when we did the experiment with potato, where the stem is also shortened, and the leaves more crowded, we did in fact, get an increase in the net assomilation rate, so we thought that probably the stem shortening was the chief factor.

Dr. Spence :

In view of the importance attached to potassium by Dr. Fujise, could Dr. Wilson or Prof. Humphries state whether they have taken this into account in their measurements of the efficiency of their models.

Dr. Wilson :

Yes, to some extent. We have looked not only at nitrogen deficiency using the rooted leaf system, but we have looked at iron deficiency, potassium deficiency, calcium deficiency, sulphur deficiency. The pattern of carbohydrate accumulation is quite similar with nitrogen and sulphur deficiency. In other words a carbohydrate saturation point was arrived at, in which carbohydrate accumulated in the laminae, petioles and in the roots.

With calcium deficiency, carbohydrates tended to accumulate in the laminae. We could not demonstrate high levels of carbohydrate accumulation in the petioles. This result, of course, is based only on a qualitative assessment of carbohydrate status of tissues by the iodine test for starch.

With potassium deficiency, the results were not conclusive in that replicates gave different results. The model rooted leaf took a very long time to become potassium deficient, but when it seemed so, as indicated by the potassium content of the laminae. then the pattern of carbohydrate accumulation was rather erratic. Sometimes carbohydrates accumulated in the leaves, sometimes in the petioles, and sometimes in the roots.

With iron deficiency there was never any marked accumulation of carbohydrates This is perhaps because, either iron deficiency reduces the rate of photosynthesis dramatically or that we hadn't produced symptoms or conditions of iron deficiencies in the rooted leaf.

Dr. Humphries :

We have not done any experiments where we varied the potassium, we only varied the nitrogen.

Dr. Royes :

I am very interested in Dr. Wilson's grafting experiments. It is known that O49 is a higher yielder or a little bit higher than C9 and that the difference in foliage is considerable. Is it possible that with the C9 scion, O49 graft, the higher production may be due either to the higher efficiency or greater leaf area of O49, and possibly the greater sink potential of the C9 scion, an do we have any method that would help a breeder to measure the sink potential?

Dr. Wilson :

I made a suggestion in the last sentence of my paper, that grafted plants could perhaps be used for determining the capacity for tuberisation of an individual variety. This suggestion was made because grafted plants highlighted only after eight weeks of growth, the already well known difference in yield potential between O49 and C9 and this, is at the time when the maximum weight of the tuber was only 132 grams. So, perhaps a calibration, with reference to Mr. Williams' question, could be devised whereby the yield potential of a new variety, could be determined by the capacity of its root stock to tuberize effectively when joined to the scion of a low yielding variety e.g. R. 38. This approach perhaps, may give results, but calibration of the processes involved must be carefully done before such experiments can be attempted.

Dr. Humphries :

I do not think that there is any method at the moment of estimating sink potential, but if we can establish that sinks are dependent on certain growth substances, then perhaps we can get somewhere near.

Dr. Carr :

I would like to ask Dr. Humphries whether he knows of any work on carrots, with regard to source and sink relationships, and secondly, whether he thinks that there are any compounds or other treatments which may induce early formation of a sink in plants like carrots or sugar beet.

Dr. Humphries :

I cannot recall, at the moment, any work on carrots. I think one reason why carrots are so very little worked on is the difficulty of measuring its leaf area. I do not know whether Prof. Milthorpe could answer this question.

Prof. Milthorpe :

No, I do not think that one can change the time of initiation of root growth in sugar beet, or possibly in carrots very much. I think that these are very fixed effects, and Dr. Humphries' slides where he looked at the effect of CCC on sugar-beet emphasises this.