Interview

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CIDEU: How long have you been breeding Eucalyptus?, what are the major changes you have seen and what do you expect will be major changes in the future?

R.Griffin: I began research on eucalypt genetics around 1975 with CSIRO in Australia and moved on to more practical breeding activities with Shell in 1991. I have been advising industrial forestry companies on their tree improvement programs since then. In 2004 I formed my own company GTI Pty. Ltd. which is based in Hobart, Australia and services clients in Asia, South America and Africa. With new Joint Ventures in South America we can now supply improved material of any of the main commercial tropical, sub-tropical and temperate *Eucalyptus* species and their hybrids.

Although eucalypts have been planted around the world for over 200 years it was really the 1970s/80s which marked the major domestication phase of eucalypt breeding. A stream of scientists from South America and elsewhere travelled through Australia making provenance collections and the Australian Tree Seed Centre provided tonnes of seed of the main commercial species and research scale provenance collections of many others. As a result the range wide variation of many species is now known in many environments and each country has focused in on the best provenances as a basis for breeding. The development of cloning technology in Congo in the late 70s, further developed by Aracruz, opened the way for large scale use of hybrid varieties. Increases in computing power in the 1990s and familiarity with BLUP has meant that large unbalanced, multi-generational data sets can now be used to predict breeding values and complex rolling front breeding strategies possible.

My views on various changes in the future can be seen in the answers to other questions below.

CIDEU: How do you see the role of genetic data in understanding how *Eucalyptus* work?

R.Griffin: Not sure I fully understand this one. Studies of population structure have been useful as a background to defining breeding strategy. If there is a large provenance level variation as in *E.nitens* then focusing breeding within the best provenances makes sense. In others like *E.dunnii* where most variation is within populations, multi-provenance breeding populations are indicated.

Progeny trials which allow estimation of heritabilities and genetic correlations allow us to predict gains from selection and indicate problems which need to be overcome such as the negative genetic correlation which exists between density and growth rate in many species. Trials which allow variance to be partitioned into additive and non-additive components also guide deployment strategies. For example *E.globulus* seems to exhibit quite a high proportion of non-additive for growth traits so it is worth identifying and deploying OSP families with high specific combining ability. If cloning is practicable then this is also a good way of utilizing the various forms of non-additive.

CIDEU: What in your opinion are the 'big' questions in *Eucalyptus* breeding that remain unanswered?

R.Griffin: That's a big question itself! Let me muse a little:

By nature and training I am an experimental scientist rather than a modeller i.e. I am more comfortable if I can demonstrate an effect rather than just predict it. It worries me that with all the increase in analytical sophistication, the tree breeding literature does not include more papers reporting that "we made these selections using BLUP methods; we predicted that gains would be xyz; here is a trial result verifying our prediction". I have even been told by some analysts that "we don't need to plant Yield Trials as we know what the outcome will be!" I don't believe that the assumptions underlying either quantitative genetic or statistical theory are so close to the real world that we can just accept this.

What if we are disappointed with the outcome of such experiments? There is obviously still huge potential for genetic improvement (we are 1 generation away from the wild, maize is thousands of generations and still improving), but it may prompt a radical change in our breeding strategies. For example, once we are through the first 2 generations and have selected a relatively productive, genetically diverse, elite population, it may be time to radically reduce the size of our Breeding Population and explore and exploit its genetic architecture much more closely. Only backward selections and tested clones would be creamed off for the deployment population rather than assuming that realized gain per unit time will be increased if we continue to use forwards selections in orchards. We probably worry too much about inbreeding….rather than diluting the problem through using large populations we should manage it. We now have molecular techniques to help us control pedigrees and we also have an opportunity to purge deleterious recessives as part of our breeding program.

I would issue the same "demonstrate" challenge to molecular geneticists. We are told that genes controlling important economic traits such as lignin content are identifiable. Who is planting progeny trials with high and low lignin selections and phenotyping to verify that the selection has been successful. Lab scientists tend to want to move on the next interesting molecular experiment, rather than doing such a mundane experiment, so breeders need to put up their hands to do this…and explain that there is in the interest of the molecular scientist to do so. If such trials work out people like me would rapidly be recommending that industry clients should increase their investment in the technology.

In hybrid breeding we are still using a "suck it and see" approach. We really don't have a decent theoretical base for choosing the parents to use as a basis for a new variety. Also we don't yet know whether we should be looking to produce an ongoing stream of improved F1 hybrids (by analogy with those

crop breeders working with highly homozygous inbred lines); or finding levels of backcross combination which are optimal for our environments or breeding objectives; or simply developing stable advanced generation introgressed populations from which to select clones (many natural eucalypt species have clearly introgressed with other in the past).

We have made the big first steps in eucalypt domestication …utilization of provenance variation and release from the neighbourhood inbreeding effects in natural populations. However there is still plenty of useful theoretical and experimental research to be done in defining the most effective ways to move forward in the future. I say "ways" advisedly….there will be no one "best way". Strategy planning always requires consideration of the species' biology (especially reproductive and propagability traits); the breeding objectives of the client; and the resources which can be spent on the program.

CIDEU: What are the potential risks associated with the 'Eucalyptus gall wasp'? For which species and in which parts of the world?

R.Griffin: I really don't know much about this but it is one of a long line of Australian pests which have followed the exotic eucalypt plantations around the world. It is a reasonable speculation that the indeterminant growth habit of *Eucalyptus* evolved as a response to selection pressures from phytophagous insects, giving it great capacity to regenerate new shoots/foliage. In Australia the populations usually arrive at some equilibrium balance of parasites/predators. I imagine that if the economic impact of the gall wasp is sustained then it will be possible to use such a biological control. I believe introductions of parasites have occurred in Israel and perhaps elsewhere.

CIDEU: Can you talk about how the field of genomics may change the way tree improvement is being conducted?

R.Griffin: Good question. I am sure that the genome sequencing project will lead to an enormous amount of work on evolutionary relationships within the genus, in developmental genetics, and in understanding genetic architecture, but applications to breeding are still very unclear. There continues to be a major disconnect between knowledge of variation at the molecular level and ability to use that information in a practical breeding program. I don't see that changing until a) costs of genotyping are radically reduced and b) the scientists demonstrate that molecular selection is in fact as reliable as a direct progeny trial in selecting for production traits. As I understand it, the main use of these tools in plant breeding relate to the identification and manipulation of traits under simple genetic control, and within genomes which are far less heterozygous than our trees..so we should not assume it will be easy. Possibly the first practical uses with eucalypts will be in backcross hybrid breeding where we want to ensure that particular genes from the non-recurrent parent are present in the progeny set which we advance to field trialling. Heritable traits which are difficult to assay (e.g. cold tolerance) or not expressed until a later age (e.g. wood density) would be obvious candidates for such an approach.

I think it is important to support molecular genetic research because I believe that greater scientific knowledge will inevitably lead to improved ability to breed. However at the moment it is more appropriate that governments/ universities make this investment than industry organizations. I would be delighted if funding agencies could find a way of rewarding scientists who are willing to actively engage with breeders.

CIDEU: What are some of the 'new tools' that should be developed to help the field of tree improvement move forward?

R.Griffin: Accurate phenotyping is a big challenge in tree improvement . Trees are large, long lived organisms and difficult/expensive to grow under controlled environment conditions so we our ability to measure/predict how a particular genotype will perform in the variable environmental conditions in the plantation is rather poor. Several approaches could help:

cheaper assays which allow more observations in more environments. The advances in application of NIR to assessment of

wood/pulp properties is an example. New portable models which can be taken to the field increase the range of applications.improved knowledge of age:age correlations leading to better indirect selection at an early age.
I have already commented on the potential for application of molecular tools

CIDEU: How that GM Eucs fit into your operation mix-ideal?

R.Griffin: It is one thing to demonstrate technical benefits from GM and another to obtain the regulatory approvals for large scale use! Also the forestry market is relatively small and fragmented so we are faced with the problem of who will pay for the necessary development programs. This is the reason that companies like Monsanto now focus their effort on a small number of crops maize/soy etc. where the market for seed is huge. To illustrate the problem with an application for the pulp industry: A large plantation of lignin modified eucalypts could certainly be of great value, but primarily because a lower capital investment would be required to build a mill with a specified tonnage output of pulp. This could not happen without a guarantee of such feedstock. Who is going to make the 20+ year investment to develop the variety, test, get approvals, plant on at least a 30,000 ha scale?

GM traits which give a cost reduction/productivity increase in the plantation are easier to justify. For example herbicide resistance would reduce costs of chemical weeding and probably reduce the total amount of chemical used. This is proven technology so the impediment is largely regulatory. FSC attitude would be crucially important to the multi-national companies who are the only realistic investors in the technology.
Fortunately eucalypt breeders have a huge pool of natural genetic diversity yet to be tapped. We therefore do not NEED GM products to make progress. My conclusion is again that this is a biotechnology which should be encouraged as an academic research field for the future. It has no practical role in the time frames on which I am planning my breeding programs

CIDEU: What do you think is the potential of *Eucalyptus* for producing biomass, bioenergy?

R.Griffin: Eucalypts are pretty much ideal for biomass production. Biomass will never be a high value crop so, unless it is a by-product of a cropping system such as sugar, it needs to be grown on poor (=low cost) land. For dry and low nutrient sites it is unlikely that a faster growing crop will be found. Because energy crops can be grown on a short rotation growers can even take more risk with frost damage. In the UK energy crops of *E.nitens* are now a real possibility even though we know that every 20 years or so there will be damaging cold episodes.

The selection criteria will be substantially different from pulp or solid wood crops, so a wider range of species come into consideration. Volume is still critical as is wood density. There is possibly some useful variation in calorific value of the wood. Coppicing ability may be important and cost structure make it more likely that seedling propagated varieties will be favoured at least at the outset. The only exception to this might be for very easy rooting species such as *E.camaldulensis*.

Because there are so many fibre and solid wood uses for Eucalypts it is not essential to produce crops solely for biomass…the value yield per hectare might be better if innovative silvicultural systems are developed. For example we might visualize an over-story of solid wood trees harvested on a 15 year rotation and an understory of coppiced trees harvested on a 5 year rotation. If a 100% energy crop is desired there is still a major decision needed re silviculture/ harvesting system. When we went through this exercise for the Shell Renewables business we came to the conclusion that where were cost and sustainability arguments for managing as a conventional short rotation pulpwood crop rather than changing to the “sugar cane harvesting” approach favoured by willow and poplar energy growers. Plenty of scope for creativity!

CIDEU: In terms of wood properties, what do you think are the major challenges in *Eucalyptus* breeding?

R.Griffin: In both the pulp and solid wood industries it is easy to estimate the economic impact of variation in traits such as density, pulp yield, stiffness, splitting. The extent to which we can/should be attempting to change these characteristics by breeding rather than silviculture and/or mill technology, is less clear. We also be mindful of the fact that we are breeding for wood which will be harvested decades into the future and that technology and market demand can change rapidly. The economic weights on a wood trait can change overnight through simple changes of business structure. To take a common example, in a vertically integrated company growing wood for its own pulpmill, it is clearly worth increasing density and even fine tuning the pulping/paper making properties of operational clones. If the mill is sold off then the breeding program, paid for by the forestry company, is faced with the challenge of capturing the value of the wood improvement in a rather unsophisticated market. In *E.globulus* in Chile, where there is a negative correlation between density and volume, trading off volume gain for density may no longer be a good strategy. So, a simple question is not so simple after all!

Assuming we want to include wood traits in the breeding objective then we need to know a lot more about the genetic parameters than is currently available. We also need to be able to partition the genetic / site /age effects in order to put the genetic contributions into better perspective. As a crude generalization wood traits seem to have relatively high heritability but do not show a lot of variation within the sub-set of acceptably fast growing populations. Applying a significant selection pressure is therefore a challenge. Using hybrids with related species may offer the best prospect of rapid progress.In most of my breeding programs I consider that Volume and Density are the major selection criteria in the breeding populations, with consideration of Pulp Yield % in selecting for the deployment population. In species/varieties which can be cloned then it is worth screening for a wider range of pulp/fibre properties before making the final operational selections.

Of other possible traits, fibre length is of huge importance to the papermaker, but there is little indication that worthwhile intra-specific variation exists. Vessel size and frequency can also be a problem for papermakers and would warrant further genetic study

CIDEU: Do you think that tree breeding is socially (environmentally) more defendable when it deals with exotic species, due to the lower risk of introgression with the native, unselected populations?

R.Griffin: When a species is grown as an exotic germplasm has to be imported from somewhere, and it is a very easy decision to invest in a breeding program. The experience in working with native species in North America, Europe and Australia (and no doubt Chile) is that the locally evolved populations are not usually the best in terms of plantation productivity. This is not surprising as they will have been through past stress bottlenecks which would have placed more value on survival than growth traits. So, if you want to make profitable plantations of native species there is a big incentive to breed.

I suggest the introgression is a separate management issue. At the philosophical level populations are not genetically static entities – they introgress naturally with other species and if we accept climate change projections gene frequencies will change rapidly as an adaptive response. It is not clear that genes from a tree crop are necessarily “bad”..they might make all the difference! However in the interests of biodiversity I think it is reasonable to do what we can to limit the extent to which introgression occurs. A number of simple practical steps can be taken, beginning with risk analysis and involving buffer zones with significant endemic populations. In Tasmania the Forest Practices Plan prepared for each new plantation proposal requires genetic risk to endemic eucalypt populations to be considered.

Ethically I don’t think there is a problem with improving indigenous species. Where would the world be if, over the centuries, farmers had not selected to improve the productivity of their wheat, maize, lentils etc.? Tree crops are not substantively different.

CIDEU: What is the 'improvement roof' for eucalypts? What is the theoretical limit for improvement?

R.Griffin: This is really a question for physiologists not breeders.."what biomass is a species capable of producing if water/light/nutrients/temperature are not limiting?" . I think it is more meaningful to ask about a particular species in a particular environment,considering the equation Phenotype (P) = Genotype (G) + Environment (E), not genotype alone. We know for example that even relatively unimproved *E.nitens* can grow at 55MAI + over a rotation on good trumao sites…can we substantially improve on that? Probably not, but we might be able to do a lot through a combination of improved silviculture and selection to extend the range of sites on which that yield is possible.

There has been discussion about whether we can improve merchantable volume by modifying the harvest index …the allocation of biomass to roots/stems/branches. Unlike grain crops where the seed biomass is small relative to total, we are already harvesting a large % so maybe less chance for progress. If we can improve stress tolerance then we can certainly increase yield very substantially in some environments (from a zero base if unimproved all die from frost/drought etc.

Once we have a well adapted variety of good form and therefore high merchantable volume I think breeders will rapidly switch to improving wood properties and stress / pest/ disease tolerances. The limits to which the value of wood as a raw material can be improved by breeding has barely been considered.

CIDEU: How do you evaluate the hybrid development program in Shell in Chile and Uruguay?

R.Griffin: Well they worked I guess! Forestal in Uruguay has shown the way in Uruguay/Brazilian forestry with operational clones of grandis x globulus and now every company in Brazil wants globulus genes. In Chile the main target variety *E,nitens x globulus* is being planted on an increasing scale by CMPC. The key lesson is that a long term commitment is required in the face of occasional skepticism from management. At least 10 years from making crosses to the first operational deployment of tested clones. You need to do it on a serious scale or not at all if you want to succeed. One of the lessons is that, although F1 hybrid traits are generally intermediate, it is hard to predict what will be a successful variety. For example it would not be predicted that 2 low density species with very different adaptive characteristics like *E.nitens x grandis* would be a successful pulp variety, yet MONDI and other South African companies are very happy with this. Let your breeder use his/her imagination at the beginning of a hybrid breeding program!

CIDEU: If hybrids are not planted operationally, what do you think is the reason? (I didn't write this one, so need to corrrect that…).

R.Griffin: Hybrids are planted widely in many parts of the world (Brazil, Congo. South Africa). Where they are not, it is more a matter of propagability of the variety and also the cost of propagation, since it is difficult to bring the cost of a cutting down to much less than 2x a seedling. Each company has to make a judgement as to whether these extra costs will be compensated by the increased yield of their plantations. Large companies are likely to invest in hybrid breeding and to keep the benefits in-house, so in many countries including Chile there are no hybrid clones available from commercial nurseries for purchase by smaller growers. Perhaps someone will identify this as a market opportunity in future.

CIDEU: What in your opinion is the key to improve operational production plantations with hybrids and what is their advantage cf. pure species?

R.Griffin: Hybrids have the capacity to produce combinations of adaptative and production traits which are not genetically possible within a species. For example the hybrid Shell developed in Uruguay combining the wood properties of *E.globulus* with the ability of *E,grandis* to grow well in that environment, or the hybrid which CMPC is now planting combining the growth and frost tolerance of *E.nitens* with the wood/pulp properties nearer to *E.globulus.*

They are also useful in making a more rapid step change in an important trait which shows rather little genetic variation within a pure species. The improvement of density of *E.nitens* via hybridization with *E.globulus* is a good example. Another is the Acacia mangium x auriculiformis hybrid which is planted extensively in SE Asia and has a density which is higher than that of the pure A.mangium.

Hybrid breeding is a long term endeavour and large populations need to be screened to find superior genotypes. There is no reason to think that F1 hybrids are the optimal combination so backcrosses may need to be made, generally but not always to the parent which provides the main production rather than adaptation traits.

A hybrid can of course only be used operationally if it can be cloned. The reason that many of the hybrids used in Brazil and South Africa include either *E.grandis* or *E.camaldulensis* is that they root very easily. In varieties like the *E,nitens x globulus* it is necessary to place a selection pressure on rooting at least in the early stages.

If you have a well adapted pure species for your environment I would make every effort to improve that before embarking on the hybrid route. In fact a serious breeding program for at least 1 of the parental species is pretty much a prerequisite.