Letter to the Editor

Estimating Parasitic Fitness: Epilogue

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There is increasing, although long overdue, awareness of the epidemiological significance of parasitic fitness, and its major role in plant pathogen population shifts and plant disease epidemics. Predicting population shifts by estimating changes in relative parasitic fitness ultimately could lead to the *avoidance* of epidemics.

My original Letter to the Editor (LTE) (3) focused on this general issue and presents one approach to estimating parasitic fitness. That approach is rooted in the concepts of biological fitness (1), a concept long accepted by population geneticists. Because I am among the first to apply these concepts from the population genetics discipline to the science of epidemiology, I was careful not to identify my approach as the *only* means of estimating parasitic fitness.

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Many scientific problems can be approached in several ways. However, the research worker must state the particular reason for studying the problem in the first place. My reason was a practical one and I suspect that that of Groth and Barrett was more philosophical.

Phytopathology LTEs are a progressive step, providing a forum for the exchange of ideas among persons interested in a common subject. Unfortunately, in this particular case, it appears that Groth and Barrett and myself are not really interested in the same subject. Their critical attempts to clarify my use of W suggests that their thinking on relative parasitic fitness versus biological fitness is considerably confused. Relative parasitic fitness W and biological fitness w are absolutely not equatable. Groth and Barrett's persistence on equating my relative parasitic fitness W to the common w is totally incorrect.

Now that I have commented on the primary issue raised in their LTE, there is little need to reply to anything else.

In my original LTE I attempt to address the issue raised by Dovas et al (2), who speculate that the differences in data recorded for epidemics induced by two different isolates may reflect differences in the fitness of the isolates. Their particular data set was amenable to the approach that I presented. My interpretation of their data in the framework of parasitic fitness should not be construed as criticism of their excellent and stimulating research.

My interests in relative parasitic fitness and population shifts stem from a considerable interest in the stability of horizontal (5) or rate-limiting resistance. Many plant pathologists measure the extent of horizontal resistance by its impact on the apparent infection rate, a trait attributable to the host. Stable horizontal resistance assumes the inability of the parasite to acquire sufficient parasitic fitness to overcome that resistance. The methodology I present in my original LTE and later elaborate in more detail (4) suggests one approach to determine if that assumption is valid. The epidemiological consequences are reasonably obvious even if the assumption is not universally applicable.

LITERATURE CITED

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