political position it rightfully should occupy. That public eulogists of scientific achievement have rarely undertaken to dwell upon anything beyond the "practical" result argues that there is in them either a want of vision, or a lack of courage to force the consideration of a viewpoint devoid of popular appeal; perhaps both.

W. L. CROZIER

DYER ISLAND

LEAF BURN OF THE POTATO AND ITS RELATION TO THE POTATO LEAF-HOPPER

THROUGHOUT the northern section of the United States, from Montana to New York and south at least to Iowa and Ohio, there has been a remarkable epidemic of leaf burn on potatoes. The margins of the leaves of early varieties turned brown, the dead areas gradually widening until the leaves dried up and the whole field took on a burned appearance. In severe cases the stalks also withered and died.

Every potato section of Wisconsin was affected and a careful study by the writer showed that in every case the injury was directly proportioned to the number of potato leafhoppers (*Empoasca mali* LeB.) present. The nymphs of this species feed on the undersides of the leaves and first produce a wrinkling of the whole surface, with a slight upward rolling of the margin, and then the marginal burning appears. Long after the leafhoppers have acquired wings and flown away it is possible to determine the cause of the damage by observing the cast skins adhering to the under surfaces and the egg scars in the mid rib or veins of the burned leaves.

In cage experiments, using large numbers of leafhoppers, typical leaf burn was produced in four days. The relation of this injury to what has been previously diagnosed as "tip burn" is an interesting subject for future determination. The characteristic marginal burn is frequently so definite that it is possible that there may be something injected that produces more definite and widespread results than the mere mechanical extraction of the sap. There does not, however, seem to be the same specific relation that exists between the beet-leafhopper and the curly-leaf disease of beets.

E. D. BALL

STATE ENTOMOLOGIST,
MADISON, WIS.

"FATS AND FATTY DEGENERATION": A RESPONSE TO BOOK REVIEWS BY BANCROFT AND CLOWES

WILDER D. BANCROFT[1] has recently reviewed in the pages of the *Journal of Industrial and Engineering Chemistry* a book entitled "Fats and Fatty Degeneration,"[2] by Marian O. Hooker and myself. He has also published in his *Journal of Physical Chemistry* a review by G. H. A. Clowes,[3] which in spirit is identical with his own. My attempt to answer both of these reviews in the pages of Bancroft's *Journal* has met with the editor's refusal.

Bancroft and Clowes's adverse criticisms are of two kinds: (1) those contradicting my observations and their interpretation, and (2) those implying unacknowledged borrowings from the works of others, more specifically their own writings. As to the first, it is the privilege of any critic to correct errors and to disprove arguments when truth and logic are on his side; as to the second, no reputable investigator would, even if moved by nothing better than the low ideal of his material future, jeopardize truth by taking it ready-made from another without noting that fact, or would pose as the discoverer of laws already set forth by authorities working in the same field. Those who know either me or the history of emulsion chemistry will easily find their way here. Yet, deferring to another article my answer to the scientific objections of Bancroft and Clowes—an answer that should be apparent to any careful reader of my book—I purpose in this note to comment upon their purely personal criticism.

Bancroft says:

> It is also interesting to note that the author does not cite Pickering's first paper, though he must be familiar with it. . . . It is certainly being overcharitable to say that the author has the unhappy

[1] Wilder D. Bancroft, *Jour. Ind. and Eng. Chem.*, 9, 1156, 1917.

[2] Martin H. Fischer and Marian O. Hooker, "Fats and Fatty Degeneration," New York, 1917.

[3] G. H. A. Clowes, *Amer. Jour. Phys. Chem.*, 23, 73, 1918.

gift of remembering what he has read but of forgetting that he has read it.

This idea is expressed by Clowes as follows:

This statement is somewhat surprising in the face of Pickering's emulsification of 99 per cent. of oil in 1 per cent. of an aqueous soap solution, and Fischer's own data and illustrations (pages 40 and 78) of emulsions (borrowed without acknowledgment from Pickering even to the stick standing up in the jelly) in which 20 parts of oil are emulsified in one part of the water phase.

The scientific aspects of these statements are covered in my book and will be more fully discussed at another time, but the implication of unacknowledged borrowing I can not allow to pass. It happens that I have never had access to this particular paper of Pickering, published, I think, in the *Transactions of the Royal Society*. I believe, however, that I am conversant with Pickering's views on emulsions from such of his papers as have been accessible to me in the original. With regard to the stick inserted in the jelly to test its stiffness, what more boyish means could any investigator employ for such a purpose? Surely he would not need to borrow from a printed illustration so simple an empirical device.

Clowes continues:

In borrowing from earlier investigators the idea of tackling the problem of protoplasmic balance by studying the reversal of phase relations in emulsions, Dr. Fischer failed to make himself acquainted with the data already available regarding the conditions under which emulsions of water in oil may be formed, and emulsions of this type transformed into those of oil in water and vice versa.

Although I do not understand the expression "protoplasmic balance," Clowes evidently believes that I have slighted his work. On the contrary, Clowes's work on the theory of emulsification and his experiments on the transformation of oil-in-water to water-in-oil emulsions are fully acknowledged on pages 28, 29 and 30 of my book. I go so far as to try to harmonize our views, although I must now confess my inability to understand much of his work owing to the fact that he writes diffusely and jumbles good experimental observations with hypotheses. Here as elsewhere, however, I have followed a principle which has guided all my writings, namely, that of discovering and emphasizing only the positive contributions of any author, and of ignoring what seem to me his mistakes or false guesses.

Clowes writes further:

In the chapter on fatty degeneration, Fischer fails entirely to give credit to Alonzo E. Taylor.

This statement is characteristically inaccurate, for Taylor's work is discussed on page 69 of my book. One is tempted to say of Clowes what Bancroft says of me, "It is a little difficult to characterize the author's methods and yet keep within parliamentary limits." Clowes might at least have done me the small justice of looking up Taylor's name in the index. Yet, as a matter of fact, Taylor was interested only in that chemical aspect of the problem of fatty degeneration which asks whether fat may be formed from protein. My own contributions to the subject have nothing to do with this; they deal instead with the physics of the question.

So far as the theory of emulsification is concerned, it is the intent in my volume to show that a union between solvent and lyophilic colloid (the formation of "colloid solvates" or "colloid hydrates") is *one* of the large and important factors in the maintenance of emulsions. This contention of mine is accepted as correct in Bancroft and Clowes's reviews. As a matter of fact the idea is looked upon by them as entirely self-evident, for Bancroft writes:

When oil is emulsified in water by means of a third substance, one has drops of oil each coated by a gelatinous film. . . . If we cut down the water sufficiently we shall get a limiting case where we have merely drops of oil surrounded by gelatinuous films.

Clowes expresses the notion in the words:

Bancroft's demonstration that the formation of one or the other type of emulsion depends not upon the relative volumes of oil and water, but simply upon whether the emulsifying agent employed is preponderantly hydrophilic or lipophilic. . . .

This complete acceptance of my views is both gratifying and surprising, since neither Bancroft nor Clowes ever said or demonstrated anything of the kind until after the appearance of my various papers[4] and of the book which they review. Never before the time of these reviews has either used the terms "hydrophilic" or "lipophilic" in any of his papers on emulsification. Indeed, when I presented the importance of colloid solvates (Bancroft's "gelatinous films") for the understanding of the stabilization of oil-in-water and water-in-oil types of emulsions, at the 1916 Urbana meeting of the American Chemical Society, both gentlemen attacked my views[5] as impossible. At that time they were following Pickering's belief that the stability of an emulsion depends upon the production of an "interfacial film" between the two liquids which, in Bancroft and Clowes's mind, when bent one way, yielded an oil-in-water type of emulsion, and, when bent the other, a water-in-oil type.

Bancroft says further:

> In so far as an emulsion of oil in water is stabilized by a hydrophilic colloid, there is nothing new about this.

Here Bancroft disparages as not new the very idea which he had previously declared impossible. Of course the fact that emulsifying agents emulsify has been known since mother first made mayonnaise. What mother did not know was why her methods worked. So far as I am aware neither she, nor Clowes, nor Bancroft knew that the hydrophilic properties of colloids were an important element in the matter until I pointed this out.

Clowes concludes as follows:

> While the writer of this review would not charge Dr. Fischer with any deliberate intention to mislead, the obvious haste with which this somewhat pretentious work has been constructed suggests an attempt to skim the cream of a new idea in a promising field of research.

[4] Martin H. Fischer and Marian O. Hooker, Science, 43, 468, March, 1916; Kolloid Zeitschr., 18, 100, 1916; 18, 242, 1916.

[5] See "Fats and Fatty Degeneration," p. 29, for an account of this.

The statement in the first clause withdraws the whole charge of the critic and is inconsistent with his earlier paragraphs. His succeeding inference is unjustified and absurd. In any case scientific research presents too bounteous a table for those who sit at it to haggle over the cream.

I conclude these quotations with an opinion by Bancroft which reveals his personal animus and embraces not only my volume on fats, but all my books:

> The author's books are all interesting reading, and this one is no exception; but they should be considered as advertising matter in the guise of scientific fiction.

Thus, from his original contention at the meeting of the American Chemical Society that my views are untrue, Bancroft has come to contend that they are not new; and then, insecure upon this ground, he turns from discussing scientific issues and discusses me.

With this brief presentation I rest my case. Decision is, fortunately, not confided to *ex parte* attorneys; it is the portion of disinterested third parties, of science and of time.

Martin H. Fischer

Eichberg Laboratory of Physiology,
University of Cincinnati

QUOTATIONS

A MEDICAL ENTENTE WITH AMERICA

We published last week an account of the very cordial reception accorded to British medicine in the persons of Sir James Mackenzie, Sir Arbuthnot Lane and Colonel Bruce by the American medical profession during the recent annual conference at Chicago. That event marks an important stage in the development of understanding and sympathy between the two countries, not only because the doctor wields in every community a large if undefined influence, but also because it is well that in the great war against disease which is now in its opening stages the two peoples should stand side by side, mutually supporting one another. American medicine has much to give, and we know that the same can be said of our own profession. The time is opportune for the