SCIENTIFIC RESEARCH AND THE WORLD HERITAGE AREA: A VIEW FROM AFAR

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Throughout the period of advancement of technology and science, and associated with an increase in human population numbers, there has been an increased recognition that areas free of western interference have substantial natural values. Such areas are relics of the natural environment which existed everywhere prior to the broadscale changes which occurred with 'advancement'. Through analytical techniques such as exploration, description and classification, scientific research has identified natural values and has therefore been a substantial contributor to the conservation/reservation process. In this sense science has been used as a political tool to justify areas being set aside from intensive human use, or (more rarely) it can be used to generate the process in the first instance. In Tasmania, the identification of natural values has been important in the initial establishment of the World Heritage Area (WHA), and subsequent additions to it.

But the juggernaut rolls on: the processes of exploration, collection, description and classification continue in the WHA, and until very recently the processes have remained virtually unchecked. They continue to be successful in highlighting values, particularly those with ecological or economic manifestations. This essay briefly addresses these continuing processes: underwhat conditions are the activities of science and scientists in the World Heritage Area appropriate?

There seems to be a perception in the wider community that scientific research can be undertaken above and beyond the normal processes of land and people management. Rarely is scientific research perceived as a threatening process in its own right; it is my contention here that there is a potential for scientific research to affect natural communities and perhaps even have a profound impact on them. Scientists therefore require regulation much like any other members of the public, and their activities require scrutiny.

The most serious threat from scientific research in natural areas is the potential for the spread of diseases or as a mechanism for the translocation of other flora and fauna. Scientific research offers many vehicles for the transport of infected soil, such as mammal traps, nets or other sampling equipment, car tyres, boots, digging or camping equipment etc. Consider the biologist or geologist who troops from one sampling location to another, in an extensive

sweep of the southwest of Tasmania without taking precautions. He or she might easily distribute diseases, or alien or native (but formerly restricted) species, throughout. For invertebrate and plant biology at least, western Tasmania is a biogeographical paradise, but, already, some of these fascinating distribution patterns may have been compromised by the very act of science that seeks to describe them. This might be considered ecology's uncertainty principle, and the probability of it occurring would increase when exploratory research takes place over a wide geographical area.

On another level, management prescriptions based on scientific research can result in a loss or a change in natural values. Examples include some deliberate but well meaning acts, and some mistakes. For instance, where management prescribes activities which will selectively advantage one species, say a threatened species, other species may well be disadvantaged. An oft cited example of this is the management of land for the orange-bellied parrot (*Neophema chrysogaster*): this programme prescribed, as a result of research into the species, a fire regime which would make available seed as food. The programme appears to have made a contribution to saving an endangered species, but in the process considerable damage has occurred to organic rich soils burnt over a wide area as a result of escaped prescription burns. The programme has therefore been to the detriment of flora and fauna requiring organic rich soils. Hazard reduction burns fall into the same category, favouring some species to the detriment of others.

Research is also selective with respect to the types of flora, fauna or landform or landscape we would like to preserve. In any research programme we commence with a presupposition about the values we would like to keep or protect from disturbance. Rainforest communities and mammalian species are good examples; in comparative terms these forms of life receive the lion's share of resources and research effort. Management effort will then be directed to these forms, to their benefit and also those species and communities that benefit from flow-on effects. Buttongrass and invertebrate communities, for instance, have been seen as poor relations of rainforests and mammals, despite their widespread occurrence in the WHA. This results in, for instance, the diversion of roads or tracks, or other activities, from forests into buttongrass moorlands so as not to violate the rainforest values, and other examples like those cited above.

An excellent example of this selectivity comes from the research and management of the Pedder galaxiid (*Galaxias pedderensis*). This fish species has been regarded as endangered for some time now; in 1991 it was recognised as urgently requiring help to recover following its demise after hydroelectric developments, the spread of a predatory native galaxiid, and the introduction of trout in southwest Tasmania. Small numbers of the species were located in one

creek system and it was deemed that the species would slide into extinction if something was not done quickly. The solution proposed and undertaken, with almost no discussion, consultation or debate, was to release a small number of these fish into an alpine lake which had no fish in it. This solution was proposed despite the fact that it violated IUCN guidelines on translocations, and despite the fact that this galaxiid was endemic to a lowland lake, the former Lake Pedder. In this instance, therefore, and under the guise of scientific expertise, it was deemed that a lake flora and fauna which had evolved without fish was less valuable than a population of galaxiid fish.

Scientific research is a potentially threatening process itself, and so are the management prescriptions which result. As the examples above show, both are selective in their approach and deal with components of the ecosystem rather than the system as a whole. Some people even suggest that science will never achieve this latter goal of treating ecosystems holistically.

Fortunately the most predominant form of scientific work undertaken in the WHA is research designed to determine appropriate management for problems of an applied nature. This includes a wide variety of activities, primarily directed towards research on the affects or impacts of processes which threaten natural features like habitats, communities or species. Such threatening processes include fire, road and track building, mining, bushwalking, effluent from septic systems, introduced species and diseases like *Phytophthora*, river bank erosion, and horse riding, fishing, bee keeping etc. There is a plethora of threatening activities currently underway in the WHA; these threatening activities will eventually disturb systems so that they no longer have the same features as they started with. These features are those that the WHA designation seeks to retain. Where these threatening processes are occurring in the WHA there is certainly justification for research programmes to be established in the WHA, but only provided the research techniques themselves don't represent too great a risk to the valued features. It might be argued that most of the work should be done outside the WHA to minimise the risk. Realistically, however, it would be better to monitor the threatening process in situ, if research is required to respond quickly to management issues.

Other research activities underway include exploration for scientific curiosity and/or systematic research, exploration of resources, undisturbed systems research (i.e. greenhouse research), or research for education purposes. A case can be made to exclude these from justifiable activities in the WHA. For instance, the intentions of exploration are often questionable. For non-economic values, why should we continue to investigate the values of the WHA when its values are already acknowledged as being outstanding? The examples above indicate that the values could be potentially weakened in the process. Exploration for

economic exploitation will have the same effect with the added impact of the invasive techniques used to extract the resource, and these impacts are usually aesthetically and ecologically extreme.

Undisturbed systems are undoubtedly valuable as benchmarks for environmental change, particularly so when the change is subtle and of a long-term nature. But undisturbed systems are also valuable because they are just that; undisturbed. Placement of monitoring equipment and regular visitation render the systems subject to the same disturbances as experienced elsewhere in the world. Unless non-invasive monitoring techniques can be developed we should consider monitoring other parts of the landscape which are presently subject to known environmental change. Viewed from this perspective there is no need to have undisturbed controls anyway.

There is no doubt that the educational value of the WHA is extremely high. On the other hand, disturbed systems like urban, agricultural and/or forestry areas are also excellent environmental classrooms and they are almost ubiquitous. Some would argue that ecological research for the purposes of education in these areas is required more urgently anyway.

Perhaps my main goal in writing this article has been to shake the pedestal of science and scientists in WHA management a little, rather than condemn it outright. From this narrow perspective, as discussed above, scientific research activities can result in disturbance to natural systems. Where the risk of disturbance becomes too great these activities should be curtailed. Where the benefits to in situ management are great these activities seem valid. Thus, to be justifiable, in my opinion, scientific research in the WHA should be issue or management oriented, and broad, acknowledging all elements of the biotic and abiotic environment. This way knowledge will continue to accumulate but potential damage caused by the research itself will be limited and outweighed by benefits to problem solving. Policies for the WHA which allow for the open slather, inventory-type ecological research which has been practiced in the past, whether intensive or extensive, should be critically re-examined. Viewed from this perspective, ecological research in the WHA of the exploratory type, monitoring type or for the purposes of education, are similarly difficult to justify.

As a final note (disclaimer), I acknowledge that a fuller treatment of this subject must include at least a discussion on the concept of "naturalness" as it applies to "wilderness" areas like part of the WHA. Similarly, a detailed discussion of the role of science in decision making, in determining the values which society upholds, is required. Perhaps these topics might form the basis for further articles.