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What we don’t know and haven’t learned about cost–benefit prioritisation of rock-wallaby management

Todd Soderquist

Abstract. Research and translocations of brush tailed rock wallabies (*Petrogale penicillata*) in New South Wales have, in conjunction with studies in Victoria and Queensland, provided extensive insights yet also document the high variability in the species’ response to management. Nonetheless, experts are being asked to quantify predicted response for cost benefit prioritisation models that will rank threatened species and populations worthy of future funding, with little consideration of the basic principles behind adaptive management. The weaknesses of these prioritisation models must be evaluated carefully by experts in order that appropriate advice is provided which genuinely assists decision making. I explore the questions facing rock wallaby ecologists as a case study of how much more we need to know and learn within adaptive approaches to conservation before our predictions are robust.

If you can look into the seeds of time, And say which grain will grow and which will not, Speak then to me. ‘Macbeth’

Introduction

For decades, conscientious threatened species managers have been applying cost benefit analyses on a day by day basis when deciding how to allocate their limited time and money to productive conservation (Bottrill *et al*. 2008). Their decisions specifically acknowledged the opportunity costs of choosing one action over others on the basis of their understanding of current issues and possibilities. They attempted to trial predictions and then improve upon them, with the flexibility of constant reassessment allowing them to seize unpredictable windows of opportunity as they arose, test novel techniques and adjust actions to accommodate unexpected responses. Where trials were not feasible many field managers have at least tried to structure their learning to improve future actions (e.g. Armstrong *et al*. 1994). Decades of experience gained in this adaptive environment provided insights and perspectives to those who paid attention, but more importantly created an awareness that static equations cannot adequately capture the variability of ecological systems and responses.

Nonetheless, policy officers increasingly demand that experts distil complex and variable results in to single value guesses. For instance, rock wallaby (*Petrogale*) ecologists were recently asked to identify all actions needed to secure each rock wallaby colony in New South Wales (NSW), the cost of these actions and the time until successful recovery (M. Roach, pers. comm., 8.vii.2010), with the expectation that these values would lead to a prioritisation of which species and populations were most worthy of saving. Because of the extensive work conducted on *Petrogale* species across the nation (e.g. Kinnear *et al*. 1988; Churchill 1997; Jarman and Bayne 1997; Sharp and Norton 2000; Hazlitt *et al.* 2004; see also Box 1), the genus serves as an excellent case study of whether such demands for expert opinion can produce useful results. It is therefore timely to examine just how much we know and still need to learn about applying cost benefit analysis to the prioritisation of rock wallaby management actions, using information gathered from translocations of the brush tailed rock wallaby (*Petrogale penicillata*) in NSW to focus the discussion (Table 1) yet assimilating insights from other areas and other species.

Questions

Within any assessment of our ability to validly conduct cost benefit prioritisation of rock wallaby management there are six prominent questions that need to be addressed:

(1) Objectives: What paradigm shifts are entailed in prioritising funding among rock wallaby colonies, between regions and across all threatened species?
(2) Critical actions: Can we identify the minimum subset of essential management actions needed to recover rock wallabies?
(3) Variability: Do the highly variable results within and among rock wallaby studies allow us to accurately interpret the species’ response to management without adaptive trials?
(4) Retrospectives: Does evidence from previous conservation projects suggest that our estimates of future costs are accurate?
(5) Predictability: How much certainty do we have when forecasting the usefulness of untested actions?
(6) Adversarial constructs: Can we integrate competing paradigms for the benefit of rock wallaby conservation?

Objectives: paradigm shifts in cost benefit prioritisation

The policy of current cost benefit prioritisation models (e.g. Joseph *et al*. 2009*a*; DPIPWE 2010) is to ‘recover’ (i.e. secure) as many threatened species as the budget allows through

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Box 1. Results of translocations of brush-tailed rock-wallabies in NSW

In NSW, brush-tailed rock-wallaby (BTRW) colonies have been 'blinking out' across the landscape for decades (Lunney *et al*. 1997). In the absence of threat abatement, many colonies have collapsed to very small numbers and become isolated (DECC 2008). Under the project 'Pulling Rock-wallabies from an Extinction Vortex', the NSW Brush-tailed Rock-wallaby Recovery Team have been supplementing key colonies, while also undertaking logistically feasible threat abatement, with the aims of raising reproductive output and restoring these colonies to viability.

Data presented below are derived from eight supplementations (between 2001 and 2010) of colonies in the south (Shoalhaven, Taralga and Jenolan Caves) and west (Warrumbungles) of the species' range in NSW (Table 1). The outcomes of these supplementations have been monitored via remote camera surveillance, postrelease retrapping and radio-tracking, the latter for at least one year after release. In the longer term, persistence of individuals at release sites as well as reproductive success will be determined by DNA analyses of faecal pellets. Detailed methods and results pertaining to these releases will be reported elsewhere.

Results of these translocations to date have been highly variable and this variability is summarised below along with brief comparisons with studies of other rock-wallaby species where feasible. The *a priori* prediction of BTRW response in the NSW Vortex release is followed by an assessment of whether the observed results were consistent with the prediction (Yes No Variable) and then further explanation. The sample size at the Warrumbungles was much larger than elsewhere (Table 1) but the conclusions presented here are consistent among the release sites.

(A) Captive-bred BTRWs will quickly switch to a typical wild diet and maintain body condition. **Yes**
Vortex BTRWs rapidly adopted a typical diet (e.g. within 36 h) and maintained health. This is consistent with other translocated rock-wallaby species, e.g. *P. xanthopus* (Lapidge 2000).

(B) Provision of free water is essential to acclimatise captive-bred BTRWs during transition to the wild. **No**
Where provided, free water was not in high demand. Need for free water is likely to vary between species and with habitat parameters at the release site such as temperature, quality of refuges and the availability of browse (e.g. Lapidge 2001).

(C) Groups socialised together before release maintain post-release contact. **No**
Presocialised BTRWs did not associate with individuals from their group any more than with individuals from other groups or native resident BTRWs. The influence of prerelease socialisation in other studies is uncertain (e.g. Lapidge 2001; DSE 2008).

(D) Released BTRWs will quickly insert themselves into the colony site among the native residents. **Yes**
Released BTRWs commenced interacting socially with resident animals shortly after release (within 24 h), and most set up stable home ranges adjacent to, and overlapping, those of native residents. Most other rock-wallaby translocations are not comparable as they have been reintroductions into empty habitat.

(E) Sending females out with small pouch young speeds recruitment into the population. **No**
All females released with small pouch young (<21 days) lost them within 6 weeks. However, four out of five replaced lost pouch young with new pouch young immediately. Lapidge (2001) found that female *P. xanthopus* carrying similar-sized pouch young retained them after release.

(F) Refuge choice and predator avoidance of captive-bred BTRWs will be inferior to 'street-wise' native residents. **Variable**
Many captive-bred BTRWs chose poor refuges and were lost to predation in the first two months. Refuge choice did improve with time since release, although relapses into poor habits were frequent.

(G) 'Hardening' of captive-bred BTRWs in predator-free natural habitat before release will improve survival and habitat choice. **Variable**
Hardened captive-bred BTRWs showed improved refuge choice and survival. However, BTRW released directly from captivity concurrently with the hardened animals as a 'control' also suffered no predation.

(H) The presence of native BTRWs will 'anchor' released BTRWs to good habitat. **Variable**
Approximately one-third of captive-bred BTRWs promptly initiated long-distance (>600 m) exploratory movements. The remaining two-thirds of released animals settled quickly into home ranges near their release point (<300 m) and established stable home ranges.

(I) We can consistently control fox densities. **Variable**
Although an intensive predator-control program is part of all Vortex releases, foxes could not be consistently controlled. Control success varies unpredictably over time, and among sites. This high variability and lack of predictability is consistent with other studies (e.g. Kinnear *et al*. 2010; Lapidge 2001; Taggart *et al*. 2010).

(J) Survival of BTRWs released under intensive threat abatement will be high (>70%). **Variable**
Survival of Vortex BTRWs was highly variable among release sites as well as at the same site through time (Table 1).

(K) Mortality will be mainly due to predators, exotic and natural. **Variable**
Predation was the most significant cause of mortality of released BTRWs. However, a significant number of deaths occurred as a result of factors that are difficult to predict or control (e.g. accidents and injuries). This is consistent with other studies (e.g. Short 2009; Taggart *et al*. 2010).

(L) The impact of native predators will be low. **Variable**
Wedge-tailed eagles (*Aquila audax*) were observed hunting BTRWs and at least one death is attributed to eagle predation. Eagles have been implicated in the deaths of other rock-wallaby species (e.g. Lapidge 2001). Photographic evidence suggests that a large diamond python (*Morelia spilota*) may have taken three out of four pouch young that disappeared around the time of permanent pouch emergence at a Vortex release site.

(M) Age or sex will strongly influence survival. **No**
There were no differences in the survival of Vortex BTRWs based on age or sex. Age, mass or condition at time of release did not significantly affect short-term survival after release for *P. xanthopus* (Lapidge 2001). Short *et al*. (1992) found that survival of male macropods after reintroduction was often higher than for females.

(N) Age or sex will strongly influence initial movements and home-range stability in a consistent pattern. **Variable**
Movement patterns were highly variable with no consistent pattern across age or sex.

(O) Dispersal away from good habitat will be rare. **Variable**
Not counting the immediate post-release movements described in (H), other long-distance dispersal events occurred months after release, generally by younger male animals. Long-distance movements of released rock-wallabies have also been observed in other rock-wallaby species (e.g. Lapidge 2001).

(P) BTRWs will demonstrate strong refuge fidelity. **No**
Radio-tracking indicated that both native resident and released BTRWs have stable home ranges but low fidelity to refuge sites. Conversely, studies of BTRWs in northern NSW and south-east Queensland indicate very strong refuge fidelity by established individuals (Jarman and Bayne 1997; Hazlitt *et al*. 2004, 2010).

(Q) Newly translocated males will become satellites until they can achieve dominance in a colony. **Variable**
Some male Vortex BTRWs initially set up a stable home range, but then later, often several months after release, made long-distance movements as described in (O). In Vortex colonies where captive-bred males have been released and there are no resident males present (Shoalhaven and Taralga colonies), males remain at the release site and successfully sire multiple young with resident females.

Table 1. Historical and current translocations of brush-tailed rock-wallabies to the wild in New South Wales

Most of the following translocations were releases into a resident colony of brush-tailed rock-wallabies (BTRWs) in order to increase its viability. The initial Wombeyan release was a reintroduction to empty habitat. Releases considered as part of the Vortex program are those from the 2001 Taralga release onward. Most of the data pertaining to these releases are unpublished except for the early Jenolan Caves and Wombeyan Caves releases (Buchan 1997; Eldridge *et al*. 2004)

Release site	Provenance of released BTRW	Release date	Number released	Survival % (first 2 months)	Survival % (to Dec. 2010)	Reproduction
Wombeyan Caves	Wild-caught, Jenolan Caves	Feb. 1980	2M, 2F	unknown	0	Successful
Wombeyan Caves	Wild-caught, Jenolan Caves	Jan. 1981	6 animals	unknown	0	Successful
Jenolan Caves	Wild-caught, Wombeyan Caves	Sep. 1995	1F	100	0	Successful
Jenolan Caves	Wild-caught, Winmalee	Nov. 1997	1F	100	0	Uncertain
Taralga	Captive-bred, ex-Kawau origin[A]	Feb. 2001	2M	50	0	Successful
Kangaroo River, Shoalhaven	Wild-caught, Jenolan Caves	Sep. 2008	1M	100	100	Successful
Kellets Creek, Shoalhaven	Captive-bred, NSW program[B]	Feb. 2009	2F	50	50	Successful
Square Top, Warrumbungles	Captive-bred, ex-Kawau origin[A]	Apr. 2009	6M, 17F	43	33	Successful
Kellets Creek, Shoalhaven	Captive-bred, NSW program[B]	Oct. 2009	2F	100	100	Successful
Square Top, Warrumbungles	Captive-bred, ex-Kawau[A] and NSW program[B]	Feb. 2010	3M, 4F	100	86	Too early
Jenolan Caves	Captive-bred, NSW program[B]	Nov. 2010	1M, 2F	100	100	Too early

[A]BTRWs identified as ex-Kawau origin are derived from repatriations of BTRW (in the 1960s, 1970s and in 2003) to Australia after original releases to Kawau Island, New Zealand in the early 1870s (see Eldridge *et al*. 2001).
[B]BTRWs identified as NSW program animals are animals that have been bred as part of the NSW recovery program, from wild-caught founders trapped from colonies across the range of the Central ESU.

implementing all actions identified as essential. Other species lower on the priority list therefore receive no funding from the process, with the recognition that they will continue their decline towards extinction unless alternative resources are found. This policy is not the only approach (e.g. Walker 1992; Pressey and Taffs 2001; Restani and Marzluff 2002; Biodiversity Decline Working Group 2005; Cipollini *et al.* 2005; Marsh *et al*. 2007; McCarthy *et al*. 2008; DECCW 2010; Howes *et al*. 2010; Carwardine *et al*. 2011; Clements *et al*. 2011; Colyvan *et al.* 2011; DERM 2011; Greyling and Bennett 2011); it is merely one untested version of 'conservation triage.' In contrast, many experienced threatened species managers practise a different paradigm: they seek individual actions that are particularly cost effective in stabilising or recovering those species that can be most efficiently influenced. For decades they have incorporated the recent advice of Possingham *et al*. (2002): 'to minimise overall species loss, we should allocate resources to recovery actions such that the marginal rate of increase in viability is equalised across all threatened species'. In some instances particular actions are pursued on the basis that a relatively inexpensive or low intensity manipulation may prevent a species from tipping over the edge into an irretrievable decline (e.g. Clements *et al*. 2011; Keene and Pullin 2011). The approach is iterative, with lessons from early trials and constant re evaluation used to further increase the cost effectiveness of their decisions. Proponents of current prioritisation models dismiss this selective paradigm as inefficient (e.g. DPIPWE 2010) and, in truth, it can be so if conducted without transparent predictions, monitored outcomes and documented decision points that allow gradual improvement. However, conscientious application of the selective paradigm is the heart of adaptive management. By tracking opportunities and results, managers can constantly rebalance the best use of their resources in preventing extinction, even if full recovery is not achieved.

However, the objective of 'recovery' is not the goal of current prioritisation models and liberal use of the word in documentation is misleading (see Carey and Burgman 2008). Fine print in the Methods (which may or may not be made clear to the public and Senior Managers) aims only to 'secure' the species in what is typically a single remnant population (Joseph *et al*. 2009*a*; DPIPWE 2010). Experts are asked to guess what level of management is required such that one viable population will remain for decades or centuries (cf. Traill *et al*. 2010). The ambiguity of this objective needs to be explained to many experts and policy officers who do not consciously differentiate recovery from stability from persistence (Fig. 1). On this continuum, the aim of prioritisation is therefore the lowest level of persistence. By having a long term one population objective for all species a level playing field is supposedly established that permits equivalent cost benefit calculations across hundreds of species. Experts may choose to increase the number of populations (e.g. to conserve genetic diversity) yet must keep in mind that a competition is explicitly created among participants to minimise management costs so as to formulate a 'wager' that gives a species the best chance to win the funding battle. If one expert costs persistence of a formerly landscape species as being a single fenced population, then it gains a competitive advantage over any more ardent interpretation for another species. While most proponents of prioritisation bemoan the shortage of explicit objectives in threatened species management, the ambiguity of the current prioritisation objective can be equally misleading and detrimental within decision theory (Maguire 1986; Carey and Burgman 2008).

Under this model the range of options to 'secure' brush tailed rock wallabies in NSW is enormous. An expert could easily defend the belief that one small population, carefully protected, could persist for centuries given that we have empirical evidence of rock wallabies surviving 8000 years under an effective

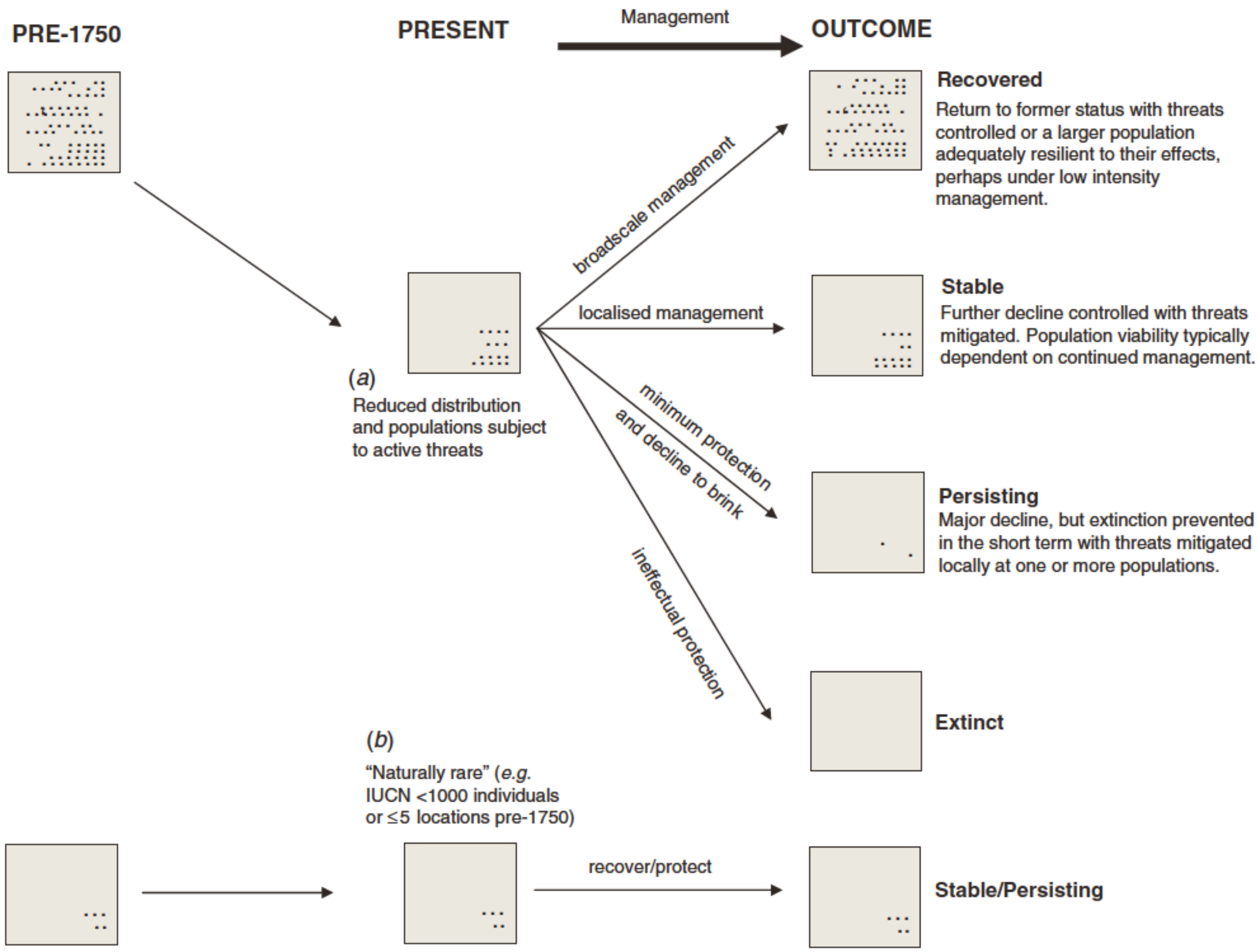

Fig. 1. A diagrammatic representation of differing outcomes for threatened species 'recovery'. When comparing champagne to cheddar to chalk in cost benefit analysis one needs to know what is on the plate, and the concepts of this diagram proved surprisingly novel to experts and policy officers developing a recent prioritisation model (pers. obs.).

population size of 15 breeding animals (Eldridge *et al.* 1999). Were the expert more cautious, two geographically separated populations would provide greater safety from catastrophic events. Or, the expert could use IUCN Red List criteria to defend the long term security of six populations, each ~200 animals, which technically allows for delisting of the species (IUCN 2010). Given that brush tailed rock wallabies are a landscape species formerly distributed over most rocky outcrops (e.g. Short and Milkovits 1990; Lunney *et al.* 1997; DECC 2008; Murray *et al.* 2008) the expert might prefer to attempt broader protection of a large population (Clements *et al.* 2011) by managing the entire Northern Evolutionarily Significant Unit (ESU: *sensu* Moritz 1994), while accepting the loss of biological diversity as colonies of the Southern and Central ESU gradually go extinct. Or, the expert might risk a dangerously high prioritisation wager by opting to protect multiple populations across all NSW, thus retaining a representative proportion of the very diverse and localised genetics (Hazlitt *et al.* 2006*a*, 2006*b*, 2010; Piggott *et al.* 2006). Experts are thus recruited as *de facto* promoters for a species and must gauge their bets in light of potential manipulation of the game (e.g. Hammond *et al.* 2000). Similar imprecision in targets is often found outside quantified prioritisation models, but these targets provide adaptive guidelines rather than a competitive gamble leading to a potentially solidified consignment to the dust bin (see below).

Critical actions: all actions are equal, but some are more equal than others

Cost benefit models assume that an expert knows the response of a species to site specific management actions and can select the appropriate suite of actions that are critical to securing a population. If the proposed actions have been repeatedly trialled then this assumption may be met. Typically, though, managers struggle on a month by month basis to adjust or even invent cost effective techniques that will work using field data, scientific knowledge and intuition. Although intuition has been criticised as a guide to daily cost benefit analyses (e.g. Joseph *et al.* 2009*b*), it is equally obvious that expert guesses about prioritisation actions will typically be based on intuition in the absence of reliable inference (Bottrill *et al*. 2011). Often managers using the same baseline data simply disagree on interpretation and without field trials are unlikely to resolve the truth (McDonald Madden *et al*. 2010). This dilemma is exacerbated by a poor understanding of the relative importance and impact of the threats themselves, much less the method, intensity and chronology of threat abatement on a site specific basis. In fact, we often do not even know what threats we are faced with until a species collapses (e.g. Groom 2010). Adding to this complexity, trying to predict actions that will ensure the long term security of a species under climate change raises the stakes for experts lacking primary data (Keith *et al*. 2008; Conroy *et al*. 2011). By assuming that an expert workshop can somehow identify all actions necessary by which to select or reject species, the prioritisation model becomes the antithesis of adaptive management.

The choice of actions in a poorly understood system will be biased if questionable dogma provides the main guidance. Brush tailed rock wallabies are a case in point. A decade ago most ecologists in south eastern Australia believed that control of red foxes (*Vulpes vulpes*) around rock wallaby colonies would be relatively easy and remarkably effective based on results from Western Australia (Kinnear *et al*. 1988, 2010). However, subsequent application of this approach in NSW has yielded anything but the expected explosion in brush tailed rock wallaby populations (NPWS 2003; DECC 2008). A dozen potential reasons for this discrepancy have been informally discussed among rock wallaby ecologists (e.g. see papers in this issue) yet no explanation has proven consistent or convincing in clarifying the inferior response in eastern Australia. The consensus to date has been that successful fox control is dependent on variable, uncertain and unpredictable local conditions that are very difficult to quantify adequately (e.g. Greentree *et al*. 2000). The crucial point in this issue is that if a prioritisation had been conducted a decade ago, experts would have confidently relied on relatively cheap fox control in their cost benefit predictions, and thus inappropriately escalated the rock wallaby's priority ranking.

With these issues in mind, what indications do we have that expert workshops are currently able to choose a useful list of 'essential' actions for the prioritisation wager, and exclude all those that are not really needed? If experts are able to filter actions then presumably we would have recovered 'easy' species long ago after thoughtfully assessing potential actions and implementing those required. Or perhaps not. During one threatened species prioritisation, modellers deduced from their final list that 'many high ranking species are at risk from a single threat such as habitat modification, and occupy small areas which are easiest to protect. They tend to be less well known, which may be why these easy to secure species have not already been recovered' (DPIPWE 2010). This begs the question as to why experts needed the stimulus of a prioritisation process to realise that recovery for some species is easy. If they were unable to recognise critical actions before, it becomes imperative to understand why their awareness so suddenly improved. (It must be understood that securing a single population under prioritisation definitions may be very different to previous objectives of stabilising and genuinely recovering these 'easy' species.)

Choosing exactly which actions are the most important out of a wide selection is also apparently fraught. The Tasmanian modellers warn against implementing single actions rather than the entire suite selected by experts, stating that an all or nothing approach is required. They add that their 'experts found it difficult to estimate relative importance of actions' among those they had chosen as essential to recovery. With this in mind, it is left unclear as to how these same experts were able to accurately identify the critical actions from the many potential ones available. In some instances the division between essential and useless may well be clear, but logically one must question how this can be so for all species, or even most given that chosen actions are usually untested (. . . or they would have recovered the species previously).

Variability: garbage in interpretations out?

To estimate a species' response to management one first needs accurate information and then hopes the results are not so highly variable that they confound interpretation (cf. Ujvari *et al*. 2011). Such dependable datasets are atypical yet modellers often expect experts to have information readily at hand that is easily interpretable. For instance, in one of the most widely applauded prioritisations of threatened species, Joseph *et al*. (2009*a*) conclude that 'if there is enough data to list the species on threatened species lists, then there should be enough data to rank management projects with project prioritisation protocol'. This view is wrong. Data regarding a species' decline, on which listing is largely based (IUCN 2010), rarely elucidate the variability among known and unknown threats, nor indicate specific recovery actions, nor their cost, nor their implementation regime, nor their anticipated success, nor their long term benefit (e.g. Possingham *et al*. 2002). Variability in site specific responses further impedes expert opinion (Chauvenet *et al*. 2010), and this hurdle should not be dismissed blithely.

Knowledge needed to predict the response of rock wallabies to management is being painstakingly gathered (e.g. Kinnear *et al*. 2010; Taggart *et al*. 2010; Tuft 2010; Murray *et al*. 2011; and papers in this issue), but is proving anything but simple to interpret. Dogmatic pronouncements on the ecology of rock wallabies from past decades are often wrong or at least highly variable. For instance, Short (1982) concluded that brush tailed rock wallabies invariably used north facing slopes and that other habitats are apparently unsuitable. This became dogma among most rock wallaby ecologists for two decades even though it ignored general knowledge that the species had been formerly distributed across the landscape, on numerous types of habitat, facing all aspects. The flexibility of brush tailed rock wallabies is frequently exhibited within the Northern ESU of the species (e.g. Bayne 1994; Murray *et al*. 2008), and even the Central ESU still demonstrates the ability to thrive on aspects other than north (e.g. Kutzner and Dodd 1996; NPWS 2003; DECC 2008). Such variability can strongly influence the input to the prioritisation models if, for instance, it leads to misinterpretation of carrying capacity or diverts attention from otherwise healthy colony sites that are deemed to be suboptimal.

If the observed results of management actions are highly variable, then predictions will be equally uncertain. Our attempts to predict species' response during recent studies of the brush tailed rock wallaby in NSW are a case in point. Management of the Warrumbungle rock wallaby population first tried goat control then added fox control then intensified both in an effort to stabilise the declining population. Only after those actions proved inadequate by themselves did we seriously consider supplementing remnant colonies as the next most cost effective action to re establish viable numbers capable of sustaining occasional predation. Translocation of rock wallabies would not have been considered at the outset and the costing would have been unpredictable anyway. If, by the time we decided it was needed, translocation was shown elsewhere to be a consistently successful technique then our monitoring of this particular project would have been costed far below the level it needed as a novel technique. There was no way of predicting these wide divergences in cost ahead of time. Other results from the initial years of translocation research in NSW have proven equally variable (see Box 1) such that it is very difficult to anticipate how a poorly known population might respond to management in any given year, much less over decades.

Conscientious managers are able to use variability to guide conservation actions if they have flexibility to react to unanticipated responses. Uncertainty is an inherent part of daily improvements in management. In some instances uncertainty may not greatly influence a result (Nicholson and Possingham 2007); in others it is critical (Gillespie *et al*. 2011; McDonald Madden *et al*. 2011). However, high variability can be the bane of prioritisation wagers because one cannot precisely estimate the parameters that dictate funding allocation among species. Hypothetical trend lines of potential response (e.g. Possingham *et al*. 2002; Fig. 1) provide only a generalised illustration of what might be subsequently determined by field trials (McDonald Madden *et al*. 2010). As prioritisation is currently conducted, most policy officers expect an expert to submit a single value for what she knows to be capricious, in an equation that comprises other equally uncertain values. Attempts to determine and control for this known uncertainty have been limited and therefore influence these guesses only slightly (e.g. Burgman 2005).

Retrospectives: we so rarely get project costs right

The human pretension to the status of expert tends to discourage retrospective enlightenment. Nonetheless, any examination of past threatened species programs repeatedly demonstrates that managers are rarely capable of predicting project costs accurately due to the complex nature of ecological systems and responses (e.g. Brashares 2010). Even supposedly simple costing of fox control can prove inaccurate by an order of magnitude when expert assumptions on localised fox response prove faulty (with, for instance, an unanticipated mouse plague causing indifference to fox baits during the initial months of our recent Warrumbungle study). If careful monitoring of fox numbers is neglected due to an assumed response and a desire to keep the prioritisation wager low, then a rock wallaby colony might be severely depleted before problems are detected and more expensive control techniques applied (e.g. Groom 2010).

Thorough sensitivity testing of cost estimates might adequately examine their influence on the initial model, but it would be better to use rigorous techniques to determine the impact of guesses concurrent with eliciting them (e.g. Burgman 2005; Speirs Bridge *et al*. 2010). No prioritisation has attempted this to date yet 'a management decision that assumes that probabilities and utilities are exact, when in fact they are uncertain, can result in management outcomes with unexpected or undesirable results' (Regan *et al*. 2005). Transmitting the relevance of this uncertainty to busy Senior Managers will be a challenge for policy officers who are directed to simply provide a prescriptive list of the Top 10 species . . . especially given that frequently policy officers themselves prefer simple answers without caveats (Briggs 2006).

In order to adaptively improve our management, transparent program costs are essential (Bottrill *et al*. 2008; Rumpff *et al*. 2011). Prioritisation models provide some transparency on estimated costs (Szabo *et al*. 2009), but the simplicity of the process tends to reduce the usefulness of these expert guesses down to single values. A full explanation of anticipated costs, plus their uncertainty, is far more informative both now and in any future reassessment. Transparency is thus a readily available benefit that could be achieved outside of formal prioritisation modelling simply by applying appropriate organisational policies and decision theory. Demanding a complex and potentially counterproductive process may not be the most cost effective way to achieve this particular goal.

The argument that prioritisation models give the public an overall cost for threatened species recovery is fallacious. Because the models used to date are designed to typically 'secure' only one population of each species, the total cost being calculated is a misleadingly small proportion of the recovery effort that the public expects, or at least believes to be happening. Great care needs to be taken in presenting the model and costs so that once the public learn the true level of the 'recovery' being announced by the government, there is not a backlash against the whole process.

In order to determine whether the expert guesses were accurate to begin with, the projects that are selected will need to be given a reasonable chance to prove themselves under a guaranteed budget. The response of most species must be anticipated to be slow, in some instances even protracted affairs lasting decades, and any attempt at revising ranks and budget allocation must take this delay into account. Interpreting whether stochastic events, such as fire or drought, have influenced population response will add further complexity when judging whether a species should be 'exempt' from yearly reranking of priorities on the basis of new information.

Continuous, adaptive revision of the prioritisation ranks is a commendable notion that will be difficult to apply in reality. Managers will be faced with an inherently defective choice if a program needs to be escalated: (1) the species is allocated more funds than the original prioritisation ranking assumed, thus being inequitable to those species not selected, or (2) the program is maintained at levels now known to be inadequate, or (3) the species is dropped, potentially before much is learned from the initial investment. Alternatively, a subset of species on the budgetary margin will shuffle in and out of funding, which is likely to be detrimental to both the species and fieldworkers. If the prioritisation process expressly allows for increasing the budget after a species wins selection, then game theory indicates that an expert should devise a minimal project that will out compete others, thereafter expanding it as necessary (e.g. Hammond *et al*.

2000). Game theory also warns of the human propensity to fall for the 'Concorde fallacy', wherein funding of a cost blowout is judged necessary in light of past investment, helping ensure that a species once anointed remains so (with anticipated delays in population response providing legitimate support to faulty logic). Furthermore, governments will find it difficult to withdraw a species from funding once public promises of 'recovery' have been announced and community groups brought on board to assist. Under these influences, iterative re evaluation of rank as each new datum becomes available will become unrealistic, and adaptive management thus further diminished. While these kinds of issues influence all threatened species management, it is important to realise that the ranking and selection within the prioritisation process makes it particularly vulnerable by inadvertently locking in decisions, for better or worse.

Predictability: if you can look into the seeds of time

As with cost, retrospective assessments of the benefit of proposed actions demonstrate a disappointing inability of experts to predict their success (e.g. Short *et al*. 1992; Boersma *et al*. 2001). In many instances threatened species managers are applying techniques that have variable responses (as noted above) or are breaking new ground with novel, and thus largely unpredictable, actions. Participants who state with certainty how a population will respond are demonstrating an 'insufficient humility about what we do not know' (Freudenburg 1999) and are in danger of playing the role of a 'Dunning Kruger expert' (see Kruger and Dunning 1999). This is exacerbated if the acceptable level of expertise is low. For example, in a recent prioritisation attempt it was explained by model developers 'that it is not necessary for the expert to know everything there is to know about a species just someone who knows basic details about the species is enough'.

As with cost estimates, most prioritisation models allow experts to record their level of confidence about presumed benefits. But then this uncertainty is rarely exploited for insights. Marsh *et al*. (2007) are frank about their use of confidence estimates: 'although these rankings are not incorporated in the final species rankings, they help overcome the reluctance of technical experts to score in the face of uncertainty . . .'. They also believe the confidence scores can be used to explore limitations and flag problems, but once the technical experts have provided answers and been dismissed, the model's advocates must engender candid internal criticism and clearly transmit concerns to policy makers. Self criticism in bureaucracies, especially of a product that has been sold by optimistic promises, is notoriously challenging (Hobbs 2009; Szabo *et al*. 2009).

Cautionary tales abound (albeit seldom published: Armstrong *et al*. 2007; Hobbs 2009) where self assured predictions of species recovery have proven misguided. For instance, after congratulating ourselves on the rapid, almost monotonic recovery and eventual delisting of the brush tailed bettong (*Bettongia penicillata*) following two decades of fox control, the species had to be relisted as 'Critically Endangered' when monitored populations crashed almost simultaneously during the first half of the last decade. 'The subsequent decline could not have been predicted, given previous knowledge or, in fact, given recent knowledge of the species biology, ecology and threats' (Groom 2010). Therefore, any wager made for this species would not have contemplated a crash of the magnitude realised. Speculative hypotheses to explain the causes are being examined in an adaptive manner (Wayne 2008), the expense of which would probably remove the species from any prioritisation listing.

Similarly, Rumpff *et al*. (2011) 'found that updated models [derived from field trials] predict markedly different transition probabilities compared with initial models based on expert opinion. This has strong implications for the apparent cost efficiency of restoration strategies'. Their adaptive approach can improve these erroneous guesses, and the rectified actions can, in theory, be reranked for effectiveness, but the material point for cost benefit prioritisation is that a decision to provide funding to some species and not others would have already been decided using an inexpert wager. Measuring uncertainty does not solve problems surrounding the application of faulty single value predictions, it just makes us more aware of the breadth of issues, fine tunes our reliance on rankings and helps experts focus on the fact that a Guess $\times$ Guess $\times$ Guess $\neq$ Truth.

Assessing the validity of predictions will be difficult under the prioritisation models as they are currently framed. The all or nothing policy of implementing actions will have to be carefully reconsidered once the species wins prioritisation because implementing multiple actions concurrently hinders elucidation of which actions are effective, and which ones are not. Progressive trials and structured learning would provide better tests by parsing the response. However, if adaptive interventions are adopted as the standard then the expert would be wise to wager only the minimum subset of actions required to start and add others as knowledge grew that they were clearly essential, thus increasing the likelihood of that species being selected for funding.

To achieve cost efficiency, the current prioritisation process focuses solely on managed populations and does not encourage the monitoring of controls to determine whether intervention was as useful as assumed by experts: adding a costly adaptive monitoring component to the wager will risk rejection of the species. Indeed, because the population 'most likely to survive' will typically be the one chosen to receive cost effective funding (Chauvenet *et al*. 2010), there will be an inherent bias in any subsequent attempts to comparatively evaluate the true benefit of the 'essential' actions (Ferraro and Pattanayak 2006). By failing to carefully monitor the supposedly 'supernumerary' populations we will not even learn much from our inaction (McDonald Madden *et al*. 2008, 2010).

The issue goes much deeper due to a perverse outcome inherent in the objective of securing only one viable population. We might hope for at least one small benefit from watching the gradual disappearance of those populations labelled as supernumerary, namely a demonstration that our expert predictions about securing one population were accurate. However, threat intensity will diverge rapidly between chosen and neglected populations if land developers are able to claim (rightfully?) that their harming of a non essential population can no longer be considered 'significant' under legislation that previously constrained the eradication of threatened species and their habitats. Quiet discussions like this are already occurring in NSW even while prioritisation is just an empty framework. Therefore, persistence of managed populations will not be suitable evidence of our predictive ability if the only comparison is to now unprotected populations whose habitat has been degraded to an unknown degree by human infrastructure, logging, mining and agricultural expansion.

Adversarial constructs: in the land of the blind, the one-eyed man is . . . well, ignored actually

No single approach to recovering threatened species can hold a mandate over all others if we are to improve our recovery efforts; we must incorporate as much value as we can from different paradigms in a broader view of the problem. If we dismiss new ideas and criticisms of them then perspectives are lost and the process will flounder.

Unfortunately, there seems a surprising lack of dialogue, or at least attention to the dialogue, among the entrenched partisans in this debate. The one eyed critic may not be king, but untested models require input from detractors as well as the converted flock if only to improve the final product. When threatened species prioritisation was initially proposed, two experienced ecologists (including T. S.) dared to criticise the concept, to which the proponent responded that 'as with most ill considered notes that are written in a state of grumpiness, they are best ignored [and] none of their points actually make sense . . .'. When opposing views are ignored, then regardless of which side wins or loses the debate, everyone loses.

All sides will agree on two of three levels in this discussion. The first is that a more structured approach to management is needed with transparent, documented testing of predictions and collection of evidence (Sutherland *et al*. 2004). Continued advances in adaptive management (e.g. Howes *et al*. 2010; McDonald Madden *et al*. 2010; Keith *et al*. 2011) could go a long way towards achieving the goal as the technique is effectively a form of iterative benefit assessment where cost can be readily incorporated as one parameter (Rumpff *et al*. 2011).

The second agreement that most parties would enter into is that cost benefit decisions need to play a main role in strategic allocation of funding. The geographic scale of implementation might remain open to debate ranging from worldwide models (e.g. Wilson *et al*. 2006) to geopolitically realistic national boundaries, to state or regional levels (e.g. catchment management authority). As scales retract the quality of knowledge held by local experts typically increases (e.g. Murray *et al*. 2009, 2011) and conscientious professionals try to balance their own cost benefit decisions at various scales depending on the species. When individuals make biased decisions on cost effectiveness it is typically not a problem of scale, but rather one of personality, professionalism, peer review and supervision, much of which is solvable on an organisational scale.

It is the third level of the debate wherein lies the rub. Once prioritisation is couched in terms of a winner take all scenario, both adaptive management and flexible cost benefit analyses are in danger of being marginalised. It is at this level where faith in gradual model improvement is not enough; a process must be developed in advance of implementation that clearly accounts for the numerous logical faults and conflicts identified in prioritisation models as currently proposed. Consensus should be reached on the model itself even before input data are collected if only to guide experts as they formulate difficult guesses. Furthermore, 'implementers of new procedures . . . need to explicitly build trust between groups' (Szabo *et al*. 2009), otherwise experts with concerns will be excluded and the only data feeding the model will derive from those with less humility.

Apologists for cost benefit prioritisation as the dominant paradigm controlling future funding note that if a species does not get ranked high enough for selection then there may be other buckets that Senior Managers can use to fund iconic species, or those important to conservation efforts by local communities, or projects that can engender outside resources and volunteers. These non market currencies represent the many other costs and benefits that conservation decisions entail. They might hypothetically be quantified within the prioritisation process, but if basic calculations like field costs and ecological benefits are confounded then conceiving of sensible ways to incorporate these more abstract values will be difficult indeed. Thus, the olive branch of multiple buckets is a return to the intuitive judgements that cost benefit models were promulgated to cure, although potentially facilitating a wider perspective on the alternative funding.

One interesting response to alternative funding that I observed is that simply having a potential for other buckets has silenced many critics. They may begrudge providing guesses to what they consider a flawed model, but assume that they will thereafter go on with business as usual. This attitude should concern all sides of the debate as the collective goal is to demonstrably improve conservation. Conscientious managers are rarely 'uncomfortable with the notion of taking a business like approach to conservation' (Possingham and Shea 1999); they do it day by day. While some managers 'might be reluctant to let factors other than biology dictate their conservation priorities', most professionals do not need to be told that 'as soon as priority setting leaves the ivory tower, a host of real world concerns, including the costs of conservation actions, must be considered' (Naidoo *et al*. 2006). What is uncomfortable to professionals is providing dubious input to weak models that will be used prescriptively without careful attention to the inherent uncertainty. Seeking a consensus on how the prioritisation model can be improved will help re engage all partners in the process regardless of whether other buckets remain available.

Rock wallaby professionals may be able to use their detailed awareness of opportunities and costs to ensure that prioritisation modelling is not detrimental to conservation while at the same time gaining perspectives on how to improve their own daily cost benefit decisions (Joseph *et al*. 2008). Any model that leads participants to contemplate alternatives in a structured fashion can be of value, helping to refocus tunnel visioned attention to certain species and guide politically motivated preferences. However, the blind acceptance of simple equations in an attempt to channel complexity will rarely facilitate insights. Criticism is critical. Unfortunately, it will be difficult for professionals to influence bureaucratic processes if agency policy is to 'support and reward staff who develop and implement the new procedures' (Szabo *et al*. 2009). If the faithful on all sides remain comfortably blind to other views of reality, the one eyed critic has nothing to offer and will continue to be ignored.

Conclusions

At times public or political pressures have weighted management decisions towards species or actions that were not the most cost effective pursuits (e.g. Miller *et al*. 2002; Restani and Marzluff

2002; DECC 2007). Some threatened species managers have been guilty of losing perspective regarding their pet projects, with no STOP sign agreed to in advance among peers or public. Others have confused output (fox control) with outcome (increased rock wallabies), failing to enunciate intended objectives or broader strategies. Although using informal models to guide their activities, many managers are only beginning to consistently structure their predictions in a manner that ensures retrospective insights (Mac Nally *et al*. 2000; Ferraro and Pattanayak 2006; Seddon *et al*. 2007; Bottrill *et al*. 2008). These lapses have recently created the belief among some individuals that prescriptive prioritisation must be used to rein in threatened species managers and teach them about cost effective thinking. Unfortunately, the focus on parameters has failed to recognise that what we don't know and haven't learned typically negates the value of priority rankings. If we are to foster cost effective behaviour in threatened species management while sustaining its adaptive flexibility, a framework needs to be devised that ensures the limitations of prioritisation models are fully recognised and results are used heuristically rather than prescriptively. This is far more difficult for bureaucracies than it might seem, as the power of a single, simple answer is enticing to busy people. The reality is that if models purported to be a decision support tool are repeatedly presented as the 'correct method for choosing management priorities' (e.g. AEDA 2009) they are likely to morph into automated decision making by those with only limited information on the underlying problems and ramifications. To provide genuine decision support, an emphasis needs to be placed not only on the priority ranking itself but also on input quality, insights derived from uncertainty, explicit recognition of constraints and the role of adaptive management in moving forward wisely.

So, rock wallaby ecologists find themselves between a rock and hard place. I have heard some public servants assert that their role is to 'make it work regardless of quality', to 'provide guesses so they don't get worse ones from someone with even less knowledge' and thus to 'just get it over with'. Nonetheless, professionals are in an excellent position to illuminate model constraints by just saying 'No' when requested to make firm pronouncements on impossible questions. If faulty approaches will sideline good adaptive conservation effort, then it is our role to educate our management partners that a process founded on contrived wagers and rankings that are carved in stone will not yield the desired result. It is our role to examine new data, especially conflicting information, in order to guide bureaucratic interpretation of conservation priorities and lift our own game. It is our role to ensure that models and expert opinion are used heuristically rather than letting the tail wag the dog. And it is our role, on a day by day basis, as we have been doing for years, to determine where resources are best placed in light of our increasing knowledge, to seize unanticipated conservation opportunities that are impossible to predict in advance and to practice management techniques that ensure cost effective advances.

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