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My view

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I have published numerous papers on methodology in a career of almost 20 yr in weed research. The aim has often been to introduce a new approach or to further develop existing methods (such as the incorporation of population dynamics into weed control thresholds or more biologically plausible regression equations for yield loss and herbicide dose–response studies). However, the motivation has frequently arisen from the frustration of seeing repeated errors in published papers. Examples have been the frequency of errors in the analysis and reporting of statistics (Cousens 1988), still widespread in weed science; the numerous statements that the Replacement Series design is always invalid (Cousens 1996); and the common comparisons of a single resistant with a single susceptible weed population (Cousens et al. 1997). In each case, my arguments were accepted by referees and editors but have been subsequently ignored by many researchers. One paper pointing out the problems that arise if variance is not considered when fitting yield loss surfaces received a best paper award (Cousens 1991), yet papers continue to appear in *Weed Science* without heeding the warnings.

As an enthusiastic young researcher, this ambiguity in response to my work was frustrating. But in my current midlife phlegmatism, I realize that, in any case, I was only drawing attention to the symptoms, not the causes. The reason that poor statistics continues is not because of a lack of knowledge of how to do statistical calculations: it is a result of the failure to grasp the philosophical basis of what we are trying to do with statistics. This arises from the way that statistics is taught to us in the first place and the way in which we learn from our peers who have no more formal training than we do. We tend to learn by copying, often with incomplete understanding, and hence errors and poor practices are passed on. Papers comparing one resistant population with one susceptible population, for example, are written and accepted because they have been accepted in the past, not because they are scientifically valid.

Methodology does not have to be technically or logically flawed for it to be a serious concern. Another, much more insidious problem in weed science methodology is the dominance of research by repetition. Even without errors, this will inevitably result in very slow progress in the advancement of science. There appears to be a strong tendency in weed science to try to advance knowledge by the accumulation of more and more data, not through the development of hypotheses and testing of ideas. Pick up almost any copy of *Weed Science,* and you will see yet another paper describing yield response of another crop to yet another weed (although it is gratifying that each one of these usually cites my publications!). Or another experiment that describes seedling numbers from several tillage treatments. But what do we learn from each repetition of these standard approaches? How much has our understanding of competition advanced since Donald's work in the 1950s and early 1960s? Or fitness effects of resistance in the 23 yr since Gressel and Segel's first paper? Eventually, we end up with review articles and summary tables, from which we can draw limited generalizations. But we have not advanced weed science as much as we should have in all that time.

Again, the danger is to concentrate on the symptoms. I would suggest that the *cause* of our tendency to do repetitious research is a general failure to formulate incisive questions and testable ideas. An interesting exercise that I have used with my undergraduates is to review the last paragraphs of the Introductions of random papers in *Weed Science.* It is easy to see the abundance of descriptive phenomenological objectives: to measure the effect of weed x on crop y; to determine whether a resistant population is less fit than a susceptible population; to describe the effects of 10 tillage × herbicide combinations on seedling density. Few pose and then test ideas that are based on proposed biological mechanisms. As a result, it is not surprising that our understanding of weed science advances only very slowly.

As in the case of statistics, I believe that the problem is in the way we teach weed science and the way we exchange ideas with our peers. In my experience, weed ecology is taught largely at a phenomenological level, rather than in a mechanistic way. We tell students, and communicate with our peers, about the relationship between numbers of seedlings and tillage, rather than the interaction between soil physics, seed biology, and soil movement. We try to communicate about the basis of competitive ability by comparing yields of unrelated crop varieties grown with and without weeds. Or we report on spatial dynamics by producing maps from widely spaced quadrats interpolated by geostatistics.

One cause of inadequate question formulation again stems from the way we use statistics. We place great emphasis (to an extent that surprises most statisticians) on testing null hypotheses. Statistics is an essential tool. However, the null hypothesis ignores biological knowledge and previous research results. In effect, it poses a very unscientific question: "Can we disprove that nothing will happen," rather than the more informative alternative hypothesis: "On the basis of my biological understanding, the following should happen: does it?" In the early stages of a research field, the null hypothesis may be useful in demonstrating whether there is anything of a measurable size worthy of

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further research. Once this first null hypothesis has been disproven, surely it is of little use to repeat the test again and again? We know that herbicides kill weeds; we conduct herbicide trials to measure the relative sizes of their effects, not to ask if we can disprove that herbicides have no effect! No reasonable statistician would ever expect us to test a hypothesis that we could not believe.

How, then, can weed scientists acquire the ability to pose more informative questions? One approach is to encourage them to review critically those papers that stand out as having achieved real conceptual advances in weed science. An interesting exercise in itself would be to compile a list of "landmark papers" for different subdisciplines. Scientists and students could then analyze not what the papers *found,* but what *approaches* they took. In weed ecology, many of the key papers (in my opinion) have started from a question, "If I want to be able to *predict* what would happen, what do I need to know?" To predict emergence, we need to understand responses to such things as temperature and soil water (Forcella et al. 2000); to predict the effect of a weed on a crop, we need to know the physiological ways in which the species responds to the availability of resources (Kropff 1988); to measure the *true* fitness effects of a resistance gene, we need to produce plant lines differing (as near as possible) only in that gene and to measure the quantities demanded by the definition of fitness (Jordan 1999). Having examined such papers, scientists would hopefully apply the principles to their own work. For example, to try to explain why some crop varieties are more competitive than others, my group has been measuring growth and developmental responses, under interspecific competition, of crop lines differing in genes for traits previously hypothesized to be important. We have crossed good and bad competitors and have estimated heritability and genotypic correlations within their progeny. By considering the dynamics of resource use within a season, we have posed and tested hypotheses of how competitive outcomes will change for plants differing in those traits and under particular conditions of resource availability. The many previous comparisons of small collections of unrelated cultivars had merely generated general hypotheses; each repetition for varieties in a different location had resulted only in the repeated statement of those same hypotheses (Lemerle et al. 2001).

I have learned a great deal from the critical evaluation of approaches taken in key research papers. I challenge others to do the same.

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