

Comments from the Editor-in-Chief

The *Journal of Economic Entomology* (JEE) has a variety of venues for publication, including Letters to the Editor, the Forum section, Book Reviews, and Research Papers. All submitted manuscripts, regardless of venue, are subject to the same rigorous review process.

Historically, the Letters to the Editor section has seldom, if ever, been used. While the general nature of what can be considered as suitable for the Letters section is still evolving through discussions in the JEE Editorial Board, at least some of the content will include comments of a critical nature regarding previously published papers in JEE. Whenever this occurs, the authors of the original paper will be given ample opportunity to provide a response. I would prefer to publish the letter and the response to the letter in the same issue. However, the authors of the original work may choose not to respond, or to respond at a later date.

John Trumble

TO THE EDITOR:

Pseudoreplication, Fungi, and Locusts

Thirty-two years ago, one of us (S.H.H.) published in this journal a statistical analysis of the effects of chlorpyrifos on duckling mortality (Hurlbert et al. 1970). Scientists at Dow Chemical (Midland, MI), the manufacturer of this insecticide, prompted in part by the fact that our findings were hindering the company's efforts to get this then new compound registered for use for mosquito control in salt marshes, criticized in a privately circulated report (Kenaga and Dowell 1970) our statistical analyses, interpretations, and conclusions. Instead of criticizing our misuse of the χ^2 test, however, they attacked the method itself, claiming that χ^2 tests generically "are acknowledged to be non-rigorous and tend to demonstrate significant differences where no such differences exist." We rebutted that claim, and when they eventually published their critique (Kenaga 1974, responded to by Hurlbert 1977), the claim was omitted. Much later, we confessed that the statistical analysis of duckling mortality was invalid and coined "sacrificial pseudoreplication" as a label for the particular error committed (Hurlbert 1984).

It is thus with some sympathy that this mister egg-on-his-face teams up with an entomologist (W.G.M.) concerned with the reliability of research reports on matters of high practical import to offer a critique of 142 errors of the same sort in another paper. This is Ivie et al.'s (2002) assessment of effects on beetle populations of insecticides and pathogenic fungi used or potentially useful for control of migratory locusts. The massiveness of this dose of pseudoreplication, combined with many misleading statements and unjustified conclusions, makes correction of this report imperative. Clarification of some general principles of

experimental design and statistical analysis may be useful to broad segments of the journal's readership. Dissection of a concrete example often is a good way to achieve this.

The experiment of Ivie et al. (2002) used five treatments (control, a standard chemical formulation, and three fungal strains) and a randomized complete block design, with two blocks of five experimental units (1-ha plots) each. To assess beetle populations, three pit traps and three flight-intercept traps were established on each plot. Beetles were collected from these traps at 5, 10, and 15 d posttreatment, identified, and counted, and treatment effects on beetle species richness and population densities were assessed using Kruskal-Wallis, Mann-Whitney *U*, and χ^2 tests. That these details are evident reflects the commendable clarity and completeness of the authors' description of their procedures.

The three principal categories of problems in Ivie et al. (2002) are their misrepresentation of the nature of pseudoreplication, their erroneous statistical analyses, and their unwarranted conclusions. Let us address each in turn.

Confusion over Pseudoreplication

In their Methods section, Ivie et al. (2002) spend almost a full page describing the constraints that limited them to only 2-fold replication of treatments, commenting on the nature of pseudoreplication, and justifying its commission, in anticipation of what they will later do in their Results section.

They state, "It may be argued that this design meets the definition of pseudoreplication . . . [, that this sin was] imposed by externalities . . . [, that] by definition all field experiments on the same planet and in the same atmosphere have some level of pseudoreplication inherent . . . [, and that] Hurlbert (1984) allows for use of pseudoreplicated data . . ." In fact, pseudoreplication is an error of statistical analysis and interpretation, and nothing more (Hurlbert 1984, Hurlbert and White 1993, Heffner et al. 1996, Lombardi and Hurlbert 1996, Garcia-Berthou and Hurlbert 1999). It makes no sense to refer to either designs or data as pseudoreplicated or to refer, as some authors have, to samples or subsamples from a single experimental unit as pseudoreplicates. Nor is it necessary to become multipanetary to remain statistically chaste. A particular design or set of constraints never can impose pseudoreplication on a researcher or a study. The only externalities that might do so are wayward power brokers (e.g., thesis advisor or committee member, journal editor). Fortunately, all but the proudest of these are educable.

Statistical Errors

It is one thing to carry out an experiment that has low power, to analyze the data properly, and to then suggest that even somewhat high *P* values (e.g., 0.10, 0.20, or 0.30), together with observed treatment means, may allow some tentative assessment of treat-