

DISCUSSION AND CORRESPONDENCE

THE PARASITISM OF NEOCOSMOSPORA—INFERENCE
VERSUS FACT

IN May of last year an article by Howard S. Reed, now of the Bureau of Soils, United States Department of Agriculture, appeared in *SCIENCE* (page 751), entitled "The Parasitism of *Neocosmospora*," this being made up largely out of a bulletin soon after published by the Experiment Station of Missouri. The article in *SCIENCE* and the bulletin are based on some confessedly incomplete work (bulletin, page 64) done at the University of Missouri with a *Fusarium* isolated from diseased ginseng plants. The contribution in *SCIENCE* is occupied chiefly with a criticism of some of my own conclusions published several years ago in a Department of Agriculture bulletin. This tardy reply is due to the fact that I have only recently read the article.

My first thought was that I must go all over my own work to see how I could have fallen into such an absurd error. On consultation, however, with one of my colleagues, who has been much engaged in recent years with diseases of this class, I found he had saved me this labor. Also on a second more careful reading of Dr. Reed's article in *SCIENCE*, and especially on reading his bulletin, I found so many unwarranted inferences that it seemed hardly worth while to consider his criticisms seriously. However, as his statements have entered into literature with the same face value as my own, especially for those who do not look into scientific writings very closely, I am compelled to make this answer.

I am not specially interested one way or another in the ginseng fungus as such. It may be a weak facultative parasite entering exclusively through wounds made by other fungi, as Dr. Reed asserts; although nothing in his writings clearly establishes this fact. The points at variance between us will be better understood if I first summarize the author's actual facts and then his inferences.

First as to the facts or supposed facts.

1. He found a *Fusarium* wilt of ginseng and also an anthracnose of ginseng. He states

that wilting ginseng plants in all cases were previously attacked by the stem anthracnose, and further that the *Fusarium* entered the ginseng plants exclusively through stem-wounds made by this anthracnose.

2. He states further that on inoculating soils with the ginseng fungus, which soils were then planted with watermelon-seeds, he obtained a wilt of the melon-seedlings and found a *Fusarium* inside the stems (one experiment, three pots). Ginseng fungus, perhaps (?). When, however, he sterilized the soil in the autoclave and then inoculated it with his fungus and planted watermelon-seeds in it, the seedlings remained healthy for twelve weeks, although the fungus (ginseng fungus, be it remembered) grew abundantly in the soil.

3. He sprayed a "thoroughly underdrained" field of ginseng with Bordeaux mixture, and neither disease appeared in it. The *Fusarium* wilt appeared in a neighboring unsprayed field, which, however, belonged to another man and was not underdrained. From this he concludes that Bordeaux mixture is a remedy for the disease.

Some of the inferences I think unwarranted are the following:

1. The ginseng-fungus belongs to the genus *Neocosmospora*.

2. This ginseng-fungus and the watermelon-fungus first described by the writer as *Fusarium niveum* are identical.

3. The watermelon-fungus can enter the plant only when a way has been opened for it by other fungi, *e. g.*, by *Thielavia*.

4. Other *Fusaria* are in the same case. My conclusions, therefore, respecting the parasitism of the melon-fungus and similar forms for which I made the genus *Neocosmospora*, are erroneous.

This sufficiently outlines the points of difference between us.

Before passing to the manifest inferences, it may be remarked that neither from the article in *SCIENCE* nor from Dr. Reed's bulletin can it be concluded with any certainty how his fungus enters the plant (bulletin, page 50), or whether, as he asserts, spraying

with Bordeaux mixture will prevent the disease, either acting directly or indirectly. He states that the ginseng fungus is a wound parasite, but, so far as I can see from any facts advanced, this is only an assumption which may or may not be true. I should like to know whether this is entirely a *post hoc* conclusion or something which was actually demonstrated, and if so, how demonstrated, and why he has not published his proofs? He nowhere says that he actually found the *Fusarium* entering the plant through wounds in the stem caused by the anthracnose, although this, from the standpoint of his hypothesis, was one of the first things to look for and to be made out *conclusively*, not *inferentially*, especially if he proposed to use it as a basis for criticism.

To come now to those things which relate specially to my own work and are manifestly unwarranted inferences:

1. How does he know that the organism he worked with belongs to the genus *Neocosmospora*? He states distinctly that he did not find any perithecia. We know that not all members of the form-genus *Fusarium* belong to *Neocosmospora*, and also that inspection of the imperfect stages does not suffice to tell. This then is an uncertain inference put forward as a fact.

2. How does he know that his identification of the ginseng-*Fusarium* with the watermelon-fungus is correct? I doubt it. He did not make any comparative study of the two fungi, although it would have been easy for him to obtain the melon-fungus, since the disease is widespread in the southern United States, and probably occurs in Missouri, possibly in some of the soils he worked with. Why did he not make comparison between the two organisms rather than between his organism and my description?

3. How does he know from his very limited experiments with one species that all *Fusaria*, and the *Neocosmosporas* in particular, are weak parasites? He states that he found the ginseng fungus to be a weak parasite, but I have just pointed out that even this is not established conclusively from his papers.

How, then, can the much larger inference be sustained? One can not reach general conclusions from a single particular. It does not need any very extensive course in logic to convince one of this.

4. How can inferences of any value respecting this group of fungi be based on such a sandy foundation? From his statements the reader is led to think that the watermelon fungus must be a weak parasite and that the plant must first be attacked by *Thielavia basicola* or some other fungus before the *Fusarium* can possibly find an entrance into it, although Dr. Reed probably never saw the melon-fungus, and has not *proved* that the ginseng-fungus can not enter the plant in the absence of wounds.

When this paper of Dr. Reed's first came out, I was in Europe, but my colleague, Mr. Orton, obtained cultures of the ginseng-fungus from Dr. Reed and carefully compared it on various culture media with the watermelon-fungus which we had in culture in the laboratory, and found that they behaved differently and were probably not identical organisms. This is the sort of work Dr. Reed ought to have done and not left for some one else to do.

Mr. Orton also made in one of our hothouses the following three sets of inoculation experiments, using autoclaved soil:

1. Watermelon-plants; the soil inoculated with the ginseng-*Fusarium*, obtained from Dr. Reed. Ten inoculated pots and ten control pots.

2. Watermelon-plants; the soil inoculated with the cotton-*Fusarium*. Ten inoculated pots and ten control pots.

3. Watermelon-plants; the soil inoculated with the watermelon-*Fusarium*. Ten inoculated pots and ten control pots.

The results were as follows:

1. No cases of melon-wilt in the pots inoculated with the ginseng-organism. (Experiment agrees with Dr. Reed's corresponding experiment.)

2. No cases of melon-wilt in the pots inoculated with the cotton-organism. (Experi-

ment agrees with Smith's earlier statements; Bulletin 17.)

3. Typical watermelon-wilt in the pots inoculated with the watermelon-*Fusarium*. All of the ten plants growing in this autoclaved soil contracted the disease. They were watered with distilled water until the plants began to develop the wilt, and then they were watered with ordinary hydrant water.

(4) All the uninoculated plants (30 pots) remained free from disease.

Because one fungus in a group is a feeble parasite, it does not follow that all are, and especially in the absence of experimental data. The writer never maintained that all species of the form-genus *Fusarium* were active producers of disease. In fact, when he began to study this group, all of them were supposed to be saprophytes, and he was, I believe, the first one to maintain and to demonstrate that certain members of the group are among our most destructive fungi. This work has been built upon largely in certain quarters, with very scant credit to the writer. Such matters, however, even themselves up in the long run and credit finally goes where it belongs.

The moral of all this is that when one assumes the rôle of critic he ought to be reasonably certain of his facts.

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ENGLISH AS SHE IS WRITTEN

EVER since it was authoritatively decided that "The United States is," and not "are," there has been increasing departure from what was not long ago considered good grammar, especially in the newspapers. We do not expect the "dailies" to lead in correct diction, however desirable this would be from the fact that the reading of the bulk of our population is done in their columns, and serves the younger generations as their preferred literary food. We are so accustomed to having the papers pervert the nation's English that we rather expect to see all kinds of grammatical and syntactic horrors perpetrated in our morning papers. And SCIENCE could hardly be expected to bring much pressure to bear

upon the journalistic world in inducing them, *e. g.*, to use the nominative instead of the accusative case when stating that "whom it is well known has been," etc., a form to be found in every daily for the last two or three years. But when SCIENCE, as well as some other journals of high standing, admits into its columns such statements as that "the underlying strata was a soft limestone," and that "this phenomena was closely observed by us," and that "we owe this data to the courtesy of Mr. —," it does seem that the restriction of the scientific curriculum to so much language study as is provided for in the high schools is proving unfortunate. Perhaps the inauguration of the much-needed spelling reform, which is considered by some as obliterating important landmarks, has contributed to the feeling of linguistic irresponsibility on the part of juvenile specialists in particular. But would it not be proper to consider the correction of such palpable mistakes as part of the duty editors owe to the public; if only to prevent us from being charged with illiterate perversion of the language by our cousins across the Atlantic?

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[The proofs of SCIENCE are read each week by three professional proofreaders, and most, though unfortunately not all, grammatical errors are corrected. Errors such as those quoted by our correspondent are like infringements of the etiquette of polite society—they are especially dreaded; but they are minor matters, and may indeed be in the line of linguistic evolution. It must be admitted that the English language is used with greater correctness and skill by men of science in Great Britain than in the United States. This is probably due to the fact that English men of science come as a rule from a comparatively small class in which the use of correct English is a social tradition.—EDITOR.]

THE ARTIFICIAL PRODUCTION OF MUTANTS

IN SCIENCE for July 19 Professor T. D. A. Cockerell gives an appreciative review of Tower's "Investigation of Evolution in Beetles of the Genus *Leptinotarsa*," a recent