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Agricultural Weed Research: A Critique and Two Proposals

Sarah M. Ward, Roger D. Cousens, Muthukumar V. Bagavathiannan, Jacob N. Barney, Hugh J. Beckie, Roberto Busi, Adam S. Davis, Jeffrey S. Dukes, Frank Forcella, Robert P. Freckleton, Eric R. Gallandt, Linda M. Hall, Marie Jasieniuk, Amy Lawton-Rauh, Erik A. Lehnhoff, Matt Liebman, Bruce D. Maxwell, Mohsen B. Mesgaran, Justine V. Murray, Paul Neve, Martin A. Nuñez, Anibal Pauchard, Simon A. Queenborough, and Bruce L. Webber*

Two broad aims drive weed science research: improved management and improved understanding of weed biology and ecology. In recent years, agricultural weed research addressing these two aims has effectively split into separate subdisciplines despite repeated calls for greater integration. Although some excellent work is being done, agricultural weed research has developed a very high level of repetitiveness, a preponderance of purely descriptive studies, and has failed to clearly articulate novel hypotheses linked to established bodies of ecological and evolutionary theory. In contrast, invasive plant research attracts a diverse cadre of nonweed scientists using invasions to explore broader and more integrated biological questions grounded in theory. We propose that although studies focused on weed management remain vitally important, agricultural weed research would benefit from deeper theoretical justification, a broader vision, and increased collaboration across diverse disciplines. To initiate change in this direction, we call for more emphasis on interdisciplinary training for weed scientists, and for focused workshops and working groups to develop specific areas of research and promote interactions among weed scientists and with the wider scientific community.

Key words: Invasion biology, weed science, weed technology.

A Critique of Weed Science

Weed science as a discipline has a long history of research across agricultural systems (Zimdahl 2010). Agricultural weeds (i.e., those in cropping systems planted and managed by humans for food, fiber, or forage production) are the greatest constraint of crop yields globally (Oerke 2006), and with continuing global demand for expanded crop production on limited arable acreage, it is clear that weed science has a vital role to play in the future. Yet how often do we reflect critically on the

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* First author: Associate Professor, Department of Soil and Crop Sciences, Colorado State University, Fort Collins, CO 80523-1170; 2nd and 18th authors: Professor and Postdoctoral Fellow, Department of Resource Management & Geography, The University of Melbourne, Victoria 3010, Australia; third author: Assistant Professor, Department of Soil and Crop Sciences, Texas A&M University, College Station, TX 77843.; fourth author: Assistant Professor, Department of Plant Pathology, Physiology, and Weed Science, Virginia Tech, Blacksburg, VA 24061; fifth author: Research Scientist, Science & Technology Branch, Agriculture and Agri-Food Canada, 107 Science Place, Saskatoon SK S7N 0X2, Canada; sixth author: Research Assistant Professor, School of Plant Biology, The University of Western Australia, Crawley, Western Australia, Australia; seventh author: Associate Professor, Department of Crop Sciences, University of Illinois, Urbana, IL 61801; eighth author: Professor, Department of Biological Sciences, 715 West State Street, Purdue University, West Lafayette, IN 47907; ninth author: Research Agronomist, USDA-ARS 1 Soils Lab, Morris, MN 56267; 10th author: Professor, Department of Animal & Plant Sciences, University of Sheffield, Sheffield S10 2TN, UK; 11th author: Associate Professor, Department of Plant, Soil and Environmental Sciences, University of Maine, Orono, ME 04469-5722; 12th author: Professor, Department of Agricultural, Food & Nutritional Science, University of Edmonton, Edmonton, Alberta T6G 2P5, Canada; 13th author: Associate Professor, Department of Plant Sciences, University of California, Davis, Davis, CA 95616; 14th author: Associate Professor, Department of Genetics and Biochemistry, Clemson University, Clemson, SC 29634-0318; 15th and 17th authors: Assistant Research Professor and Professor, Department of Land Resources and Environmental Sciences, Montana State University, Bozeman, MT 59717; 16th author: Professor, Department of Agronomy, Iowa State University, Ames, IA 50011-1010; 19th author: Landscape Ecologist, CSIRO Ecosystem Sciences and Water for Healthy Country Flagship, G.P.O. Box 2583, Brisbane, QLD 4001, Australia; 20th author: Group Leader, Weed Ecology, Rothamsted Research, Harpenden, AL5 2JQ, U.K.; 21st author: Associate Researcher,. INIBIOMA-CONICET, Laboratorio Ecotono, CRUB-UN Comahue, Bariloche, Argentina; 22nd author: Associate Professor, Facultad de Ciencias Forestales. Universidad de Concepción. Casilla 160-C, Concepción, Chile; 23rd author: Assistant Professor, Department of Evolution, Ecology & Organismal Biology, Ohio State University, Columbus, OH 43210; 24th author: Senior Research Scientist, CSIRO Ecosystems Sciences, Private Bag 5, P.O. Wembley, WA 6913, Australia. Corresponding author's E-mail: sarah.ward@colostate.edu.

way we go about weed science research? How often do we rigorously debate our directions or evaluate our progress? Weed scientists, as a body, may have a broad concept of what we are trying to achieve, namely the scientific understanding of weed biology and managing weeds to minimize their impact, but are current practices the most effective for reaching those goals? Are we asking the right questions and using the right tools? Are we collaborating effectively? How well are we communicating our results with the intended audience?

Many scientific disciplines require moments of reflection, introspection, and debate and emerge stronger and more focused as a result (e.g., Peters 1991; Scheiner 2013). In June 2012, a group of weed and invasive plant scientists met for a 4-d workshop to share experiences and examine current directions in weed science (Murray et al. 2012). One-third of the group consisted of young scientists launching new careers, whereas the remaining group members had many years of experience. Group research interests ranged from agricultural weeds to invasive plants in natural systems to global climate change and speciation genetics; group expertise included weed control, modeling, population and evolutionary genetics, ecology, and molecular biology; members of the group came from across four continents. During the workshop, this diverse international group was challenged to consider the aims of weed science and how weed science research is currently done, and to identify how we could achieve more effective research that would push the frontiers of weed science in general.

This paper is the result of 4 d of intense debate as a group, which led to collaborative discussions and exchanging notes over the following months. Our intent is entirely positive and constructive: to identify what we, as weed scientists, need to do to move our discipline forward in the most effective way. However, to do this we must be prepared to conduct a critical and perhaps brutally honest assessment of the current situation. Here we report our conclusions, propose some new ideas, identify outstanding challenges, and invite others to think about how we can be more effective.

What Are the Goals of Weed Science?

Most weed scientists would argue that two broad aims guide their work. First, many researchers work to identify or refine management solutions for plants growing in places where they are not wanted. Some weed scientists have argued that the primary need is for research resulting in practical applications that will reduce the abundance of undesirable plants and alleviate the problems they cause (e.g., Breen and Ogasawara 2011; Moss 2008). Given the predominance of chemical weed control in many production systems, this has led to a constant output of herbicide efficacy reports—valid and necessary research, but arguably technologically oriented gap-filling rather than breaking new scientific ground. Other examples of research aimed at solving agricultural weed problems over the years include (1) the many crop competition papers justified by the need to calculate economic thresholds; (2) spatial mapping driven by the potential for site-specific management; and (3) currently a wide range of activities to develop and promote integrated weed management. Although some of these approaches have never been widely adopted as management tools (such as economic thresholds, which nevertheless are still often used to justify competition research), there has been a clear management objective to all this work.

Second, we seek to understand weeds and the systems that they populate. For some researchers, this aim is also driven by the desire to better manage weeds. Many weed scientists believe that by understanding the biology and ecology of weeds, we will discover more sustainable ways to manage them (e.g., Hall et al. 2000). Although this assumption has been challenged (Cousens and Mortimer 1995), it remains a strong philosophical driver of research. However, biological systems involving weeds also have their own intrinsic scientific interest regardless of the need to manage them, as weedy plants often demonstrate rapid adaptive evolution in response to strong selection pressures. For example, agricultural weeds inhabit highly disturbed environments dominated by short-lived species, ecosystems that were rare globally before the spread of settled agriculture. This results in the predominance of particular life histories in agricultural weed communities, and population dynamics that are strongly influenced by recruitment from the seed bank. Weeds in these intensively managed ecosystems have repeatedly evolved to persist in agricultural conditions by evading different control methods, most recently with the proliferation of resistance to different herbicides. Agricultural weeds are therefore potentially valuable—but insufficiently exploited—experimental models for exploring rapid evolution and adaptive response. Similarly rewarding model study systems are provided by nonnative plants in natural

ecosystems. These plant invaders allow ecologists to examine the relative importance of preadaptation, phenotypic plasticity, and postinvasion adaptation in determining species range limits, as well as fundamental concepts such as the ecological niche (Webber et al. 2012).

During earlier decades of weed science, it was not uncommon for researchers to combine research on weed ecology and biology with weed control (see Zimdahl 2010 for examples). However, it has become increasingly difficult to be at the forefront of thinking in both practical weed management with an emphasis on technology, and in weed biology and ecology with an emphasis on science. These subdisciplines have steadily diverged, as demonstrated by the Weed Science Society of America's publication of separate journals (*Weed Science* and *Weed Technology*) devoted to each. Over the years, there have been calls for greater investment in weed biology and ecology and "the development of basic weed science principles" (Wyse 1992). This has resulted in an increased volume of weed ecology research, but are we addressing significant questions that will ultimately lead to better management? The same arguments can be made about management-oriented research in weed technology. This is an area that has been the subject of considerable investment, but what constitutes the most useful weed technology? How can we maximize outcomes from the resources invested?

It is easy for researchers working in the biology and ecology of weeds to argue that management practitioners focus too much on herbicides and do not take more fundamental aspects of weed biology sufficiently into account. It is also easy for weed technologists to argue that weed biologists and ecologists focus on research that is of little practical value. We propose that it would be to everyone's benefit for the two sectors to come closer together. More recent reviews of the current state of weed science continue to echo Wyse's 1992 critique by again calling for greater integration with other areas of biological research (e.g., Davis et al. 2009) and for expanding agricultural weed research beyond a narrow focus on chemical control to reemphasize integrated weed management based on ecological principles (Mortensen et al. 2012). It seems that little has changed despite repeated calls over more than two decades for a broader interdisciplinary approach to weed science research.

Why? And how can we change our approach? Blame has been laid at the door of reduced resources, including both limited funding and declining numbers of weed scientists (Davis et al. 2009; Derr and Rana 2011). However, an equally large impediment to progress may be the way we organize ourselves. Many weed scientists work alone rather than in collaborative teams, a situation often dictated by lack of resources at a local level. When weed scientists do form collaborations these are often with researchers of similar rather than complementary interests. We seldom put together truly interdisciplinary teams that bring in people from very different backgrounds, and perhaps the lack of such collaborations results in failure to embrace novel ideas. Individual weed scientists cannot be expected to be effective in obtaining the best outcomes from multiple disciplines, and therefore we need to become smarter at working together. One approach to developing effective multidisciplinary teams is to hold more workshops and working groups on specific issues, where people can exchange ideas and work on problems together in an environment different from large conferences where the focus is on presentation of completed research. This paper derives from just such an experiment.

What Drives the Direction of Weed Science Research?

Agricultural weeds became a serious focus for research internationally in the late 1950s. However, directions in weed research have not been driven by deliberation about appropriate goals and the pathways necessary to get there. Like most branches of science, reality more closely resembles multiple correlated random walks. Like-minded agricultural weed scientists pursue broadly similar research with occasional changes of direction in response to major new developments (e.g., selective herbicides, zero-till farming systems, widespread herbicide resistance) or the appearance of new research methodologies such as population modeling or molecular biology. This is not to argue that individual researchers do not have aims—they clearly do. However, these aims often reflect a reactive response limited to the immediate tasks at hand, such as managing a particular weed species, or characterizing a new case of herbicide resistance, rather than the broader advancement of the discipline. The weed science community rarely debates deeper issues, such as what research will deepen our understanding of weed biology, ecology, and evolution? What are the major research challenges we need to address in weed management? What are

our goals and how do we get there? Although our multiple random walks have brought us thus far, and serendipity occasionally leads to unexpected breakthroughs, we believe that a more rigorous critical analysis of weed science could move the research community onto a more direct path to its larger goals.

The hallmark of outstanding science is that it relates to and further develops existing theory in novel ways. Such research is presented with a clear theoretical justification for why it was done, describes how it adds to our current understanding, and discusses the next steps to be taken. How well or how often does agricultural weed science research do this? There is an extensive and constantly growing ecological and evolutionary biology literature, but few papers on the biology or ecology of agricultural weeds link to this body of theory. Too many weed science papers simply describe facts, such as results of herbicide efficacy trials, seed germination experiments, or other case studies describing some aspect of a weed life history or herbicide resistance mechanism. Although this information is useful—and we are in no way advocating that such work should not be performed and communicated—we are increasingly troubled by the extent to which such papers dominate weed science journals and meetings, and by the paucity of publications and presentations addressing broader theoretical issues.

Research on the biology and ecology of weeds by weed scientists generally does not make good links to more general theory, and perhaps this is why plant biologists outside weed science do not see our work as contributing to the cutting edge of broader research. Linking weed biology, evolution, and management to larger questions would raise the profile of weed science and encourage scientists from other disciplines to explore weeds as research models, potentially expanding the scope of weed science journals and meetings. Connecting research on agricultural weeds and weed management to broader biological theory is not a new idea. Herbicide development has generated insights into plant physiology, and some researchers outside traditional management-focused weed science have used agricultural weeds to explore larger questions. Examples include work in evolutionary biology by Baker (1974) and De Wet and Harlan (1975), and John Harper's pioneering work in plant population biology and ecology (Harper 1977). However, such use of weeds as model systems has been sporadic at best and remains uncommon (although see Vigueira et al. [2013] for a recent example).

Given that weeds are plants where we do not want them, it is useful to compare agricultural weed science as we define it in this essay with the parallel field of plant invasion biology. Exotic plant invaders in natural systems have attracted much more attention as models for exploring theoretical questions in evolutionary biology and ecology (for examples, see Barrett et al. 2008; Ren and Zhang 2009; Schierenbeck and Ellstrand 2009; Shea and Chesson 2002). Inevitably, some of these explorations went up blind alleys—for example, the weak (but highly cited) empirical evidence for the evolution of increased competitive ability hypothesis of plant invasion (Blossey and Notzold 1995). However, weedy invasive plants in noncrop systems have attracted a more diverse cadre of scientists than have agricultural weeds. This cadre includes ecologists, population and evolutionary biologists, geneticists, and others who are less focused on the technology of management and more interested in using plant invasions to explore theoretical questions. Although several futile random walks have occurred in plant invasion biology, this field has generally seen broader interdisciplinary thinking that is largely absent from agricultural weed research. We note, however, that development of a more robust theoretical context for understanding invasion does not automatically lead to useful results for managing invasive species. In invasion biology as in agricultural weed research, the need is not only for better theory but also for bringing together biologists and management-focused technologists.

The Stranglehold of Repetition

A further hindrance to agricultural weed science is the repetitive nature of many experiments and the high volume of descriptive case studies. Whether in the area of weed management or understanding weed biology, the first paper on a topic, proposing a new idea, is often inspirational. Once a novel general principle has been established, however, further research rapidly narrows to case studies that support the idea for this specific weed in this specific crop, in this specific region. Rather than actually developing management methods, many studies merely describe the consequences of existing techniques so that we can better appreciate them (e.g., minimum tillage on weed populations). Although this provides strong "proof of concept," it consolidates rather than advances research. Moreover, like most scientists across all disciplines, weed scientists specialize according to their interests

and training, and tend to follow similar paths throughout their careers. Once we have identified a productive area of research, too often we stick to it and become method-driven, developing a vision based around a skill-set. We focus on new tools and approaches as they become available: we apply them repeatedly and become familiar with them. We equip our labs to use those methods, generating many case studies illustrating the same things again or with slightly different—and more interesting—outcomes. We achieve a reputation for this work and we obtain research grants; promotions may be awarded more on the number of publications than on their impact. Unfortunately, the utility of novel research technologies in agricultural weed science is too often undermined by the substitution of descriptive studies and mere fact collecting in place of the pursuit of more rigorous questions, a trend that was noted and criticized almost 40 yr ago (see Knake 1975) but still persists. Plus, in terms of the development of our science, the law of diminishing returns kicks in: we learn relatively little from the next study, and even less from the next, but it is difficult to know when to stop. Too often in weed science we fail to recognize the point at which the value to the discipline of yet another case study is outweighed by a switch to a different objective.

Unfortunately, funding for weed science research is generally easier to obtain from applied science agencies and industry if we follow popular trends and themes rather than risk real novelty. Often research support is conditional on the likelihood of ''success'' in confirming investigator expectations, and on results having immediate and direct applications to weed management. Consequently, perhaps, many investigators gravitate toward projects with short-term payoffs and are afflicted by ''copycatatonia'': endlessly repeating the same experiment with a different crop, or weed, or herbicide. A recent example of this phenomenon is the proliferation of descriptive herbicide-resistance case studies: many papers have been published, and conference presentations given, describing the growth of resistant and susceptible biotypes and characterizing the molecular or physiological basis for individual cases of herbicide resistance. However, other than linking herbicide resistance to herbicide application, there has been limited progress in developing a robust theoretical base for research that would address broader questions, such as why does resistance evolve faster to some herbicide modes of action than others? Why is resistance common in some weed species and rare in others? How can we progress from reactive post hoc characterization of resistance cases to proactive forecasting and prevention?

Patterns of consolidating reputations within a small subfield and consequent repetitive behavior are human nature, and the authors of this paper do not claim to be immune from such tendencies. However, our conclusion is that we need to fight against the ease of staying on the bandwagon, and of repeatedly applying established recipes, by constantly asking whether our research agenda is truly advancing our science.

The Way Forward

Two key elements emerged from discussions among members of the workshop group. We are not proposing that these are the only solutions, but we offer them as suggestions that could improve the outcomes of weed science research and move our discipline forward.

First, as others before us have argued (e.g., Davis et al. 2009), agricultural weed science—no less than invasion biology—should encompass disciplines as diverse as plant physiology and biochemistry, agroecology, climatology, paleobiology, community ecology, evolutionary and ecological genetics, sociology, psychology, and economics using the array of methods that continue to appear in each discipline. These tools hold promise for narrowly focused questions, such as predictive modeling to project rates of resistance evolution to a given herbicide under different management scenarios, or transcriptomic analysis to pinpoint the molecular basis of a weed's response to an environmental variable. However, although novel research technologies make new experimental directions possible, simply adopting such technologies without a more profound rethinking of weed research is not a panacea.

Most weed scientists would agree (at least in principle) that successful weed management requires an integrated approach, even if we are still groping toward consensus on what that really means. However, truly interdisciplinary agricultural weed research will not generate spontaneously: it must be argued, championed, developed, and then sold persuasively to our funding agencies. We need to develop active collaborations with nonweed scientists in the many disciplines that intersect with weed science. In addition, global collaboration among scientists from different cultures provides alternate perspectives and encourages novel approaches

that can contribute to the development of central disciplinary hypotheses. International networking in ecology has resulted in unique and rapid advances that otherwise could have not been achieved (Sagarin and Pauchard 2012) and interdisciplinary collaborations are becoming routine in the rapidly growing subdiscipline of plant invasion biology. There has been some movement in this direction among those working with agricultural weeds—for example, the inclusion of sociologists and economists in discussions of barriers to grower adoption of improved weed management practices. However, we have been slow to respond to the urging of Fernandez-Quintanilla et al. (2008), Wyse (1992), and others. How many scientists from other disciplines did you meet at the last weed science meeting you attended? And how many weed scientists attend meetings outside our primary discipline?

Second, we must rediscover the ability to pose critical research questions that are rooted in and will advance the theoretical underpinnings of our science. Members of our workshop group have reviewed too many manuscripts and grant proposals that lacked testable hypotheses or failed to connect with existing theory. Too many weed scientists restrict their funding sources to industry or applied agricultural programs because they are unwilling or unable to frame their research in terms of broader questions that use agricultural weeds as models. Nevertheless, the opportunity is still open for weed scientists to make real contributions to overarching theory in areas such as ecology and evolution, and beneficial cross-pollination could flow both ways. Decades of work by weed scientists in agricultural systems have produced large amounts of data and well-honed experimental approaches that could prove useful for investigations in invasion ecology. In addition, weed scientists' experience with short-generation plant species provides a unique vantage point for understanding patterns and testing theories, an advantage over other researchers studying longer-lived organisms.

To exploit these research opportunities successfully and expand the theoretical basis of weed science, as individuals we need to read and publish more widely in the literature of plant biology, evolution, and ecology, and we should encourage our graduate students to do so. As a weed science community, we need to develop training programs that help students develop good research questions with scientific rigor, encouraging them not to limit their focus to the particular crop–weed system they plan to study, but to develop a broader vision of what they are trying to achieve. As a discipline, perhaps through activities initiated by our professional societies, we need to take time to debate our goals and directions in ways that link us more tightly together outside our individual programs. We are not proposing that all weed scientists should pursue the same goals, or that applied management-oriented experimentation has no place in our discipline—far from it. However, we do argue that too much weed science research is impoverished by our failure to think widely and creatively, by our lack of innovation and consequent repetitiveness, and by our self-imposed isolation from researchers in other areas of biology and beyond.

Some agricultural weed scientists have expressed concern that immersion in a wider biological pool will lead to weed science losing its distinct identity (e.g., Breen and Ogasawara 2011). Surely this underestimates the ability of the weed science community to explore new research horizons while continuing to meet the needs of growers and weed managers. Given the challenges that weedy plants pose for sustainable agricultural production and natural ecosystems on a livable planet, we cannot afford to allow the lack of interdisciplinary research and resulting intellectual and scientific impoverishment to prevent us from doing our best work. Weed science could—and should—be an exciting fusion of multiple disciplines attracting talented people from diverse scientific backgrounds. We challenge our fellow and future weed scientists to make it so.

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