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‘KNOWLEDGE WITHOUT APPLICATION IS WASTED’

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Dr Stephen Moss has been a weed research scientist since 1975 specializing in the agro-ecology of grass weeds, and most recently, herbicide resistance. He has published over 200 research publications and always attached a high priority to knowledge transfer. As well as presenting research findings at many conferences worldwide, he also gave numerous talks to farmers, consultants and agrochemical company technical personnel. In addition, in the last 20 years he has contributed to over 250 articles in the popular farming press and it was largely due to his Knowledge Transfer (KT) activities that he was awarded the Royal Agricultural Society’s Technology Award in 2010.

Many Congrats Stephen, Keep it Up-IWSS.

Having been in weed research for 37 years, and heading for retirement, it seems like an opportune time to reflect on the state of weed research and what should be the priorities for the future.

Weed research is emphatically an *applied* scientific discipline. Or at least it should be. It is a specialised branch of plant science, and the only reason to study weeds is to improve our understanding of them with the aim of improving their management and control. Why else study weeds? Any academic study of plants, which just happen to be weeds, which contributes nothing to their management, should surely be considered as ‘plant science’ and has no place in the weed literature. One really has to ask the question repeatedly; ‘why study weeds?’

This is not to say that all weed research needs to have an immediate, practical application. Indeed, we need a continuum of research from basic through to applied where due recognition is given to the research, and researchers, at all points across this spectrum. This is patently not the case in the UK. I could cite at least a dozen references from reports written by people far more eminent than me to support this view, but one quote from a farmer sums it up nicely; “Part of the problem with most modern scientific research is a lot of the eggheads in our research institutes concentrate on pure science and find it hard to communicate their ideas widely or simply enough for them to change everyday life.” (Matthew Naylor, Farmers Weekly, 2 October 2009).

It’s not hard to understand why the ‘eggheads’ concentrate on pure science. The mechanism of reward and career opportunity in universities and research institutes is heavily influenced by the ‘impact’ of publications in scientific journals. A culture has been created in which successful research careers are determined by how well our science impresses other scientists. It never ceases to amaze me how this narrow-minded view has become so prevalent in many countries. It should never be forgotten that, however great the ‘impact’ of a publication, it achieves nothing in terms of improving our ability to manage weeds, and feed the world, until the information is used in practice. Man emphatically does not live on a diet of high impact journal papers alone.



In the UK, practical research tends to be frowned upon in academia and this has tilted the balance of agricultural science (including weed science) towards basic research and away from applied (Taylor Review, 2010). Does that apply in your country? I hope not, but fear this is a trend that is spreading.

My plea to those of you who are working in countries where weed research is a relatively new discipline is to think very carefully about what you want to achieve from your research. Do you want merely to publish results in journals or do you want to improve the practical management and control of weeds? These two objectives are not, of course, mutually exclusive, but will you get equal

personal recognition and reward for both?

I would also urge you not simply to duplicate what has already been done in other countries. I do think there is too much ‘reinventing the wheel’ in weed research. For example, the principle that some crop cultivars are more competitive against weeds than others has been demonstrated for numerous different crops in many countries. So what is the point of demonstrating this principle yet again in another country in another crop? What is needed is some means of using such research to continually assess new cultivars as they are developed, and make this information available to farmers. There have been some attempts to do this, but very few that have proved successful and durable. This would be a much more useful objective than merely comparing the competitiveness of existing cultivars that may well be outclassed by the time the results of the research programme are published.

I am not suggesting that you ignore previously published research, but I do think that you should be constructively critical, and question its relevance to your own county. For example, there have been many studies on ‘fitness’ of plants in relation to herbicide resistance, but is there any evidence that such knowledge has actually helped in the practical prevention and management of resistance anywhere in the world? It may be interesting, and may explain why certain target site mutations are commoner than others, but are such studies a good use of scarce resources if the ultimate aim is to better manage resistance?

Weed research has a bit of an image problem, and many people find it hard to take weeds seriously. This is despite the fact that globally, weeds reduce crop yields to a greater degree than other pests. With increasing world population and changing diets, there will be a consequent need to increase food production. Better weed control will be vital to achieve this. Weed research has a bright future, especially in the rapidly developing countries of Asia and South America, but only if greater emphasis is placed on delivering better practical weed management rather than focussing on ‘academic impact’. What is needed is truly integrated research, across the whole spectrum from basic to applied, with all elements contributing to real improvements in weed management.



To those of you who are, or may be in future, responsible for allocating funding for weed research, I make one last suggestion. That is, for every \$ spent on basic research, a \$ is spent on applied research. This concept might need refining a little, but would be one means of achieving a better balance across the research spectrum. Basic research can be an end in itself – and there is nothing wrong with that. Applied research can be considered a means to an end – that end for us is better weed management. With weed research, knowledge without application really is wasted, and the world simply cannot afford that.