# Standing Committee on Science and Innovation, Inquiry into the coordination of the science to combat the nation's salinity problem

Submission by David Pannell

## Credentials

- Associate Professor, School of Agricultural and Resource Economics, University of Western Australia;
- Chair of Research Evaluation Committee and Leader of People, Land and Water Program, CRC for Plant-Based Management of Dryland Salinity;
- Director on the Board of Land and Water Australia;
- Member, Salinity R&D Technical Committee, NRM Council of WA;
- Member, Salinity Investment Framework Steering Committee, NRM Council of WA;
- Member, Ministerial Task Force into Salinity Policy and Management in WA, 2001;
- President, Australian Agricultural and Resource Economics Society, 2000;

(Note: this submission is made independently, not on behalf of any of the above organisations.)

I have personal research experience on many aspects of dryland salinity, including:

- Statistical analysis of groundwater data to quantify long-term trends.
- The economics of perennial options for salinity management at the farm level.
- Regional scale economics of salinity management.
- Economic benefits of salinity prevention.
- Market-based economic policy instruments for salinity management.
- Economics of desalination technologies.
- Farmer adoption of salinity management practices.
- Farmer attitudes to and perceptions of salinity and its management.
- Explanation and measurement of farmer monitoring of groundwater.
- Synthesis of economic, hydrological, social, environmental and political aspects of salinity to assess government policies for salinity.

I have also overseen the development and evaluation of all research projects of the CRC for Plant-Based Management of Dryland Salinity in my capacity as chair of its Research Evaluation Committee. These projects include a full range of salinity related science topics encompassing plant science, farming systems, soil science, hydrology, social science, economics, animal science, ecology, remote sensing, spatial analysis and modelling.

## The roles for science in addressing dryland salinity

There are three distinct roles for science in addressing dryland salinity in Australia.

1. Information to aid selection and prioritisation of interventions (e.g. by farmers, by catchment management bodies and by governments)
2. Development of new salinity management methods and technologies.
3. Information to aid the design of the major government programs

I note that the information paper for the review recognises the first two roles. I suggest that the third role for science is also important in ensuring that public funds are invested well.

The conclusions reported here draw on a number of publications in refereed research journals, particularly: Pannell (2001b). A version of this paper is available at http://www.general.uwa.edu.au/u/dpannell/dpap0101.htm

Underlying my submission is a synthesis of findings from hydrological, economic, social and agricultural sciences. The key findings and implications of this synthesis are as follows:

Hydrologists now believe that the extent of perennial vegetation needed to prevent groundwater rise on a catchment scale is very much greater that the sorts of levels discussed in the 1980s and early-to-mid 1990s. Clever placement of small areas of perennials is now seen to be rarely a realistic option. To fully manage watertables at most locations throughout Australia, establishment of perennials on at least 50% of the landscape (and perhaps more) is needed (e.g. National Land and Water Resources Audit, 2000; Campbell et al., 2000; George et al., 1999b; Hatton and Nulsen, 1999; Stauffacher et al., 2000). Smaller areas would have predominantly local effects. For example, George et al. (1999a) found most stands of woody perennials in WA had no measurable effects beyond 30 metres from the edges of the stands. As well as large scales being required, the off-site salinity benefits of establishing perennials can often be long delayed (by decades or even centuries in the case of salinity in waterways) (e.g. Hatton and Samala, 1999; NLWRA 2000; Bell et al. 2000). Notwithstanding differences between east and west, these conclusions about scale and long lags apply fairly generally. There can also be off-site costs from establishing perennials in some locations.

Research into farmer adoption of new land management systems highlights that large-scale adoption depends substantially on the financial attractiveness of the proposed systems (e.g. Cary and Wilkinson, 1997; Sinden and King 1986; Pannell 2001a).

Economic research has shown that the farm-level economics of currently available perennials are positive in some locations, but very rarely on a scale that would be sufficient to fully manage rising watertables (Hajkowicz and Young, 2000; Kingwell et al. 2003; Bathgate and Pannell 2002). If it was intended that farmers should be compensated for the economic losses they would incur in establishment of perennials on the scale required to fully manage salinity, the extent of public funding provided through the NAP would need to be increased by perhaps 100 fold. (This is not to imply that this would be a good use of public funds.)

Clearly, the NAP provides only enough money to effectively contain or manage salinity on a small proportion of the landscape. If we attempt to protect a larger area (meaning that the available funds are spread more thinly), the probability is that little of the money will achieve effective outcomes. In terms of terrestrial impacts (on infrastructure, biodiversity, land), the practical reality is that the NAP can only afford to protect a small number of assets of outstanding public value, and these would often be assets that are amenable to protection by localised engineering treatments, rather than by revegetation of agricultural lands (Pannell, 2001b). In the case of water resources, only a small minority of subcatchments, with particularly high salt loads and other key characteristics, would qualify for public funding support under a well designed NAP.

The consequence of this is that comprehensive establishment of perennials on a large scale will not be achieved by the NAP. If the regional NRM bodies spend their money wisely, they will not attempt it. If they do attempt it, they will fail.

These observations highlight the outstanding importance of R&D of the second type listed earlier (development of new salinity management methods and technologies). The new technologies need to be profitable at the farm level in order to be adopted on a sufficient scale. This R&D should be part of a strategy of industry development to complement the role of regional NRM bodies and ensure that more than just iconic assets are protected from salinity (Pannell, 2001b). There seems to be no prospect of adoption of perennials on anything approaching the desired scale without outstanding success from industry development efforts.

The findings reported above also mean that options for making productive use of salinised resources are of great importance, since most of the salinisation that has already occurred will not be reversed, and a significant proportion of the prospective salinity is not practically preventable (George et al., 1999b; Campbell et al., 2000).

I was a member of the Ministerial Task Force into salinity in WA in 2001. On reviewing the science and consulting widely with many different stakeholders, including the farming community, we reached conclusions consistent with the above. We concluded that the NAP was too narrow in several respects, including its serious neglect of R&D, of options for living with salinity, and of issues that should be prioritised at a scale other than the NRM regions.

## Responses to the Terms of Reference, particularly "The Commonwealth's role in managing and coordinating the application of the best science in relation to Australia's salinity programs"

The Australian Government should be providing guidelines to the NRM bodies making them aware of the scientific realities presented above, spelling out their implications for the broad types of investments that should and should not be undertaken, and enforcing the guidelines through the accreditation process for regional plans. In reality, none of these things is happening.

It seems that few of the regional NRM bodies have a realistic appreciation of the limits of their effective role in relation to salinity, and few if any will target their investment in salinity in a way that is as tightly focused as the research indicates they should. Much of their planned efforts to encourage establishment of perennials on private land, even if successful in fostering adoption of perennials by farmers, will achieve little other than highly localised effects on watertables. Achievement of larger scale adoption, necessary for off-site benefits, will depend primarily on commercial drivers rather than policy instruments or provision of information.

In developing their specific regional plans, NRM bodies are constrained from making adequate use of available science and data by the limitations of time and scientific resources that are put at their disposal. Any devolution to regional bodies of powers to plan public investments in such a complex and difficult issue as salinity would ideally be accompanied by well resourced systems to make the best science available to the NRM bodies, identify and prioritise knowledge gaps and set about filling them. In reality, the process is *ad hoc* and there is minimal coordination between regions. From my interactions with these regional bodies, it seems that it is currently easy to get away with very superficial use of science in the planning process. Given tight timelines, limited resources and limited technical expertise in many cases, such a strategy becomes very attractive to them.

For science providers to obtain funding under the NAP, it is necessary to invest considerable transaction costs in engaging with each individual regional body and endeavouring to have that science embedded in their regional NRM plans. This is highly inappropriate and inefficient and will result in very patchy application of science across regions. There are a number of key aspects of the science that would need to be coordinated and conducted on a state-wide or even national scale.

The development of new salinity management methods and technologies is perhaps the most obvious area requiring coordination at a scale above any individual region. The R&D needs in this area include:

- Development of new types of perennial plants that are profitable (new trees, shrubs, pastures, crops). A portfolio of these is needed for many different climates and soil types so that the total area of perennials is enough to make a difference to salinisation rates. Note that this category is not about salt tolerance; it is about using fresh water before it leaks from the root zone into the groundwater table.
- Development of profitable options for making productive use of salt land and salt water.
- Testing and design of engineering methods, including assessment of downstream impacts.

There will be substantial overlap between the regions in their needs for new systems and technologies. This work also needs to be conducted over a longer time scale than that in which the NAP regional bodies are operating. By constraining science to operate in this regional planning environment, we are effectively constraining the NAP investment in science to minimal levels, which is what we are seeing. As I argued earlier, the success of Australian efforts to contain and manage salinity in the long run will depend substantially on the success of efforts to develop new farming options and farming systems that are commercially competitive with existing farming systems. It seems quite inappropriate that the setting of the level of investment in R&D in this area is left to chance -- the actual level is whatever emerges out of funding sources and processes independent of the national salinity program.

Science needs to be dealt with in a much more serious and sophisticated way in the design of national salinity policy. For example, my synthesis presented earlier reveals that there is a serious imbalance between investment in short term direct interventions on private land, and the longer term indirect approach of supporting industry development. It indicates that the NAP is seriously misconceived in its neglect of R&D, and particularly of R&D in the second category.

Furthermore, there is a strong tendency for NAP funds to be spent on private land, whereas many of the more valuable investments in salinity containment would be on public land. Given the expectations that have been created in regional NRM bodies, it seems very hard to get this recognised.

Research has highlighted the importance of targeted engineering works in achieving practical salinity outcomes for some assets, but this too seems to be at odds with the expectations of most regional NRM bodies, and the managers of the NAP program have not provided information to help change these expectations.

Another aspect of policy design in need of greater science input is the allocation of NAP funds between regions. The determination of the existing allocation has been completely non-transparent, and the strangeness of some of the decisions about which regions were included or excluded make one seriously doubt that any rigorous process has been used. In addition, a rigorous science-based allocation process would result in considerable diversity in funding levels between regions, but there is no sign that this will occur in practice, or if it does it will not be on the basis of scientific analysis of needs and opportunities.

The original NAP plans included a substantial emphasis on airborne geophysics to assist with regional planning. It is now clear that this emphasis reflected a failure to understand the real factors limiting large-scale land-use change. It is not lack of such information, but lack of profitable land-use options and systems that can be widely adopted by land managers to manage groundwater recharge. Airborne geophysics has an important role to play in some situations, but its application needs to be carefully considered and targeted.

One unfortunate consequence of the failure of Commonwealth agencies to appreciate the scientific and economic realities of dryland salinity has been the merging of planning processes for the NAP and the Natural Heritage Trust (NHT). The biophysical and economic characteristics of dryland salinity mean that the policy and planning approach taken should be substantially different from that which is appropriate for the NHT. However the current system encourages regional bodies to treat the two problems similarly, and fails to highlight their important differences and the implications of these differences for appropriate investment strategies.

Finally, I note the possibility that this review may result in proposals for the Commonwealth to take a greater role in coordinating salinity science nationally. There are some significant dangers in this if it is not handled well. Relations between the Commonwealth and some states in relation to the science are already somewhat strained due to the Commonwealth's poor handling of science-related issues to date. Some of the state agencies are already investing in salinity science in a more balanced and realistic way and have been frustrated by Commonwealth resistance to proposals for better funding of science within the NAP. Among the states, confidence in the quality of thinking about salinity science in the core NRM Commonwealth Departments is at a low level. If a Commonwealth Department attempts to take a coordinating role in this environment, it may cause more problems than it solves. I suggest that if any national coordinating role is judged to be needed, then it should be managed somewhat at arms length from the Department of Agriculture, Fisheries and Forestry and the Department of Environment and Heritage. A possible vehicle for this already exists in the form of the National Dryland Salinity Program (NDSP), which is well established and well respected. It appears that the commitment of some states to the NDSP has reduced and that its continuation beyond the current financial year is in some doubt. A commitment of resources by the Commonwealth to ensure its continuation would appear to be timely and appropriate.

In summary, the science of salinity has moved on, but the new insights are not reflected in the design of the core national salinity policy, the NAP. Furthermore, the NAP is set up in a way that makes it extremely difficult for research to be adequately funded (particularly research in the category of technology development) or for scientific results to be adequately considered in regional planning. With a number of fairly obvious changes to the design of the NAP, it would be possible to substantially improve its effectiveness in achieving important outcomes for Australia.

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