Strategies for organic research

Willie Lockeretz
Friedman School of Nutrition Science and Policy,
Tufts University, Medford MA 02155, USA

ORGANIC AND CONVENTIONAL RESEARCH: HOW DIFFERENT?

In this paper I offer three suggestions for increasing the value of organic farming research. However, these suggestions could apply just as well to conventionally oriented research, if conventional researchers were so inclined. Moreover, later I argue that organic research should be brought into closer contact with mainstream science.

This might seem surprising, since a commonly expressed opinion in organic circles is that organic and conventional research are, and should be, carried out in fundamentally different ways. Therefore, in thinking about possible ways that organic research could be improved, it is necessary to consider first a related question: how different are organic and conventional research, and how different should they be? Otherwise, we may be unnecessarily limiting the range of strategies we consider as we think about future organic research. (I should emphasise that in what follows I will be considering only *how* the research is done, not its broader goals or underlying values. For example, organic research should and often does emphasise preventive solutions to the adverse environmental and other side effects of modern agricultural systems; the idea is to develop production systems in which the problems never arise, rather than waiting for them to reveal themselves and then trying to deal with them with additional technology.)

I have yet to be convinced that the differences in how organic and conventional research are done are as fundamental as they are made out to be in the usual rhetoric heard in organic circles. Some organic researchers seem almost obsessed with doing things differently from conventional researchers. I think, rather, that we should concentrate on doing our job in whatever way is best, regardless of how similar or different it is from what conventionally oriented researchers do. We should be concerned about what the research accomplishes, and not worry if it is done on an experiment station rather than on farmers' fields; nor if it is researcher- designed rather than participatory; nor if it is 'reductionistic' rather than the more fashionable 'holistic'; nor if it is merely single-disciplinary when multi-disciplinary is the way to go; nor if it remains grounded in an obsolete 'Cartesian' or 'mechanistic' worldview rather than a new 'epistemology' appropriate for our new age; nor if it still clings to 'positivism', rather than whatever the opposite is (negativism?). I also believe it is time to stop being obsessed with creating new 'paradigms.' (To digress for a moment about that greatly overused vogue word: originally it was a technical term in grammar, where it meant a model, an example that shows how to conjugate a verb, say, or to decline a noun. In our modern understanding of what grammar is, in contrast to an older, more prescriptive view, first the people talk a certain way, and *then* the grammarians offer paradigms that codify the way people are already talking. The moral for organic researchers is that instead of announcing in advance what the new 'paradigm' should be, we should look over what we have actually done and figure out what has worked best, and let that set the standard. I expand on this point at the end of this paper.)

Why does it matter if we overstate the differences between organic and conventional research strategies? There are three reasons. First, discussions about the nature of knowledge in agricultural science often are so removed from practical considerations, so abstracted from the world of real research, that they are sterile and fruitless, a waste of time and effort. Second, the claims made regarding the special nature of organic research often are simply wrong; the bulk of actual organic research does not look very different from conventionally oriented research. It may be personally satisfying to think that one is doing something special, and presumably better, but I would worry about any branch of science where the rhetoric is so removed from the reality.

Most important, though, is that the obsession with being different means that some research approaches, such as on-farm research, participatory research, and multidisciplinary research, have been exalted to a stature that they do not deserve. They have become ends, not means, presumably because keeping a distance from conventional research is thought to be desirable in its own right. Even worse is to assume that because these approaches are more common in organic research, they are not just different, but better. A great variety of research approaches – if wisely chosen for the question at hand – can be appropriate and effective, just as, if not wisely chosen for the question at hand, they most decidedly will be neither appropriate nor effective. Working on-farm is very good for some organic research questions, but the growth chamber, the glasshouse, and the experiment station all have something to contribute too. Similarly, some organic research should involve farmer participation, but some should be conceived and executed entirely by researchers. And single-discipline research definitely has a place alongside its more fashionable relatives, variously known as *multidisciplinary, interdisciplinary*, or *transdisciplinary*. Nor should we forget that all these 'organic' research strategies could be and in fact are used in conventional circles as well.

ORGANIC RESEARCH AND MAINSTREAM SCIENCE

Therefore, my suggestions about how to improve organic research go against the prevailing current in that they are intended to make it *more* like mainstream science, not less. But note that I said mainstream *science*, not mainstream agricultural research. It is important to distinguish the two because conventional agricultural research as actually done today does not necessarily reflect the best that can be done within mainstream scientific thought, and it, too, could benefit from these suggestions.

My first suggestion is that agricultural science should be connected more closely to the broader disciplines from which it has separated. Such a connection could infuse it with imaginative, fresh insights and help put it on a sounder conceptual foundation. In the US, at least – this may be less true in the UK – the agricultural disciplines are largely separated from their parent disciplines, and have been for about a century. Thus there is economics, and there is agricultural economics; there is zoology, and there is animal science; there is botany, and there is crop science; and so on. These specialities are not simply portions of the larger disciplines' territories. Rather, they are self-standing disciplines in their own right, with their own academic departments (which in turn are part of their own schools), their own research institutions, their own professional organisations, their own journals, and their own cultures.

When agricultural science started to gain importance around the start of the 20th century, the US policy of giving it its own institutions might have made sense. The country was rapidly industrialising, and agriculture was the unwanted child of mainstream science. It is possible that agricultural science would have suffered from neglect without its own institutions that gave it a special protected status and allowed it to grow and develop (Rosenberg, 1971). But whatever national goal might once have been served by separating agricultural science from its natural parents, that goal has long since been achieved and has long been out of date.

The result has been to isolate agriculture intellectually and culturally, turning it into what Mayer and Mayer (1974) dubbed 'the island empire.' The price of agriculture's special institutional status has been that it has become parochial and self-satisfied, accountable only to itself, and deprived of the challenge and stimulation that would come from more contact with mainstream scientific thought.

To bolster this point, I offer three related questions, which are intended for any agricultural scientist, no matter whether organically or conventionally oriented. First, suppose that Darwin, Einstein, Mendeleev and Pasteur had become agricultural scientists: how would the field be different today? I don't know the answer, but I think the question is worth pondering. Second, who are agricultural science's intellectual giants, past or present, those who achieved what the rest of us could only dream of? Are there any? Finally, how often does it happen that an agricultural scientist reads a research paper and thinks: 'This is a really great piece of work. I wish I could think as deeply as that!' Ever? If not, there is serious cause for concern, because it says that the shortcomings of today's agricultural research are deeper than questions of methods, such as whether we should be doing more research on farmers' fields instead of experiment stations.

This proposal to rectify agricultural science's intellectual isolation is a call for a kind of interdisciplinarity, but a different kind from what one usually hears advocated in organic circles. The usual call is for horizontal connections among the various agricultural disciplines, such as agronomy, economic entomology, and agricultural economics. But in addition to these connections, I am advocating a vertical reconnection of the agricultural disciplines with their parents, to undo a separation that perhaps never should have occurred.

However, the terms of this reconnection must be right. I am not calling for more of what some agricultural science departments have already done, which is simply to abandon their essential mission of dealing with the problems of real agriculture and real agriculturists, and make themselves over into look-alikes of departments of molecular biology that just happen to specialise in agricultural applications. What we need instead is a true intellectual partnership, in which more scientists from the parent disciplines are recruited into the agricultural research effort because of what they can contribute towards the advancement of agriculture. There is nothing to be gained from turning the island empire into a colony of a larger and more powerful empire – and a willing colony, at that. To avoid that would require the non-agricultural scientists we work with to be of a special sort, those who acknowledge the legitimacy of 'applied' research – which agricultural research always was, still is, and should remain – and who are keen to see the non-agricultural sciences be put into the service of applied agricultural research, rather than turning it into something very different.

'What's my motivation?', the non-agricultural scientist might ask. The answer is that the right kind of partnership could be mutually beneficial. Not only would agricultural researchers benefit from being in contact with the broader currents of scientific thought; their non-agricultural counterparts would benefit from being able to work with enormous quantities of detailed empirical data from a domain they do not usually work in, a domain subject to a unique combination of natural and human forces, unlike the purely natural systems studied by most non-agricultural ecologists, for example (Levins, 1973). Collaborating in agricultural research could be very stimulating and challenging for both parties. Many areas of agricultural research could benefit, but especially organic research, which is supposed to be based on a deep understanding of fundamental ecosystem processes. So far, however, not a great deal of progress towards developing that understanding has been apparent; perhaps the more basic scientific disciplines have something to offer.

AGRICULTURAL RESEARCH AND CUMULATIVE SCIENTIFIC KNOWLEDGE

My second suggestions is that agricultural researchers – whatever their orientation, but especially if they are concerned with organic systems – should pay more attention to the fact that an individual research project is not self-contained. Its value lies not simply in what the project itself has learned, but also in what it contributes to our cumulative understanding of some phenomenon, because that is how science advances. We are seeking an *understanding* of organic systems – not simply a large body of unsorted empirical data. We commonly hear that conventional agricultural research is too fragmented, that it breaks the system being studied into unrealistically small and unconnected pieces. The criticism often is valid. But at least as serious is a kind of fragmentation that organic research is just as guilty of, namely, the failure to relate one's study to other studies of the same question (Lockeretz & Anderson, 1993:177-181).

Overwhelmingly, the agricultural research literature consists of self-contained papers. Of course, there is always the *pro forma* literature review at the beginning, but that is usually the last of it. Typically, there is little to indicate that previous research influenced the current research at all, neither in how the research was designed nor in how the findings were interpreted. It is as though the only question on the investigator's mind was: what happens when I do exactly this, in this particular place, under precisely these conditions? If the literature cited at the beginning is referred to at all at the end, usually it is to say no more than 'Our results agree [or conflict, as the case may be] with those of … ', as opposed to actively making the most out of the fact that now we have two results where formerly there was only one (or eight where formerly there were only seven).

True, sometimes a public-spirited agricultural scientist will write a review article covering the research results on some question. This is a valuable service that often is severely undervalued compared with primary research. However, such articles typically involve just compiling and summarising the research, and perhaps reporting some general trends in the results. This falls short of what I am calling for, which is to analyse the results actively, on a higher level, with the hope of understanding them more deeply..

This call runs up against the endlessly repeated truism that organic systems are highly site-specific, and that each farm is unique. This truism is in fact true, and is very relevant when one is seeking to help a farmer solve a particular problem. But all it means is that a particular practice will not necessarily give the same results as it gave somewhere else. It definitely does *not* mean that one cannot get a better *understanding* of what is going on in one place from what has been learned somewhere else. Let us not be so obsessed with site-specificity that it paralyses our efforts to improve organic farming systems by understanding them in a more general way.

An analogy with human health might be informative here. If every farm is unique, so, too, is every human being, even an identical twin. Suppose a person has some health problem and goes to a doctor. Would that person really want the doctor to say: 'Well, there has been a lot of research on how to cure your condition, but it won't do you any good, because it was done on other people, and every human being is unique, so let me start from the beginning and figure out what should be done in your case'?

Good health care, like good farming, involves a mix of the general and the particular. In our obsession with the particular, let us not forget about the general principles that underlie how a particular farm functions, because if we do forget, we are abandoning the hope of ever getting a scientific understanding of farming. Science is built on general principles. This does not mean that a given action will have the same result on every farm. Certainly not. But it does mean that the results will be determined by the same principles that determine the results of that action everywhere. The results will differ because the conditions differ, not because the principles governing them differ.

An understanding of these principles will emerge, if at all, only from many studies that in some ways repeat but in other ways change previous experimental conditions and procedures. The need to change some things from one experiment to the next is obvious: otherwise, why do it the next time? Less obvious is that experiments must still have enough in common with what went before that larger patterns eventually can emerge, not just a confusion of separate, unrelated data points.

In seeking the more general understanding that could emerge by actively integrating the results of many different studies, it would be valuable to record more experimental information than might seem relevant to one's particular purpose. Since we do not yet fully understand the system we are studying, it would be prudent not to assume that we know in advance which variables matter.

This does not mean that we should indiscriminately throw everything into the mix. It does mean, however, that we should think seriously about what additional variables are likely to be important for a more integrated, higher-level analysis. We then should measure and record them so that we, or other researchers, can return to the data when we have a better understanding of the system we were studying. Including additional variables does not necessarily mean including them as ‘treatment’ variables, i.e., variables that are varied in a controlled way to see their effects. It could also mean simply recording more experimental conditions than are routinely recorded for that kind of experiment. Granted, either of these takes effort, and may mean that fewer (but more thorough) experiments can get done with the available resources. But often it will be worth the trade-off. Haven’t we all at one time or another said, ‘If only I had to thought to take down such-and-such?’

ORGANIC FARMING PROGRESS AND ORGANIC RESEARCH: WHAT IS THE CONNECTION?

My third and last suggestion is that in deciding what research approaches are most valuable, we should not simply speculate and theorise, but rather learn from experience. The fundamental question is: where have the improvements in organic agriculture come from? Improving organic agriculture is the reason we do research, and therefore should provide the ultimate standard by which we judge it.

This proposal starts from commonly heard statement that organic farming isn’t just ‘farming the way we all did in the old days.’ Of course it isn’t. It’s much better, and it’s getting better all the time. But we should delve into the implications of that statement. In what specific ways have organic techniques improved, and how did those improvements come about?

The first question is the easier of the two. It involves comparing organic production practices of today with those of, say, 20 years ago. One could start answering this question by interviewing organic farmers who already were farming organically at least 20 years ago, asking them what changes in production methods have been most valuable to them, whether because they reduce environmental damage, reduce risk, increase income, improve food quality, or contribute in any other way towards the many goals of organic farming. One could also look at recently published case studies and surveys of organic farmers and compare them with those of some years ago.

The second question is more difficult to answer: where have these improvements come from? There are several possibilities. One obvious one is that improved methods have come from the accumulated experience of organic farmers, i.e., they have had more opportunity to learn by doing.

A second possibility is better scientific understanding in agroecology, soil science, plant pathology, etc. As has been said repeatedly, organic farming is supposed to be based on more sophisticated understanding, so this source of improvements is plausible.

Another possible source is applied research leading to improved techniques in agriculture in general, e.g., better tillage implements, better crop varieties and livestock genetics, and better pest control methods of the kind used in both conventional and organic practice.

Finally, there is the possible contribution from applied research aimed at improved techniques for organic farmers in particular, that is, the kind of studies that typically are suggested by the term 'organic farming research.' This is an area that has grown greatly in the past two decades: witness this conference.

All of these may have made some contribution. (If the last of these has not had at least some role, we should seriously rethink what we are about.) In any case, the study I am suggesting would trace the most important improvements back to their sources. Again, one part of this would involve talking to organic farmers and researchers with long memories. We should also go backward through the published literature to learn what earlier research has been most picked up by current researchers. We then will have learned not only how big a contribution research as a whole has made, but also which kinds of research have been most worthwhile. Has the most fruitful work mainly been conducted on farms, on experiment stations, or in laboratories? Has it mainly been single-disciplinary or multi-disciplinary? 'Basic' or 'applied'? And so forth.

Whatever such a study finds, the results will provide valuable guidance in thinking about organic research strategies for the future. This returns us to the question that this paper began with: how should organic research be done? I will not propose an answer, but I certainly propose that we try to answer it. But we should answer it empirically, rather than by just speculating about it or declaring the answer as a matter of doctrine.

ACKNOWLEDGEMENTS

I appreciate the valuable comments that Engelhard Boehncke and Sarah Wernick made on a draft of this paper.

REFERENCES

Levins R (1973) Fundamental and applied research in agriculture. *Science* **181**, 523-524.

Lockeretz W; Anderson M D (1993) *Agricultural Research Alternatives.* University of Nebraska Press: Lincoln.

Mayer, A; Mayer J (1974) Agriculture, the island empire. *Daedalus* **103**(3), 83-95.

Rosenberg C E (1971) Science, technology, and economic growth: the case of the agricultural experiment station scientist, 1875-1914. *Agricultural History* **45**, 1-20.

From: Powell et al. (eds), *UK Organic Research 2002: Proceedings of the COR Conference, 26-28th March 2002, Aberystwyth*, pp. 25-31.