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## INSIGHTS

# Weed research: is it delivering what it should?

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### Summary

I believe the overall balance and current direction of much weed research is wrong, with too much emphasis on 'scientific impact' at the expense of practical application. For example, despite considerable research effort, Integrated Weed Management has not been widely adopted by farmers. Weed research, as a whole, has delivered less than it should have done in recent years, because of lack of appreciation of the difficulty and costs involved in scaling up experimental results to be applicable at a realistic field scale in real farming systems. In addition, there is often a lack of awareness of the complexities and resources needed to translate research results into actions that farmers, who may be

counted in their millions, are willing to adopt. 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. 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 until the results are used in practice. Effective technology transfer is essential. Weed research is an applied discipline, and the question needs to be asked repeatedly and critically, 'Why study weeds?'

**Keywords:** weed research, integrated weed management, publication, technology transfer.

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### Why study weeds?

'Weeds are unwanted and undesirable plants which interfere with the utilization of land and water resources and thus adversely affect human welfare' (Rao, 2000). From a philosophical perspective, consider what would happen if mankind suddenly disappeared from the face of the earth. Surely, weeds would disappear at the same instant. Plants would remain, but weeds would cease to exist because they, by definition, are inextricably linked with human activity. It follows that the primary objective of weed research should be to improve our understanding of this relationship, with the aim of improving the management and control of weeds.

The practical impact of some weed research has been questioned recently. For example, Mortensen *et al.* (2000) considered that the contribution of weed biology and ecology to the development of weed management

strategies had been 'modest'; Cousens (1999) commented that weed thresholds had '...seldom been used in practice'; and Sanyal (2008), stated that '...IWM (Integrated Weed Management) is still not widely adopted.' If such comments are true, and I believe they are, can we explain why so much weed research has failed to deliver meaningful practical benefits?

Cousens (1999) criticised the preponderance of phenomenological experiments and over-reliance on repetitive superficial case studies, and argued that much 'weed science' is, in reality, 'weed technology'. He called for a more scientific approach aimed at a greater understanding of the fundamental principles explaining 'why' things happen, rather than merely documenting 'what' occurs. I agree, but for this to be credible it is essential that any increased focus on principles is shown to result in improved practical outcomes. Weed research is emphatically an applied research field (after all, why

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