Donald Oration 2004

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Introduction

The Donald medal that you have given me today is, in my mind, the greatest professional award and honour that I could have received. I am aware of the strong competition that there is for the award, and both exalted and humbled that I was chosen. I warmly thank my proposers and the Committee of the Australian Agronomy Society for the award, and for this opportunity to speak to you on a subject of my choosing for 45 minutes, at a Congress when even the Plenary Speakers get only 30 minutes. It is considered good form to give a brief summary of what one intends to cover in a talk, but I feel today I can dispense with good form, and just let it flow. However as a concession to as a concession to our President, I will try to avoid the word "extension".

Professor Colin Donald

It is valuable to spend a few minutes considering the achievements and influences of Professor Colin Donald. His breadth of interests stamped him as a true agronomist and agricultural scientist, for his research and writings ranged across both pastures and crops, and from ecology to crop physiology, to cereal breeding, and to advocacy on progress in Australia's agriculture. He went to Hawkesbury Agricultural College and the University of Sydney, graduating in agricultural science with first class honours. Those were the days when you studied all science in agriculture, and did a fair bit of practical training, to receive that degree. He worked for the NSW Department of Agriculture, CSIRO Plant Industry, and finally as Professor of Agriculture and Head of the Department of Agronomy at the Waite Institute, University of Adelaide. His early work was on pasture nutrition and evaluation of sub clover, and he soon became an expert in and strong advocate for leguminous pastures and phosphorus in southern Australian agriculture (eg Donald 1965), the ley farming revolution that transformed the scene beginning around the middle of the last century. In the early 1960s there appeared for the first time his famous graph of the changes in decennial wheat yields in Australia after the mid 19 century (Donald 1962a), updated since then by several agronomists, including myself, and a great lesson in agricultural progress and sustainability.

After becoming Professor at the Waite, Donald's thinking matured in another field in which he had been working, that of competition in crops and pastures. He had written a much cited review on the subject in Advances in Agronomy (Donald 1963), with the memorable line about the "substantial independence of competitive ability and yield", meaning yield per unit area. However it was in his talk and paper to the Third International Wheat Genetics Symposium in Canberra in 1968 that he spelt out the implications of this for breeders and launched his second famous figure, that illustrating the wheat ideotype (Donald 1968). He was only rivalled at that meeting by a pre-Nobel Peace Prize Norman Borlaug laying down the law, perhaps for the first time to an international audience, on the green revolution in wheat, and railing against the world's useless bureaucrats . There would have been a mighty contrast between their styles, and their approaches to breeding, but Colin Donald was not to be dissuaded by Norm's fierce belief in tillering ability. He went on to elaborate his ideas on low tillering communal plants, partly through the efforts of graduate student John Hamblin, and to evaluate his suggested ideotype through breeding uniculm barleys (Donald 1979). In 1976 there appeared, coauthored with John, another very influential review in Advances in Agronomy (Donald and Hamblin, 1976), namely "Biological yield and harvest index of cereals as agronomic and plant breeding criteria", and finally in 1980 at the Frankel Symposium, his presentation entitled "Competitive plants, communal plants and yield in wheat crops" (Donald, 1981), followed by a Chapter "Developments in wheat agronomy" by myself (Fischer, 1981), the only time I shared a podium with Colin Donald.

Donald's influences on me

John Hamblin was only one of Donald's many famous graduate students over the years. Alas I was not among them. I had only listened at a respectful distance to Donald, and Borlaug, at that famous Canberra conference in 1968. And it was not until 1976 that I had the first of two or three serious discussions with Colin about wheat improvement. Growing up on the farm at Boree Creek in southern NSW, I had encountered the wonders of subterranean clover, even the long superseded variety Dwalganup, with which Colin had worked so much, and we had both worked in the NSW Department of Agriculture and in CSIRO Plant Industry, but there was a large time gap separating us..

Although I didn't interact with Donald until 1976, his writings on competition and on the wheat ideotype mentioned above, substantially influenced my work beginning in 1970 as the first ever wheat physiologist and agronomist employed at CIMMYT, and Donald showed great interest in that work when he eventually learnt of it. At CIMMYT my boss was none other than Norman Borlaug, by then the giant of wheat breeding and the undisputed father of the green revolution. And there was I, before long convinced that CIMMYT should be selecting Donald's communal plants, adapted to perform at crop densities, rather than Borlaug's heavy tillerers that looked so impressive amongst the thousands of spaced F2 and F3 populations that CIMMYT wheat breeders trawled through each cycle. Performance as a spaced plant was just as useless as competitive ability was, as a predictor of yield at normal crop density, a fact about which Don Duvick often reminds us in his comprehensive studies of yield progress in hybrid maize in USA (eg Duvick, 2005).

CIMMYT in the early 1970s was simply wonderful for a young scientist, despite the intense work pressures and the outcome/impact orientation (well before the donors had invented the word incidentally). Resources were good, field research facilities unenviable, excitement levels high, and Mexico as always, frustrating but colourful and fascinating. Only the heady graduate student days in California in the mid 60s outshone this period for me, when research for knowledge's sake was the goal and we were all idealists and revolutionaries. Of course in CIMMYT ideals were OK also, but their focus had to be different, and I learnt to sublimate the revolutionary notions and the sympathies for Che Guevara, just as I kept my thoughts on yield selection in wheat pretty much under wraps. I had seen what had happened to visitor Jim Syme, a successful young Australian breeder himself at the time, when one day on a visit to CIMMYT during the F2 selection ritual, he suggested to Norman Borlaug that measuring harvest index may be a better way to select the spaced plants: it took 30 minutes for Norm to calm down, but to his credit and as always, he apologised to Jim the next day!

Colin and Jim were right. There were new traits that could be used for selection amongst spaced plants for per hectare yielding ability, and there were physiologically-sensible traits that stood a reasonable chance, if deliberately sought and built into wheat, of improving yield potential, ideotraits if you like. Sometime later Richard Richards and Jim Morgan started to employ this approach to breeding better wheats for dry environments in Australia, and after 25 years, are seeing successes, with varieties in farmers' fields, but that's another story.

Golden years in CIMMYT and more on Donald

Back in CIMMYT in the 70s and for sound reasons, it was traits for selection for yield potential of wheat in which I was especially interested. Norm Borlaug actually gave me a fairly free hand to look at selection criteria (in my own plots), and we did a lot of work on harvest index and on selecting for small erect leaves.

With Zoltan Kertesz from Hungary, we found that harvest index measured on a central shoot of a spaced plant had a much better genotypic correlation with yield potential that yield per plant itself, but alas it took time to measure harvest index, and the take home message for breeders visually selecting amongst thousands of plants was try to place more emphasis on dry matter distribution than on amount (Fischer and Kertesz 1976)….shades of Donald's ideotype. As were the short erect leaves that my Dutch post doc colleague, Dirk HilleRisLambers sought from wherever, putting them into the best breeders lines of the day. We could never prove that they boosted yield but the CIMMYT breeders made their own crosses to them, and lo and behold when I returned to CIMMYT in the late 1980s much of the advanced material, both in durums and bread wheat, was of the small erect leaf phenotype. By then Dirk was spending his days boating around plots in Thailand as a deep water rice breeder, but he probably achieved more through his largely unrecognized pre-breeding work on wheat at CIMMYT.

It was in the mid 70s while pursuing early generation selection criteria in CIMMYT that we latched onto the illusive relationship between yield potential and stomatal conductance. It's worth spending a few minutes on this one. It owes nothing to Colin Donald, who did little work on detailed cereal physiology. How did it happen? I had done my graduate work on stomatal mechanisms at UC Davis, a glorious period of curiosity research which had earnt me a paper in Science (Fischer 1968), a handsome science prize of 1000 Swiss francs, and had brought me brief fame and proximity to the big names in plant physiology. It also meant I knew about air flow porometry, old fashioned (dating back to Francis Darwin) but quick and inexpensive. I took a porometer "gun" from my previous position at ANU to Mexico in the early 70s, added a small aneuroid pressure gauge, and by timing a given pressure drop, or better still, reading pressure change over a given time like 10 seconds, I could measure leaf permeability in the field on 3 or 4 leaves a minute, permeability (LP) showing a robust linear relationship to stomatal conductance in wheat (Fischer et al 1977). It worked well in irrigation scheduling experiments but we quickly learnt that LP in wheat was very sensitive to other things as well, including variety. About the same time, a paper from Dan Shimshi in Israel showed a nice correlation of yield potential with the same leaf permeability (Shimshi and Ephrat, 1975), as well as with photosynthetic rate, across short spring wheat varieties. The first serious look at this in Mexico in 1975 confirmed the relationship with LP, and as well, showed that LP on a spaced plant bore a significant relationship to yield potential (Fischer et al 1981).

At that point I left CIMMYT and my good colleague, Pat Wall, had the task of validating LP as a selection criteria in the breeders' own populations. Porometer guns were constructed and the work began in earnest. There is a brief mention of the results in a CIMMYT Annual Report around 1978: LP selection performed quite well and certainly beat breeders' visual selection. A victory for physiology but alas Pat Wall was sent to South America as an agronomist. Back in Australia I subsequently tried along with colleague Ken Quail to see if stomatal conductance (actually LP) could be used as an early generation selection criteria for yield potential in spaced F3 plants in a random population derived from a composite cross. We did a lot of measurements in Canberra and yield testing in the MIA but the final results were equivocal (Quail et al 1989), one of my best but most unrecognized papers.

To cut a long story short, when I got back to CIMMYT 12 years later, Ken Sayre had been using the new fangled infra red thermometer (IRT) gun to measure canopy temperature, and again saw relationships with yield, including ones where the yield potential variation was genetic. Of course, other things being equal, canopy temperature is simply an inverse function of stomatal conductance. We eventually showed that the yield potential progress in short wheats between 1962 and 1988 of about 30% was quite closely correlated with increased stomatal conductance, which increased 60% overall (Fischer et al 1998). We had been so close to this relationship in the mid 70s!

Over the last 5 years Matthew Reynolds and Tony Condon have yet again been trying to understand and utilize this unexplained yet enduring relationship, aided by a fancy and even faster air flow porometer, and have again come up with very reasonable relationships between what they have baptised as SATs (Stomatal Aperture Traits) and yield potential in random populations; use in regular selection by breeders is recommended. And remember the latest technology will now permit the measurement of one SAT, namely canopy temperature, across thousands of plots in a matter of seconds, using aeroplane-borne IRT!

Other work at CIMMYT flowed from Donald's papers. We wanted to know what limited yield potential. In 1971 a Mexican student, Immer Aguilar, did a simple planting density experiment with one of the newest semidwarf varieties of wheat, an experiment modelled on the brilliantly innovative experiments of Puckridge and Donald a few years earlier (Puckridge and Donald,1967), but done under irrigation and high inputs, where the only limiting resource would appear to be light. I recommend density experiments to all young crop physiologists. We found that density made absolutely no difference to grain yield, at least over the range 60 to 300 plants/m2, nor did row spacing up to at least 45cm (Fischer et al 1976). The promising high density narrow row crops with large amounts of vegetative growth and up to 1500 tillers/m2 produced a few more spikes/m2, but no more grains/m2. Everything was squeezed though the crop net photosynthesis and dry matter distribution bottle neck in the month leading up to flowering, the outcome of which determined grains/m2, regardless of the potential number of grains represented earlier on by the spikes initiated in all the tillers, and their spikelets and floret primordia numbers. I advanced the hypothesis that provided full ground cover was reached by the onset of spike growth, what happened earlier in terms of growth didn't matter, and as a corollary, all the work that physiologists loved to do studying meristematic spikes, made for nice microscopy, but was irrelevant to yield in the crop situation! The sort of conclusion Donald applauded, and a hypothesis now forgotten , but one which I believe still stands.

There were applications of this dawning knowledge about the control of yield under potential conditions. The best crops were mostly sink limited during grain filling in 1975, something Matt Reynolds has shown to still be the case with current varieties (Reynolds et al 2004). Back then we went on to pin down the importance of the spike growth phase, the period when the sink size in terms of grains was being set up, showing that temperature and solar radiation had their largest effects on grain yield then, (and that the yield advantage of short wheats and their superior successors seemed to derive from extra efficiencies then). Several important papers flowed from this (Fischer 1983, 1985). Modellers took on board much of this, they stopped trying to simulate grain number via the numerical yield components and approached it from the direction of total or spike dry matter at anthesis, something Colin Donald was getting at when he wrote his influential little paper "In search of yield" in 1962 (Donald 1962b).

As a result of our recognizing the importance of light interception around flowering it was possible to calculate the yield bias which could arise due simply to height and spreading differences in yield trials if the whole of a small plot was harvested. This was pointed out to the CIMMYT breeders in 1975, and from then on plot ends were trimmed off most yield trials, and edge rows discarded, facilitated by leaving a larger gap between the first and second row and penultimate and last row, because it also had been shown that yield potential was insensitive to row spacings up to at least 45cm. This also led to a paper I had the temerity to present at the International Wheat Genetics Symposium in New Delhi, entitled "Are your results confounded by intergenotypic competition" (Fischer 1978), reflecting my belief that many results were thus invalidated, something Donald would have applauded.

Thus Donald had a big influence on how my research unfolded in CIMMYT in the 1970s, and in turn, on many of the useful outcomes of the research for wheat improvement 15 and 30 years later

Challenges in designing wheats of higher yield potential

When I left the physiology of yield potential in wheat I had a fairly simple model of yield determination as outlined for example in a paper given at IRRI in 1980 (Fischer 1983). Yield variation was seen to be largely driven by variation in grains/m2 which in turn was related to two seemingly independent components, namely the total dry matter in spikes (g/m2) at anthesis and the number of grains per unit spike dry matter. Spike dry weight in turn was related to the total above ground dry matter produced during the spike growth period (approximately 30 days preceding the end of anthesis), and the proportion of this DM invested in reproductive structures, namely the spikes. Dry matter accumulation in the spike growth period could be expressed as a function of duration of the period, radiation interception then, and radiation use efficiency (RUE), but under potential conditions interception was always close to 100% in this period, while duration appeared to be largely an inverse function of temperature, and RUE with optimal crop management almost a constant. Grains per unit spike weight could also be considered the product of subcomponents, namely competent florets formed per unit spike dry weight and of grain set. The latter was close to 100% in bread wheats. Studies then clearly pointed to the semidwarf wheats having more grains/m2 simply because they partitioned more dry weight to the spikes during the preanthesis spike growth period; no other component was changed much relative to predecessor tall varieties.

What has happened since? The model is still around. Grain yield and grains/m2 has continued to increase at the hands of the breeders and this appears to be due to further increases in dry matter partitioning to spikes, but as well, at least in some modern cultivars, more grains per unit spike weight (eg Abbate et al 1998). We now know a lot about environmental effects on RUE, but there is still not much evidence in wheat that before anthesis it has been increased by breeding (eg Calderini et al 1997; Fischer et al 1998). My colleague Pablo Abbate in Argentina has tried to prove that grains/m2 is driven more by spike N than spike dry weight (see Abbate et al 1995), but excellent work from France under a wide range of N levels seems to support the notion that dry matter investment is the key (Demotes-Meynard and Jeuffroy 1999, 2004).

But there are many unknowns as we face the challenge of seeking analytically, as would Colin Donald, the next yield increment. One unknown, that I have already mentioned is the association with stomatal conductance, which in my view still has no clear explanation. But from listening to Prof Horie two days ago I have learnt that in paddy rice in Japan there is a similar relationship between genetic yield progress and stomatal conductance pre heading, which he has been able to relate to greater photosynthesis and crop growth rate, and he believes, greater N uptake and leaf N.

Back to wheat, if we assume that yield is still sink limited during grain filling under potential conditions, then we still need more grains, or potentially bigger ones, for more yield. We could ask whether partitioning to the spikes can further increase, for example by diverting more dry matter to spikes during spike growth from growing stems and flag leaves, or from accumulating stem sugar reserves. Here is a puzzle: as yield potential has increased in winter wheat in the UK, grain numbers have risen, but so also have stem sugars at anthesis, to reach levels of several tons/ha (Foulkes et al 2002). One might expect stem sugar deposition to be competing with spike growth: 10 mg DM in the spike will grow an extra competent floret, which can produce a 40 mg grain, but 10 mg in stem sugars is always only 10 mg at best for grain filling.

What about the number of florets and hence grains per unit spike weight? Can this not be further increased, why do we need investment in awns or even large glumes, if all the radiation is intercepted by green tissue somewhere in the crop for many weeks after anthesis, as it usually is under potential conditions? We know that more grains per unit spike weight tends to be associated with grains of potentially smaller size (Fischer and HilleRislambers 1978). Besides historically there is a strong tendency for grain size to fall with yield progress, and breeders must struggle to maintain grain size as they push for higher yields. Why is it almost inevitably harder to produce high yields by selecting for larger grains?

What lessons are there from the other cereals? Maize and rice have also been intensively studied. Rice seems also to be sink limited during grain filling in most environments, and the key parameter, the % unfilled spikelets, sort of equivalent to grain setting in wheat, but related more to the survival of the newly fertilized grains than to production of viable florets as it is in wheat. Most interesting results have come from work on the latest hybrid maizes. Don Duvick in Iowa has pioneered studies on this progress (eg Duvik 2004), and Thys Tollenaar in Ontario has shown that the most modern hybrids photosynthesise more and produce much more dry matter after silking (eg Valentinuz and Tollenaar 2002). Their vital physiological processes seem much more resistant to the “little” stresses of potential conditions, like cold nights, and windy and cloudy days, and both Duvick and Tollenaar point out that most modern hybrids are clearly more stress resistant in general. Stalk sugar levels are higher and this is one reason for the important improvement in stalk lodging resistance in the most modern hybrids. Harvest index increases somewhat because of the increase in dry matter production during grain filling, but not because of shortened stalks or improved pre silking partitioning. This contrasts with tropical maize improvement studied by CIMMYT physiologists (my colleague Peter Goldsworthy, and later Ken Fischer and Greg Edmeades), where height has come down and early DM partitioning to the growing ear has improved (just like the wheat story). Anyhow the yield physiology of cereals remains fascinating and there is much we don’t know and we can learn from looking across species. I just mentioned lodging resistance in maize; it is a vital factor in progress in all cereals, and it is still a serious weakness, poorly researched, in modern wheats, especially autumn-sown spring wheats.

And before I finish these speculations, I must mention some results buried in the literature when crops, grown in hydroponics in the field, have reached RUE levels well above what we thought were the limits of RUE. One report is from Professor Akita working with rice at IRRI in the 1980s, and the other is again from the Ontario maize group (Miller et al 1989). What is going on here?

The crop physiology I have been speaking about may be retrospective when we look at past breeding progress, but has not been without influence on the direction and probably success of breeding for yield: that was the whole thesis behind the Donald's postulating an ideotype for higher yield. I cannot finish this section on yield improvement, without reference to the whole new predictive scientific activity, namely functional genomics. This is elegant, sophisticated and exciting science without a doubt, but even if it does operate at the level of the very genes determining yield, it is no less empirical than the crop yield physiology that I have just described. This is because functional genomics is revealing more complexity than anticipated: that there are many genes involved, hundreds or thousands perhaps, and there are complex regulatory processes controlling gene activity, such that I believe it will be a very long time before a way forward can be predicted and engineered. This point was made not so long ago by molecular biologists Morandini and Salamini (2003). And we should not be deceived by the advances in medical science which are largely attempts to overcome simple genetic weaknesses in humans; breeders and nature have already weeded out the deleterious single genes from our crop plants. I also draw your attention to a couple of recent papers in Plant Physiology. Minorsky (2003) believes computer simulation at the level of the molecular processes will point the way forward, an especially courageous form of *in silico* breeding. Hammer et al (2004) vigorously challenge this view as unrealistic, and propose simulation at a higher level of organization, utilizing key genetically-controlled physiological processes . I am not sure that Hammer et al are not also wildly optimistic. We must remember we are dealing with highly refined crop plants, refined by nature and breeder alike. I salute the success of GM crops to date, but the engineered traits are relatively simple. Indeed I believe that functional genomics is unlikely to deliver practical results in terms of yield potential advance for a long time and then only if it is associated with much more investment in plant research at a higher organizational level, namely plant and crop physiology. I think there is a danger that resources, and along with it the brightest minds, are being sucked into functional genomics at the cost of physiology and conventional breeding and genetics. At the least we should be very cautious about the claims of functional genomics, as was Donald with the exciting and fashionable areas of plant biochemistry and physiology, when he proposed his essentially morphological ideotype nearly 40 years ago.

Beyond yield potential in wheat to performance under drought

Of course my earliest research scientific interests was in wheat performance under water limited conditions (eg Fischer and Kohn 1966), probably as a result of upbringing on a wheat farm in the Riverina where it never seemed to rain enough and still doesn't. Under the benign directorship of Albert Pugsley, another Waite Institute man, and guided by Peter Kohn and others, including Rex Oram, Alan Taylor and Ray Storrier, I had great fun investigating wheat yield physiology and haying off at Wagga in the early 1960s. And as I write this, I recall that my first encounter with Professor Donald occurred then, at Wagga in the early 1960s. As part of the 10th Anniversary of the Agricultural Research Institute, in 1961 Albert Pugsley invited Colin to give a public lecture at the Wagga Civic Theatre, and later to conduct a "farm walk"; you see the ARI then led Australia in many innovations, and hopefully still does.

Later, as a post doc I was back at wheat drought research in the Canberra phytotron, and then again in Mexico, where I convinced myself and hopefully some others that the new semidwarf wheats had plenty of ability to outperform under water limited conditions. Some of the work was done with my old friend and colleague Douglas Laing. I recall a much quoted paper, where we showed that the short wheats beat the old tall ones at all water-limited yield levels above as little as 1.5 t/ha (Laing and Fischer 1977). This was controversial stuff then, and still leads to discussions with Salvatore Ceccarelli of ICARDA, but the semidwarfs went on to occupy not only the irrigated wheat lands of the developing world but most of the rainfed areas as well. Soon after I got embroiled in a debate with the English wheat physiologists on the importance of pre-anthesis reserves to grain filling under drought. It stemmed from Fran Bidinger's thesis work at CIMMYT and was a lot of fun. I got a paper in Nature (Bidinger et al 1977), one that Antonio Hall and later Anthony van Heerwaarden showed to be only partly right, leaving this field open for more research.

Finally back in Australia, I wrote a couple of reviews on the subject of water limited plant production, including one with Neil Turner (Fischer and Turner 1978), another Donald Ph D student, in which we got a few things wrong and some right as it turned out, but probably the most significant was the notion that crops could grow very efficiently water-wise in winter rainfall environments because transpiration efficiency was high (due to low vpd) and because transpiration substituted for soil evaporation such that greater leaf area barely increased evapotranspiration. This was followed by the famous little speculative paper in the Journal of the Australian Institute of Agricultural Science (Fischer 1979), but in the meantime along came a couple of captivating review papers by John Passiuora where all was reduced to a simple equation with three independent variables (Passioura 1977). This was followed by Reg French and Jeff Schultz (French and Schultz 1984) with their simple notion of plotting yield against growing season rainfall, or better still crop ET, Cornish and Murray's interpretation along these lines for the Wagga district (Cornish and Murray 1989), and finally Farquahar, Condon and Richards with delta, and a host of others with plausible simulation models. All this pretty much squeezed me out of the field of drought resistance strategies, which still remains so vital for Australian cropping and many other parts of the world.

I moved back to agronomy for a while, working on direct drilling in southern NSW for almost a decade in the 1980s, when there was great excitement in this field, but as so often happens, our greatest impact probably came through serendipity, when with my colleague John Angus, we saw just how much more wheat yield we could squeeze out of most situations, whether direct drilled or not, with tactical supplemental nitrogen (eg Angus and Fischer 1991).

At about the same time I joined the SIRAGCROP project in the MIA as it rose to fame. Computer-aided decision support was the new tool, and with this and some new farmer participatory approaches, SIRAGCROP was going to lift the productivity of irrigated winter cropping. It was probably premature, but it helped keep CSIRO Griffith on the map in the hiatus period between the end of classical irrigation research and the beginning of work targeting environmental aspects of irrigation. Besides with colleagues like Maarten Stapper we did some good irrigated wheat crop physiology and white peg field agronomy (eg Stapper and Fischer 1990; Fischer 1993), some of which later found its application in the developing world.

This came about because I left full time research in Australia in the 1988 and returned to CIMMYT in Mexico, and took with me an enthusiasm for reduced tillage and better N management in irrigated wheat. But that is another story. Overall, second time around it was tougher in CIMMYT, as too much success had caught up with the place, donors had been seduced by ill informed northern fads, and budgets faltered. But as Wheat Program Director, it was a wonderful opportunity to get to know the developing world and its problems, which had little to do with the northern perceptions. Highlights were no longer papers in good Journals, but new funding from some recalcitrant donor, or travel to developing wheat producing nations and seeing more evidence of the penetration of CIMMYT germplasm or technologies into National Programs, witnessing the impact of this work in such disparate places as the Punjab, Bangladesh, the Nile Valley, the highlands of central Africa or Paraguay and southern Brazil, especially the impact on the farm economy which was often huge.

The experience with CIMMYT was something which stood me in good stead when I moved on to being a program manager for ACIAR, a glorified tourist as someone unkindly called me. There I got to know Bob Clements and a bunch of other scientists committed to the ideal of international agricultural research. There were exciting development trips to North Korea and to many parts of India and China including Tibet; only the project in Afghanistan remains to be checked out. However I trust I have moved beyond notching up countries in my passport and frequent flyer points with Qantas, to being a committed and credible advocate for international agricultural development. A lot of poor people benefit from this application of agricultural science, and agronomists from Australia have a knack of doing this well. Besides its very good for Australia….read my old professor, Derek Tribe, on this if you have any doubts!

The family farm

Through all these years, from when I left the farm to go to boarding school in the early 1950s, then to study agricultural science in Melbourne, until this very day, there has been the thread of continuity provided by my involvement in the family farm at Boree Creek. This is where I grew up and where I was, just the other day, checking crops. An oasis of calm, replete with memories which go way back, a real world challenge to my scientific training, a self indulgent distraction from my professional responsibilities, who knows, but it still has that magnetism, despite drought and fickle prices. And one learns a great deal, from close observation, from trying different things, and from keeping good records. The new technologies started with sub clover, then came 24D-based herbicides, banishing the skeleton weed forever, then short wheats, grass herbicides, narrow leaf lupins, knock-down herbicides and reduced tillage, canola, lucerne, bag N, soil testing, and lime. Pulses remain a challenge for the future, as do full conservation tillage and controlled traffic. Progress has also been made with the Merino sheep enterprise, operating in a 50:50 crop pasture rotation, and it has rescued net farm income in the recent droughts. Fortunately dryland salinity appears to not be a problem, and I have no doubt that the soil is in better shape that it was when my father purchased the farm midst the dust and scalded paddocks of the mid 1940s.

And to show that all the record taking was useful let me indulge a little more here.

Rainfed cropping is a dodgy business in Australia, as seen in the farm wheat yields (Figure 1).

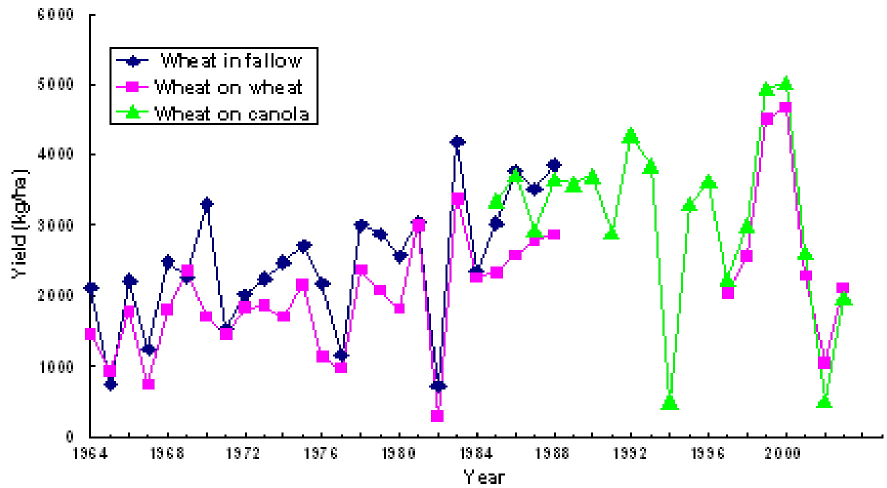

Figure 1. Progress in wheat yields at Boree Creek, NSW.

But ignoring the droughts of 1982, 1994, and 2002, and allowing for ET and calculating water use efficiency (WUE = Yield/(ET – 100)), we can see that there has actually been some good progress. The slope is 0.27 kg/ha/mm per year (R2 = 0.542), or currently about 1.5% pa.

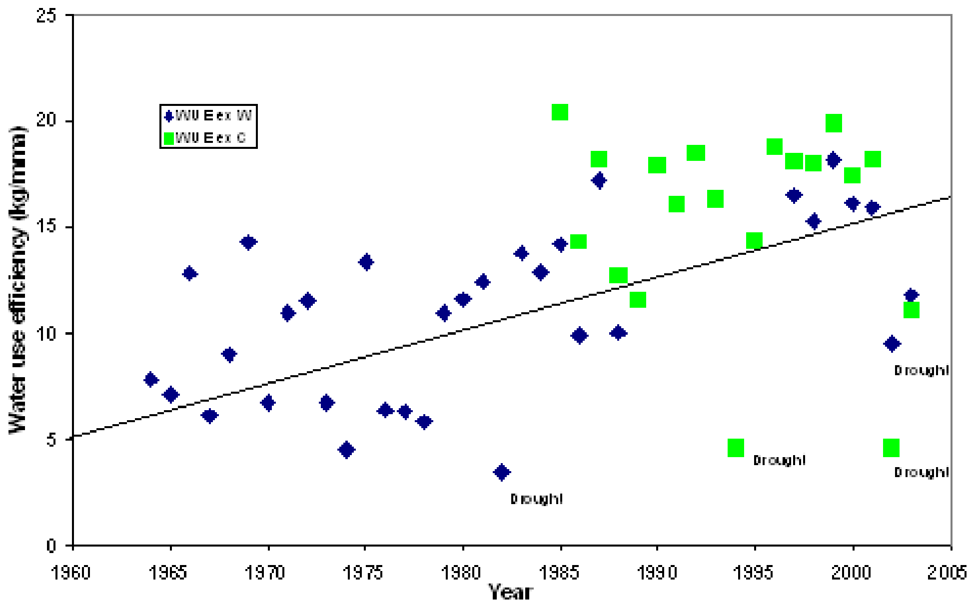

Figure 2. Progress in wheat water use efficiency for wheat after wheat and wheat after canola at Boree Creek, NSW.

The factors which could be involved in this progress are summarized in the Table 1. Cropping intensity has changed little, but higher yielding semidwarf wheat varieties have taken over, biocides have replaced cultivations, canola now precedes all wheat crops and although wheat is now again following wheat, this wheat is always after canola. Finally fertilizer use (N and P) has steadily increased, and not shown, lime every 8-10 years, at the end of each pasture phase, is a regular feature.

Crop	Attribute	Period for which attribute averaged				
		1964-1974	1975-1984	1985-1988	1989- 1995	1996- 2003
Farm	May-Oct rain mm	244	239	293	270	235
Farm	Cropping intensity %	45	44	41	48	49
All wheat crops	% semidwarf vars	1	89	100	100	100
Ditto	No.of cultivations/crop	3.5	5.4	1.9	1.3	1.5
Ditto	No. of biocides/crop	0.3	0.9	1.5	1.9	2.9

Ditto	Fertilizer N kg/ha	0	0	3	12	38
Ditto	Fertilizer P kg/ha	8.5	10	11	17	21
Wheat yield on long fallow kg/ha		2048	2468	3534	--	--
Wheat yield after wheat kg/ha		1597	1936	2635	--	2800
Wheat yield after canola kg/ha		--	--	3408	3157	2983
Wheat after wheat	WUE kg/ha/mm	8.9	9.7	12.8	--	14.8
Wheat after canola	WUE kg/ha/mm	--	--	16.4	14.2	15.8

Table1. Evolution in time of wheat crop management and effect on yield and water use efficiency. Boree Creek, NSW.

More careful records latterly reveal a remarkable fit to a French and Schultz-type relationship between yield and ET. But with the clear exception of 2003, a year in which yields were down relative to ET and grain was very shrivelled. But was the explanation a cut off in grain filling moisture? I don't think so. In fact I don't think we have a good explanation at all as to why grain shrivelling was so widespread, and yield so disappointing, in 2003.

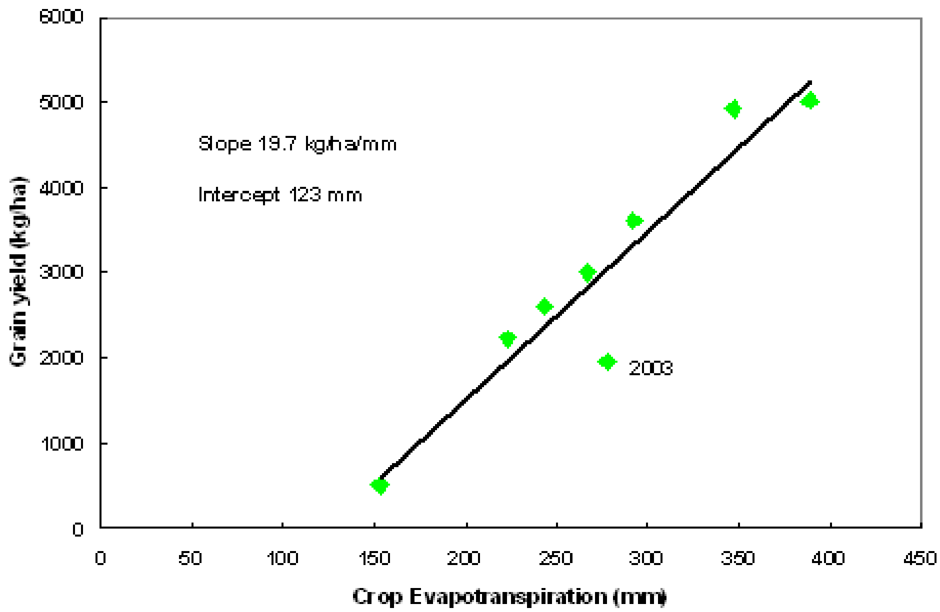

Figure 3 Grain yield versus crop evapotranspiration, Boree Creek,1996-2003.

Concluding reflections on a mottley career.

I will make three points in conclusion. The first stems from reflections on the home farm data that I just presented. Much of it appears to make sense and fits with the conventional wisdoms about crop yield progress. But at closer range, and as we saw with the outlier year 2003, when grain shrivelling was severe, there is a lot we don't understand. What's more, most of our conclusions are quite equivocal. The latter arises partly because there are so many variables influencing crop yield, and many varying together, that it is difficult to draw clear conclusions; essentially there are no control treatments. Still it has become fashionable especially amongst farming system scientists and agricultural economists to try to overcome this by surveying many farms, collecting panel data with large sample sizes, and then subjecting them to multivariate analysis, drawing conclusions on causality based on partial regression coefficients that are often only significant at the 10 or 20 % level of confidence, and often in my way of thinking, inextricably linked to covariables. My point is that this approach has its limitations and there remains a continuing need for old fashioned white peg agronomy with replicates and controls and carefully managed plots and treatments, and above all careful measurements of the standard variables, such as weather, soil water and nitrogen, crop growth and yield components. Many examples of such agronomic work are to be found at this Congress. It remains an essential backup to the paddock surveys and simulation modelling.

The situation is similar with breeding with its inevitable reliance on multilocation multiyear yield trials; in this case the high powered statistics are essential and seem more credible. Breeding can progress with little understanding of key physiological mechanisms, although genotype by year interaction poses serious constraint on progress. And we have made only limited advances in understanding these widespread genotypic or agronomic interactions, because I believe the biology is complex, we usually lack the extensive and careful measurements to properly characterize the sites, and besides, our models remain coarse approximations in the face of 5 and 10% yield differences and interactions that we seek to exploit. So this is a plea for more careful field experiments and better measurements, including attention to Henry Nix's minimum data set.

The limitations of our simulation models and understanding that operates at the crop level of organization, pale into insignificance when one contemplates the complexity being revealed by functional genomics and the challenges that this poses for prediction of interventions for enhancing performance, as I touched upon earlier. My concern is heightened by the relatively high level of investment that Australia, and developed countries in general, are making into functional plant genomics, at a time when research resources are scarcer than before. Since we are not talking about new money, other types of crop research suffer. Functional genomics is clearly a field has reached the status of "fashionable band wagon", and starting to look like other bandwagons I have known. I paraphrase Norman Simmonds who listed several during his long career, in chronological order they were induced polyploidy, induced mutations, crop physiology (that one hurt), the protein gap (in the developing world), farming systems research (why that, read his paper), and finally biotechnology (Simmonds 1991). Curiously, he missed simulation modelling! About biotechnology, and referring to molecular biology, he said that "quantitative genetics was just too complex for chemists". He was wrong in predicting little early impact in farmers' fields of genetic engineering, but right I believe, in condemning the hype and diversion of resources, and warning of the complexity. As I said earlier the complexity, while dazzling in its beauty, is looking like a serious barrier to purposeful engineering of things like yield. It is to be hoped that the whole is simpler than the sum of its molecular parts, a principle I have always found useful, and in this case implying that there are bottlenecks in performance which are much easier to seek at the plant or crop physiological level of organization, and may be easier to overcome at that level also. I admit that I might have been in a similar position 30 years ago as a physiologist, predicting routes to yield improvement, but since then and despite some successes from predictive physiology, experience has led to a healthy scepticism about easy solutions in biology and agriculture.

However I don't want to finish on a sour note. I rejoice at the progress of useful GM crops, and at the stunning complexity of plant life being revealed by functional genomics. It's just a question of balance: we shouldn't forget the soil, the weeds, the animals, the environment, and last but not least, the farming communities that we serve. There is still a lot we can do in these traditional research areas and that needs a fair leavening of broadly trained agricultural scientists seeing the whole picture but able to exploit the modern tools. That is where I have tried to be, where Colin Donald was, and where there is plenty of need and room for those of you less inclined to be specialists and reductionists, especially of the molecular kind.

Following this notion further, my third concluding remark relates to my career, and to careers in general in agricultural science. As I think about my career, I can see great fortune in the face of many changes. I have been able to move back and forth from strategic to applied research in crops, principally wheat, to keep a sort of balance that is also an importance balance for a national crops research portfolio. I had the farm as a foundation, where a 6 t/ha wheat yield in 2000 was a great thrill, but I actually got an equal sense of achievement out of publishing across a wide range of fields, even if in retrospect many of the papers were too turgid and read by only a few. Nor in this age of emphasis on output and impact, do I resile from admitting excitement and enjoyment in the small amount of basic and considerable amount of strategic plant science that I did: it will have its impact one day! As for the applied science of which I did some, and the on-farm participatory work of which I did little, I recognize that it is equally important agricultural science, even if an unfavoured field amongst our brightest science students, but one suited to people-oriented scientists, like many of our agricultural economists, and one that can bring plenty of professional satisfaction. Because of my farm background I am comfortable amongst farmers anywhere, but not all of us can be Peter Carberry's "change agents". Australia may be neglecting this end of agricultural science, but as farm yield gaps indicate, the neglect is currently much greater in developing countries. Many have pointed to how to well Australian agricultural scientists, maybe because of their breadth of training and experience, have been able to perform in developing country agricultural development, so Peter shouldn't worry too much.

Acknowledgements

I gives me much pleasure to formally acknowledge my wife Miriam whom I met in those exciting days in California and who has supported me strongly all these years since. I would like also to salute the other recent "retiree" agronomists, and apologise if I have overlooked other worthy recent one: these days it's hard to know when people have retired. There's Howard Rawson, Neil Turner and John Hamblin, Ph D students of Colin Donald, then there's Tim Reeves and John Lovett, Ann Hamblin, and some locals, John Passioura and Bob Clements. I think they have all formally left full time employment lately, and I am sure that they each have contributed massively to agronomy in Australia. Let's salute them as well.

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