THE ERRONEOUS PHYSIOLOGY OF THE ELEMENTARY BOTANICAL TEXT-BOOKS.*

By W. F. Ganong,
Smith College, Northampton, Mass.

Most of us can remember when plant physiology was a study only for the favored student in a European university. To-day its elements are taught throughout the country in the schools. The interval has been marked by an effort so to simplify its appliances and methods that even a child may use them. The spirit of the movement is admirable, but its results are not wholly fortunate, mainly because it has been carried too far. For this extreme of simplification has too often produced appliances so crude as to encourage slovenliness of hand and mind, methods so slipshod as to inculcate wholly wrong ideas of the nature of scientific work, and results so illogically grounded that it is only the badness of our teaching which saves us from having our own students point out their fallacy. Most of us concerned with this subject are guilty of perpetuating if not originating some of these errors; and as I observe them passing along without question from book to book, seemingly in secure immortality, I am led to exclaim, truly the evil that men do lives after them! But we have now, perforce, reached a limit in the development of simplification, and it is time for a new mutation. It should take the direction of a critical examination of our existent material to the end of rejecting the bad and further perfecting the good. It is, I think, true that a majority of the physiological experiments in our elementary books are scientifically objectionable from some point of view, and I propose here to point out some of the worst of the errors involved.

We turn first to the vital subject of photosynthesis. Perhaps we are already freed from that experiment, not long since recommended in some of our foremost books, in which a leaf of a land plant, such as lettuce, etc., when immersed in a tumbler of water and placed in the sun, was said to give off copious bubbles of oxygen; and to make you believe it there was a picture showing the bubbles on their way to the surface. As an old chronicler would have put it,—"this is a lye." Those bubbles are not oxy-

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gen from the leaf, but air dissolved in the cold tap water generally used, and released by the rise of temperature in the warm room and bright light. They collect on the sides of the dish as well as on the leaf, on dead leaves as well as on living ones, in darkness as well as in light when the temperature is raised as high, while analysis shows that the gas contains no more oxygen than is proper to dissolved air. And besides the bubbles do not rise unless the dish is shaken. It is true that some oxygen can be given off by land plants immersed in water, especially by such as keep a film of air about them, but the quantity is insignificant and it dissolves in the water without showing perceptible bubbles. This experiment is, however, vanishing from our books; not so another almost equally bad which is found in every elementary book known to me, including two of my own, viz., the one in which, to demonstrate the necessity for light in starch-formation, corks or pieces of tin-foil are pinned to the opposite sides of a leaf, the effect exhibited when the iodine test is applied being supposed to be due to the absence of light. In fact it is due as much to the absence of carbon dioxide, as is readily shown by control experiments. Even a loose arrangement of cork or tinfoil is not sufficient, since the carbon dioxide, owing to its low tension in the atmosphere, must have the freest possible access to the leaf in order to afford it a sufficient supply. It is an evidence of a general awakening to our deficiencies in this direction that not only do some of our later books insist that the covering substance must be loosely arranged, but also that the flaw in this experiment has been pointed out in at least three publications of late, by Miss Haug in the *Botanical Gazette,* November, 1903; by C. W. King in *Torreya,* April, 1905, and in the *Plant World,* October and December, 1905. The ideal way to perform this experiment is this,—to apply the opaque object only to the upper surface, where there are usually few or no stomata, (preferably selecting a leaf with none above), and to cover the corresponding lower surface with a perfectly ventilated dark box, which will permit free access of the carbon dioxide to the stomata.

Another error is involved in our usual method of proving the need for carbon dioxide in photosynthesis. Two similar plants are placed under separate bell-jars to which access of the atmosphere is allowed; in one case, however, the opening is screened by a carbon-dioxide absorbing substance and in the other by a mechanically similar but chemically neutral material. The iodine test, when applied after some hours, is supposed to show the presence of starch in the latter but not the former plant. In theory this experiment is faultless, but as usually carried out, and especially as pictured in the books, it is practically fallacious, because the neutral material is used in such quantity and fineness as practically to stop all diffusion of carbon dioxide into the bell-jar. Under these conditions the results are always imperfect, though they are the better in proportion as the bell-jar is larger than the plant; if the plant is large and bell-jar small the result is negative. The reason is plain,—the larger bell-jar may enclose enough carbon dioxide at the start to permit an appreciable color with the iodine test even though no more enters. The fact that the result depends upon the relative sizes of plant and bell-jar shows the fallacy of the method, for it ought to be independent of this factor. The remedy is to be found either through use of very small plants (or single leaves) in very large sealed jars, one of which contains a carbon dioxide absorber, or by drawing air continuously through the screens into both bell-jars by means of an aspirator.

An experimental defect in connection with the release of oxygen by water-plants in photosynthesis is implied by those books which figure a cluster of *Elodea* or other water-weed giving off copious oxygen when enclosed under a funnel which rests upon the bottom of the containing vessel, which, further, is often shown of little greater diameter than the funnel itself. In fact, under such circumstances, the release of oxygen is soon checked, because the small quantity of carbon dioxide caught under the funnel is exhausted, and the way is almost closed to the accession of more. For success in this experiment one should keep the funnel well up from the bottom and should use either a large containing vessel with an abundant surface for the absorption of carbon dioxide, or better, should add that gas from a generator. Further, the gas released by water plants in this experiment, especially when the plants are resting in winter and when no carbon dioxide is added to the water, is by no means all oxygen, but is frequently not rich enough in that gas to enflame the classical glowing splinter.

We turn next to some erroneous experiments connected with the water stream through the plant. Of these the greatest relate to root pressure. There is much confusion between root-exudation or bleeding and root-pressure, two very different phenomena. Almost universal is the recommendation for the determination of pressure, of the use of open mercury gauges of several mm. diameters, which not only require a large amount of exudation to push the mercury up to any appreciable height, but also are of such proportions that only a fraction of an atmosphere can be registered before the water pushes past the mercury. I know the use of such gauges has the sanction of the highest authority, but they are bad, nevertheless. The method is fallacious for the reason that it ignores the fact that there is no relation whatever between the quantity of water given off and the pressure under which it is given off. Frequently the quantity given off is very small, though its pressure is high; and when, in such a case, a large gauge is used, the quantity is enough only to push the mercury a short distance, which would be taken to mean a very low pressure. It is for this reason that the open gauges nearly always give very low pressures, while in fact the pressures of even common greenhouse plants frequently equal or exceed an atmosphere. The remedy is to be found in the use of gauges so small in bore and length that the pressure is indicated by an inappreciable quantity of water. For this purpose small closed gauges, in which the pressure is calculated by Mariotte's law, are best, though open gauges may be used if of very small bore and provided with a mercury reservoir in the descending arm. Nor do I find it possible to fill the lower arm of a pot-hooked shaped gauge between mercury and plant with water, as the books glibly tell me to do, without use of a method too involved for ordinary use. The ignoring of the independence of quantity and intensity in pressures is seen also in the method sometimes recommended of measuring the swelling-power of seeds by making them press against a spring balance. It is only by elaborate preliminary experimentation and adjustment of the tension of the balance in correlation with the amount of swelling that this method can be made to give results anywhere near the truth.

In connection with transpiration minor errors are current. One is too great faith in potometers, which in reality require careful adjustment of parts in construction and many precautions in use to give results of any value. Again that experiment in which a plant, or a membrane, affixed air tight to the top of a water-filled tube dipping into mercury at its bottom, shows a rise of the mercury as transpiration or evaporation proceeds is said to illustrate the lifting power of transpiration. If it is clearly understood that it is not the transpiration but the external atmospheric pressure which does the actual lifting, and moreover, that the physical conditions of the experiment, viz., atmospheric pressure on a free surface forcing liquid into a vacuum-forming tube, do not exist in connection with the living plant, then the experiment is accurate enough, though of little use in throwing any light upon actual processes in the plant.

Among minor errors may be mentioned the assumption that the extinguishment of a candle inserted into a closed space means the presence of carbon dioxide, whereas it may mean this or it may mean the presence of any other neutral gas, or it may mean simply a deficiency of oxygen. Again it is sometimes said that by means of a flame the oxygen may be burned from a confined space and replaced wholly by carbon dioxide, and I know a book devoted to the chemistry of plants which gives supposedly experimental evidence in support of this error. In fact, an ordinary flame will not burn as a rule over about 3 per cent of the oxygen from a confined space before it goes out.

It is plain that our physiological errors are due to a combination of circumstances. As a rule they originate in somebody's carelessness, perhaps in his recommendation of an experiment devised in the study arm-chair and never properly tested in the laboratory. Then they are perpetuated through our slavish reverence for authority, which has so great a grasp upon us that when we do seriously test an experiment we usually feel bound to make the result come right; and we even blame ourselves, and not the original author (and much less the phenomena of Nature), when these results fail to come as advertised. Besides we are usually perfectly satisfied when a result comes according to expectation, never stopping to ask whether that result may not be only accidentally and not logically correct,—something which would never be possible if we but employed in demonstration the same critical-control spirit we always use in an investigation. Then, too, we have acquired the makeshift habit (when we should have the efficiency habit), to such a degree that many of us actually prefer an inefficient thing improvised in the laboratory to an efficient one already properly made; and we often go to more trouble and expense to obtain materials to clumsily home-make something than would be required to obtain it in efficient form by purchase. It should be remembered that while the improvisation of physiological appliances from odds and ends does inculcate ingenuity and manual dexterity, these are not the ends of botanical study; and I believe that observation must show others, as it has shown me, that the student's time and energy are usually so completely absorbed by the difficulties of constructing the apparatus and making it work that he has little of either remaining for the study of the phenomena of the plant. I believe, too, that we commonly attempt too many experiments, and further, that we tend to select them for their practicability, perhaps even for their showiness, rather than for their physiological importance.

The first step is to know our deficiencies; the second is to correct them. First of all we should cut loose from authority and examine every experiment in our elementary books with the same critical and control methods that we would apply to an investigation, and we should always use those control methods in presenting them to our classes. Then we should abandon the attempt to improvise or home-make all our apparatus, doing only enough of this to give the students some training in manipulation. We should purchase efficient instruments whenever possible, precisely as is done in chemistry and physics, gradually, as there, accumulating a permanent laboratory supply. And whenever home-made apparatus is constructed we should insist that it be done in a workmanlike manner, with greater mechanical neatness and exactness than now usually prevails. Then we should concentrate upon the experiments illustrating the more fundamental topics, and treat them fully and logically. Under all circumstances we should insist that every experiment be performed with cleanliness, neatness, mechanical exactness, even a kind of artistic attention to details, such as reflects the spirit of genuine scientific inquiry. It is only by such reforms that the scientific spirit can be trained and plant physiology made a factor of value in education.

The Production of Sulphuric Acid is one of the most important chemical industries. The following are the producing countries in the order of their output:

England, ...	1,100,000 tons per annum.
Germany, ...	880,000 tons per annum.
United States, ...	870,000 tons per annum.
France, ...	500,000 tons per annum.
Italy, ...	200,000 tons per annum.
Austria, ...	200,000 tons per annum.
Belgium, ...	164,000 tons per annum.
Russia, ...	125,000 tons per annum.
Japan, ...	50,000 tons per annum.

The production of other countries is insignificant.—*Revue de Chimie Industrielle.*