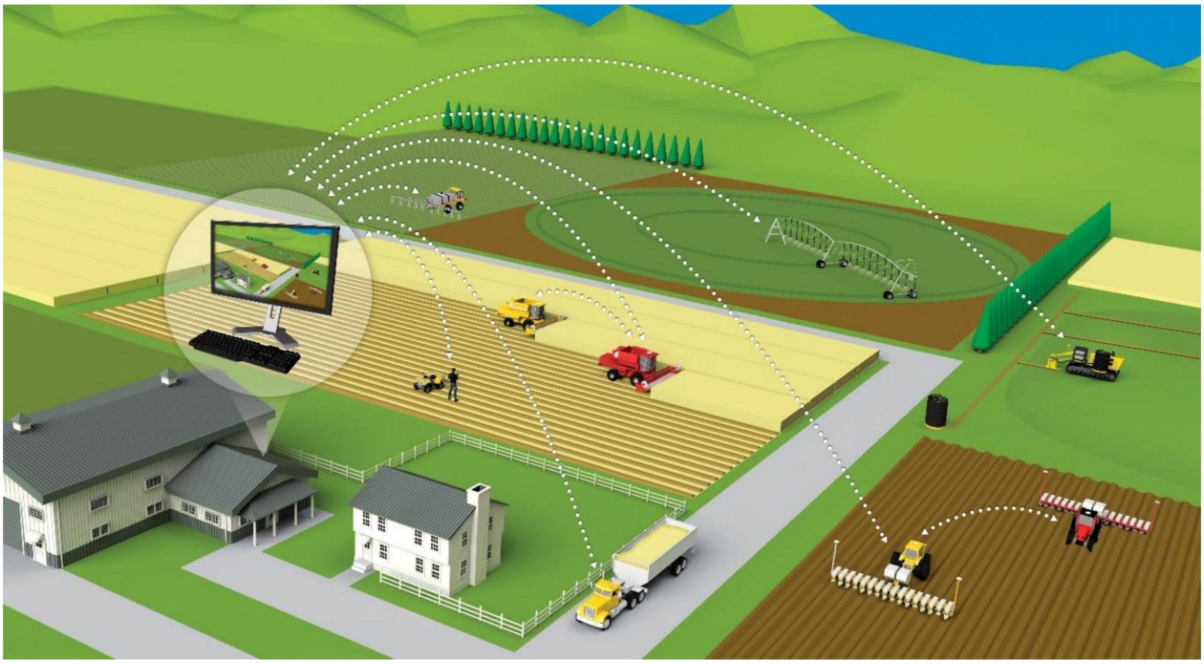
PART VI – TO THE FUTURE



Connected farm: increasing connectivity allows data and local information to be shared instantaneously between sensors, equipment and people (Courtesy: Trimble Connected Farm)

Swarmbot: Equipment automation is replacing large farm equipment with ‘swarms’ of smaller, lighter robotic units capable of a diverse range of tasks (Courtesy: SwarmFarm Pty Ltd)

The DOT autonomous platform driving a fertilizer spreader (left) and boom spray (right) on display at Ag-in-Motion field day in Saskatoon Canada July 2019. (Courtesy: John Kirkegaard)

Chapter 23

Transformational Agronomy: restoring the role of agronomy in modern agricultural research

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Introduction

The global food security challenge has prompted many to propose the need for 'transformational change' in food production systems through technological 'breakthroughs'. These transformative technologies are often distinguished from the 'incremental' advances generated by agronomy and breeding which are dismissed as business as usual, and inadequate to achieve the productivity improvements sought. Since the prequel to this book was published in 1987, the reduction in yield gap achieved by the Australian grains industry has been formidable. In the 10 years prior to 1987, Australian wheat growers averaged 34% of water limited potential yield. In the 10 years prior to 2017, they averaged 52%, a 35% gain relative to the most recent period, or 1.2% per annum (Hochman *et al.* 2017). When viewed over the 30-year period this change is truly transformational, but the transformation has come through incremental gains (Kirkegaard 2019).

It may seem curmudgeonly to be critical of aspirations to achieve transformational breakthroughs, but in a world of diminishing expenditure in agricultural research it will be important to target dwindling funds well. Proposed transformational changes often focus on one component of a system championed by largely disconnected research disciplines. In reality, and throughout history, few individual technologies have been singularly transformational either in the scale or the speed with which they have influenced productivity (Evans 1998). Rather, step changes in productivity have come only when combinations of technologies, often a mix of old and new, synergise within a system. In the context of Australian wheat production, the productivity gains of the last 30 years have been due to many disparate technologies combining to form a coherent system. The advent of glyphosate and grass selective herbicides drove the rapid adoption of no-till (Llewellyn *et al.* 2012) which improved soil water conservation and allowed earlier sowing (Stephens and Lyons 1998). Wheat was increasingly grown in rotation with broadleaf break crops (canola and pulses – industries initiated through substantial public investment) rather than other cereals or weedy pastures which reduced yield losses due to root disease. Meanwhile breeders consistently achieved genetic yield progress of 0.5% per annum (Siddique *et al.* 1990, Sadras and Lawson 2011, Fischer *et al.* 2014, Kitonyo *et al.* 2017) and overcame significant biotic and abiotic constraints to production which interact with management (cereal cyst nematode, stripe rust, acidity, boron). Early sown, disease-free crops responded profitably to N fertiliser, applications of which tripled over the 30-year period (Angus and Grace 2017).

To fulfil the goals of sustainable intensification, Fischer and Connor (2018) estimate that similar gains (1.1-1.2% per annum) are required over the next two decades to keep pace with increased global demand for food. Whilst it is an oft cited cliché that agricultural productivity must increase to feed a growing global population enjoying an increasing quality of life, the challenge is real. It lies not so much in producing enough food to feed the world, but in producing enough food to keep prices sufficiently low that the poorest citizens of the globe can reasonably afford it. The second challenge is then keeping growers in business whilst they grow food that remains affordable to the world's poor. Australian growers need these increases to remain competitive in the global market. There is evidence that the 0.5% genetic yield progress historically achieved by breeders may be slowing in at least some breeding programs (Fischer *et al.* 2014, Flohr *et al.* 2018b). The obvious question arises – where will future yield increases come from, and what role will the profession of agronomy play to deliver them? We argue that to meet these challenges, the role of agronomy should be restored and the frameworks in which agricultural research in this country is conducted reviewed.

Defining agronomy

Agronomy is generally defined as the science and practice of understanding how agricultural systems work in order to improve production, profitability and/or sustainability (Manley *et al.* 2019). It is an integrative profession – requiring an understanding of many scientific disciplines related to agricultural systems, including plant and animal science (ecology, physiology, nutrition, genetics and pathology), soil science (soil physics, chemistry and biology), meteorology, economics, sociology, geomatics, statistics and data science. This makes agronomists unique in the field of science – most other scientists specialise deeply within these disciplines. Many individual scientists devote their entire careers to researching and improving understanding of a small component of these fields.

Agronomists are generalists by definition. This is perhaps where agronomists can be underestimated in academic and scientific circles. Whilst their knowledge must be broad, it can only ever be relatively shallow, and findings (though extremely useful and impactful) are rarely universal but instead highly context dependent. In many ways their activities are more akin to engineering than science, and the chances of conducting research that truly advances human understanding and is deemed worthy of publication in high impact journals such as *Science* and *Nature* is consequently low. In addition to broad (if shallow) science knowledge, agronomists must have good working knowledge of the farming systems that they study. Whilst this knowledge is often informed or underpinned by science, many times it also requires an appreciation of on-farm logistics, economic realities and social and cultural norms.

There is also an important distinction that needs to be made within the field of agronomy. Many that term themselves agronomists work in commercial roles advising farmers on management practices, particularly regarding inputs of fertilisers and biocides. Here we refer to these as *commercial agronomists*. Other agronomists (typically employed by government agencies, grower groups and universities but also including private businesses) discharge research roles, conducting experiments to improve understanding and improve management. These we refer to as *research agronomists* and they are the focus when we use the term ‘agronomist‘ in this chapter.

Restoring agronomy

In recent times the integrative and generalist view of agronomy has been lost. Agronomy has been increasingly viewed as the ‘left-over bits’ of agricultural research once plant breeding, crop nutrition, crop protection and farming systems are moved into their respective silos. Whilst there has been an increasing effort to understand the interactions between genetics (G), environment (E), and management (M) in crop production systems, unfortunately agronomy has become synonymous with the ‘M’ in the term ‘G x E x M’ (Messina *et al.* 2009). This is reflected in the management structures of numerous research organisations and funding bodies both within Australia and beyond. This has had the effect of relegating agronomic studies to hypothesis-free empirical dabbling involving the management factors that remain within control of the farmer, including time of sowing, seeding rate, row spacing and the like. Except for time of sowing, yield effects of these factors are uniformly small and variable and rarely interact meaningfully with other aspects of management. Whilst growers often appreciate hearing results of these experiments (largely to confirm that they are doing the right thing), they are unlikely to lead to the transformational change growers require to stay profitable, or the world needs to feed itself. They are frequently revisited, often when a new piece of technology is made available *e.g.* precision seeding. This view of agronomy we refer to as *reactive agronomy*.

If the required yield increases are to be achieved, this is not the role that agronomy must play. Agronomists instead must act as directors and integrators of multidisciplinary research teams that are formed specifically to address significant constraints to production. They must oversee and optimise the G x E x M system, and not be concerned only and lastly with ‘M’. The argument for this is compelling – agronomists understand farming systems context and have a better appreciation of the factors that are limiting production (sometimes more than the growers themselves). They have the generalist science knowledge to understand which specific disciplines of research can be brought to bear on a challenge or opportunity, and how different disciplines must interact with each other to exploit synergies and avoid trade-offs to form tractable solutions. They also have familiarity and credibility with growers and commercial agronomists that is required to test research findings in the right context to ensure adoption and impact. Agronomists must be the leaders, the translators and the communicators, accessing the best discipline-based knowledge and expertise where relevant to deliver transformational change.

In this chapter we define a framework that allows multi-disciplinary teams led by (or at least involving) agronomists to identify and quantify constraints to production, profitability and sustainability, propose and test solutions, and work with farmers and advisors to integrate them into farming systems. We refer to this process as *transformational agronomy*. It builds on the concept of *systems agronomy* (Giller *et al*. 2015), which emphasises that agronomy must not merely focus on production or environment but consider social and economic factors and interactions and trade-offs in context. As systems agronomy argues that ‘principle’ based approaches (*e.g*. maintenance of permanent soil organic cover as a principle, regardless of negative impacts on yield in some contexts) are unlikely to lead to sustainable intensification, we further this to argue that ‘discipline’ based approaches are equally unlikely to be transformational. This is simply because substantial constraints to production are complex and involve trade-offs in a broad range of factors beyond the scope of any one discipline. Multiple disciplines working in balanced unison with integrating leadership are required.

Transformational agronomy also borrows from participatory research (Pretty 1995) or collective inquiry in recognising the importance of participation of end-users (*i.e.* farmers and their advisors) from the outset. This is essential not only for appropriate framing of research questions and conduct of experiments, but to ensure successful adoption of proven interventions.

The context and examples that we use here to describe transformational agronomy are from dryland crop production in southern Australia, but we argue that the same framework could equally be applied to any agricultural system in the world.

The role of agronomists: closing yield gaps vs increasing potential yield

The concept of potential yield (PY) and yield gaps is crucial for the following discussion and we follow the nomenclature of Fischer (2015). The most important definition for dryland crop production in Australia is water limited potential yield (PY_w), defined as the yield of the best cultivar under optimum management with no manageable constraints (*e.g.* nutrient deficiency, weeds, disease) except for water supply. Farm yield (FY) is yield achieved by farmers in their fields. The difference between FY and PY_w is termed the yield gap. Economic yield (EY) is the yield attained by farmers when economically optimal practices and levels of inputs have been adopted while facing all the vagaries of weather. Economic yield is typically 75-85% of PY_w (van Ittersum *et al.* 2013). The difference between EY and FY is the exploitable yield gap.

Hochman *et al.* (2017) estimate that Australian wheat producers are currently achieving 55% of PY_w. However, van Rees *et al.* (2014) demonstrated that leading farmers have closed the exploitable yield gap and are achieving 75-85% of PY_w. This implies a split (or a continuum) among Australian wheat farmers between those that are regularly achieving EY, and those with a substantial exploitable yield gap. This split raises a question about what level of limited agronomic research resources should be spent closing yield gaps by assisting farmers to implement better management, and what level should be spent overcoming current constraints to PY_w. We argue that as optimal management practices are usually in the public domain, it is predominantly the role of commercial agronomists to work with farmers to close yield gaps. It should be the focus of research agronomists to look for ways to increase PY_w. Some exceptions to this general distinction are discussed later.

Transformational agronomy

Our proposed framework for transformational agronomy is described below, and schematically represented in Figure 1. Our restoration of the definition of agronomy is indicated by the grey box.

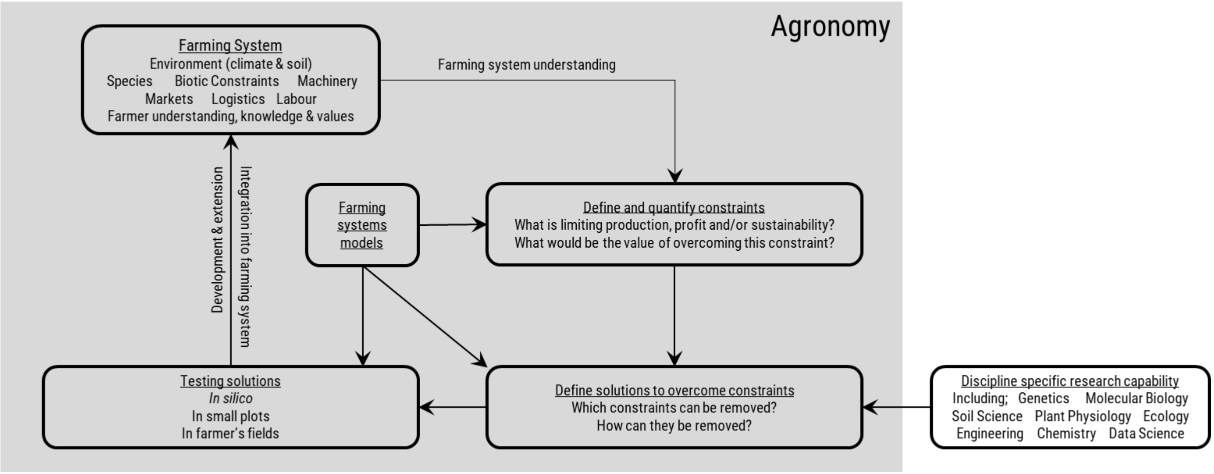

Figure 1. A framework for achieving transformational agronomy. Agronomists must understand farming systems sufficiently to define constraints to production. The value of overcoming these constraints must then be quantified. Expertise from discipline-specific researchers forming multidisciplinary teams coordinated by agronomists must be accessed to form tractable solutions which are then tested and re-integrated back into the farming systems context from which the constraint derived.

Definition and quantification of constraints to production

The critical first step to develop research programs that can transform farming systems is accurate definition of a constraint to production, and/or an opportunity to overcome such a constraint. As we discuss later, this can be harder than it seems. Agronomists are often best placed to identify constraints, as they are grounded in real farming systems, but also have knowledge of what discipline-based science and technology may have to offer in the way of solutions.

Often experiments are necessary to accurately define and quantify a constraint to production before the value of a solution is known. This is where the science training of agronomists is critical. The importance of accurately identifying and quantifying genuine constraints to production cannot be overstated. The history of agricultural research is littered with examples of research addressing assumptions which were subsequently revealed to be poorly founded or erroneous. Often this has been caused by proponents of principle- or discipline-based research pushing what they considered to be a constraint to production, with only weak prior quantification. Ryan *et al.* (2019) use the case of arbuscular mycorrhizal fungi (AMF) to demonstrate how readily discipline specialists sabotage the real needs of sustainable intensification (or even common sense) to promote their own narrow disciplinary interests as central (Ryan and Graham 2018). Mycorrhizal specialists propose that food security can be served best by moving towards AMF-sufficient farming systems that mimic natural systems (Rillig *et al.* 2018). A systems agronomy approach instead considers sustainable intensification against defensible physiological benchmarks, and then diagnoses the constraints in such a way that AMF, if important, would become part of the solution (Ryan *et al.* 2018).

Such behaviour is not uncommon and is completely rational in a research environment where funding is scarce, and funding bodies are increasingly seeking short- to medium-term impact over novelty. It is easier for researchers to adopt a narrative that places their discipline as central to deliver transformational change than it is to work with other disciplines and generalists to solve properly defined and quantified constraints. There is no greater evidence of this than the vast pile of plant molecular biology papers (many working on model species) that commence their introduction with rhetorical outline of the need to increase crop production to feed a growing world population. Whilst this narrative has helped capture an astounding level of resources and prestige, it has done very little to change what happens in farmers' fields anywhere in the world (Porter *et al.* 2018). In many ways this situation is merely the result of the deeply human adage 'when all you have is a hammer, all you see is a nail.

For instance, the notion that crop yields in the high rainfall zones of SE Australia were limited by the predominance of sodic sub-soils was first raised by Gardner *et al.* (1992) and repeated by many others (*e.g*. Zhang *et al.* 2006, Adcock *et al.* 2007, MacEwan *et al.* 2010). None of these studies reported empirical evidence that sodic sub-soils actually reduce root growth and crop yield. On the contrary, Gardner *et al.* (1992) cite a study by Whitfield *et al.* (1992) that measured roots of canola and wheat down to 1.0 m and water extraction beyond this depth. Similarly, Zhang *et al.* (2006) cite the study of Lorimer and Douglas (2001) that observed wheat roots growing into dense sodic clay subsoils. The substantial negative effects of sodic soils on crop yields appear to be overstated and based largely on assumptions. The ripple effect of these assertions has been millions of dollars on research attempting to ameliorate sodic sub-soils with gypsum and organic amendments. Many of these experiments injected high rates of manure into the sub-soils, and erroneously claimed substantial yield responses due to amelioration of subsoil constraints (Gill *et al.* 2008, Gill *et al.* 2012, Sale *et al*. 2019). The likely explanation for the (at times physiologically implausible) yield increase was provision of nutrients in manure in a nutrient-limited environment (Celestina *et al.* 2018, Celestina *et al.* 2019). Measured changes in soil physical properties are explained more parsimoniously by improved root growth in response to alleviation of nutrient deficiency.

This situation could have been avoided if researchers had more accurately defined and quantified the constraints that were limiting yields prior to embarking on sub-soil amelioration treatments, instead of simply assuming sodic sub-soils were the major problem to be addressed. To do this would have required characterising soil physiochemical properties and plant available water capacity to confirm that soil water extraction by plant roots was indeed restricted (Celestina *et al.* 2019) – a potentially costly and time-consuming, but necessary undertaking. Even without the identification of genuine constraints to production, the inclusion of proper control treatments and use of appropriate sampling protocols in these experiments would have allowed nutrient- and non-nutrient effects on crop yield to be separated (Celestina *et al*. 2019), thereby revealing nutrient deficiency – not sodic sub-soils – as the critical constraint to crop production in the high rainfall zone of SE Australia. Until clearer attribution is provided, yield benefits attributed to deep placement of organic amendments beyond nutritional impacts will remain contentious.

Since Gardner *et al.* (1992) was published, yields in the high rainfall zone of SE Australia have increased dramatically without any broad scale amelioration of sodic subsoils (Robertson *et al.* 2016). This has been achieved by installation of raised beds to alleviate waterlogging, better crop rotation reducing root disease and weed burden, timely sowing of high yielding cultivars either specifically bred or imported for the region, and a dramatic shift in the levels of fertiliser (particularly N) applied to crops. It was root disease, seasonal timing of crop development, foliar disease and nutrition that was limiting yields, not sodic soils. The direct cost to the Australian grains industry and taxpayer is many millions of dollars. An unaccounted cost is the opportunity cost of what research could have been funded with this money. This example highlights how critical it is to define and quantify constraints to production accurately prior to conducting research to avoid potentially costly mistakes.

Defining solutions to overcome constraints

Defining solutions requires discipline-specific knowledge. Frequently constraints have more than one viable solution, and some solutions interact either positively (synergies) or negatively (trade-offs). Agronomists need to have enough cursory knowledge of associated disciplines to be able to seek input from experts at this point. At the same time, the agronomist must act as an independent evaluator of solutions and choose those most likely to succeed. They must also be prepared to collaborate with disciplinary specialists with whom they may not normally interact to engage fundamental scientists to develop applied solutions.

Testing solutions

Testing solutions in silico

Once constraints have been accurately defined and quantified, plausible solutions need to be tested. Some constraints and solutions can be well represented by crop simulation models such as APSIM (Holzworth *et al.* 2014) and, in these instances, it is extremely cost-effective to initially test solutions *in silico*. The benefits of using models for testing are that they allow solutions to be evaluated over a very large number of sites and seasons at very little cost. Environmental and farming systems interactions can be properly investigated, and outputs of variables that can be expensive or difficult to measure in the field can be cheaply obtained.

This process is often termed *pre-experimental modelling*, and there are many examples in Australian dryland crop production of simulation studies that have quantified constraints to production or evaluated solutions to constraints. This includes potential for summer weed control to improve capture of summer fallow rain; this was tested *in silico* (Kirkegaard and Hunt 2010, Hunt and Kirkegaard 2011) as part of the Grains Research and Development Corporation (GRDC) 'water use efficiency' initiative (Kirkegaard *et al.* 2014). Responses were promising and were subsequently tested in field experiments (Haskins and McMaster 2012, Hunt *et al.* 2013, Kirkegaard *et al.* 2014). Likewise, simulation studies identified that slow-developing cultivars of wheat sown early could achieve higher yields than the current practice of fast cultivars sown later (Moore 2009, van Rees *et al.* 2014). This was tested experimentally and found to be the case (Flohr *et al.* 2018c, Peake *et al.* 2018, Hunt *et al.* 2019). Simulation studies on canola have shown that the principles extend beyond wheat (Christy *et al.* 2013) and are currently being experimentally tested (Brill *et al.* 2019).

There are other situations that do not lend themselves to modelling. This is particularly the case when constraints are biotic. Simulation models, particularly APSIM, do not incorporate well-validated modules that can simulate the dynamics of biotic constraints such as weeds, invertebrate pests or disease, or their effect on crop growth and yield. Whilst there have been efforts to incorporate these or build new tools with dynamic modules (*e.g.* DYMEX, Whish *et al.* 2015), this stands as a significant gap in the utility of crop simulation. Whilst many biotic constraints are manageable, this relates less to the improvement of PY_w and more to the closure of yield gaps, but there are notable exceptions. A recent example is the refinement necessary in defining optimal sowing and flowering times for canola (Lilley *et al.* 2019) due to increased risk of upper canopy blackleg (*Leptosphaeria maculans*); this would have gone unrecognised had the agronomists and physiologists involved not been working closely with pathologists as the new early sowing systems were developed (Sprague *et al.* 2018).

Testing solutions in experimental plots

Promising solutions identified by first-principles or simulation must be experimentally tested to determine their efficacy in overcoming the intended constraints. Small plot experiments are still the most cost-effective way to achieve this, particularly if many factors are involved. Testing solutions effectively in plots requires good science training and critical thinking to ensure appropriate experimental designs and conduct, and that valid controls are used (see Celestina *et al.* 2019). Input from the discipline of statistics is critical at this point.

Because agronomy is so context dependent, it is crucial that experiments have the same context as the farming systems they are intending to emulate. This is yet another reason why it is of value to seek the involvement of leading growers and commercial agronomists in research from the outset. They are invaluable to provide feedback on the relevance of research, particularly experimental context.

The skill and level of dedication required to conduct plot experiments successfully cannot be overstated. Skilled operators are needed to ensure that factors other than those explicitly under evaluation in experiments do not dictate results. For instance, Peake *et al.* (2018) point out that many published field experiments that have sought to compare long and short duration wheat crops (McDonald *et al.* 1983, Ortiz-Monasterio R *et al.* 1994, Gomez-Macpherson and Richards 1995) have done so under nitrogen limitation, meaning that any inherent yield differences were unlikely to have been expressed.

Testing solutions in farmers' fields

As adoption rates of controlled traffic farming increase (Umbers 2017) – and with it, the precision and ease with which solutions can be tested at large scales – the potential to use swathes in farmers' fields as statistical units in field experiments is increasing. This has tremendous power from both a statistical and adoption perspective and can be a positive aspect of collective inquiry. However, the types and number of interventions that can be tested via these means are limited. Conduct of these sorts of experiments will always slow down agricultural operations, increases expense to the grower and likely reduces yield and profitability. These experiments require extremely dedicated, benevolent or well compensated co-operators to be successful. Even if experiments in farmers' fields testing interventions against controls are not possible, proposed interventions need implementation on farm if they are to achieve their intended aim. It is thus critical for research agronomists to work closely with growers and commercial agronomists on farm.

In many cases the interventions being tested by research agronomists emerge from techniques already practised by leading growers. Often there is overlap between the adoption phase, and the research and development phase. An illustration of this is the case of summer fallow weed control in southern Australia. Leading commercial agronomists and growers had identified the effect that controlling summer weeds had on crop yield (van Rees and Smallwood 2000) well before the constraint was properly quantified by research agronomists, or the mechanisms fully understood. The constraints were subsequently quantified by experimentation and simulation and the mechanisms clarified (Hunt and Kirkegaard 2011, Hunt *et al.* 2013). Scientific quantification of the constraint then helped drive further adoption of summer weed control as growers and commercial agronomists in southern Australia could attach a robust monetary value and risk level to the practice (Kirkegaard *et al.* 2014).

Integration into farming systems

Until solutions to constraints that have been tested by research agronomists are adopted in farmers' fields, this investment in RD&E has not generated any return. Integration of solutions to complex constraints into farming systems is far from easy but, despite this, the returns from agricultural research are generally high (Alston *et al.* 2009). Barriers to adoption are many and diverse. There are always production trade-offs with proposed solutions and often costs or changes in risk profile. There are also social and economic barriers that are frequently insurmountable. Tools such as ADOPT (Kuehne *et al.* 2017) can provide valuable insight for researchers as to the likelihood of adoption of solutions and what potential barriers might be, although ideally such analysis would take place in early stages of research. Adoption will always be greatest if research is conducted in close collaboration with growers and their advisors.

Sometimes trade-offs are perceived rather than real. In the case of summer fallow weed control, a frequently cited reason for letting fallow weeds grow has been the provision of feed for livestock at a time of year when supplementary feeding was often needed. Such assumptions need to be challenged with data. Whole farm modelling demonstrated that the small reduction in supplementary feeding due to uncontrolled growth of summer weeds could not compensate for the associated reduction in crop yields (Moore and Hunt 2012). Similar perceptions that sheep trampling causes lasting damage to no-till soils and reduces crop yields were also overturned with careful testing and measurement (Hunt *et al.* 2016).

Modelling also has a role to play in this step of the process as it can scale-up findings from small plots to the level of the whole farm. This we refer to as *post-experimental modelling*. The impact of some interventions on yield can be magnified at the level of the whole farm, whilst others can be diminished. For instance, a modest yield increase (0-10%) from slow developing cultivars sown early could scale up to a 20% increase in whole farm yield (Hunt *et al.* 2019). Conversely, numerous crop sequencing experiments have demonstrated the superior economic performance of break crops in comparison to long fallow. However, model-based scale-up to continuous cropping at the whole farm level diluted the impact due to operational and logistical considerations (Cann *et al.* 2019).

The role of impact evaluation

A difficult question to answer for the Australian grains industry is what level of resources should be invested to evaluate the adoption or other impacts from research. In contrast to the USA, where centralised management records for most grain crops are kept as a requirement for state subsidised insurance, there are very little standardised or comprehensive data available on management practices used by Australian growers. Organisations that have sought to acquire these data have had to do so through the use of surveys (Umbers 2017), which are deeply limited in terms of their sample size and the qualitative information that they tend to obtain. Some records are available through databases from commercial services accessed by growers (*e.g.* both Anderson *et al.* (2016) and Flohr *et al.* (2018c) used the Yield Prophet® database to evaluate temporal changes in growers' sowing time), but these are also limited in size and often skewed towards progressive growers.

There are emerging technologies that could be extremely helpful to solve this problem. Remote sensing could be used to detect many practices, such as summer fallow weed control, sowing time and crop species, but there are few published examples. Quality of historic satellite images has previously been a barrier (Gobbet and Hunt 2017) which may be removed with improved satellite systems. The power of this would be the incredibly large sample sizes and veracity of information (what actually happened as opposed to what the grower said happened, as is obtained in a survey).

Much is made of the potential for online storage of farm management data and subsequent analysis using advanced data science techniques (*e.g*. machine learning). These analyses are only as good as the data that go into them; with little financial incentive for detailed record keeping, growers generally are currently poor at maintaining accurate records. This may change with shifting societal expectations of provenance and traceability of food commodities, or if the data analytics provide more utility in decision making. Some funding organisations such as GRDC in Australia invest in their own assessments of impact. Interestingly a recent assessment found the work on early sowing systems described here as an example of transformational agronomy had an estimated internal rate of investment of 152%, more than double the nearest project area (snail and slug management), and well above a series of other high-profile projects on rust control, weed control and legume N fixation (11 to 64%, GRDC 2019).

In summary, we think it vital to demonstrate impact of research through rigorous evaluation of changes in management practices, but currently available techniques are expensive, and the methods flawed; it seems prudent not to spend too much growers' money finding out what they are doing. This will hopefully change as the technology and expectations outlined above change.

Novel transformational agronomy

Below are three constraints that we believe could be overcome with the multi-disciplinary research that is embodied in transformational agronomy. Indeed, if these could be achieved we believe it would lead to transformational changes in production and profit for Australian growers. These are complex problems and will not be overcome cheaply or easily, but the pay-off from doing so would justify the investment.

Removal of N limitation

Nitrogen deficiency remains the single biggest factor contributing to the sizeable exploitable yield gap in Australian wheat production (Hochman and Horan 2018) and likely other non-legume crops (barley, oats, canola) as well. It is true even of elite growers in favourable seasons (van Rees *et al.* 2015). At first this appears somewhat paradoxical; nitrogen management in grain crops is extremely simple – crop requirement is well related to yield as described by the simple rule of thumb taught to all budding agronomists: 40 kg/ha N per tonne of anticipated wheat yield. Sources of N are also readily quantified – mineral N in the soil prior to sowing can be cheaply and easily measured from intact soil cores. Mineralisation is more difficult to estimate but it is possible and is self-correcting (spring rain leads to higher yield potential but also more mineralisation). The complexity comes in reliably estimating anticipated yields. This requires no less capability than the accurate prediction of weather several months in advance! The difficulty arises from Australia's extremely variable rainfall. For instance, in southern NSW when growers need to make decisions regarding N inputs (July-August) in seasons with no stored soil water prior to sowing, possible yields range from 0 to 7 t/ha, all dependent on September and October rainfall. In addition, over-fertilisation with N can reduce both yield and grain quality through haying-off (van Herwaarden *et al.* 1998). N fertiliser is also a costly input and, mindful of perhaps exaggerated environmental losses (Turner *et al.* 2012, Schwenke *et al.* 2014), growers tend to err on the conservative side in their applications.

There have been consistent attempts to improve prediction of yields to make N management more precise. This has included the use of forecast systems (Asseng *et al.* 2012) and decision support systems that integrate soil resources and management variables and present likely response to N inputs in probabilistic terms (Hochman *et al.* 2009). Whilst seasonal forecasts are likely to improve in their skill as computing power increases, they will never be perfect. Given the substantial nature of the problem a fresh approach is required. One such solution that may work in environments with low N losses (*e.g.* low rainfall areas with high soil water holding capacity) is the use of N fertiliser to maintain a base level of soil fertility (‘N bank’) sufficient to achieve water limited potential yields in the majority of growing seasons (as is currently done for phosphorus). Implementation of this strategy would need to consider the amount of mineral N in the soil profile to adjust inputs for carry-over of previously applied N fertiliser not used by the crop. If applied appropriately at the time of rapid crop uptake, environmental losses from the ‘N bank’ would be low in stubble retained farming systems where the majority of applied N is either taken up by the crop or immobilised into organic forms. Losses could be further reduced through use of higher efficiency N application strategies (*e.g.* deep and mid-row banding). Once the N banks are built, the cost of N fertiliser for growers is deferred into the season following (rather than the season of) high yields; this could have substantial economic value through improved cash flow and tax benefits. It would also reverse the mining of soil N that has occurred under Australian crop production since the decline in area of legume-based pastures (Angus and Grace 2017).

Whilst this solution represents a closing of the yield gap rather than an increase in potential yield and therefore defies the general statement about the role of research agronomists made above, the complexity of the constraint and gaps in knowledge are such that research is required. A multidisciplinary team is also essential to test this solution effectively. It requires accurate measurement of N losses and N cycling within the soil, and this requires discipline-specific expertise from within the field of soil science. Economic assessment would also be critical, and it also requires investigation of management techniques to minimise possible negative effects on yield and quality from high levels of soil mineral N. Pre- and post-experimental simulation would be essential first to test assumptions, identify locations and treatments that would be promising to test in the field, and then extend field results over multiple sites and seasons. If found to be successful, GIS tools (yield and protein mapping) would allow even greater efficiencies through spatial mapping of N removal in grain.

Crop establishment in the absence of autumn rainfall

From the time of Farrer, much of the agricultural research conducted in Australia has aimed to coincide critical periods of yield determination in crop species with climatically optimal conditions for growth. The cool, wet winters during which crops are grown in southern and Western Australia transition rapidly into hot, dry summers at which time temperatures become supra-optimal and water highly limiting. When combined with spring frosts, this creates a reasonably narrow period during which crops must undergo their critical development phases in order for yields to be maximised (usually associated with flowering, Dreccer *et al.* 2018). Whilst the concept of such optima has long been known (Anderson *et al.* 1996), it has been the advent of computer simulation that has allowed them to be quantified for multiple locations across many seasons, firstly for wheat (Flohr *et al.* 2017) and then canola (Lilley *et al.* 2019) with other crops likely to follow. Shifting crop development closer toward optimal flowering periods has been the major mechanism behind many of the transformational changes in Australian crop production. This includes such iconic advances as the release of Federation wheat with its faster development pattern (Pugsley 1983), the rise of no-till which allowed much earlier sowing (Stephens and Lyons 1998), and more recent shifts to dry and early sowing (Fletcher *et al.* 2016, Hunt *et al.* 2019).

Recent quantification of optimal flowering periods (Flohr *et al.* 2017, Lilley *et al.* 2019) has revealed that leading growers are now coinciding critical periods with seasonal optima for the first time in history (Flohr *et al.* 2018c). The only times they do not achieve timely flowering is when they have been unable to do so due to dry autumns providing insufficient soil moisture to allow seeds to germinate and emerge. Somewhat ironically, this new period of enlightenment regarding optimal sowing times of major crops has coincided with declining autumn rainfall (Pook *et al.* 2009, Cai *et al.* 2012) making it harder than ever for growers to achieve optimal flowering periods. This defines our second opportunity to overcome a major constraint to crop production – achieving crop establishment in the absence of breaking autumn rain. Once again, an integrated solution to this constraint demands multidisciplinary expertise led by a generalist with appreciation of G x E x M context. Input is required from disciplines of agricultural engineering, plant physiology, genetics and soil physics.

Our knowledge of the regulation of seed germination has developed greatly in recent times yet understanding of the basis of variation of seed establishment in the field remains limited. This is probably because most seed biology experiments are performed in laboratories on Petri dishes or under optimal conditions, whereas seeds in the field are subject to a complicated soil matrix where they experience a variety of different stresses (Finch-Savage and Bassel 2015). Domestication and breeding have provided incremental improvements in the ability of crops to germinate and emerge under sub-optimal conditions, but here we discuss ways in which agronomically directed research could be applied to transform seed performance when surface soil is dry.

Soil water potential is a major factor in determining germination and establishment. Many species can germinate at soil water potentials well below those that maximise plant growth (Wuest and Lutcher 2013). Distinguishing between adequate and marginal water to enable germination can be difficult for growers – there are no well-defined criteria for determining if a soil contains a high enough water content to germinate different crop species. At water potentials above -1.1 MPa, germination rates are rapid (Wuest and Lutcher 2013). Further decreases in water potential slow the speed of germination; below -1.6 MPa, germination ceases. Pawloski and Shaykewich (1972) showed that these effects were similar between soils, even when soils differ in hydraulic conductivity. Crop establishment could be enhanced by the ability of seeds to germinate at lower water potentials. This could be achieved by genetic or other means. Singh *et al.* (2013) examined differences between wheat cultivars as a function of water potential and found significant variation in the ability to germinate at low water potentials. Genetic variation for rates of seed water uptake (which initiates germination and is the first stage in the malting process) exists in barley, and it has been suggested that this could be exploited by breeders for the benefit of the malting and brewing industries (Cu *et al.* 2016). The same principles and expertise could be applied to field germination at lower water potential. An obvious trade-off that may arise with the genetic ability to germinate at low water potentials is susceptibility to pre-harvest sprouting (Rodríguez *et al*. 2015). Expertise from plant physiologists concerned with the regulation of dormancy would be essential to harness this opportunity.

Beyond genetic means, strategies for manipulating germination processes used in horticulture crops and rice could be evaluated. Seed priming techniques limit the availability of water to the seed so there is sufficient to progress metabolism, but insufficient for completion of germination (Halmer 2004). Seed priming has potential to reduce the lag time between imbibition and emergence and synchronise seedling emergence. Seed priming has been shown to improve emergence of wheat under low temperatures (Farooq *et al.* 2008), but not necessarily under low water potentials (Giri and Schillinger 2003). The inclusion of plant growth regulators, hormones or micronutrients during priming can also improve germination and emergence (Jisha *et al.* 2013, Ali *et al.* 2018). It is clear from the literature there are many potential solutions that could improve seed germination and establishment at low water potentials. Extensive field appraisal of these techniques is required.

Inadequate moisture at ideal sowing depth has led to growers sowing deeper to 'moisture-seek' (placing seed into moist soil below a layer of dry soil) to make use of residual moisture stored from summer rains or the previous growing season. Their ability to do this is currently restricted by the availability of sowing equipment capable of placing seeds into moist soil at depth, and the ability of plants to emerge from depth. Coleoptile length is an important trait determining the success of emergence from depth (see Chapter 20) but there are also other genetic factors involved (Mohan *et al.* 2013). Modern Australian semi-dwarf wheat and barley cultivars show poor emergence when sown deep (greater than 8 cm) due to shortened coleoptiles (Rebetzke *et al.* 2007). Warmer soils in future may further exacerbate poor establishment and with deeper sowing.

Pre-experimental modelling indicates substantial benefits for crop yield in southern Australia if machinery and genotypes could be developed that allowed placement and emergence of seed at depth (Kirkegaard and Hunt 2010, Flohr *et al.* 2018a). Establishment of crops in this way is routine in the drylands of the Pacific North West USA, where seeds of winter wheat and now other crops are sown deep using deep furrow drills into moisture remaining from 13-month fallows and can emerge with 10-15 cm of soil covering them (Schillinger and Papendick 2008). Rebetzke *et al.* (2016) have argued the case for Australian breeders to use novel dwarfing genes that do not suppress coleoptile length. Larger seed size is also known to improve deep-sown crop establishment. Large-seeded canola improved the timeliness of establishment and subsequent grain yield of canola when rainfall for crop establishment was marginal but there was moisture available deeper in the seedbed (Brill *et al.* 2016).

Frost, drought and heat

Whilst optimisation of flowering times has allowed the combined stresses of drought, frost and heat to be minimised, these abiotic stresses still take a large toll on crops every year. They would continue to do so even if establishment in the absence of autumn rain (see above) could be achieved. With all avenues for avoidance of frost, drought and heat explored, the only means remaining to increase yields in the face of these cardinal abiotic stresses is through crop tolerance. It is our opinion that this will most likely be achieved via genetic solutions, but that these must be considered in an appropriate G x E x M context.

Frost, drought and heat are inextricably linked. Frost risk declines as flowering moves later into the spring, but risk of drought and heat increases. This means that tolerances to all three stresses are not necessary to improve yields. If tolerance can be found to either frost on the one hand, or drought and heat on the other, then optimal flowering will shift accordingly to reduce the likelihood of occurrence of opposing stress. That is, if we can solve frost stress then we have solved drought and heat stress, and vice versa. The value of this approach has been demonstrated by economic analyses of potential frost tolerance. The benefit of shifting flowering time to avoid drought and heat has also been quantified (An-Vo *et al.* 2018). Therefore, the important question is which of these stresses will be cheapest and easiest to solve?

Drought and heat are perhaps easier targets compared with frost in that it is reasonably easy to screen for tolerance in different genotypes. For various reasons, frost tolerance is extremely tricky to phenotypically screen for, and frost itself is virtually impossible to recreate under controlled conditions. Thus, we believe it likely that the breakthrough will come through combined drought and heat tolerance rather than frost tolerance. The trick with heat and drought is that they interact. Studies that have attempted to identify sources of heat tolerance in the absence of drought have found tolerance is associated with stomatal opening and rapid water-use that depresses canopy temperatures relative to the atmosphere (Reynolds *et al.* 1994). For heat tolerance to be useful in the Australian context, it must be effective under limited water supply (Hunt *et al.* 2018).

Whilst there may be some promise in selecting morphological traits known to confer both heat and drought tolerance (Hunt *et al.* 2018), the greatest and most cost-effective progress may be made by breeders selecting for high yield at late flowering times where crops would be routinely exposed to concurrent drought and heat stress. However, this is where wider crop physiology and management context becomes important – it would be crucial that late flowering be achieved with slow developing cultivars that could be sown early and thus exploit a full growing season rather than by late sowing of faster developing cultivars where yield potential would be limited by shallow rooting depth and low biomass accumulation (Kirkegaard *et al.* 2015, Lilley and Kirkegaard 2016).

Barriers to transformational agronomy

In this chapter we have outlined some of the factors that have diminished the role of agronomists in the research process and, though we recognise individual responsibility for change, various barriers for young agronomists can be identified (Table 1). Broader thinking in assessing opportunities to increase water limited potential yield or close yield gaps, foreseeing interactions and developing field experiments and simulations to test them must be encouraged – as opposed to narrow disciplinary and single factor experiments. Such a change must commence with training, which has already been mentioned as a deficiency in recent years (see Chapter 3). This will inevitably lead to better collaborations and partnerships; institutional arrangements should encourage and support rather than dictate or obstruct such interactions. Siloed research and funding organisations discourage these interactions as do overly aggressive intellectual property (IP) and legal arrangements, yet active encouragement and facilitation is required to initiate and maintain them.

Table 1. A range of barriers to transformational agronomy and behaviours that can minimise them.

Barrier	**Description**	**Improvement**
Conceptual	How we think about things	Think more broadly about interactions (G x E x M)
Statistical	How we design/analyse	Embrace systems designs and experiments
Cultural	How we approach research	Partnerships and collaboration
Structural	How we organise teams	Reward integrators as specialists
Institutional	How we are rewarded	Reward impact, not just impact factors
Training	How we are taught	Elevate agronomy in university courses

Structural and institutional arrangements must encourage and reward those working in, and leading teams that deliver real impact through increased adoption of practices that improve yield or reduce input costs, risk or environmental damage rather than simply rewarding individual researchers based on publication metrics and impact factor. Unfortunately, there is evidence that success in funding proposals is negatively correlated with multi-disciplinary teams, suggesting the problem is entrenched (Bromham *et al.* 2016). Career progression is generally slowed in science ranks for those who are not seen to specialise, and in technical ranks for those working in field-based agronomy – yet these staff are crucial to the quality, relevance and rigour in on-farm experimental research.

Achieving these changes must start with the training of the next generation of transformational agronomists, who are motivated by the prospect of a rewarding career of high-quality research delivering real impact in future food security.

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