Abstract There is much research in “weed science” that is not science or that will have little impact on the future development of the subject. Examples are presented to support this argument, including the preponderance of phenomenological experiments, an over-reliance on repetitive superficial case studies and the application of erroneous methods. It is argued that a future agenda for weed science should include an approach based more on answering questions, the facilitation of multidisciplinary teams, a greater desire to learn from the work of others and greater self-examination of approaches.

INTRODUCTION

The title of this paper clearly implies that, in the view of its author, much research that is called “weed science” is not science at all. This is a bold statement, especially coming from someone with no training in philosophy (like most other so-called Doctors of Philosophy in science, I have read little philosophy – Medawar’s “Pluto’s Republic” bored me so much that I never finished the first chapter!). The title is also bound to upset many dedicated researchers who consider that they have achieved much in the name of science. However, I will certainly argue that their efforts have, in a great many cases, not achieved as much as they might.

Not all research is science, and hence not all experiments on weeds constitute scientific research. It is, of course, easy to counter such a claim with the rejoinder that it depends on your definition of science, and that definitions are both vague and varied. We must therefore begin by discussing the definitions of science.

Most definitions of science (see, for example, those in the Oxford English Dictionary and Webster’s) refer to the acquisition of knowledge. Philosophers have, of course, been arguing for centuries about what knowledge is and whether, indeed, we can ever know anything! Probably more books have been written on this narrow area of epistemology than on the mix of contrasting disciplines that we now call “weed science”.

The vast majority of practicing scientists would, however, feel strongly that through their research efforts they are adding to knowledge. Whether or not we can indeed truly know anything, I am satisfied that through our research efforts we are steadily increasing our understanding of the world around us; this, for me, is the essence of science.

Definitions of science usually also include reference to the way that this knowledge is collected or assembled. For example, they often refer to “organised knowledge”, “knowledge gained by systematic study” or “truths systematically arranged”; systematic is in turn defined as involving a system, method or plan. It can thus be argued that science involves putting various pieces of information together into a higher level of organisation, such as a set of principles, general laws or theories. Hence science is far more than simply listing or accumulating empirical data.

Many of us would have learnt in high school that there is an accepted basic scientific method, involving hypothesis generation and testing, through which we develop our level of knowledge. Popper and others have argued the fallibility of this method and it would certainly be difficult to argue that research is only science if it uses this hypothetico-deductive method. However, most of us would accept that some form of logical inference is central to science. Hypothesis generation, estimation, observation, logical argument, experimentation and theoretical abstraction (using mathematics) are all undoubtedly tools for what we call science. But we need not use every tool for every job.

It may be argued that weed science is an applied science, and hence involved with the solution of problems rather than with knowledge for its own sake. But is this any reason for accepting a different set of values? Surely if we understand the workings of a system, we will be able to harness it more effectively? To me, weed science should by definition be the application of science to weeds (and their control), rather than the application of the outcomes of science. Much of current “weed science” can thus be argued to be technology, rather than science: concerned with designing

WEED SCIENCE DOESN’T HAVE TO BE A CONTRADICTION IN TERMS!

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tools to achieve some purpose. We may use experiments and statistics to validate a machine’s performance, but this does not make it science (though the phrase “scientifically proven” is often used by marketers!). It is significant that the Weed Science Society of America now has two journals – Weed Science and Weed Technology, recognising this distinction.

In summary, to my (non-philosophically trained) mind, what makes some research “science” is the deliberate use of appropriate tools to generate a greater level of understanding about the world around us. Many papers on weeds merely add items to our database; except in the most infinitesimally small way, they add nothing to our understanding and are therefore not science. Experiments that try out different weed control methods (or herbicide doses) without attempting to understand why they achieve those results are not science. Incorrect application of the tools of science, such as the application of flawed logic or the use of incorrect statistical tests to support an argument, are also, in my opinion, not science. Hence, there is thus much in current weed science that indeed makes it a contradiction in terms.

My objective in writing this paper is not, however, to initiate a debate on what is or what is not science. Instead, it is to raise serious concerns about the slow rate of progress in developing a detailed level of knowledge within our field of research. Apart from the occasional introduction of a new analytical method from elsewhere, little changes from year to year in weed science, while other research areas seem to advance in leaps and bounds. It is easy to answer, as some philosophers have, that science relies on occasional flashes of inspiration, and that there must necessarily be intervening periods where not much seems to happen. However, I am far from convinced that in weed science we are making the best use of the research tools available to us. I am concerned that this is, to a major part, because we have lost track of what science is and how gains in knowledge are best achieved. I will therefore attempt to show examples of where scientific practices are poor and will indicate how more meaningful advances could be made through better applications of science. Even if we may not all agree on a definition of science, hopefully we will be able to agree that there are some approaches that help our understanding to increase rapidly, while others add little to knowledge and may even prevent progress.

**EXAMPLES**

Rather than pick on particular papers, thereby singling out their authors for criticism, I will draw on generalisations of articles from the recent weed literature.

**Phenomenon Fixation** In weed studies there seems to be a particular emphasis on the description of the direct outcomes of experimental treatments. A herbicide is applied and the level of weed control measured; various tillage methods are used and the number of weed seedlings counted. The emphasis is thus on the phenomenon and not the process. We tend to ask “what” questions rather than “why”. I would argue that progress in understanding any system will be slower if we treat it as a “black box” and just describe its output, rather than trying to understand the processes going on inside.

When I picked out a copy of a recent weed journal at random, every paper had as its objective the description of data. For example, “There are no data on the effects of this weed on this crop, so the aim was to describe yield losses at a range of weed densities”. “There are few data on the effects of this herbicide on these weeds….” Not one paper had an objective of testing an hypothesis, only passing reference was made to underlying processes (usually in the Discussion in trying to interpret a result), and few explained why the piece of work was needed (either from a practical point of view or in order to advance understanding). None of them justified their descriptive experiment in order to understand some process at a higher level. Although hypotheses may not be a necessary part of science, they do reflect an attention to understanding a system rather than pure description. “If my understanding of these processes is correct, then if I do this, the following should happen…” Moreover, given that hypotheses are sometimes raised in Discussions, why are they so seldom tested?

Tillage is an excellent example of how repeated phenomenological experiments have added little to understanding. There have been many tens of papers describing the weed populations after differing tillage treatments. Yet we still have little ability to predict which species in a community will increase and which will decrease under particular tillage regimes. It seems remarkable to me that Mohler’s (1993) paper in which he established a framework for looking at seedling emergence in relation to tillage is almost never quoted in the weed-tillage literature. It remains to be seen whether Forcella’s recent work on predicting emergence will have any major effect on the way research is done on weed populations.
One powerful tool for trying to generate an understanding of the way something works is to build a model. We are not short of models in weed research. However, there is a tendency to separate modelling from the mainstream of research (even within a single research organisation). For example, we have a small group around the world using growth simulation models to try to understand the processes of competition (and, in part, thereby obtain better predictability of yield loss), but there is a much larger number of researchers still working at a phenomenological level, writing their papers as if the underlying biological processes are of no consequence to their work. I am aware that models are an object of incomprehension or fear for many, and the skills are not easy to acquire; however, they provide a powerful way of summarising knowledge and making testable predictions.

No doubt one reason for the preponderance of phenomenological work is the desire to obtain better weed management. Indeed, this is the very justification for some agencies to fund our work. Many experiments thus ask simply whether some management tool works, or which of several control methods is best. In concentrating on the outcome, however, we miss much. Why did some methods not work? How could we improve their performance? Again, I would argue that the shallowness of the objectives coupled with a lack of critical reflection about processes, results in poor knowledge of our subject.

Catalogues of Case Studies As I skim the contents pages of weed journals, I seem to have seen so many of the titles before. The only difference seems to be the name of the weed or the herbicide. The contents are also familiar, with the usual set of treatments generating the traditional sets of measurements. I therefore wonder why this should be. What does it add to our knowledge to have research producing so many copies of the same thing (albeit on different focal objects)? There may be a limited need to compare a particular organism with another, but how much do we learn from repeating this again and again? This is not confined to weed science. Following Harper’s (1967) seminal paper on plant populations, the ecological literature was flooded with case studies of “The population biology of x” or “the demography of y”. It would seem that it is in the nature of researchers that when they see a new method or approach, they readily apply it to their own system. Unfortunately, the repeated application of the method often seems to take over from the answering of questions. “If we apply this great method often enough, some gem of wisdom or interesting observation will fall out”.

The mere generation of case studies does little to advance our understanding of a subject. It takes a great many such studies to be able to generalise, and even then the conclusions may be weak. Consider an area in which I have worked, weed-crop competition. In 1985 I reviewed over 300 data sets where crop yields were described at a range of weed densities. There have been probably as many such studies in the years since. Many of the studies were (and still are) justified by the need to calculate economic thresholds (even though thresholds have seldom been used in practice) or to be able to assess the economics of controlling the weed (even though the economic analysis is seldom done). But what have these shown us? We have found that species differ in competitiveness (but still have little idea why) and that within a species competition varies between years (ditto). So, we try out different regression models based on leaf area, and find that there is still enormous variation in their parameters (Lotz et al., 1996) and we can still only speculate as to the reasons for this. Rather than feeling proud that my 1985 paper receives so many citations, I am disappointed because researchers are blindly copying the analytical method, churning out case studies for little benefit. My high citation rate is thus an indictment of the poor state of our science!

Advances in understanding of competition have mostly been achieved by moving away from descriptive case studies. We are now combining population studies with measurements of plant physiology and observations on plant morphology and phenology. We are thus concentrating on understanding processes and using models to make predictions. Under what circumstances would we expect competition to be most intense? Can we predict the outcome of competition from studies of monoculture; if not, why not? How important is competition for water in comparison with competition for nutrients in various Australian environments? What plant traits would we expect to enhance competitiveness; do studies using isogenic lines with these characters support our hypotheses? Such an approach will advance our understanding far more than even several hundred more case studies of density responses. It may be easy for an academic to say this, since I am not constrained by the expectations of a government department or chemical company. However, I would argue that a little more “academic” content would add value to most practical research in the long term.
Catalogues of case studies can be found in many other areas of weed science. How often do we see papers describing the genetics, fitness or other attributes of the latest biotype of weed resistant to yet another herbicide? Or (increasingly) a map of another weed produced by kriging (impressive though the graphics might appear)? It is easy to argue that it is important to know about the latest case of resistance so that we may better control the weed, but how often have such studies led to better weed management? More importantly in the current context, how much have they increased our understanding? We already have enough studies to generalise that resistance is almost always due to a single, partially dominant gene and that it may or may not be due to increased metabolism or a modified site of action. But other than adding to publication records, can several dozen more case studies add significantly to science? They may throw up an occasional interesting anomaly, but this is very hit-and-miss. What are the more conceptually important questions that will really advance knowledge more rapidly? Maps of weeds are increasing in number and it is already obvious that there are many key questions about the processes determining spatial pattern and the scale of aggregation. Surely it is worthwhile concentrating our efforts more on these “bigger” questions than on generating yet more case studies of patterns of certain weeds at particular scales in a specified location?

Case studies are clearly of value, but to a limited extent. We need case studies to help develop theoretical frameworks for science, to provide parameters for our models and to test hypotheses. They are needed to tell us about the generality of our conclusions, but there is a law of diminishing returns. I therefore question whether we have our balance right in many areas of weed science. I would argue that we learn more from a few highly detailed case studies, looking on a fine scale at the processes of seedling emergence in relation to the physical environment in the surface region of the soil, than from a catalogue of several hundred superficial observations of seedling numbers after disc ploughing vs zero tillage. Likewise, a small number of biochemical and glasshouse studies of resistance linked intimately to detailed field studies of pollination ecology, gene flow, population genetics and population dynamics will tell us far more for the same effort than the accumulation of case studies as each future resistant biotype appears.

**Careless Copy-Cats** One of the pitfalls with copy-cat research of the type described above is the fact that errors or poor science are perpetuated. A method is applied, it looks interesting, so it is copied – along with its errors. Even when these errors are pointed out, the popularity of the method (and the apparent previous endorsement of its acceptability) may make little difference to its use. How often does this occur?

One of the commonest errors in agricultural and biological papers is the mis-use of statistics. Figures of 70% of papers containing statistical errors have been recorded (see references in Cousens, 1988). The many papers that have been published by statisticians and others to point this out, have had little impact on the use of statistics. The criticisms of the use of multiple comparison tests on structured experiments, especially the rote application of Duncan’s Multiple Range Test in North American journals, led Weed Science to adopt the LSD – another multiple comparison procedure and just as inappropriate! The reasons for this apparent inability to address statistical errors are no doubt varied, but probably include: a feeling by many researchers that statistics really doesn’t matter anyway; a lack of interest in articles on methodology; a narrow scope of journal readership (articles on mis-use of statistics in weed research appeared in European journals, and those on the problem in North America were not in journals on weeds); and the fact that many research habits are set during university education.

Other copy-cat errors involve the experimental methods rather than their analysis. For example, the logical flaw of deducing the pleiotropic effects of resistance genes from comparison of means of a single resistant population and a single susceptible population collected from different sites has been pointed out (Cousens et al., 1997), but the same types of study continue to be published. Do the researchers in the area and the editors disagree with these arguments? Do they consider that the arguments may be correct but that they won’t matter to their conclusions? Do they consider that the more appropriate methods will be too difficult to adopt, and hence they make a conscious decision to stay with poor science? Or do they again not bother to read articles on methodology. After all, the current method has been considered good enough to publish until now, so why bother to change approach? However, three referees and an editor considered that the criticism was correct…

Another type of copy-cat error is from mis-application of definitions. An example of this is the use of the term “fitness” in studies of herbicide resistance (see also Jasieniuk et al., 1996). Many researchers measure fitness as biomass or seed production at the conclusion of a pot experiment. Seed production is only
one component of fitness: the fitness of an individual should include other aspects of its lifecycle which contribute to the plant’s ability to leave offspring in the next generation. A 25% reduction in seed production may be accompanied by a one week delay in germination, a 5% increase in seed longevity and a 10% increase in plant mortality. Fitness is the net outcome of all these effects (Kremer, 1998) and can better be assessed by the intrinsic rate of increase. In the resistance literature we now see more references to the “components of fitness”, perhaps in response to a realisation of this; however, if the aim of the research is to measure the cost of resistance, assessment of individual components of fitness will not have achieved the objective. Moreover, fitness is relative to the habitat in which the plant will grow. A small reduction in rate of emergence or seed production of a single plant in a pot may be magnified into a very large reduction when the plant competes with a crop in the field. Given that resistance is not always complete, fitness in the presence of herbicide may not be the same as fitness without the herbicide.

The problems with assessment of fitness do not end there. If the aim is to assess the cost of the fitness gene, then taking the average of a mixture of homozygotes and heterozygotes will give a poor assessment of this. The resistant and susceptible populations may have different genetic backgrounds (as discussed above). Even within a single population in which resistance is evolving, the resistance genes may occur in a biased sub-set of the population genotypes, hence a comparison with the population mean of susceptibles will give a biased estimate of the cost of the gene.

The message to be appreciated here is that a detailed consideration of methodology is essential in any field. It is important to recognise the situations in which errors may occur from lack of knowledge and to know when help is needed. Most researchers are not experts in statistics and must recognise that, despite having done a superficial course at college, they will be prone to errors. Most weed researchers with a general agricultural background will not be experts in population genetics and evolutionary theory, surely essential for anyone working in the area of the evolution of resistance.

**Statistical Straightjackets** Statistics is often taught as a set of recipes. There is thus a tendency to believe that it is necessary to conform to particular procedures and to apply them without thought. As before, a failure to appreciate the methods being used can result in poor science: research progress can be impeded and even nonsensical conclusions can be reached.

The randomised complete block and other experimental design recipes have been significant tools in the development of agricultural science. However, their rote application can also be argued to have held back knowledge considerably. The essence of ANOVA is that by filtering out sources of variance we have more confidence in our estimation of the effects of treatments. We filter out the noise in the system in order to see the signal. So we throw away the variance, assuming it to be uninformative, and we concentrate on the direct effects of the treatment. When designing the layout of the experiment, we choose a site that is as homogeneous as possible and block replicates so that as much noise due to location in the field as possible can be filtered out. We often repeat the experiment across years and sites in order to confirm the generality of the results, and again we throw away the variance. If this “unexplained” variance was all experimental error, then little harm will have been done. However, consider the case of a soil-applied herbicide: we see the mean effects of its application, but we miss the interaction with soil type variation across the site (thrown away) and the effect of weather across years (thrown away). This effect of soil type may be critical in understanding variability in the performance of the herbicide. We may say that there is a G (or other treatment) × E interaction, but how often do we investigate the reason for the environmental component? An alternative approach is to deliberately set out to study variation in its own right.

Of far more concern is the influence of the null hypothesis. This is the very antithesis of science, replacing the formulation of hypotheses based on understanding by the approach “I know nothing, can I prove otherwise?” (Cousens and Marshall, 1987). Rather than examining results in relation to logical expectations, we see papers simply concluding that the effect of this or that treatment was or wasn’t (statistically) significant. The next step (illogical, but all too easy) is to equate statistical significance with biological or economic importance and to conclude that non-significant results are proof of no effect of a treatment. All of these interpretations are common in the weed science literature and are flawed in logic. For example, a real biological difference between treatments might have to be greater than 20% may be able to be de-
tected as statistically significant, and a difference of only 5% may be of economic relevance. Just because an effect cannot be detected above a large amount of background noise does not mean that it is not there.

The null hypothesis is merely a way of quantifying the level of reasonable doubt in an experimental outcome. If the null hypothesis cannot be rejected it simply means that we cannot be sure of the outcome, not that the outcome is zero. Unfortunately, most statistics courses for agricultural students emphasise the null hypothesis and fail to discuss adequately the existence of this “Type II” error in its interpretation. A paper of mine pointing out the prevalence of misinterpretation of the null hypothesis in agriculture was once rejected by Frank Yates (of Fisher and Yates tables fame). The gist of his argument was that its subject was irrelevant: why would anyone want to test a null hypothesis that they could not entertain accepting? He argued that if you do an experiment on the effect of fertiliser on yield because you expect it to increase yield (and it always has done in every previous experiment), why on earth test the hypothesis that fertiliser has no effect? Such a question is pointless and uninformative. So it is, but Dr Yates was apparently unaware of just how often such pointless hypotheses are tested and how often their meaning is misinterpreted.

One way in which the null hypothesis (and statistical significance) has been commonly misinterpreted is in comparing quantitative series of treatments, such as a range of herbicide doses or weed densities. Each treatment is (inappropriately) compared with the control and if the difference is not significant the null hypothesis of no difference from the control is accepted. The shape of the relationship thus inferred may bear little resemblance to the trend shown by the data (Cousens, 1988). The reason for including a range of levels of a treatment is to describe the overall relationship between level and response; the appropriate technique for this is regression, not pairwise tests of untenable null hypotheses.

Regression is also mis-used. In weed papers we commonly see straight lines fitted to clearly curvilinear responses, peaked (quadratic) curves fitted where there is no sign of a peak (or other cases of fitting of wrong-shapes curves), lines allowed to have an intercept where no intercept could logically occur and inverse polynomials fitted without considering their error structure. A paper pointing out problems with fitting these inverse polynomials to competition experiments was given an award for best paper in Weed Technology (Cousens, 1991), but almost every application of the method in Weed Technology and Weed Science since that date has ignored its advice.

Other statistical errors include failure to correct for unequal variances by using transformations. On so on. The mis-uses of statistics are legion! However, I have no doubt that many of these are the result of researchers feeling that they have to use particular recipes of designs, tests and null hypotheses. If they understood the philosophy of statistical analysis better, and were trained in how to choose the appropriate tools for the job, these errors would occur less often. I remember a seminar by a Professor of Statistics in which he stated that in his opinion the biggest problem with statistics in biology was its over-use. How often do you see straightforward biological discussions swamped with statements about which means did and didn’t differ significantly and at what level of probability? It is difficult to see the scientific wood for the statistical trees!

**CONCLUSIONS**

If, as I argue, there is little science content in much of weed science, what should we do? Surely such a state of affairs is serious if we are to hold our heads up high. Clearly, if we stop referring to our research as weed science we could avoid the problem. Let us accept, with no loss of pride, that much of what we do is weed technology. That still, however, leaves much weed research that clearly has nothing to do with technology. Moreover, avoiding the issue is equivalent to accepting that we are happy not to be regarded as scientists. What we really need to achieve is a raising of standards in the discipline. We can try to do this through a range of mechanisms based on debate and education. No change will occur, however, until we achieve ownership of the problem and a widespread acceptance that standards are poor.

“Holier than thou” papers (such as this one?) will no doubt achieve little. For example, a paper on the misinterpretation of statistics in weed research, published in a major weed journal and receiving a very large number of reprint requests (Cousens, 1988) has so far had little perceptible effect on statistical usage. The problem of statistical mis-use is not yet owned by the practitioners, their managers or their teachers. Those people who have accepted the problem have had little impact on the editorial boards of their journals, and have not achieved significant changes in the way that statistics is taught.
The example of statistics shows how we need to educate both the active researchers and the educators of the next generation. How do we do this? A direct assault on their credibility is doomed to failure! One essential approach is to lead by example. We must ensure that top quality pieces of research that clearly advance our science are published in the journals that weed scientists read. But this requires a considerable level of altruism. I am currently trying to publish one of my best pieces of research in a top international ecology journal in order to get to a wider audience. How many others do the same? Arguably, if I want to change thinking in weed science, I should publish in a weed journal. But my ego also needs massaging!

There are other ways of advancing the debate, if not the solutions. Many conferences have review papers. These often present the status quo and are delivered by experts, but they tend to be self-congratulatory for those in the area and somewhat tedious for most present (as those of us at the Second International Weed Control Conference can vouch!). Instead, what about asking a respected scientist from outside of that area to do a critical review, directing them not to pull their punches and to examine the bigger picture rather than the detail? Another approach would be to have a retreat of respected scientists from a range of disciplines, asking them to review a number of core areas and to lay out an agenda for future research. Again, the focus must not be on the detail but on the general state of our “organised knowledge” and the approaches being used to develop deeper understanding. We should also reorganise more of our conferences to focus on weed science rather than weed management; focus on management has not encouraged thought about underlying processes. For many of us, it has been a disappointment that the word Control is in the title of the International Weed Control Congress (though we accept that this will help achieve sponsorship); this could have been the forum in which to achieve a new agenda for weed science. The same may be said for the CRC for Weed Management Systems.

I have stated repeatedly that the aim of this new agenda should be achieving greater knowledge and understanding rather than the accumulation of data or (directly) the achievement of better weed control. What are the changes that therefore need to be achieved? The following are some initial suggestions:

- A focus on asking critical questions. The simplistic question “what happens when I apply this treatment” advances science only slowly. Better questions are “why does this happen? What are the processes involved? What information do I need to collect to be able to predict the outcome?” Thus we build up a theoretical framework from which we can pose real hypotheses, not uninformative null hypotheses.

- Appreciation of limitations in experience. We are not trained as experts in every one of the diverse components of weed science. Most weed scientists are generalists. Many questions in weed science, however, would benefit greatly from intimate knowledge of fundamental disciplines. There is little doubt that my own knowledge of statistics is inadequate for many of my research questions, and although I have been working on competition for 16 years further advances in this area require trained physiologists. As a Clint Eastwood character said, “A man’s got to know his limitations”. I can read up on a subject, but this does not make be an instant expert. We see very few multidisciplinary teams in weed science. Rather than nobly trying to tackle problems on their own, perhaps weed scientists should see their role more as facilitators of groups of discipline experts. For competition, my research applications should include a collaborating team of a physiologist, ecologist (me), breeder, quantitative geneticist, agronomist, modeller, and statistician. The team may be difficult to assemble, but surely the potential is far greater than if I continue to catalogue phenomenological case studies on density responses or if I try to dabble in weed physiology on my own?

- Lifelong learning. Each of us has our own “comfort zone” in research, and we are no doubt knowledgeable within it. However, we need to make sure that we continually work to update and extend this knowledge. Do we all do this sufficiently? I have certainly heard a weed ecologist at a conference saying “this next session is on herbicide physiology, so I won’t bother to go”, and an academic in my own crop production department saying “the seminar doesn’t interest me this week, it’s on biotechnology”. No doubt that happens in other subdisciplines as well. Does this reflect an attitude that we can’t learn from the approaches of others, or that our time is too precious to waste on these diversions? I certainly encourage my own weed students to join me in
attending zoology and botany seminars. We need to make sure that we keep up to date in our own areas and that we read widely in related areas. For example, I would argue that every weed ecologist should read articles in Ecology, Oikos and Oecologia on a regular basis, and that every weed scientist in one continent should read weed journals from others. I had assumed that this was standard practice, but at my last WSSA meeting I met an experienced weed scientist who commented that he hadn’t realised that Weed Research was still published!

Self-examination. Without this, there can be no ownership of the development of a real weed science. The objective criterion for this should be the question “when was the last time I made a conceptual advance in my research”, not “how much data have I been successful in collecting”. This is exactly why I am presenting this paper now. I was faced with my mid-life crisis: what have I achieved and why haven’t I done better. I am certainly not holier than thou, as you will see from reading my publications. I have learnt as my career has developed and from making mistakes.

REFERENCES


