drops have fallen far enough, the field between the earth and the bottom of the cloud is thus reversed, in confirmation of what experiment shows. The polar character of the cloud's charges is also in agreement with experiment. An objection that I have to this theory, to which its advocates have not given a satisfactory answer, is that it completely ignores the great air disturbances which accompany all thunderstorms, and which, it would seem, must in some way be one of the essential conditions for lightning production. According to the process postulated, lightning should be as likely to occur during a gentle shower as during a turbulent storm; in cold weather as in hot; near the Pacific coast as often as near the Atlantic. But such is not the case.

Another theory that has attracted much attention is that of Simpson.[19] This theory does not make use primarily of either the small or the large ions already present in the atmosphere. It postulates that the storm manufactures its own ions. It is well known that when a water drop is disrupted by an air blast, for example, the droplets which result from the disruption are found to be positively charged while an equal negative charge appears on ions in the air. Simpson seized upon this process as the one active in a thunderstorm. Swiftly ascending currents of air in the front part of the storm meeting falling rain drops tear them asunder with the resulting separation of electricities that has been described. The positively charged droplets may in time grow by coalescence, only to be disrupted again on reaching a certain size. Eventually, the drops fall away from the turbulent region of the cloud, the negative ions are carried upward by the air currents, and the high potential differences requisite for lightning are gradually built up.

This theory has a strong appeal because it makes use of the high vertical winds characteristic of thunderstorms to generate the large charges found in them. The comparative lack of lightning during cold weather and its entire absence in the Arctic regions may have a possible explanation on this theory. The argument is like this. After the wind has pulled a part of a drop into a film, the film breaks and snaps back into a number of droplets. These are charged positively because, in jerking back, the film loses some of the negative charge from a double layer supposed to exist on its surface. The faster the film contracts the greater its loss of charge. The speed of contraction depends largely on the viscosity of the liquid, and this decreases rapidly with rise of temperature, so that a warm film will contract faster than a cold one. Experiment has confirmed this reasoning,[20] for it was found that the charges obtained by disruption of drops falling through an air blast increase rapidly as the temperature of the drops is raised.

Hence we should expect potentials sufficient for the production of lightning to be reached with less violent air currents when the drops are warm than when they are cold. The only question that remains open is whether at the altitudes where the drops are being disrupted, the temperature is really different when it is undergoing large changes at the earth's surface.

The main argument directed against Simpson's theory is that the charges predicted by it in the clouds are more often than not of the wrong sign. Simpson[21] has countered these objections, but the arguments on both sides are too involved to be considered here.

The conditions in a storm are complicated and seem to vary with time and place, and the information about the actual distribution of the charges in a thunderstorm is still meager indeed. A more accurate knowledge of the location of these charges would be helpful in reaching a decision between the various theories, although in the end it may turn out that more than one of the processes which have been described, play some part in the phenomenon of lightning production.

[19] G. C. Simpson, *Phil. Trans.*, A 209: 379, 1909.

[20] J. Zeleny, *Phys. Rev.*, 44: 837, 1933.

[21] G. C. Simpson, *Proc. Roy. Soc.*, 114: 376, 1927.

BOTANICAL RESEARCH BY UNFASHIONABLE TECHNICS[1]

By Professor NEIL E. STEVENS

UNIVERSITY OF ILLINOIS

NEARLY two years ago in suggesting that among certain crops in the United States there was a relation between disease damage and pollination behavior, I ventured to use volume of publication on the diseases of these crops in relation to their farm value as a measure of the commercial importance of diseases in their culture.[2]

The main thesis that, at least among the crops classed as "grains," disease losses are much more important in those groups which are wholly or largely self-pollinated than in those which are largely cross-

[1] Address of the vice-president and chairman of the Section for the Botanical Sciences of the American Association for the Advancement of Science, Philadelphia, December 29, 1940.

[2] SCIENCE, 89: 339–340, 1939.

pollinated, has not yet been attacked. Indeed, if we are to take the teachings of the breeders seriously and actually credit the existence of selection as a vital force rather than just something in a book, it could hardly be otherwise.

On the other hand, a good many have found a source of amusement in the type of evidence offered. A characteristic comment has been, "this is interesting but not very scientific." Now, when some one makes the comment of "interesting but not very scientific," I find myself somewhat in the position of John Wesley regarding music. When some of the orthodox complained that the songs which thousands of his followers were singing throughout England were not sacred music, he replied with the question, "Why leave all the good tunes to the devil?" Why should we in our search for evidence on live problems leave all the interesting fields to the economists?

The general thesis advanced is, in view of the present great interest in breeding for disease resistance, of real, even basic, importance. Such generalizations are dangerous enough and hard to prove, but the available evidence should at least be considered on its merits and without any attempt to find too difficult an explanation. It is in dealing with just such general observations that present-day biologists lay themselves open to the criticism once leveled at a well-known writer by the late Newton D. Baker that he "discards the obvious as unreasonable and embraces the unreasonable because it is not obvious."[3]

In this case the inference drawn seemed to be in line with the general impression among informed agronomists, yet the suggestion was largely disregarded because of the unusual source of the figures presented. This question then has very much broader significance, which is my justification for bringing it up here, a significance as wide as the whole field of biological investigation. For it is only one phase of the more general question, Shall we permit easy and fashionable methods to determine our lines of research, or shall we attack interesting and important problems with the best tools we can find or devise?

Since the publication of the earlier article I have examined three other possible sources for evidence on this point and find them in essential agreement. For example, in a series of articles discussing progress and possibilities in plant and animal breeding, prepared by specialists of experience and high standing in their respective fields, and published in the United States Department of Agriculture Yearbooks for 1936 and 1937, relatively much more space was given to discussing disease resistance in wheat, oats and barley, all largely self-pollinated, than in corn, which is, of course, cross-pollinated. The relative amounts expressed as percentage of the total space which was given to this phase were: wheat, 11.5; oats, 10.5; barley, 7.0; corn, 1.9. Moreover, the disease loss estimates compiled and published by the United States Department of Agriculture from figures sent in by thousands of crop reporters in all parts of the United States are in agreement with the foregoing. The estimated average annual reductions for the decade 1916–1925,[4] which are slightly higher than for the earlier years, but fall in the same order, are in percentage: wheat, 5.2; oats, 2.8; barley, 2.7; corn, 0.4. Rye is not mentioned. Finally, the estimates compiled by the Plant Disease Survey for the years 1917–1937, inclusive, show a striking agreement with the foregoing. This, of course, raises the question of the significance of these estimates, or indeed any estimates relating to plant life.

Three years in the Plant Disease Survey followed by four seasons of renewed contact with cranberry growers and their problems led to much consideration of estimates and their place in biology, with conclusions which may be worth sharing, in spite of, or perhaps because of, the fact that they may not agree with current fashionable concepts.

First of all, what is an estimate? An estimate, according to Webster, is a "judgment of opinion, usually implying careful consideration or research—a judgment made by calculation, especially from incomplete data." This definition applies well to both estimates of crops and estimates of crop losses. Comparison of the actual shipments of cranberries by the New England Cranberry Sales Company with the estimates turned in by the members in September, only a short time before the harvest begins, over a period of 24 years showed errors of 5 per cent. or over in one or both of the two major varieties in eighteen of the years and errors of 20 per cent. or over in six years. Moreover, in most cases the causes of the errors seem to have been psychological rather than observational. The inherent caution of real New Englanders appears in a decided tendency to underestimate the Early Blacks, the variety first harvested. A conspicuous cause of error appears also in the influence of the previous crop. Very large crops tend to be greatly underestimated if they follow small crops and small crops to be greatly overestimated if they follow large crops. May I hasten to add that these serious, demonstrated errors led to no suggestion on the part of the cranberry growers that estimates be dropped. They apparently agreed with Poincaré that "it is far better to forsee even without certainty than not to foresee at all."[5]

It may not be impertinent to add that only academic minds question the usefulness of estimates. Businesses

[3] *Yale Review*, 30: p. 40, September, 1940.

[4] "Crops and Markets," Vol. 3. Supplement 10, pp. 321–322, October, 1926.

[5] "The Foundations of Science" (Authorized Translation by G. B. Halsted), Science Press. 1913.

are projected, factories built, professions chosen and crops planted on the basis of estimated future needs. Money is loaned, houses are purchased, horses are bought and are (or used to be) swapped on the basis of estimated value. Husbands are selected, married and, with increasing frequency nowadays, also swapped on the basis of estimated present or future worth. Many scientific investigators, on the other hand, only with the greatest reluctance, hazard their reputations (to say nothing of their money) on even the most carefully qualified estimates, preferring to devote their attention to phenomena which may be "measured."

Admittedly—indeed by definition—a high degree of numerical accuracy can not be expected of estimates. If in the case of the crop estimates just cited, a group of highly intelligent and vitally interested cranberry growers dealing with quantities which could be rechecked very soon after they were estimated, made frequent errors of 20 per cent. or over, there are probably very much larger errors in our crop loss estimates. The real question is, are they adequate for or at least the best available means of serving their purpose?

The purpose of crop estimates is clearly understood by all. It is to furnish those concerned in handling the crop with the most nearly accurate advance information possible. What is the purpose of the estimates of crop losses? Primarily, I take it, to furnish some basis for the comparison of one year with another, one region with another and one disease with another. So far as can be judged from reading the introductions to the various published summaries of estimates it was recognized from the first that they could not be very accurate. They were not measurements, they were "judgments of opinion." Into such judgments certain psychological factors undoubtedly enter as causes of errors. There has been, I believe, a general tendency on the part of most collaborators to err on the side of caution and to underestimate losses from diseases. This may be in part a reaction from the fantastic estimates of years ago. Certainly in some cases where it has been possible to measure the effects of disease, the figures have been much higher than the usual estimates. Study of the estimates over a period of years leads me to the conclusion that there is some irregularity as a result of special interest or knowledge, and that estimates are influenced by general interest or lack of interest in a disease. Local prejudices certainly seem to influence some of the estimates, and it is charged that there is some deliberate distortion.

It might be no less than fair also to call attention to the fact that estimates differ in degree only—not in kind—from many of our so-called measurements. That there are always subjective sources of error, and that in general, the larger the problem and the wider the field studied, the greater the probability of error.

It is my impression, gained through several years of study and observation, that the tendency to discredit the estimates of crop losses results from the inclination to expect of them wholly unattainable degrees of numerical accuracy, and that this tendency is fostered by the fact that the estimates are expressed in numbers. If no claim is made to numerical accuracy in the crop loss estimates, why are they numerically expressed? Simply because of the limitations of any other method of expression. The case is admirably summarized by Sorokin in a discussion[6] of methodology in another field. His arguments, which seem to me unanswerable, are that the numerical method is more concise and economical, and that verbal quantitativism has a very limited number of gradations. A glance at Table I which contrasts actual published figures with attempted verbal equivalents, will illustrate the point. After all, we have learned to use figures with discretion in some fields. If a text we use in 1939 states that certain plants grew a million years ago, we do not say in 1940 that these plants grew a million and one years ago.

TABLE I

ESTIMATED LOSSES DUE TO BACTERIAL WILT IN SWEET CORN IN PENNSYLVANIA

Year	Numerical (percentage)	Verbal quantitative
1931	..	did not notice it
1932	45	a massacre
1933	25	heavy losses
1934	20	less heavy losses
1935	15	still less heavy losses
1936	8	losses still less heavy but still heavy
1937	3	losses light but still noticeable

In view of all their admitted inaccuracies, are crop loss estimates worth while? I believe the answer to this question should be based on two further questions—How important are the problems to which they relate? Are they the best tools available? Obviously they are our only available means of studying the fluctuations in the intensity of many diseases from year to year. Just how important this is depends, of course, on one's judgment of the scientific and practical importance of epidemiology.

In this field, at least, I admit myself a prejudiced witness, for I am deeply interested in the history of plant diseases. On the basis of estimates collected by the Federal Government, some of them before the Department of Agriculture was organized, I was able to trace the spread of potato blight in 1843, '44, '45[7] and to publish for the first time a record of the

[6] P. A. Sorokin, "Social and Cultural Dynamics," Vol. 2, p. 22.

[7] *Jour. Washington Academy of Sciences*, 23: 435–446, 1933.

extent of losses from the disease in another great rot period of 1885–1886.[8] I confess myself not greatly concerned by the fact that New York records the loss in 1885 as 38 per cent. and adjacent Vermont as 17 per cent., nor do I care whether either figure is accurate. The fact is that we are able to learn from them that the losses in both states were large in 1885 —much larger than in the subsequent year.

At some risk to what little may be left of my scientific reputation I wish to add that for a study of the fluctuations of disease from year to year, estimates may be just as useful as figures based on laborious measurements and calculations. A figure published a few years ago[9] gives some information about the incidence of fruit rots of cranberries on Cape Cod during 5 years. It is based on the results of storage tests of eight to ten lots of the most important cranberry variety in Massachusetts taken from the same bogs and from nearly the same sections of these bogs year after year. The lots were adequately sampled, carefully sorted and accurately counted. For purposes of comparison they show that the crops of 1931 and 1933 were of very poor keeping quality, the crop of 1932 very good, and the crops of the two other years good but not exceptional. These facts were already well known to inspectors, sales agents and growers throughout the area on the basis of their own experience and observation long before my tests were concluded.

There is another and very practical angle to the study of plant disease control in which estimates of disease, yes, and forecasts of the probable incidence of disease, are of direct significance. How else can we evaluate our control measures? I have already quoted more than once the reply I received when a few years ago I asked a plant pathologist who had spent several years in the study of the control of damping-off in forest nurseries for a brief statement of his recommendation for disease control. His first sentence was "If your loss is less than 15 per cent., forget it." I think it would be an excellent practice and would tend to establish us in the eyes of practical men if all our recommendations for disease control began, "If your loss is less than — per cent., forget it." In order to determine what this figure should be, we have at present no other source of information than estimates of plant disease losses. However interesting it may be to work out theoretical controls, in the actual practice of plant pathology we have no right, outside the field of ornamentals, to recommend the use of any control measure which does not cost demonstrably less than the probable loss from the disease. On a very much larger scale, how is it possible to evaluate the results of campaigns for the eradication of long-established diseases, for instance, fire blight and peach yellows, unless or until we know something about the extent of the fluctuations in these diseases when there have been no eradication campaigns?[10]

The difference of opinion as to the value of crop loss estimates would seem to boil down to this: a difference of opinion between those whose first concern is an alleged mathematical accuracy and those who are seriously concerned with practical disease control and adjustment to disease conditions in a practical world. For my own part, I can see little justification for a plant pathology—certainly not for a plant pathology supported by public taxation—which is more concerned with methodology than with objectives.

I should like to go even further than this and insist that we are in serious danger of needlessly limiting our interests and usefulness, to say nothing of making ourselves ridiculous, by insisting on professionally ignoring all phenomena which do not lend themselves readily to measurement by fashionable technics.

Fungus spores can be measured in fractions of microns and the geographic ranges of fungi only approximately determined, while the causes of their distribution can be little more than guessed at. Shall we, therefore, spend our lives measuring spores and neglect questions relating to distribution and its causes?

Because it is impossible to determine whether the loss caused was 18.2 per cent. or 22.4 per cent. must we refrain from recording the fact that *Diplodia zeae* caused severe losses in Illinois in 1938?

Shall we fearfully protect our precious reputations by refusing to publish any suggestions or opinions not bolstered with measurements which seem to be statistically respectable?

Through the indulgence of the Wisconsin Academy of Science, we have been able to place in print a conviction held by myself and two associates that attempting to utilize alkaline flooding water in the cultivation of cranberries in that state is apt to lead to financial disaster. Of this, as yet, we have not a shred of experimental evidence, nor is there any chance of getting that within the next few years. We know, however, that of the properties now under cultivation those with alkaline water are the hardest to manage successfully. We know that an entire cultural area, once the largest in the state, which has alkaline water, is now almost abandoned; while another with acid water is now cultivated by the third generation of successful cranberry growers. We know that the only property in the earlier area still under cultivation is in the red and has been in deep red for at least a quarter of a century. We have case history after case history, some running back 45 years.

[8] *Phytopathology*, 24: 76–78, 1934.

[9] N. E. Stevens and J. I. Wood, *Bot. Rev.*, 3: 277–306, Fig. 18, 1937.

[10] *Jour. Econ. Entom.*, 31: 39–44, 1938.

In spite of all this evidence some of our colleagues (fortunately not the cranberry growers themselves) find it hard to take the conclusions seriously. If, in contrast to the above, we were reporting results obtained from six 4-inch pots for one year, our results would be far more respectable.

A realization of the possibility that even in science there is danger from too devout worship of the fetish of alleged accuracy has recently been found in papers from two fields which are commonly supposed to be much more characterized by accuracy than biology can reasonably hope to be; namely, astronomy and physics. Professor Henry Norris Russell, in his presidential address before the American Astronomical Association[11] in 1937, discussed "the place, utility, and limitations of approximate methods in astronomical work."

Professor Russell's paper is short enough to be easily read and too compact to be easily abstracted. He gives a number of instances in which approximate methods have given highly significant results, and raises the important point of what he calls "astronomical economics," that is, the question as to the extent to which the director of a great modern observatory should spend money and energy in securing more and more accurate observations. He indicated that spending effort and funds for accuracy is justified to the extent that the problem under study requires accurate measurements for its solution.

Even in astronomy it appears there are those who would let methods dictate problems. He quotes E. C. Pickering as saying that shortly after he had become director of the Harvard Observatory he was severely and publicly criticized by a conservative group because "instead of putting his time on meridian observations, which can be made with an accuracy better than one part in a hundred thousand, he is working on photometry, with errors of ten per cent. or worse, and in spectra, with no accuracy at all." And, he adds, "Pickering had the courage of his convictions, and kept on with the results that we know."

Charles Galton Darwin in a discussion of "Logic and Probability in Physics"[12] says: "What is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges; and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline."

A drastic change in my surroundings a few years ago led to my reading a number of books in the field of sociology—which is after all a sort of biology. I find these workers are frequently faced with this same choice, relatively accurate measurements in a less interesting and significant field or obviously crude measurements in a highly interesting one. May I then close with quotations from two of them, "We have been choosing the problems of study not so much by their importance as by a possibility of making a 'fine and accurate study of a topic.' . . . Pushed too far in that direction, these investigations become a worthless parody on science. To avoid this situation, once in a while, somebody has to take upon himself the doubtful privilege of selecting an important topic for his study, though it does not lend itself to an exact investigation."[13] "Method must conform to material and not vice versa . . . the first loyalty of a scientist is to his material; . . ."[14]

SCIENTIFIC EVENTS

THE RESEARCH SECTION OF THE ROCHESTER ACADEMY OF SCIENCE

A NEW Research Section of the Rochester Academy of Science has been organized under the constitution of the academy. Meetings will be held on the first Tuesday of each month giving opportunity for social and professional discussion.

The Research Section has as one of its objects the promotion of the more active publication of the *Proceedings* of the academy. Its members pay an annual fee of $3, in addition to the fee of $2 for general academy membership. The additional $3 will be used entirely for the benefit of the *Proceedings*. It is planned to issue from two to four numbers annually, four numbers of about 200 pages constituting a volume. The subscription price will be $5 a volume, with a guarantee that the cost will not exceed this sum.

The first meeting of the section was held on January 13. Karl Patterson Schmidt, director of the division of zoology of the Field Museum of Natural History, Chicago, gave a lecture entitled "Desert and Highland in Peru," which was devoted to a résumé of impressions gained while on an expedition to Peru in 1939. In his introductry remarks he presented his views on the value and purpose of an Academy of Science. The second meeting, on February 4, provided a round-table discussion on "Speciation in Plants and Animals" conducted by Professor Sherman C. Bishop and Dr. Richard Goodwin, both of the department of biology of the University of Rochester. The third meeting will be held on March 4, when William A. Ritchie, of the Rochester Museum of Arts and Sci-

11 H. N. Russell, *Publications Am. Astron. Soc.*, 9: 108–114, 1938.

12 SCIENCE, 88: 155–159, 1938.

13 P. A. Sorokin, "Social and Cultural Dynamics," Vol. 2, p. 270.

14 John Dollard, "Caste and Class in a Southern Town."