CHAPTER 8: CONCLUSIONS

8.1 Role of conservation science

Caughley's (1994) 'Directions in Conservation Biology' has been described as the defining publication in the field (N. Leader-Williams, verbally). Caughley surmised two philosophically and procedurally divergent approaches to conservation biology; the small population paradigm, which 'deals with the effect of smallness on the persistence of a population,' and the declining population paradigm, which engenders 'an investigation to discover the cause of a decline and prescribe its antidote.' I.e. the small population paradigm concerns the impact of Allee effects and stochastic processes on the survival of numerically small populations, and its seminal contribution is population viability analysis PVA, which has contributed conceptually to protected area planning as well as the management of some individual populations. The declining population paradigm is in contrast an essentially pragmatic approach aimed at discovering the cause of a decline once it has been recognised. Caughley laid out a 4-step ideal process, requiring a general ecological study, listing of potential explanations of decline, statistical tests of spatial or temporal correlations associated with potential explanatory factors, and finally an experimental test to confirm the implicated cause.

Whether the entirety of conservation of conservation biology can be shoe-horned into this dichotomy is questionable; there is a macro-ecological dimension to the field which does not sit particularly easily in either. But it certainly does encompass much and highlights a fundamental difference in focus, which is one of the factors confounding the correlates of extinction literature mentioned in chapter 1. Pimm *et al*'s. (1988) suggestion, that studies on correlates of extinction risk should focus on 'normal' extinctions, which are 'not clearly attributable to man's destructive influences', i.e. not related to clear exogenous drivers of decline, is in clear contrast to those studies concentrating on links between local extinctions and patterns of human activity (Kerr 1995; Channell 2000; McKinney 2001).

The academic appeal of the small population paradigm is that it is 'amenable to theoretical examination,' which makes it all the more unfortunate that it 'has not yet contributed significantly to conserving endangered species in the wild' (Caughley 1994). Quantitative measures of environmental and demographic stochasticity require many years of field data, and have only ever been estimated in tandem for a handful of species (Tufto *et al.*2000). The quantitative impacts of genetic factors over time are all but unknowable. The use of PVA models with static vital rates for long-term predictions is hampered by the fact that outcomes tend to converge on a survival probability of either 0 or 1, either side of a knife-edge balance point at which expected growth rate = 0 (Dennis *et al.* 1991; Ludwig 1999; Fieberg and Ellner 2000). Halley's (2003) attempt to stabilise forecasts by adding noise to the population parameter estimates serves only to highlight the issue and does nothing to rectify the problem that predictions of survival probability will be very sensitive to small changes in estimates of vital rates when the net growth rate is close to zero. Failure to take into account changes in the human processes ultimately driving population dynamics limits the usefulness of traditional PVA approaches (Lacy and Miller 2002).

The impression of an applied science desperately in search of an application is reinforced by authors who attempt to demonstrate the relevance of the small population paradigm unfettered by constraints of common sense. Drawing on a perceived lack of genetic diversity in cheetahs, O'Brien *et al.* (1985) inferred that a population bottleneck 10000 years ago was to blame for recent declines in the species. The ensuing technical arguments over whether O'Brien's measurements were representative of levels of heterozygosity within the cheetah genome as a whole (see Caughley 1994) notwithstanding, surely the decline in cheetah populations within the last few decades has far more to do with the spread of human impacts within that time-frame, rather than their genetic status throughout the last ten millennia. In fact the most common practical implication of the small population paradigm has been the adoption of rules of thumb for minimum viable population size, typically the 50/500 rule (Soulé 1987), but these are little more that numerically convenient guesstimates, which hardly justify the theoretical investment in small population processes.

The declining population paradigm is conversely 'relevant to most problems of conservation', but suffers from an 'almost total lack of theoretical underpinning'; i.e. it applies existing ecological techniques rather than defining a new field. Caughley also criticises its application as 'often short on scientific rigour,' citing the Californian Condor as an instance where a lack of due scientific circumspection and thoroughness led to the wrong conclusion being seized upon. It is easy and perhaps instinctive for scientists to decry imperfect empirical rigour, but they need to also consider the constraints under which conservation operates. There are generally two components to any conservation scenario, the ecological and the social environment. Both follow general principles of organisation, but no two communities are identical, be they human or ecological, and neither are they static. In few circumstances are time or financial resources abundant. Rigorous empiricism is expensive, and even in the developed world, were the entire budget of a typical protected area dedicated to research, it could encompass only a fraction of the resident species. Empirical science is efficient where the results from a single intensive study can be applied to many other settings, but without a 'theoretical underpinning,' transferability is low. Where urgent, low-cost answers are required for unique, evolving and irreplaceable systems, a little more pragmatism is required. Methodologies promoted by conservation practitioners emphasize opportunistic learning from natural experiments and proper monitoring of management outcomes (e.g. Margoluis and Salafsky 1998). Idealistic calls for the broadscale application of formal empirical methods, such as the suggestion of setting aside a large proportion of the world's protected areas estate as an unmanaged scientific control (Arcese and Sinclair 1997), have fallen on deaf ears. Managers who follow hunches rather than investing precious resources in detailed experimental studies may on specific occasions be wrong, but that does not necessarily mean they are irrational.

So the small population paradigm is largely irrelevant and the declining population paradigm too methodologically cumbersome. Authors even from within its own ranks have questioned the effectiveness of conservation biology (Ehrenfeld 2000; Whitten *et al.* 2001), but what can be done about it? Caughley suggested bringing the two paradigms closer together by putting more practice into one and theory into the other. Combining the two would seem to suggest models which encompass both the dynamic processes driving declines and the stochastic processes that determine the long-term stability of system equilibria. The problem again is the particularity of the circumstances; how can the process of decline be generalised if its causes are inevitably case-specific? Some types of decline may indeed be intrinsically non-generalisable, but if we allow ourselves a broader perspective than that implicit in Caughley's discussion, then common threads may be apparent between a far greater proportion of cases.

Caughley stated that the purpose of the declining population paradigm was to identify the problem and its cure, but the process of implementing the solution is not discussed and his 4-step approach makes no mention of it. I.e. the implicit assumption is that once the cause has been elucidated, the solution suggests itself, and its implementation is largely a formality. This may well be appropriate where strong and effective conservation legislation exists, and substantial resources can be allocated to individual species, i.e. in the case of protected species in western countries. The cases in which the cause of decline has been extensively researched are naturally those where it is obscure. Usually the species involved or their habitats are not subject to extensive direct exploitation because they are either protected or have little economic value. By limiting himself to such cases, Caughley guarantees that the drivers of decline will appear diverse and intrinsically non-generalisable precisely because they are incidental to human activities, rather than instrumental to them. His examples include species affected by an alien introduction, a specific pollutant and particular crop harvesting practices.

In most other settings and throughout the developing world, the situation is typically reversed; it is the cause of decline which, at least in broad terms, is often all too obvious, and the method of achieving a solution that is obscure. If we wish to generalise in conservation, our models must focus on the commonality of human activity, rather than the specificity of the ecologies involved. Where those activities are incentivised by the status of the conservation resources themselves, quantitative models of human behaviour can be used to predict system equilibria, upon which stochastic biological processes can be overlaid. Even where unaffected by stock levels, a quantitative understanding of how incentives influence behaviour is needed in order to effect anything other than the most crude control measures (see chapter 7).

8.2 Use of economics in conservation

The bioeconomic approach adopted in this study was not pre-determined, but emerged from consideration of processes relevant to the stability of a harvested system in chapter 2. The use of economic behavioural models is predicated on the assumption that incentives can be expressed in monetary terms, regardless of their actual form. The move towards economics from behavioural considerations mirrors the incorporation of explicitly behavioural consideration in economics. The shift from simply identifying theoretical social optima situation to assessing the achievability of outcomes given individual behavioural incentives was noted in chapter 2 in respect to fisheries bioeconomics, where it was realised that ‘models based on global principles, such as “optimal efficiency” or “maximum profit”, are clearly of dubious relevance to the real world’ (Allen and McGlade 1986). This reflected a general movement in economics, however, involving the extension of ‘behavioural economics’ into the sphere of the social sciences (Becker 1957, 1968, 1976; Tommasi and Ierulli 1995) and the reciprocal realisation that behavioural constraints and information flow had profound effects on classical economic processes from land tenure to inflation (Ray 1998; Stiglitz 2002).

Biologists may be more comfortable with behavioural ecology, which has been used to model aspects of human hunting behaviour (e.g. Alvard 1993, 1995b; Rowcliffe *et al.* 2003). But in fact the tools of behavioural ecology, optimality models and game theory, have been borrowed from earlier economics applications, and the provisos to them are analogous between the disciplines. This parallelism is increasingly being realised by economists themselves (e.g. Hirschleifer 1995) and perhaps in future the disdain expressed by the humanities for the application of biological models of behaviour to humans (e.g. see Pinker 1998 for the vitriol directed towards E.O. Wilson over socio-biology), will be tempered by the fact economists were successfully applying the same frameworks long before Tinbergen’s (1958) wayward wasps ever gave birth to the science of behavioural ecology. The only real difference is the choice of currency, which in almost all cases serves as an approximation to the hidden property of real interest anyhow; typically time or energy is used as a proxy for inclusive fitness in behavioural ecology, and monetary values as a proxy for utility in economics. Given the almost universal penetration of market economies, human decisions and preferences are expressed to a greater extent than ever in monetary terms, which makes the adoption of a monetary currency logical even in cases where there is no commercial market for the wildlife products in question.

Economic models of behaviour provide a powerful tool for integration. Hunter decisions of where, what and how to hunt, and of whether to hunt, and managers’ decisions of how best to influence their behaviour can all be evaluated in a common monetary currency, albeit with some difficulty at times. A monetary currency is not flawless, and it has the potential to introduce serious biases into the measurement of value, but it is difficult to imagine another currency which could integrate both individual and social values.

Within the public arena, economic arguments hold most weight, so beyond its direct use in management, conservationists must engage with economics to achieve political gravitas. This does not mean that moral imperatives should be ignored, but they must be couched in economic terms or at least it must be explained why conventional economic considerations fail to account for them. Moral absolutes alone are not workable. Some use, and even loss of biodiversity are inevitable, and once we admit that there is a trade-off, we return to the question of how to value conflicting social objectives, the first recourse of which is again economics.

8.3 Objections to economics

It has proved fashionable in some circles to attempt to disprove economics (Ormerod 1994), or reject the maximisation models upon which it is based (van den Bergh *et al.* 2000). But considerations such as bounded rationality, satisficing and game theory are simply behavioural constraints which act upon the general principle of optimisation. As these modifications allow novel results to be explained in terms of the existing theory, they act to reaffirm, rather than replace it. For biologists accustomed to accommodating analogous constraints within their optimality models, the objections to economics are typically more philosophical than theoretical. Ehrenfeld (1988) summed up the distrust of economics felt by many conservationists, especially biologists:

"It is certain that if we continue in this crusade to determine value where value ought to be evident, we will be left with nothing but our greed when the dust finally settles. . . . . economic criteria of value are shifting, fluid and utterly opportunistic in their practical application. This is the opposite of the value system needed to conserve biological diversity over the course of decades and centuries."

These objections are typically associated with the debate over sustainable use, and 'economic criteria of value' probably refer to market values. Many have rightly attacked the naivety of those who assert that conservation can and should be self-financing:

- It is typically assumed rather than demonstrated that conservation-friendly economic uses are financially viable, and Salafsky *et al.* (2001) found that the majority of their biodiversity enterprise schemes required open-ended subsidy. Even where variable costs are met, the fixed cost of establishing schemes are rarely accounted for and, where foreign consultants are required, may be huge (see the costs of the development initiatives surveyed in 7.6.3 for an indication). Conservation assets are rare almost by definition, and they are generally so not by coincidence, but because they can be replaced with economically more productive alternatives (MacKinnon unpublished). Of course there are cases where the commercial potential of a resource will not have been properly exploited due to a lack of know-how, capital investment or market structures, but assuming that this is always the case is patronizing to local communities, and making it a pre-condition for conservation is extremely dangerous.
- Often the potential profits are overstated because the problems of sustainable harvesting – exclusivity, monitoring, natural fluctuations – are ignored (Struhsaker 1998).
- At the heart of Ehrenfeld's objection is the fact that there is no such thing as sustainability in market economics; commodity values change with technology, fashion, and the influence of substitutes and complementarities. Furthermore, as human populations grow, demand will rise whilst supply falls (Barrett and Arcese 1995, 1998), given which it may be more logical to

> break the cycle of dependency on extraction early, rather than attempt to institutionalize it. Chapter 7 contains some discussion of how short-term interventions may conflict with long-term objectives.

There is a need to recognize common ground in the debate, however. No one would prevent the use of any natural resource, and few would fail to recognize that some elements should be preserved inviolate. It is therefore a question of degree and how to integrate different strategies for different areas (e.g. Peres and Zimmerman 2001).

It is important, however, to distinguish between market values, which are predicated on an ability to derive and exchange exclusive benefits from a resource, and public values not recognized in conventional market transactions, but which should nevertheless be included in economic analyses. Costanza *et al.* (1997) famously tried to evaluate services and products derived from Nature, but they overlooked the fact that economics concerns choices, and nothing has a value in the absence of alternatives. The value of the living world is no more meaningful than a valuation of sunlight, and conservationists must concern themselves with the marginal values of biodiversity or wildlands (Bulte and Van Kooten 2000).

Some economists, e.g. Skonhoft (1998), Bulte and Van Kooten (2000), tend to assume that the marginal value of nature is low, concluding respectively that we must use it or lose it, or that economic estimates of value should be rejected altogether in favour of a precautionary principle. But they do this on the assumption that marginal existence values are negligible for all but the last few of any species. This is not necessarily the case; people may value expansive wildland systems and large-scale natural spectacles far more than remnant curios of biodiversity maintained under intensive management. Economic measures of these values may underestimate them as estimates of willingness to pay presume that respondents have the psychological flexibility to deal with public goods as if they were private commodities. It is perfectly possible for an individual to recognize a public value, but make private decisions contrary to it, acting in their capacities as an individual and citizen respectively (Sen 1981). This capacity for rational but mutually inconsistent public/private behaviour was noted in chapter 7, and suggests that where public values are concerned, relevant decisions should be public, political ones.

Economics is accused not only for failing to fully account for intrinsic values, however, but also of subverting their basis. This is intimated by Ehrenfeld and stated more directly by Henderson (1978 quoted in Ponting 1991): ‘Economics has enthroned some of our most unattractive predispositions: material acquisitiveness, competition, gluttony, pride, selfishness, short-sightedness, and just plain greed.’

On a surface level, such criticisms are facile: normative economics aims to maximise human welfare conditional on existing preferences and subject to technological and resource constraints, and positive economics provides a value-free set of tools with which to accomplish that (Lipsey and Chrystal 1995). Yet, the approach and preoccupations of a discipline inevitably does colour the perspective of those who practice it; it has been shown for example that business students exhibit less environmental concern than their peers (Benton 1994). The fact that market values are far easier to assess than either non-market material values or intrinsic existence values means that they tend to dominate the work and perhaps therefore the psychology of economists. Similarly, discounting and technological progress lend economists a very limited time perspective, even if the current rate of technological change is an historical anomaly and there are good reasons why discounting should not be applied or applied at much lower values to considerations of social value (Price 1993; Portney and Weyant 1999). Keynes famously dismissed long term concerns; ‘In the long run we are all dead’ (Sloman 1997). There is a willingness amongst some economists involved with the efficiency of commercial markets to assume that all human endeavours are best managed according to a single paradigm, and certainly a passing familiarity with the tenets of classical economics can give the public impression that self-interest and an absence of social meddling are desirable.

Economics has to be embraced in order to combat its misapplications and, in some cases, its shortcomings, but conservationists must be careful not to adopt the myopic priorities of many of its existing practitioners. Conservation is defined by the presence of intrinsic existence values, and if it must pay for itself then it is not conservation, it is natural resource management. Most conservationists are far more likely gain inspiration from Aldus Leopold than a balance sheet, and if they lose sight of the source of their own convictions, then they will make themselves an irrelevance. The ultimate choice to conserve is not a private economic decision, but a political decision based on public values.

8.4 Practical problems with economic models

The predictive use of economic models in conservation does present serious problems, however. Economic analysis takes preferences as its starting point, saying little about what they should be. In the absence of markets conveniently expressing these preferences in monetary terms, their valuation is challenging. Chapter 4 illustrated the particular difficulty where the relevant agents represent only a small fraction of the population, so that we are potentially concerned with subjects selected from the tail of some distribution, rather than the hump.

The difficulties are not limited to the present study. Wilkie and Godoy (2001) drew on data from interviews in 443 households and 2.5-year consumption records from 32 households to elucidate patterns of demand for bushmeat in Latin American Amerindian villages, but their results were little more than suggestive. They were unable to estimate own-price elasticity of demand for bushmeat as there was no market and so no prices available for it. On the basis that income elasticities for bushmeat were not significantly different from zero, they ‘tentatively conclude that bushmeat is a necessity, bordering on being an inferior good,’ which is of course a type II error. Cross-price elasticity could only have been assessed by comparison between different villages, which limits their sample size to 43, and the results are doubtful because of the potential for extraneous variables to effect the outcome. For example, if wildlife is more scarce in one village than another, then consumption of it may well be lower and the prices of a substitute higher due to the increased demand, but this does not imply that a negative cross-price elasticity exists in regard to that substitute. Differences between demand points assessed at different locations or times can either represent movements along or shifts in the position of the demand curve. So direct measurement of a demand curve is nigh on impossible unless the host of extraneous variables which might account for a shift of the curve can be controlled. Even if the considerable problems in achieving a reliable quantitative measure can be overcome, we then return to the stumbling-block of transferability; will the results be applicable to another village with a different economy, ethnicity or history, or even to the same village in 10 years time?

The practicability of predictive models depends on the extent to which their components are generalisable across various hunting scenarios or need to be investigated in each new case. In terms of its impact on behaviour, the most important endogenous component of the bioeconomic models developed in this study was the hunting cost function. The form of the relationship between hunting cost and prey density may be affected by many processes. Landscape heterogeneity may provide spatial refuges in which very small populations may enjoy high protection from hunting. This may be combined with changes in prey behaviour as in the NTS simulation, where it appears that the different susceptibilities of prey during the initial decline and the later equilibrium phases may be due to naïve animals not being prepared to exploit the spatial refuges that become available at low density. Herding behaviour provides another clear example from chapter 6 of how prey behavioural responses to low density impact significantly on hunting costs. Another possibility, unexplored within the models presented, is that hunter behaviour may also change significantly with prey density. Hunters in the simulation were able to adapt their search patterns and some of their gross hunting strategy decisions to changes in ibex numbers, but they were not able to develop novel hunting techniques such as tracking animals rather than relying on visual searches.

The abundant potential for qualitative changes in system behaviour at different prey densities goes beyond the simple problem of accurate parameter estimation. This was illustrated by the sampled S&D models in chapter 6, where increased sampling effort did lead to a narrowed range of equilibrium predictions but the fundamental problem with the accuracy of the model was the difficulty of predicting the change in herd size with ibex density, which would not be helped by more scrupulous surveys. The purely economic processes, such as demand and effort adjustment, probably are intrinsically generalisable if the critical determinants can be identified, but of course suffer from the difficulties of measurement discussed above. The final judgement on the performance of the S&D models from chapter 6 must be that they are good for describing the general properties and characteristics of the system, and if the desire is to produce quantitative predictions solely within the correct order of magnitude, i.e. whether equilibrium population size is likely to lie in the 10s, 100s or 1000s, then they also appear satisfactory. But for making precise quantitative forecasts, they are not reliable, even when data are available over virtually the full range of population densities as it was for the construction of the fitted model.

8.5 Future directions

Chapters 2 and 3 showed that to make sense of an exploited system, human behaviour needs to be incorporated into models, and the abundant potential for mis-interpretation if all factors are not considered explicitly and systematically was illustrated with a spatial harvesting model. In practical terms, chapters 4-6 showed that such models to situations where realistically low levels of information exist and still return meaningful results, if not detailed forecasts. But there remains abundant potential for discontinuities and other unforeseen effects to throw predictions out of kilter even within a relatively simple system. Chapter 7 shows that a coherent approach to modelling human behaviour not only provides a platform for analysing the existing state of resource use system, but also for integrating and comparing management approaches for influences their outcomes. The remainder of this section attempts to reveal some of the implications of these lessons and how they can be expanded upon.

8.5.1 Tactical models

One way of avoiding the difficulties of generating accurate quantitative predictions of system equilibria may be to ignore them altogether and concentrate instead on models which identify robust adaptive management strategies which can produce superior if not theoretically-optimal results. Imagine, for example, the manager of a hunting reserve interested in determining his optimal stock density and setting hunting quotas, and budgets for monitoring and anti-poaching patrolling accordingly. As a simplification for conceptual purposes, we might assume that the potential equilibrium hunting quota and therefore revenue will show a monotonic increase with stock level. Density dependent effects on growth might suggest it is a decelerating function of stock level, whereas if demand for sport hunting is sensitive to stock levels, it could be an accelerating function. At any given target density, greater investment in monitoring is likely to allow more precise quotas to be set, and hence to increase revenue, but monitoring expenditure is likely to show diminishing returns. Obviously optimal expenditure will lie at the point where marginal return equals marginal cost, but it is not inherently obvious how the form of this solution will vary with target stock size. Poaching will reduce the offtake available for sport hunting, so investment in patrolling will increase revenue in a similar manner, and again the form of the relationship between optimal investment and stock density may take a range of forms depending on the particular geographical and socio-economic circumstances.

The manager might aim to maintain stock at the point at which the difference between revenue and cost is greatest or at the highest stock density at which revenue covers costs, depending on whether his primary motivation was profit or conservation. The point is that with two sources of cost and one of revenue, all of whose relationships with stock density depend on numerous processes, the optimal solution is not at all clear *a priori*. Resources could be expended on measuring a range of biological, socio-economic and physical parameters that would allow optimal stock, quota and budget levels to be determined, but the solution may well be sensitive to uncertainties in the measurements, and parameter noise or time-related trends. Even if robust, it may not be very transferable to similar problems elsewhere.

The alternative to attempting to define a precise quantitative solution would be to test the outcomes of various adaptive management rules against a whole range of feasible parameter values and assumptions. The advantage of this approach is that it would allow the robustness and transferability of solutions, as well as their expected maximal performance to be evaluated. As noted in chapter 6, even though the specific quantitative predictions of bioeconomic models may differ, they show many qualitative similarities, and it is possible that, as Axelrod (1984) found in a different context, high-performing strategies might also be extremely robust and simple. Even if they were not, the modelling should define critical information needs, help to determine where the performance gains from research justify its costs, and produce rules of thumb and decision tools for use where site-specific investigations are not feasible.

In fisheries science, the parallel development of adaptive management and the International Whaling Commission's management procedure evaluation have led to a Management Strategy Evaluation methodology which incorporates the testing of different sampling and decision strategies (observation and conditioning models respectively) against simulations of the harvested population (operating models) (Sainsbury *et al.* 1999; Punt *et al.* 2001, 2002: also known as Management Procedures – Butterworth and Punt 1999; Geromont *et al.* 1999). Increasing awareness of the biases in traditional stock assessment models and availability of computing power have led to an increasing number of applications in recent years (e.g. Kell *et al.* 1999; Polacheck *et al.* 1999; Adkison *et al.* 2003). Theoretically, the advantage of Management Strategy Evaluation is that it demands the inclusion of uncertainty at all stages, not just in a stock model's parameters, but also in the selection of operating model, and in the observation and conditioning models (Butterworth and Punt 1999; McAllister *et al.* 1999). Practically, the emphasis on responsive management strategies, rather than simple prescriptions of stock level or offtake derived from complex stock assessment models, has facilitated greater understanding, participation and compromise amongst stake-holders (Butterworth and Punt 1999; Geromont *et al.* 1999; Smith *et al.* 1999).

Stochastic operating models may take the form of Monte Carlo simulations or stochastic dynamic programming, and examples of both are starting to appear in the conservation literature for the evaluation of management strategies (e.g. Milner-Gulland 1997 for Monte Carlo; McCarthy *et al.* 2001, Milner-Gulland *et al.* 2001 and Westphal *et al.* 2003 for stochastic dynamic programming). The latter has the advantage of guaranteeing that the optimal solution is found (Westphal *et al.* 2003), but is less accessible and restricts the range of models that can be used. Besides, in real scenarios, the robustness of a management strategy will probably be more important than its absolute performance at any given set of parameter values. There is scope to extend these tactical management models to include a greater element of human behaviour both within fisheries and conservation. In conservation in particular, general models assessed over a wide range of parameter space may help to define the broad categories of management approach appropriate to different scenarios. The common thread to all these approaches, and to the researches of the "virtual ecologist" referenced in section 6.1, is the emphasis either explicitly or implicitly on some kind of decision analysis framework. Approaching models as decision tools inherently tends to their use as interactive environments for developing management strategies, rather than simply as calculators of extinction risk or stock level.

8.5.2 Development of quantitative behavioural models

Possibly Caughley was correct to ignore explicit considerations of human behaviour as his concern was with conservation biology, not some more inclusive academic treatment of conservation. If conservation biology cannot accommodate such areas, however, then maybe it should be replaced, as disciplinary aloofness will prove costly. But however we choose to define the subject, there is a need for biological insight, not just in the ecological components of our theory, but in the behavioural component as well. The foundations of economic analysis are the assumptions that private choices should reflect private utility, social utility is the sum of private utility, and as many choices are perforce expressed in monetary terms, money is a good proxy currency for utility. Factors such as wealth and risk preference distort the correlation between money and utility; but it is usually considered a close enough working relationship for practical purposes. Economists find it harder to accept that choices themselves may often not reflect utility. They are of course aware of non-utilitarian preferences such as Verblen effects (Verblen 1970), but these are uncomfortable peculiarities, that do not find their way into general models. Hence, as touched on in chapter 7, the economic treatment of risk preference cannot account for enjoyment of risk or addiction to gambling, except as a form of pathology, because risk for its own sake cannot be understood from a utilitarian perspective.

Biologists bring a very different perspective, however as they are used to working closer to the ultimate currencies in which behaviour is reckoned, and are aware that the force which has shaped psychology – evolution – operates on relativities, not absolutes. To economists, observations that wealth does not equate to happiness are inconvenient. For biologists it ought to be axiomatic that much consumption is for the purposes of gaining and advertising relative status and therefore represents an unbounded, zero-sum arms race. Whether the glass is half empty or half full depends on the status of the next man's glass. Perceptions of poverty are relative, and if inequality increases, the psychological effects of poverty mentioned in chapter 7 may deepen even if absolute poverty is reduced in general. This is important to the prosecution of ICAD because many modern, market-based development initiatives may actually increase inequality (Robinson and Bennett 2002).

Biological insights into behaviour are important in the broad, philosophical debates; optimism that environmental problems can be solved entirely through increased efficiency (Hawkin *et al.* 1999) for instance, should be tempered with the reality that consumption has always managed to outstrip productivity gains. If broad evolutionary insights and the specific constraints of evolutionary psychology can be added to quantitative behavioural models, however, they may allow us to estimate preferences not expressed through markets, including a more mature understanding of risk and time preferences, and provide more precise definitions of the conditions under and extent to which human choices do not promote social welfare.

8.5.3 Inclusivity

Conservation science must be applied, and must be concerned with increasing the capacity of managers to achieve conservation, rather than simply providing an erudite analysis of the loss of biodiversity. The essential levers for manipulating human behaviour – penalties, inducements and distractions – have been used since the dawn of society, and to this extent, new additions to the managers toolbox should not be expected. There is still much work to be done in determining where and how to use these tools, however. Too often the debate could be characterised by the question of which is the best tool, a hammer or a screw-driver, and has been partisan and acrimonious. Instead it should focus on where to apply a nail and where a screw, recognising that all conservation approaches extend our capabilities, but none is infallible.

Fortunately, we now have modelling approaches and the requisite computing power to provide the practical guidance and decision tools mentioned in section 8.5.1. Whilst spatial models clearly show that global harvesting is more productive (chapters 2 and 3), the greater ease of assessment may yet make spatial population management at the metapopulation level more efficient than density-based management. Suitably broad spatial models should be able to reveal which biological characteristics of the resource and social characteristics of the exploiters favour management options such as no-take zones or rotational harvesting. Multi-agent systems show how the outcomes of principal-agent models evolve as the composition of the pool of agents is influenced by the conditions placed upon them, and are clearly relevant to many instances of conservation management and resource exploitation (e.g. Bousquet *et al.* 2001; Walker and Janssen 2002; Parker *et al.* 2003). Conservation practitioners are beginning to demand the freedom to flexible, pragmatic approaches, rather than being constrained by a single overarching management philosophy (e.g. Baird and Dearden 2003; Polet and Ling 2004), and it should be the goal of conservation academics to enhance their ability to do that.

Whilst we must be more inclusive in regard to the means, we must be more focussed in regard to the ends. Poverty, corruption, conflict and a host of other social ills may make effective conservation more difficult, but their end will not come soon and cannot be the responsibility of or a prerequisite to conservation. Where tackling social obstacles provides a demonstrably efficient mechanism for making conservation gains, or where a conservation issue attracts the attention of those agencies in whose remit they lie, then all is well and good, but conservationists should not tie their own hands with social preconditions or have the temerity to imagine that their twopence-worth will make a fundamental difference (MacKinnon unpublished).

8.5.4 Sustainability

Conservation is not the same as development. Whilst the latter is in the direct private interest of those it impacts, the former, though in the public interest, may or more typically may not coincide with individual economic interests. The principle of sustainability applied to local development projects, i.e. giving beneficiaries the capacity to achieve the ends for themselves, cannot be transferred wholesale to conservation. Sustainable use is a dynamic outcome, not a system which can be set in motion and then left to function in perpetuity. Those cases where conservation can pay for itself by reorganizing or redirecting systems of use are the relatively easy ones. But even here it cannot be assumed that a few years of funding during the transitional phase will be sufficient, firstly because the changes involved may take a long time to effect (Salafsky *et al.* 2001), and secondly because the relative values of products and services are not set in stone. Economics is largely a science of change, and continual reassessment of strategies which rely on current market conditions. Whilst these cases might survive on periodic external inputs, in most cases continuous external funding will be necessary even if elements of use are included in the approach (Wells *et al.* 1999, Salafsky *et al.* 2001). In addition to being inadequate, short-term funding can even be prejudicial (MacKinnon unpublished).

The principle of sustainability applied to conservation must therefore be financial sustainability and the political sustainability needed to achieve it over the long-term. The seminal contribution of the ICAD revolution has been the realisation that unnecessarily antagonistic and insensitive approaches to conservation can undermine political sustainability at the local and eventually national and global level. There is a wider point that development is about providing choices (Sen 99), and in order to conserve, people must be able to choose to do so. But economic growth alone is not sufficient (Dietz and Adger 2003, Naidoo and Adamowicz 2001) and conservationists need to avoid throwing out the baby with the bathwater.