2010 Donald Oration

Peter Cornish

The University of Western Sydney Email: p.cornish@uws.edu.au

Prologue

As I reflect on the outstanding skill and contributions made by many agronomists I'm humbled that you've chosen *me* to receive the 2010 Donald medal. To be acknowledged by one's peers is the greatest professional honour possible. I'm grateful and thankful to my proposers, the Committee and you, the members of the Society, for granting me this honour. I now plan to speak mostly about my evolving interest in farming systems, but first I want to acknowledge the work of some Australians that has particularly influenced me.

Prof Donald was once described as "the ideotype of an agronomist, a person with the ideal attributes and characters for agronomic investigation". The appellation arose from Donald's ideas on competition, which led him in 1968 to publish the concept of the ideotype with his famous diagram of the 'uniculm' wheat. This was the antithesis of high tillering, the goal that ruled breeding at the time. The 'ideotype' embodies the set of characters needed by crop or pasture plants to perform well *in dense communities*. I never met Colin Donald, but I was familiar with his work on subterranean clover and competition and I spent my first two years after graduation in the Plant Introduction Section of CSIRO that he had established in the 1950's.

Donald's work had a significant influence on me, particularly as a young pasture ecologist at Condobolin in central NSW. Although rainfall there is equi-seasonal, we assumed the region was ecologically "Mediterranean-like" because evaporation so greatly exceeded rainfall in the warmer months. Donald's influence led me to question this assumption and the prevailing view that adaptation was a matter of selecting Mediterranean legumes with hard seeds to cope with 'false breaks' to the growing season, and early flowering to avoid terminal water stress. Donald's work had contributed to this view. Using the new tool of water balance modelling I contrasted central NSW with regions receiving most of their rainfall in winter, and concluded the strategy carried a significant *production cost*. The work was published much later (Cornish 1987a) but is still referred to occasionally today. Taking my cue from Donald, I proposed an ideotype for annual legumes that I supposed might be better adapted and more productive in this environment, leading to interests in seedling drought resistance or avoidance (Cornish 1974) and flowering behaviour in annual medics (Cornish 1985). It also led to a plant collection mission in 1974, almost following in the footsteps of Colin Donald ... but not quite, as we went to Central Asia and the Caucasus seeking different traits in the so-called 'Mediterranean' species (Broue et al. 1975). I became an advocate of summer active species for pastures in this environment, including lucerne and native grasses. The real significance of this surfaced much later when we understood the *environmental 'cost'* of water 'leaking' beyond the rootzone in farming systems based on winter-active crops and pastures. Water balance modelling has played an important role in identifying the 'leakage' of contemporary farming systems, including my own small modelling contribution in central-western NSW with my colleagues Richard MacCallum and Neil Fettell. The issue is widely acknowledged now as we hunt for ways to put perennials back into farming systems.

Others have greatly influenced me, including **Tony Fischer**, a Donald medallist whose work on rainfed wheat crop physiology underpinned much of my work on water use in farming systems. **John Passioura**'s (1979) paper on 'Accountability, Philosophy and Plant Physiology" helped me formalise my thoughts and practice as an agronomist in terms of systems theory. **Tony Gregson** represented farmers when reviewing my work on the agronomy and physiology of malting barley. My results challenged the conventional wisdom for managing malting barley in areas with safe rainfall, pointing to earlier planting and planting earlier in the rotation to take advantage of higher fertility and use more of the available water (Cornish et al 1987). I anticipated a new farming system, but I hadn't researched it. I was really chuffed when Tony said years later that I had made him a lot of money, but *he* had researched the new system, not me - this anecdote illustrated Passioura's systems theory in practice and the power of farmers to do research, and reminded me that agronomy is ultimately about improving livelihoods.

Introduction

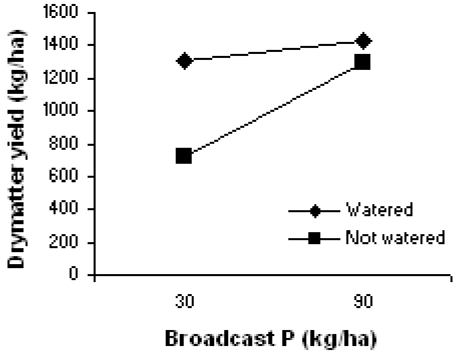
Farming systems appear complex and yet farmers manage them and most somehow make a living despite the vagaries of the weather and the markets, and the banks. Farmers manage every day with issues that we as scientists struggle to understand. Our training equips us well to simplify complex systems into bits we *can* understand but, try as we might, it does not equip us well to integrate in the way farmers must if they are to prosper. One way a farmer copes with complexity is to concentrate on more effectively pulling the few levers that make a difference to the bottom line; such as crop choice, variety, tillage/machinery choices, and fertiliser type/rate. As agronomists we have tools that can identify new levers to pull, or develop new ways of more effectively pulling old levers, but we only see parts of the picture. It is the farmer who sees the whole picture, and takes the risks. Moreover, without diminishing the importance of science in the progress of modern agriculture, an honest reflection reveals that farmers have contributed a great deal of innovation. It follows that a partnership between farmers and scientists ought to be a good way of advancing agriculture. I suspect that Colin Donald would agree. It has taken me 40 years to grasp what this means in practice.

To the extent that this paper has any purpose, other than to afford me the luxury of sharing my reflections, it is to pay homage to many wonderful pioneering farmers, including those of the Cornish family who once farmed in the South Australian mallee. It is a collage of my experiences whilst learning about 'farming systems'. It is essentially practical, and neither deeply theoretical nor philosophical. I promise not to use the words 'paradigm', 'facilitate' or 'empower'.

Nutritional drought, deep P placement, and improving P-fertiliser prediction

An experience as a student in the late 1960's shaped my life over the next 40 years and laid the foundation for my interest in farming systems as '*human activity systems'*. This was the 'sub and super' era. Wool prices were high, there was still a subsidy on superphosphate, and it was the heyday of grazed fertiliser experiments. We were in Canberra to meet CSIRO scientists who told us all about the good work on 'sub and super' ... and also about the slow uptake of ideas in parts of the nearby Yass Valley where the farmers were apparently 'a bit slow'. An extension organisation, the Yass Valley Organisation, had even been set up to deal with the problem by establishing 'demonstration' farms.

My first job as an agronomist a year later was with CSIRO and NSW Agriculture working in the Yass Valley on the ecology of 'improved pastures'. The 'problem farmers', it turned out, had sedimentary ('skeletal') soils with low water holding capacity in the surface and above average P fixation capacity. I learnt that broadcast superphosphate was often stranded in dry surface soil and therefore less effective than in field experiments elsewhere. A regular sprinkling of water made a modest rate of P more effective (Fig. 1a) but had no effect on plant water relations. A pot experiment with reconstructed soil profiles and sub-surface watering confirmed that P uptake and plant growth from the 'problem' soil could equal other soils at 'recommended' rates of P if the surface was kept wet (Fig. 1b). Deep placement of fertiliser doubled P uptake in the infrequently watered treatment (4 days/month).



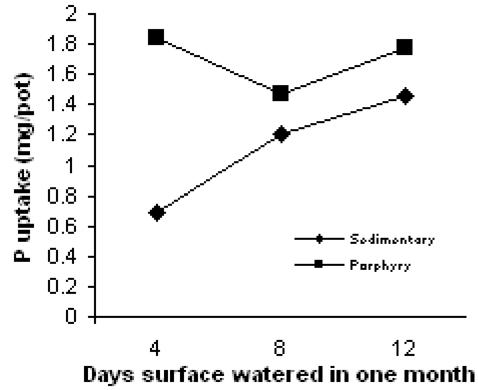


Fig. 1a. Response by subterranean clover to surface-applied P and supplementary water in the field. Murrumbateman, NSW, 3/03 – 3/05/69.

Water (43 mm) applied twice-weekly at (0.7*E_{pan})-rainfall. Rainfall for the period was 194 mm and E_{pan} 171 mm. (From Cornish 1977)

Fig. 1b. P uptake by subterranean clover in response to surface watering - pots.

P broadcast at 15 kg P/ha. Plants watered by sub-irrigation at 30 cm. Pots surface watered for establishment and then allowed to dry. 'Sedimentary' from field experiment (Fig. 1a)**.** (Cornish 1970, 1977)

So, far from being 'slow', the farmers had tried 'sub and super' and found it wanting. My enduring lesson about farming systems was that farmers and their knowledge are central to this *human activity system*. If we want to make progress we need to engage with them ... and agronomic interventions need to turn a profit.

What I've described is '*nutritional drought'*, a phenomenon that Brendan Scott subsequently worked on at Condobolin calling it 'positional unavailability'. Scott showed how important it was in a ley farming system to fertilise the crop sufficiently to provide residual P at depth for the subsequent pasture, rather than broadcast fertiliser ineffectively on the surface in the pasture phase. This was an important systems concept.

Crops also experience nutritional drought. In Wagga in 1982 I worked on an idea inspired by John Passioura and Richard Richards. They were selecting wheat with greater hydraulic resistance in seminal roots. Their idea was to reduce subsoil water-use early in the season to make more water available during grain filling. I wanted to know if deep sowing (using long-coleoptile varieties) could achieve the same result by increasing the length of the sub-crown internode and so increase the resistance to water flow. It was a drought year and the results were stunning - deeper planting doubled yields, but not for the reasons I'd supposed. I had confounded deeper sowing with deeper fertiliser placement. Deeper placement into moist soil had prolonged P uptake resulting in big differences in plant P concentration, water-use and yield.

Later Shane Norrish, a PhD student of mine, showed that a little irrigation at stem elongation increased soil P uptake by wheat on a vertosol soil at Moree (Fig. 2). Just 35 mm increased yield by ~40%, *after* deducting any direct effect of water (10 kg grain/ha/mm water applied). Crops were P deficient, but P-fertiliser didn't help much.

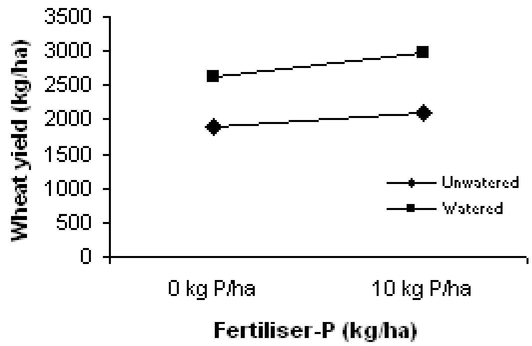


Fig. 2. Wheat yield response to P fertiliser and 35 mm supplementary 'irrigation'. Plant P concentration response to watering, P<0.001; to fertiliser, n.s. Norrish et al. (2001). Subsoil P was low in this particular vertosol.

In his thesis, Norrish revealed the importance of subsoil P in some other vertosols. As subsoil P reserves in northern vertosols are depleted, winter crops will depend more on fertiliser-P, which may not be very effective because of dry surface soils in this predominantly summer-rainfall environment.

More than just 'nutritional drought'

In the early 1980's I became interested in the surface stratification of P in new methods of tillage we were working on. I was amongst the first to show that P stratifies in the surface following direct drilling, and that this could increase the requirement for fertiliser-P (Cornish 1987a). To me this was quite an interesting story that went beyond the unavailability of soil-surface P ... even with fertiliser, plants could not take up all the P they required. Although direct drilled crops needed more P to achieve 90% of the maximum yield, young plants in cultivated seedbeds were always larger than with no tillage, regardless of the P rate applied (Fig. 3). Plant weight was closely related to tissue P concentration, so it seems inescapable that *direct-drilled plants were short of P, even at high rates of fertiliser application*. To complicate the story further, whether differences in early growth resulted in yield differences or not depended on how much water there was for grain filling, and this reflected rainfall and tillage method, as well as paddock history. I'll come to that later.

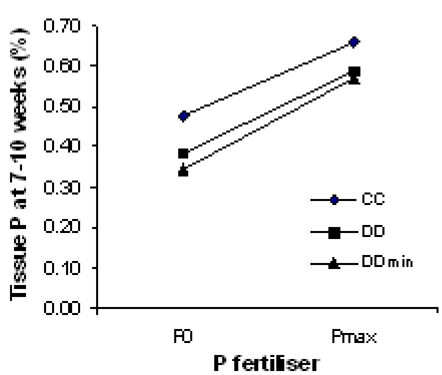


Figure 3. Tissue P concentrations in wheat seedlings (7-10 weeks) in conventional cultivation (CC) and direct drilled with conventional combine (DD) or with minimal soil disturbance (DD_{min}). Means of 7 field experiments over 3 years at Wagga Wagga, NSW. Rates at P_{max} were 24 to 50 kg P/ha for the different experiments.

So, whilst surface drying may be a factor in the greater fertiliser requirement of direct-drilled wheat, the evidence suggests that effectiveness of banded fertiliser was reduced by limited root physiological uptake capacity, thus favouring plants in the tilled seedbed with access to a greater diffuse source of P below planting depth. This ran against the wisdom of the time that P uptake was always limited by the rate of supply to the root, not by root uptake capacity. Presumably it was because a relatively short length of root took up most of the plant's P requirement, even when roots proliferated in the P-amended soil. I would expect physiological P uptake capacity to be most limiting in low P soils or where most of the soil P resource is stratified near the surface and either above seedling roots or subject to surface drying.

It has taken 20 years for P stratification to be researched more widely and demonstrate that soil-conserving tillage practices can indeed increase the requirement for fertiliser-P (see AJSR 2009, Issue 47). Whether response to that fertiliser is limited by P uptake capacity remains to be seen.

Deep placement of P

This has been a recurring theme for nearly 40 years. Researchers have been motivated by seeing dry surface soils, stratified P in reduced-till systems, and by farmers asking if they can take advantage of the capacity in newer seeders to vertically separate seed and fertiliser. Most recently the evidence that crops on some vertosol soils in the northern grains region have been drawing on subsoil P has prompted interest there. Despite this effort, results are still hard to predict. Recent synthesis and re-interpretation of the literature has helped, but the challenge now is to put the findings into practice using some robust rules-of-thumb.

Indirect effects of water on P uptake

We tend to think about the direct effect of soil water on P uptake but overlook the indirect effect through soil strength and root extension; which is important because P uptake by most agricultural plants is roughly proportional to root length (Cornish et al. 1984). This is because P diffuses slowly in soil, and uptake occurs from within a cylinder that generally does not extend much beyond the root hairs. Roots deplete the labile P in this cylinder of soil within a few days of entering it - further root extension is needed to maintain P uptake.

The effect of drying on soil strength and therefore root growth and P uptake varies between soils. This is shown in Fig.4 for two undisturbed surface soils of equal bulk density from pastures near Armidale, NSW. Root extension in the silty loam is about halved by lowering water potential from near saturation (-0.01MPa) to -0.03 MPa ('field capacity'), and drops to ~20% of the maximum when water potential is just -0.1MPa. The reduced root growth has nothing to do with plant water deficits. It is entirely the effect of increasing soil strength upon drying. Root growth is much less affected in the sandy loam.

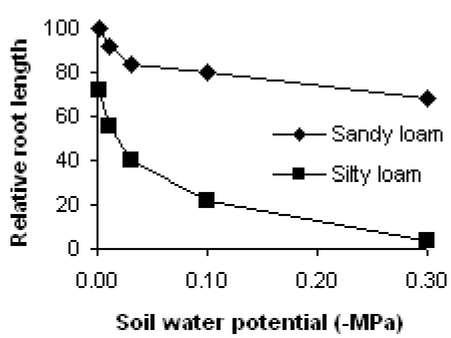


Fig. 4. Effect of soil water potential on root growth in undisturbed pasture soils (0-5 cm depth) with equal bulk density (1.3 g/cm^3). Root length is relative to the maximum in this experiment (Source: re-drawn from Cornish 1987b).

Assuming that P uptake is a function of root extension, it follows from Fig. 4 that P uptake from the silty loam will approach zero when the soil is not much drier than field capacity. P-deficiency will limit pasture growth in the silty loam for much of the time, regardless of the water available for growth, and almost regardless of soil P concentration. I showed this for the sandy loam in Fig 4 by modelling surface soil water, and from this predicting root growth and P uptake (Cornish 1987b). There were long periods when water was available for growth but P uptake was limited by high strength and slow root extension. P will limit growth in the silty loam regardless of water availability, unless plants can access P from fertiliser banded near roots, absorb significant P from subsoil (only likely in some vertosols) or mobilise P from reserves within the plant. For grazed pasture (without banded P) this amounts to root reserves. High shoot-P concentrations have been associated with P-efficiency but this would be an appropriate strategy only in crop plants.

A new look at interpreting soil test values

We all know that P uptake is a physico-chemical process that can't be predicted from measures of soil

chemistry alone. Yet this is pretty much what we try to do with soil testing. It's easy to understand why farmers seem to place so little faith in soil-P testing when you compare the *theory* of P response curves with the *reality* (Fig. 5). On top of that, as always with agronomy, responses in a controlled experiment may be different from commercial practice where other variables also determine yield, not to mention economics. Our response to the variation in Fig. 5 is typically to develop another test or combination of tests, and that is what Gourley *et al.* (2009) did, by using PBI to group soils; but this makes little difference. Not that there is anything wrong with their work, which is a compilation and synthesis of many experiments, but they are missing the essential soil water data to *interpret* the variation and improve predictions. Soil water is missing because it is not usually measured, and in some P-rate experiments rainfall has not even been reported.

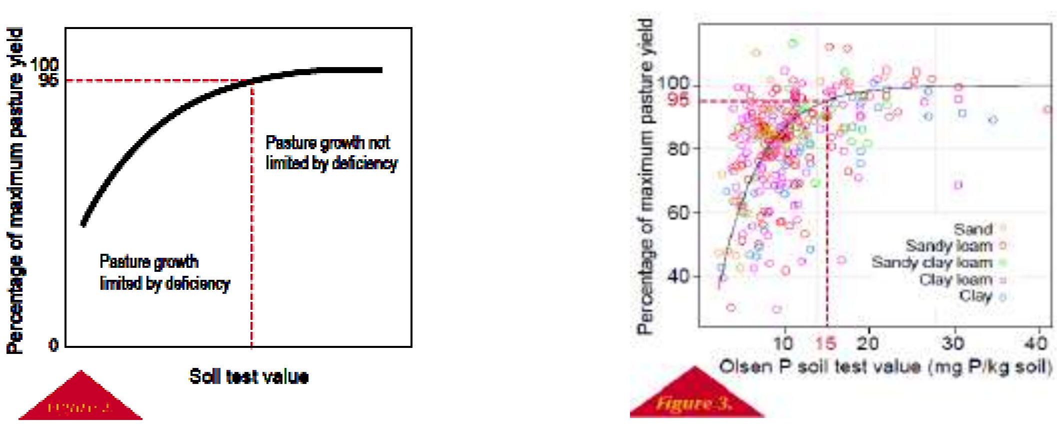


Fig. 5. Theoretical and actual P response curves: (Gourley et al. 2009)

Much of the variation in Fig. 5 could be explained, I suggest, by variation in soil water; but such detailed explanations have been absent from most of our work on P responses. We are familiar with the effect of water on *demand* for P, but less familiar with the direct and indirect effects on P *uptake* that I have discussed already. We have been far too descriptive, as John Passioura might observe. **As for future research**, I see potential for improvements in the predictive value of soil-P testing, which is important to underpin improved fertiliser efficiency, and improved predictability for the response to deep placement of P. This will be underpinned by reconsidering the mechanisms that contribute to P (in) efficiency.

The Tillage Revolution

No-till cropping, to me, is the most important advance in agronomy in the last 50 years, up there with the release of semi-dwarf wheat. Not only did it reverse soil degradation, but it made more intensive cropping systems possible, and it underpinned the rise in wheat yields that started in the mid 1980's (documented by Kirkegaard and Hunt 2010). I'm pleased to have been a small part of this development amongst many notable contributors, including farmers who took all the risks, including the risk of social exclusion.

It seems hard to believe now, but for three centuries the argument had raged about why we cultivate soil – what are the main reasons for the evident responses, apart from the practicalities of getting seed into the ground. Arguments about the reasons for fallowing in Australia are an echo of this. The arrival of bipyridyls in 1961 and glyphosate in 1971 gave us the ability for the first time to grow significant crop areas without cultivation, and so to experimentally test the competing theories. Thus commenced one of the most exciting eras ever for agricultural science, one in which Australia was undoubtedly a world leader.

The role of ICI

ICI commenced research in Australia (and NZ) early in the 1960s to see if crops could be grown without cultivation using Paraquat?, Diquat? (Spray.Seed?) as the main agent for weed control before planting. ICI pioneered 'direct drilling' that evolved into the "Burn-Graze-Spray-Wait-Sow" *system.* It was initially developed with farmers and later promoted widely through ICI farmer groups. The idea was to burn crop residues, graze hard after the autumn 'break' to manage the weed biomass and get some grazing (pastures using water at the expense of the later crop), then spray to knock down remaining weeds and wait for 'root release'. The crop was sown a few days later, preferably with a combine modified to deal with tough soils. ICI in Australia promoted a new *system*, to the extent that even the implications for autumn grazing and lambing time were considered. ICI arranged national and regional conferences as early as 1977 to share what was being learnt. They can take great credit for backing this research and development[i].

The research 'Establishment'

In the early 1960's Kohn and Storrier at Wagga used herbicides to replace cultivation in field studies of soil N. They inadvertently showed that good crops could be grown without cultivation. McNeill, also from Wagga, then carried out multi-site field experiments comparing conventional cultivation, reduced cultivation and direct drilling from 1969 to1975, showing that direct drilling reduced yields, but only by an average 10%. At about this time, research was also underway in Victoria (Reeves and Ellington), SA (Catt) and WA (Tennant, Anne Hamblin, Marsh). Research on no-till was getting underway in the northern grains region (Felton), where residue retention was a higher priority than further south because of higher summer rainfall erosivity and lower stubble cover on soils. Tony Fischer, then with CSIRO, joined the research around 1978. There were many other contributors, too numerous to mention, but all important.

In Australia by the early 1970s there was an acute awareness of the damaging effects of cultivation, but no obvious alternative. The Soil Conservation Services still backed earthworks as the primary weapon in the fight against soil erosion. The issue was brought to a head by the widespread use of trifluralin herbicide in the early 1970's, because it required such thorough incorporation by cultivation. So at this time, the main motivation for reducing tillage was to reduce the risk of soil erosion. Other potential benefits emerged later.

By the 1980's farmers engaged in the development of direct drilling were mostly able to establish a crop, but there were issues they couldn't resolve. Apart from weeds, the most important issue was the reduced early growth and unpredictable effect on yield. Also, by now breeders at Wagga were asking how they should respond[ii]. My research in the 1980's focused mainly on these issues. The effect on yield was complicated, but most of it could be explained by water. First, grazing after the autumn break reduced soil water at planting. This could reduce yield if in-crop rainfall was low. Modelling suggested a short fallow was useful in 40% of years at Wagga (Cornish 1987c). With this knowledge farmers could weigh up the costs and benefits of any extra grazing against water storage in a short fallow. Second, after planting, reduced early growth reduced pre-anthesis water-use which saved water for grain filling. Although *potential* yield was reduced, the *actual* outcome depended on growing season rainfall and any stored water at planting (Cornish and Lymbery 1987). In our experiments the net result was a 2% *increase* in yield, not the deficit McNeill reported earlier, possibly because of earlier weed control and the short fallow effect. As for why early growth was less, we could provide only part of the answer in reduced root growth and P uptake, and the need for more P-fertiliser mentioned earlier. Roberts, Kirkegaard and others later provided the rest of the answer

We also explored the effects of tillage and stubble retention on infiltration and soil evaporation, concluding that stubble retention was good for the water balance. This related more to improved infiltration than to reduced evaporation (Cornish 1987e). These benefits were much greater in the northern grains region. By the 1990's many agronomists realised that the water needed to be used or it would drain somewhere. At first the production costs were obvious, and later the potential environmental implications became clear.

When the Soil Conservation Service of NSW finally embraced soil management as the primary attack on soil erosion in the 1980's, they coined the slogan "Conservation Farming – Good farmers manage it". The inference that farmers who didn't embrace 'conservation farming' were poor managers was far from the truth. Despite research and development there were serious difficulties to overcome before conservation farming was a reliable, economic proposition – not the least being the need for new equipment and the capital to buy it. Add emerging problems with annual grass weeds, herbicide resistance, N 'tie-up', and newly emerging diseases like yellow spot and we had a formidable set of issues. It is to the great credit of many pioneering farmers and agronomists that solutions to these problems have largely been found, and that improved soil management has been adopted almost universally. Farmers routinely cultivated 10-12 times in the 1970's, but this had dropped to 1-3 times by the 1990's. Recent surveys have shown that most farmers now either practice no-till or plan to (machinery and herbicide resistance are barriers to adoption). This is a remarkable, given that soil erosion due to excessive cultivation was the nation's most pressing environmental issue at the time! How did it happen?

The role of farmers in innovation, adoption and dis-adoption in the early years

The ICI approach to direct drilling became relatively complex with implications for many aspects of farm

management. It was evident quite early that farmers would need to see clear benefits for these new ideas to take root, and simple 'demonstrations' alone would not bring about change. Farmers everywhere are keen 'experimenters', and ICI took advantage of this to engage them in much of the pioneering work with on-farm comparisons. By the 1980's farmer groups and some individuals were involved in the development of new approaches to weed management, especially grass weed problems. Ideas of spray-topping or pasture cleaning dating from the early 1970's were further developed mainly by farmers. Some farmers were 'experimenting' with rapeseed (canola) in rotations to help with weed management. These were not formal experiments, but they were often assisted by public and private agronomists. Farmers reported that trifluralin could be used at high rates in rapeseed without much incorporation to control grass weeds after pasture, ensuring fewer grass weeds in the subsequent wheat that could be direct-drilled. Rapeseed also made good use of the high N following pasture. Farmers were also central to machinery development. There was a lot of 'learning' about new farming systems, but little of this came from formal experiments.

You might think that 'extension' of direct drilling was a tough ask. After all, it was promoted to tackle soil erosion, but most farmers at that time thought they had little erosion, even if they saw it elsewhere in their region. Farmers (or their wives) expressed concern about toxic chemicals, and rightly so, especially in the case of the bipyridyls. Direct drilled seed beds looked awful, early growth was reduced even if yield didn't always suffer, and the early pioneers reported having a hard time in the pub! Yet, in spite of these significant challenges, the area of crop sown by direct drilling or reduced tillage increased very rapidly, especially in WA in the 1970's. This is remarkable when you consider how slow change on farms can sometimes be. On reflection, I attribute this first to the active participation of farmers in the early research, and from this the clear benefits that farmers perceived, apart from any potential reduction in soil erosion.

Direct drilling and reduced tillage took off first in WA, with rapid adoption between 1972 and 1974, but this came to an end with an explosion of annual ryegrass. It was not until the advent of Hoegrass? that direct-drilling took off again, growing to 2.3 M ha by 1983. Of course, herbicide resistance emerged as early as 1982, and with it a new set of recurring challenges. In NSW, the area direct-drilled grew to 400,000 ha during 1980-1983, only to crash in the wet summer of 1983/84 when sales of disc ploughs boomed. In retrospect, direct drilling had grown mainly because dry summers had prevented cultivation, so direct drilling was a straightforward default option in some years. Many of the 'converts' slipped away. Nevertheless, there remained a small but growing group of farmers who were committed to making this new approach to farming work. *It's worth recalling that what we called 'reduced tillage' in our early experiments had, by the mid 1980's, become 'conventional'*.

A system, not a new practice

What started out as the simple substitution of herbicide for tillage evolved into quite a complex sequence of operations: burning-grazing-spraying-waiting-sowing. This pointed to rather complex 'systemic' changes for the farmer. In 1987 Jim Pratley and I produced a monograph for the Agronomy Society called "Tillage - New Directions in Australian Agriculture". We ensured the systemic elements were captured as well as the basic science. We also commissioned a chapter dealing with 'new' approaches to extension that focused on 'adult-learning' rather than 'technology transfer'. While doing the monograph, we were asked to contribute a paper to the 1985 Agronomy Conference, in which we answered the latter to the question 'Is Conservation farming a crop establishment alternative or a whole- farm system?'. We questioned the capacity of agronomists to deal with the complexity and called for new approaches to research, extension and education.

It had emerged mainly from farmers that one of the greatest benefits of direct-drilling was that a larger area of crop could be planted, and in a more timely fashion. Most researchers had trouble with this because our experiments were designed to control any extraneous variables. So we had different tillage treatments, but generally insisted on sowing all treatments on the same day. As our work became more sophisticated we introduced crop residue management and N as treatments, and rotational crops. But still we had trouble varying planting time to reveal the true agronomic benefits of these new approaches to soil management. I've tried more systemic designs for farming systems experiments that have been publishable (see Wells et al., 2000)[iii], but as a consultant to GRDC from 1994-2000 I learnt that we steadfastly resist change in the way we run our multi-factorial 'systems' experiments. Peter Carberry would recommend 'modelling experiments' instead, to explore farming systems options. I see the merit in this, but I'm wary of over trusting the virtual reality of APSIM. In any case, a farming system is more than the biophysical aspects that can be modelled. Although, again, Peter and others would say that models can be used by managers to inform their management. I see their point, but I remain unconvinced about the value of complex simulation models as farm management tools, other than to re-set farmer's thumb-rules.

Efficient use of rainfall – crop benchmarking

Earlier I mentioned my experiments in the early 1980's with ways to improve crop water-use. In 1983, following the 1982 drought, my experimental crop yields were ~6 t/ha. I was surveying them with pride one day and glanced over the fence at the neighbour's crop, languishing at around 2 t/ha. The penny dropped. We all got about the same yields in dry years, but most farmers badly missed out in better years. Understanding and addressing this yield 'gap' in better years would do much more to improve farm livelihoods than any amount of yield improvement in poorer years. Farmers were rightly concerned with *risk* management for dry years, but they either did not see that *opportunities* also needed to be managed (or managed for), or unknown constraints were undermining their capacity to benefit from good rainfall.

I met Reg French (a Donald medallist) about this time, before the famous French & Schultz (1984) paper appeared. I was impressed by his analytical approach and the vast amount of data he had access to, for which we had no equivalent in NSW. Inspired, I worked some thoughts on 'rainfall-limited yields' into a talk at the 1984 Riverina Outlook Conference which actually had the theme of Soil Compaction. I boldly said that long-term Wagga yields could be over 4 t/ha, although the district average was less than 2 t/ha. Afterwards Bernie Hart, a well-known and highly regarded farmer from near Junee, told me he liked the idea that he could double yields for the same rainfall, but he didn't think he was ready yet. This would soon change.

Subsequently, Gordon Murray and I modelled potential yield for crops over a 25-year period and compared this with Wagga district average yields (Fig. 6). The title of our paper, *'Low rainfall rarely limits yields'*, was rather cheeky (for me) as the esteemed statistician EA Cornish had convinced us that low rainfall explained everything. We established local values for 'achievable' apparent transpiration efficiency, i.e. the slope of the water-use – yield graph (15 kg/ha/mm), and inferred soil evaporation, i.e. the 'threshold' (70 mm). We also stressed the importance of estimating water storage during the short summer fallow and factoring this into crop water use rather than rely on growing season rainfall alone. Farmers met in groups using these figures to benchmark crop performance. As crop benchmarking expanded, farmers used the values from French & Schultz that were being promoted in particular by ICI in 'Crop Production Groups' they had set up in 1983 to "identify and solve crop production problems" (Neil Clarke – stakeholder report).

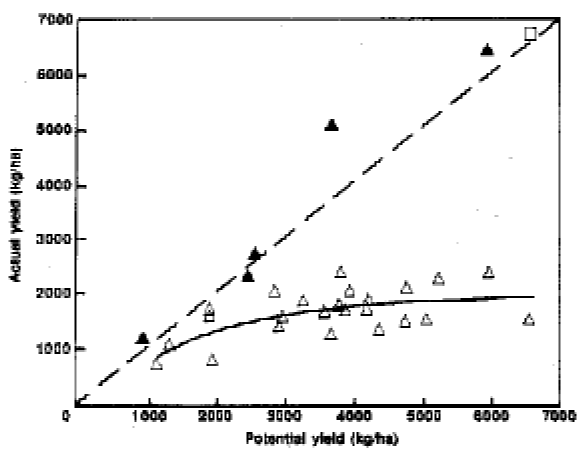


Fig. 6 "Low rainfall rarely limits yields". Potential yields are modelled. Wagga district yields 1960-1984r; experimental yields 1980-1984p,; a 1ha experimental field in 1983 o. (Cornish and Murray 1989).

I left Wagga without understanding why the yield gap occurred, although as far as nutrition was concerned it seemed that P might be an issue in dry years and N in wetter years. In my Outlook paper I also referred to soil-borne diseases and soil physical constraints that 'deserved careful consideration'. In a paper at the Golden Jubilee Symposium of the AIAS in 1985, I noted that 'virtually nothing is known about soil biology' – all foreshadowing research that was to come. Most importantly, we knew soil borne diseases reduced yield, but not by how much, or what to do about it. But well-adapted, black leg resistant, high oil quality Canola varieties were about to appear, and with them a new era in farming systems.

Bernie Hart was a prodigious record keeper and benchmarker. By the mid 1990's, his records showed he had almost doubled his crop WUE and wheat yield, and his sheep carrying capacity. Tony Fischer reported wheat WUE and yield increases of around 30% from his farm at Boree Ck. over the same period. This trend swept across Australia, as Kirkegaard and Hunt (2010) nicely documented in an update of Donald's famous graph of wheat yield trends (Fig.7). Most agronomists agree that break crops like Canola were the key.

On looking back, however, the efforts by farmers to control grass weeds before direct drilling wheat helped to bring soil-borne diseases under control even before the adoption of Canola. It also sensitised farmers to the value of Canola for grass weed control, and thus prepared the way for rapid adoption following the release of much improved varieties. Bringing soil-borne diseases under control allowed cereal crops to respond to N built up under ley pastures and also to respond to the higher rates of N recommended from a new generation of research (by Angus and others). Sadras and Angus kindly noted in a paper in AJAR in 2006 that “The recognition that water supply did not generally limit yield of rainfed wheat in southern Australia was important in promoting the yield increases of the 1990s (Cornish and Murray 1989)”.

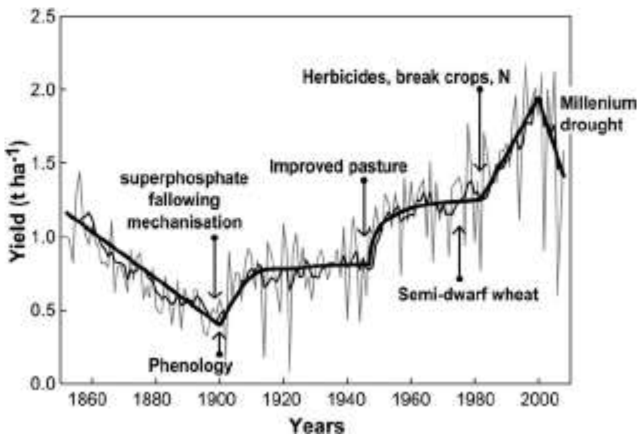


Fig. 7. Trends in Australian wheat yields - Kirkegaard and Hunt (2010) after Donald (1965)

Note increasing instability in yield, which I argue is not necessarily a bad thing, and may present opportunities[iv]

Bernie Hart and other farmers who estimated their crop WUE were **benchmarking**, and using this value as the starting point in a structured review of crop performance and its possible future improvement. Instead of comparing a crop with past performance, there was an independent benchmark of potential. Benchmarking used indicators that provided a summative assessment of performance (e.g. WUE) or information about key determinants of performance (e.g. plant population). It provided insights into the performance of individual fields, the overall farming system, or the key determinants of profit if appropriate indicators were used.

Even before French and Schultz published their benchmark, farmers had met in groups variously known as ‘2’ or ‘3 ton clubs’ (t/acre) to learn from each other how to improve yields. ICI were behind some of these ‘clubs’. With a much improved benchmark, and growing knowledge from crop physiology about the key indicators of crop performance, benchmarking really took off in the late 1980’s and it was a very important contributor to subsequent yield increases. I participated in two of these groups in the Wagga area, and they were exciting places to be with lots of sharing and learning between farmers and agronomists.

The TopCrop program developed from these groups, using principles of benchmarking and group learning. They were a happy meeting of science and industry. By the late 1990’s GRDC had turned TopCrop into a top-down catch-all extension program. To my mind, the key elements of its success were lost. That isn’t to say the TopCrop that emerged was unsuccessful – it was just different, and could not satisfy the needs met by the original bottom-up benchmarking groups.

Farming Systems Research

Farming System Research developed in the 1970’s to try and bridge the gap between theory and practice; to achieve greater impact from agricultural research, mostly in developing countries. It was widely adopted within the CGIAR Centres although it seems to have fallen out of favour since. The approach acquired its own rich theory and yes, there was a gap between the theory and the many diverse practices. Now, with so many different meanings, the term FSR has largely lost meaning. The common element between various approaches to ‘farming systems research’ is that *the technical production system is linked, at one level, to research on the underlying processes and, on another, to the people who manage the systems across multiple fields and enterprises* and ultimately undertake any innovation.

They all seek practical solutions in complex situations. Rarely are there simple solutions to complex problems, so FSR commonly seeks to *improve situations rather than solve problems*. FSR is underpinned by the principle that all stakeholders 'participate'. I interpret this to mean creating 'space' for farmers to develop their innate desire to 'experiment', that I referred earlier, but to do so with support from agricultural professionals and to our mutual benefit.

There have been exhaustive (and exhausting) reviews of the philosophy and theory of FSR. Here I simply draw on my own experience, and that of my students, to point to what worked for me. When I joined UWS in 1993 I joined an institution with a long and distinguished history as the Hawkesbury Agricultural College (Donald studied here). It was also undergoing radical change and forging a new reputation for experiential learning and new approaches to 'extension' (adult learning). It was natural then for some of my students to try and engage more directly with farmers, a level of engagement beyond what I had used for previous research, although most of that had been on farms. Some staff doing higher degrees tried likewise. None of them found it easy, whatever the nature of their enquiry. Something was missing that I had observed a long time ago, and that was the genuinely active participation of the farmer. Looking back over the previous 30 years it seemed that the most exciting developments with real 'spark' occurred either with ICI working directly with farmers, or in the Crop Production Groups of the 1980's, some of which I mentioned earlier. To be frank, the advances were more at the Descriptive (N) or Significance (N+1) levels of Passioura's (1979) hierarchy, rather than at the Understanding (N-1) level. So solutions were often local and rarely published, but progress was real and exciting. Most of my own research on farms had missed this 'spark'.

It was Peter Hayman or Peter Carberry who helped me to see the important distinction between 'research on farms' and 'on-farm research'. The difference, the 'spark', lies in the quality of farmer's participation: do they merely help choose sites and attend any field days; or do they help frame the research question, design the enquiry, collect data and share in the interpretation? This defines the strength of any links between the technical production system and the people who manage it, and the underlying processes – the goal of FSR.

When Bob Martin and Peter Grace reviewed long-term experiments (LTE's) in 1994 and questioned the value of large soil management x crop rotation trials, the stage was set for a major change in the way applied rural R&D was funded and managed by GRDC (not unique to GRDC). Pressure was on to close down most LTE's and introduce new approaches to applied agricultural research. I was a member of the Northern Panel of GRDC at the time and had the unpopular task of guiding transformation in the north, where my own research had revealed little or no improvement in yields since the mid 1970's (Cornish et al. 1998), even though improvements were occurring elsewhere. The case for a fresh approach was compelling.

With encouragement from GRDC I worked with Bob Martin (and others) to conceive a new approach that drew on systems theory, our collective experience and, we thought, some common sense (Martin et al. 1996). The schematic in Fig. 8 shows the influence of Passioura's philosophy. GRDC subsequently funded 3 major 'farming systems' projects in the northern region. All of them involved farmers in their development and management, but none were said to be 'farmer-driven'. They differed according to local needs, and none really developed as the model in Fig. 8 sets out. Reasons for this varied from researchers clinging to old habits through to industry politics. However, the initiative set a precedent for strong farmer engagement in applied research and GRDC took some key principles nationally. I departed at this time, but GRDC later nominated the Farming Systems Program for the 2002 Prime Minister's Award for Excellence in Public Sector Management which they won, kindly acknowledging my contribution on the awards night. Evaluation of the projects by Agtrans Research in 2009 reported good returns on investment, both past and anticipated.

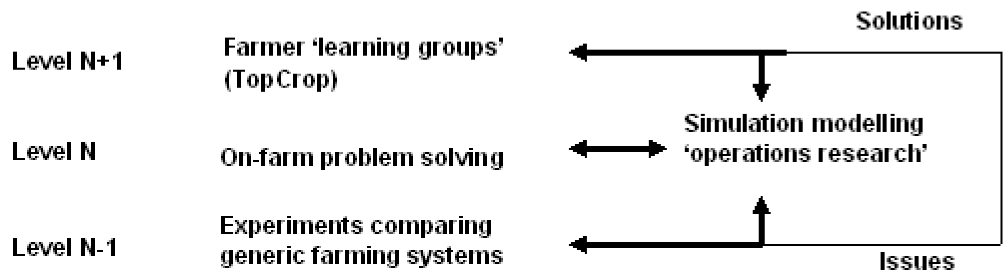


Fig.8. Organisation of a program of on-farm research funded by GRDC (Western Farming Systems) - Theory

Theory in agricultural science may be out of the sight of the practitioners, but like the foundations of a building which are also out of sight, both need to be strong. The original thinking behind the farming systems projects was informed by both theory and practice but there was slippage in the implementation, and there has been evolution since. It is timely to review the farming systems programs in Australia, checking for balanced effort at the relevant levels of complexity and examining the quality and quantity of the outputs.

Rural development – putting it all together

At times I feel like a butterfly, flitting from one topic to another at will. But there has always been a purpose, and I've maintained two themes throughout – P and water. I've been fortunate to have spanned spatial scales from the root surface to the catchment, 8 orders of magnitude, at each scale picking up new knowledge and skills and learning to integrate across scales.

This came together for me in 2003 when Tony Fischer asked me to consider developing and possibly leading a new project in India where ACIAR was keen to diversify its operations into poorer regions by working directly with the poorest farmers. Their hope was to achieve demonstrable impacts in a short period of time, probably in partnership with a non Government Organisation. He needed someone with broad experience in agronomy and farming systems, and sympathy for participatory research. He anticipated a relatively small project to identify research opportunities in East India. He regarded it as difficult, with a significant risk of failure. It took 3 trips to India and what seemed like interminable negotiations to finalise a project, but we were ready to go by the end of 2005. We had a partnership with an excellent NGO, PRADAN, which has highly trained development professionals in the 5 poorest States of India. PRADAN wanted to become more scientific in their work, but they did not want to become scientists.

Our project centred on the East India Plateau. I've described the work elsewhere at this Conference (Cornish et al 2010) and won't discuss it here in detail. Briefly, though, the people are amongst the poorest in India, many are Tribal. The area is the heartland for the Maoist (Naxalite) insurgency, India's greatest security threat. The population of East India is around 300 million, 70% of whom depend on farm-based livelihoods and 50% of whom live on less than $1/day. The region has been almost untouched by the Green Revolution – there has been little productivity growth in agriculture in 50 years. Despite high rainfall, only one crop is generally grown per year – almost all of it transplanted rainfed rice. There is little irrigation.

In India, as in much of the developing world, the gap between achievable yields and on-farm yields is greatest in rainfed areas. India's only hope of future food security lies in big improvements in productivity in rainfed areas, and nowhere more so than East India, which many people have referred to as a 'basket case'.

By the time the project was signed off by ACIAR, we had all convinced ourselves that any agronomic work needed to be (i) driven by livelihood improvement, putting families right at the centre, (ii) set in the context of watershed development which aims to more effectively use all water resources and (iii) genuinely participative. The scope and budget were larger than Tony had originally foreshadowed. So the stage was set to draw on all of my knowledge and skills, ranging from soil physics to catchment hydrology, and from soil P chemistry to fertiliser experiments designed for co-learning by farmers and the rest of the team. A strong theme of improving community engagement developed, including the effective engagement of women.

It was important for us to remember that this was research, not rural development, although we had an expectation of improving the situation of our collaborators. All activities have had a research question, even those that appear to be ‘simple demonstrations’, and all have followed an action-learning approach that starts with a participatory assessment of issues, followed by joint **planning** for appropriate actions/activities, then **doing** the activity together whilst **observing** the outcomes, and at the end **reflecting** on what it means and what can be done differently. This is followed by planning for improved actions as the start of a new cycle of learning. Many of the activities actioned within this framework were statistically valid experiments, often calling for a great deal of creativity and flexibility in designing activities that were both meaningful to farmers and scientifically rigorous. Many activities have been designed to change the self perceptions of farmers and perceptions of their land – the idea being that families can generate decent farm-based livelihoods from their own land, if they have the right attitudes, knowledge and skills.

As our project draws to a close we see potential for the East India Plateau to be a ‘food basket’. It does not need to be the ‘basket case’ it now seems to be. We have learnt there are good biophysical reasons for the lack of productivity growth in rice, in addition to any other socioeconomic or political reasons - population pressure has pushed rice from safe lowlands onto unsafe terraced and bunded uplands, setting farmers up for failure. Weak research and extension support combined with high risk aversion have conspired to give farmers few viable alternatives to transplanted rice, which is the crop best able to cope with low external inputs. Together with farmers, we have learned how to improve rice in their best lowlands, giving them financial scope to try alternatives to transplanted rice in their terraced and bunded uplands. They have learnt how to grow direct-seeded upland rice that has performed well under ‘drought’ conditions, and to grow vegetables in the monsoon for cash income when market prices are high. They have learnt how to ‘harvest’ water to provide small local water resources for irrigation, and how to use this to grow a second crop after rice. They have learned about residual soil water after rice can support a second crop with supplementary irrigation and a good dose of P (‘no phosphate-no crop’ the farmers say, and they are right).

Our research shows that farmer’s self perceptions have changed, and also they now say their best land is the uplands, rather than the traditional lowlands of which only rich farmers have a significant area. With new attitudes and experience, participating farmers are readily adopting new ideas and doing their own ‘experiments’ to test new crops, going well beyond ideas introduced by our project. Our colleagues in PRADAN now say they see an end to *‘NGO-dependency’*, and the farmers have expressed confidence that they will soon be independent of PRADAN - and unreliable government handouts. These are not a few families with whom we’ve directly engaged, but whole villages and groups of villages within catchments.

Amongst many indicators of change that we have monitored, to me the most significant is that girls are now going to school. These are girls whose mother’s once said that their greatest hope was for their daughters to go to school and not be illiterate like them. Of course there are many other positive indicators such as increased family cash income and reduced forced migration.

Much of the credit goes to the professionals in PRADAN, but they also say that they have learnt how to ‘create local knowledge’ – just what farmers were doing in Australia in the 1970’s and 80’s as they struggled with growing crops without cultivation and dealing with all the issues that emerged, setting the scene for the more intensive and less-unsustainable cropping with much higher yields that followed.

I’ve become a keen practitioner of highly participatory approaches to research, but I’m also cautious. Not every problem requires or can sustain a high level of farmer participation. Also, what passes for participation may not be at all participatory. I have a 'checklist' for participation (Table 1) that I refer to in addition to any socioeconomic and cultural considerations that may influence the research approach.

Table 1. A checklist for participatory on-farm research (farming systems research)

Check	Indicator
Is the level of participation appropriate for the question being asked	Problem is clearly defined and likely to have a simple solution: ▪ participation in the framing of the question might be enough Problem is poorly defined, complex, and incremental improvement likely: ▪ a higher order of participation is needed
Does the level of participation match my claim? (be alert to terms like 'farmer-driven' if this reflects actual practice)	At the deepest level:- ▪ All stakeholders meet to plan, do, observe & reflect, plan the next steps ▪ Farmers say they have 'space' to experiment ▪ Research questions shared by all, or at least activities address questions asked by each stakeholder (questions may differ, and stakeholders construct different meaning from the same data or activity) ▪ Researchers honestly say 'I don't know' when asked for a solution
Is it extension (or rural development) dressed up as research?	There is a clear research question & planned research output including publication (or is the science outcome largely predictable?)

And now ...

As I reflect on 40 years of professional life I feel happy to have done what I've done, and to conclude with this award today is a tremendous thrill. But even that pales beside the joy of applying a lifetime of learning in India and seeing lives transformed. I hope in time that all of you share in that same joy.

Acknowledgements

Many colleagues including students have enriched my life over the years. I thank you all. Most, however, I thank my wife Lynette who has patiently supported me in my comings and goings for 40 years.

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[i] Monsanto came later, emphasising applications for Roundup ?) (market development) rather than research.

[ii] From our research we later concluded that broadly adapted varieties selected under either tillage system would be OK, although some additional traits might be needed, the most obvious being yellow spot resistance.

[iii] This paper reports a comparison of intensive horticulture systems that started my major interest in relationships between land management and off-farm water quality. It was the first of a number of projects with a close engagement with hydrologists, from whom I've learnt a lot. Baginska et al. (1998) was the first of a series of papers reporting on nutrient, pesticide and coliform runoff from various land-uses. I have not discussed these here.

[iv] Yields since 1985 appear less stable than earlier. National data complicates interpretation, but to me the instability is not as bad as it first seems to be. I interpret it to mean that farmers are moving closer to effectively using the water available to them, in that regard more closely mimicking natural ecosystems. More water is being transpired by crops in good years, and less water is draining to groundwater so the prospect of dryland salinity is less. As agronomists and farmers get better at what they do the instability will increase further, unless we can predict the droughts and not sow at all. Consequently, farmers will have to manage wildly fluctuating cash flows, and we have a nightmare for marketing. Farmers will continue to develop ways of coping with this, including a lot more grain storage. Wild fluctuations in national crop production will impact on price, presenting the astute farmer with opportunities to make more money.