DOCTOR OF PHILOSOPHY

Cost-benefit analysis for global environmental issues with a case study of biodiversity conservation in Madagascar

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A dissertation submitted to Bangor University in application for the degree of Doctor of Philosophy

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Abstract

Humanity faces a formidable challenge in deciding how to respond to global environmental issues, such as climate change and biodiversity loss. This thesis explores the use of cost-benefit analysis (CBA) in this context. I start in Part I by highlighting the problems with conventional CBA, which include its treatment of the future, and its approach to aggregating costs and benefits across individuals. The rest of the thesis comprises empirical investigations of these two issues.

In Part II, I carry out the first rigorous statistical analysis of the economic scenarios developed by the Inter-governmental Panel on Climate Change, which have been criticised as being implausible relative to the historical data. I show that for two of the three regions considered, the criticisms are unfounded, but for the other, the IPCC scenarios are considerably more optimistic than is suggested by the data. In addition, I show that economic growth is poorly described by an exponential function, undermining conventional approaches to discounting. Next, I use data on global and national Ecological Footprints to consider whether environmental limits will prevent future economic growth. I find that previous studies using this data have been biased towards pessimism by ignoring technological progress, and I present two novel analyses that incorporate it. I find no evidence that future economic growth will necessarily result in increasing global Ecological Footprint.

In Part III, I carry out a partial CBA of a biodiversity conservation project to protect the Ranomafana-Andringitra forest corridor in south-eastern Madagascar. I begin by investigating the linkages between economic growth, urbanisation, and forest cover change and then develop projections of deforestation and species extinction. I show that the local opportunity costs of conservation are likely to be positively linked to its biological urgency, and hence its global benefits. Although the benefits of conservation strongly outweigh the costs in monetary terms, when corrected for the diminishing marginal utility of income, the net present value of the project is strongly negative. Therefore unless compensation is complete and efficient, conservation will reduce social welfare, due to the extreme poverty in the region. Because complete compensation is difficult to achieve, these results call into question the present emphasis on directing conservation efforts towards low-income countries.

The thesis shows that the simplifications inherent in conventional CBA can produce misleading results when applied to complex global environmental issues. In particular, assuming that compensation will be complete and costless could encourage decision-makers to adopt projects that are seriously detrimental to social welfare. CBA cannot be both value-free and decisive. I therefore outline a generalised CBA approach that is capable of incorporating important societal value judgments.

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Glossary and abbreviations

Acronyms	
ALM	*Africa, Latin America and the Middle East.*
ASIA	*Asia, including the pacific.*
CBA	*Cost-Benefit Analysis*
CBNRM	*Community-Based Natural Resource Management*
CV	*Compensating Variation*
EKC	*Environmental Kuznets Curve*
ICDP	*Integrated Conservation and Development Project*
MER	*Market Exchange Rate*
OECD90	*Member countries of the Organisation for Economic Cooperation and Development in 1990: North America, Western Europe, Japan, Australia and New Zealand.*
PPP	*Purchasing Power Parity*
REF	*Countries undergoing economic reform: Eastern Europe and the former USSR*
Foreign terms	
Fady	*Taboo*
Fokontany	*Fokontany are the lowest administrative unit in rural Madagascar,*
Teviala	*Swidden agriculture*
Transfert de Gestion	*Transfer of forest management rights and responsibilities to communities*

Table of Contents

1. Introduction

What should we do when scientists tell us we are changing the planet's climate or driving its species to extinction at an unprecedented rate (Chapin et al. 2000, IPCC 2007)? These vital questions face humankind today. Failure to act may have very serious consequences for the welfare of future generations. Yet even to slow the rate of anthropogenic climate change or extinction may require considerable sacrifices from the present generation, of which one in six are undernourished (UNDP 2007).

These questions are a severe test of the predictive capabilities of science. The causes and effects of climate change and biodiversity loss span continents and centuries: the full environmental consequences of burning oil in our cars will not be felt for many years to come, and could affect the lives of people thousands of miles away. Any decision we make must take account of this, yet predictions of the environmental consequences of action or inaction are clouded by large and unquantifiable uncertainty (Grübler & Nakicenovic 2001). This uncertainty only increases when we try to understand the implications of environmental change for people living in different countries and generations (Solow 1999).

These issues do not just test our technical abilities, but also raise difficult moral questions. How should we weigh our own welfare against that of the as-yet unborn (Price 1993, Portney & Weyant 1999a)? What rights do we have to use natural resources (Howarth 1995)? Do the rights of the poor differ from those of the rich? How much should we sacrifice today to avoid highly uncertain catastrophes (Arrow 1999, Weitzmann 2007)? Is it irresponsible to trust in continued technological progress (Costanza 1995)?

Perhaps the severest test posed by global environmental issues is of our capability to aggregate diverse information, weighing the interests of rich against poor and generation against generation, in order to decide whether and how to act. Cost-benefit analysis (CBA) is a procedure which aims to assist society to make decisions about public policy and has been applied to global environmental issues

(e.g. Balmford et al. 2002, Stern 2006). However, such analyses have been controversial.

Some dispute the particular methods used (e.g. Dasgupta 2007c, Nordhaus 2007b, Spash 2007b); others reject the practice of CBA itself (e.g. Collar 2003, Ackerman & Heinzerling 2004). Certainly, global environmental issues provide a stern test of CBA, stretching it well beyond the circumstances for which it was originally conceived (Hanley & Spash 1993). Methodological conveniences that may be harmless when deciding whether or not to build a bridge might, if repeated in an analysis of climate change, lead to recommendations that would be catastrophically detrimental to human welfare. And yet, even in these complex issues, "the beans have to be counted" (Solow, 1999:vii). Decisions have to be taken, and whatever method is used, the same information must either be aggregated and weighed, or else ignored. This suggests a need to re-evaluate CBA for application to complex issues (Sen 2001). Reappraising CBA from the perspective of global environmental issues might pay other dividends too: many of the issues raised relate to the fundamentals of economics - its "scope and method" (Harrod 1938). How should economics relate to ethics (Hausman & McPherson 2006)? Does economic science have predictive power, beyond the minutiae of everyday life (Ormerod 1995)? How *useful* is economics to mankind (Harrod 1938, Adler & Posner 2006)? Does the advice of economists deserve to be heard by statesmen or confined to the marketplace?

Objectives and plan of the thesis

The preceding is full of big questions. These have intrigued me, and motivated this thesis, but I can make no claims to have answered any of them. What follows in subsequent chapters is an account of my own explorations of this extremely important issue. The thesis is organised in four parts. The next chapter (the second of part I) reviews the theory and practice of CBA from the perspective of global environmental issues. The chapter has two aims: the first is to show that CBA can make a useful contribution to decision-making in society - to defend it against its critics; the second is to demonstrate that CBA, as currently practiced, must be modified to be defensible. In particular, I explain the importance of making explicit one's assumptions about the future (including future income growth), and

argue for a reformed approach to aggregation and compensation. The former point underlies the work in Part II, and the latter provides the rationale for Part III.

Chapter 3 begins Part II, by investigating the controversy over the best-known long-range economic projections, those of the Inter-governmental Panel on Climate Change (Nakicenovic & Swart 2000). I enter the debate on the terms set out by the protagonists (Castles & Henderson 2003a,b), asking whether the projections are 'reasonable' compared with the historical data. Through these analyses, I develop extrapolations from past income growth and income convergence, which are used in subsequent chapters.[1]

Chapters 4 and 5 address another question that is always important, but especially so in CBA of environmental issues: can income growth continue indefinitely, as assumed in most CBAs? After reviewing the wider debate, I focus more narrowly on empirical analyses that use Ecological Footprint to investigate this issue. After an analytical review of these studies (Chapter 4), I carry out an analysis of the relationship between Ecological Footprint and income per capita at the global level (Chapter 5). Chapter 6 concludes Part II, by combining the results of the three previous chapters to project per capita income and Ecological Footprint over the coming century.

In Part III, I use a case study of a proposed biodiversity conservation project in Madagascar to explore issues of aggregation and compensation in CBA. Chapter 7 introduces biodiversity conservation in general and the case study in particular, while Chapter 8 develops projections of deforestation in the absence of conservation action. Chapter 9 further develops this scenario, linking deforestation to species extinctions, and asking what might be the value of avoiding these extinctions. Chapter 10 considers the local opportunity costs of conservation, first describing the considerable difficulties associated with measuring them and then developing internally consistent projections. Chapter 11 concludes Part III, by aggregating these costs and benefits of the conservation project, demonstrating the enormous importance of adopting an appropriate approach to aggregation and compensation. Finally Chapter 12 concludes by summarising the key findings of

[1] In the appendices to this chapter, I delve deeper into this debate, investigating the internal consistency of the purchasing power parity scenarios.

the thesis, and making some general observations on the potential of CBA to contribute to decisions on global environmental issues.

2. Cost-benefit analysis for global environmental issues: a review

> "[Cost-benefit analysis] is a practice that has no theoretical justification. The original objections to CBA have never been rebutted". (Adler & Posner 2006:12).

Abstract

Cost-benefit analysis (CBA) is an explicit valuation procedure used by economists to inform societal decision-making. CBA was originally used to evaluate relatively small projects whose benefits and costs were largely monetary. Now, however, it is frequently applied to much more complex policy questions, including those relating to global environmental issues such as climate change and biodiversity loss. I review the literature on the theory and practice of CBA in order to determine whether conventional approaches are able to meet the challenges posed by these issues. I join previous authors in recommending that economists should abandon their expectation that meaningful results can be obtained from a value-free procedure and should reject the quasi-decisive role of conventional CBA. Furthermore, I show that the products of conventional CBAs (benefit-cost ratios or net present values) are uncertain in meaning, or socially constructed, and therefore unsuitable subjects for scientific meta-analysis. I outline a generalised CBA procedure which allows for the incorporation of decision-maker preferences, including deontological concerns, as well as explicit compensation mechanisms, thereby addressing the problems of the conventional approach. I also highlight the importance of explicit income projections for inter-temporal CBA.

I. Introduction

The previous chapter introduced the global environmental issues facing mankind in the 21st century, including climate change and loss of wild nature. These issues share a number of important characteristics which make them challenging for society to confront. They affect many different nation states, spanning numerous cultures and systems of government, and long time periods. There are very large disparities in wealth, information and power between the individuals affected. The consequences of action or inaction could be serious, and costs and benefits large relative to incomes, yet considerable uncertainty surrounds any estimates.

Faced with these challenges, society must have a way of deciding what actions should be taken. Although elected or appointed officials will make the final decision, experts, including economists, may assist the process. One tool developed by economists for assisting public decision-making is cost-benefit analysis (CBA). The aim of the chapter is to review the theory and practice of CBA, keeping in mind the difficulties posed by global environmental issues.

What is CBA?

Sen (2001) proposed three "foundational principles" of CBA, which represent the lowest common denominators of rival approaches, and would be expected to command the widest support. The first is "explicit valuation": tradeoffs are explicitly acknowledged and measured. The second principle is "broadly consequential evaluation". CBA is concerned with the consequences of a project, but Sen allows for a broad scope, in which consequences might include the violation of rights, not just effects on income or welfare. Sen's third principle is "additive accounting". The consequences of a project are rendered commensurable and then aggregated, culminating in an ordering of the project and status quo on the basis of their social desirability (or some aspect of it).

These principles allow for a diversity of approaches to CBA. However, what I will term conventional CBA, which accounts for the majority of analyses, is considerably narrower (Sen 2001). It adopts a narrowly consequentialist perspective, tending to ignore motives and rights. With some exceptions, the product of a CBA is usually a simple sum of the monetary valuations of project

consequences, which may have been limited in scope. Costs and benefits occurring in the future are normally discounted exponentially (Price 1993). This is the CBA most often practiced and the CBA which has recently been defended by Adler and Posner (2006) in what the authors set out at as a fundamental reappraisal of CBA. It is also the CBA which is advocated, sometimes with some adjustments, in most textbooks (e.g. Johansson 1993, Pearce 2006). Finally, it is the CBA which is employed in each of the five studies identified by Balmford et al. (2002) as being the only CBAs of nature conservation published at that time. One aim of this chapter is to discuss what conventional CBA can tell us.

CBAs of global environmental issues, for example climate change (e.g. Stern, 2006) have been hotly debated (e.g. Nordhaus 2007b, Weitzmann 2007, Spash 2007b). This is not surprising, since the application of CBA to even relatively simple issues has been controversial (e.g. Ackerman & Heinzerling 2004). This controversy has probably been fiercest in the US, where CBA has been adopted by government agencies and the courts, to a greater extent than elsewhere (Pearce 1998, Adler & Posner 2006, Hahn & Tetlock 2008).

The criticisms of CBA take many forms, and motivate this chapter. However, before introducing the valid criticisms, it is worth dispensing with one criticism which lacks merit. In some cases, critics of CBA reject the existence of trade-offs, believing that their preferred policy has no costs (e.g. Ackerman & Heinzerling 2004:10). Since all government actions require at least some resources, this is unlikely to be true. Policies without losers are rare (Just et al. 2004:14-15), and not always desirable (Hausman & McPherson 2006). It is also inconceivable that decision-makers[2] would ever be faced with exactly one, costless, alternative to the status quo. For a decision-maker faced with two or more options, choosing one policy over another will always have opportunity costs (e.g. Hahn et al. 2000). One role of CBA can be to improve a policy, even once an agency has committed to its implementation (Hahn & Tetlock 2008). While the ubiquity of trade-offs may be regrettable, shooting the messenger is not constructive.

[2] Throughout the chapter I use “decision-maker” to refer to whoever makes the final judgment over which if any project to implement. This might be elected representative, appointed officials, or the electorate as a whole. It could conceivably be the members or board or any organisation concerned with social welfare, whether public or private, or even a private citizen.

Another, related objection to CBA is to hold that one's preferred policy should be implemented whatever the costs to others. A good example from conservation is Collar (2003). Many conservationists have expressed concern that CBA may not give the result desired, at least by conservationists (e.g. Bulte & van Kooten 2000). While it may be appropriate for conservationists to fund only those CBAs of conservation which are likely to prove helpful to their cause, when applied to wider society this becomes profoundly dystopian (Price unpubl.).

Once the ubiquity of trade-offs is acknowledged, and elitist disregard for other's preferences ruled out, all of the remaining objections to CBA, either in principle or as currently practised, have some merit. I introduce them briefly here, and return to them throughout the chapter.

Some critics argue that explicit, quantitative valuation of costs and benefits is neither possible nor a helpful way to proceed (e.g. Heinzerling 1998) or that CBAs are systematically biased because they ignore intangible benefits or overstate the costs of regulation (e.g. Ackerman & Heinzerling 2004). Because it focuses on welfare effects, CBA is criticised by some for ignoring the moral validity of an action, or whether that action infringes an individual's rights (Sen 2001). Others object to the assumption of a strictly utilitarian framework, which assumes that benefits to one person can compensate for losses to another. Also, CBA may give greater weight to the preferences of rich people and be indifferent to the distribution of costs and benefits in society (Adler & Posner 2006). CBA often appears to treat catastrophic losses in the same way as small ones, and to ignore non-linearity and irreversibility, endangering the environment and future generations (Edwards-Jones et al. 2000:122). Finally, the use of discounting in CBA is held to be unfair to future generations and to lead to myopic policies (e.g. Price 1993, Ackerman & Heinzerling 2004, Spash 2007b).

The role of CBA

Whenever decisions are taken all pertinent information has either been weighed and aggregated or ignored, whether implicitly or explicitly. The importance of this point cannot be overstated: howsoever society arrives at a decision, it must have assigned relative importance to material prosperity, environmental quality, human health, equity, property rights, individual freedoms and more else besides. CBA,

broadly defined, represents an attempt to make explicit the various factors which are pertinent to a decision. The question at hand is; how should CBA aid society's decision-making processes? Should the results of CBA be decisive? Should CBA attempt to incorporate everything, or do some things lie beyond its scope? CBA does not stand alone, but forms part of society's institutions. It is not possible, therefore, to consider what makes a good CBA method, without anticipating the way in which it will be used. CBA cannot be designed or evaluated on theoretical grounds alone.

Sunstein (2001) has argued that CBA should be viewed primarily as a mechanism for countering cognitive biases present in the general public. For him, the chief advantage of CBA is that it brings "on-screen" those costs and benefits which might otherwise remain "off-screen". For example, he argues that when considering safety legislation, the public is inclined to see only the benefits (reduced accidents) and not the costs (lower wages, higher prices), which may be dispersed but significant.

Sunstein's point is undoubtedly valid, but we must surely also consider the cognitive biases and limits of at least two other groups: CB analysts, and decision-makers. If analysts act as gatekeepers, determining what is included in a CBA, it is possible that cognitive biases among analysts would lead to some important considerations being omitted from the CBA. If an incomplete CBA appears comprehensive, and therefore decisive, to the decision-maker, CBA may actually do more harm than good. For example, Ackerman & Heinzerling (2004:7) document cases where important benefits have been omitted from CBAs, which have then been used to undermine the case for a particular policy. The same is probably true of the many caveats which are, or should be, attached to conventional CBAs: if these caveats are omitted or receive less attention than the headline results, CBA may distort rather than inform decision-making.

If decision-makers realise that CBA is neither comprehensive nor decisive, they must weigh up the results of the CBA against other factors which have been excluded. If decision-makers have a tendency to over-weight numerical results over caveats (or vice-versa), then once again, CBA as a partially explicit procedure may do more harm than good. Heinzerling (1998) argues that decision-makers do overweight the results of CBAs. However, the evidence reviewed by

Hahn and Tetlock (2008) suggests the opposite: US decision-makers tend not to accept the results of CBAs. Whether their decisions reflect a considered opinion that omitted factors swing the result, or a simple rejection of CBA as flawed, is difficult to tell.[3]

However, it is not necessary to invoke the cognitive biases of decision-makers in order to argue that CBA could be improved. By convention, CBA leaves out a host of important factors, which decision-makers must then weigh against its results, and offers no guidance as to how this might be achieved. Do economists not possess skills which could assist decision-makers in factoring in the various "intangibles"?

In the context of global environmental issues, and in conservation in particular, insufficient attention to issues left out of CBAs could result in society adopting undesirable projects. For example, if conventional CBA showed that biodiversity conservation was desirable (e.g. Balmford et al. 2002), despite the costs being born disproportionately by poorer people (e.g. Balmford & Whitten 2003), then inadequate attention to issues ignored by conventional CBA (e.g. rights, compensation and distributive justice) might lead to conservation being pursued in a way which, when properly considered, was socially undesirable compared not only to ideal conservation, but even potentially to the status quo of no conservation.

[3] Hahn & Tetlock (2008) note the poor correlation between CBA results and the decisions taken, but does not consider that this reflects the perceived limitations of the CBAs themselves, despite documenting the far from perfect nature of most CBAs reviewed.

II. Explicit valuation of project consequences

In this section I review the way in which CBA approaches the first two of Sen's fundamental principles; how it explicitly values the consequences of projects. Although this is a far from trivial matter, my aim is to show how the many criticisms can be either rebutted, dealt with, or deferred until the next stage, additive accounting.

Sen's (2001) foundational principles do not define which consequences CBA should measure. A project may have consequences for the welfare[4] of an individual and for their rights. Rights have conventionally been excluded from CBA (Sen 2001:100), and I discuss the degree to which these can be separated from a consideration of welfare.

The focus of CBA: welfare, utility and preferences

The meaning, measurement and moral importance of welfare have been the object of considerable investigation and debate by economists (e.g. Sen 1982), philosophers (e.g. Griffin 1986) and psychologists (e.g. Kahneman et al. 1999), and no definition receives unanimous support. Against this backdrop, economics defines welfare relative to the individual's own preferences.[5] Thus the possession of something is defined as increasing welfare if the individual (after the fact and with full knowledge) prefers having it to not having it. The same object or experience may be welfare enhancing for one individual, and welfare decreasing for another. In this, and all subsequent chapters, I use 'utility' to refer to this conceptualisation of welfare. Later, it will become necessary to distinguish between that which the individual maximises (utility), which may include 'disinterested' (altruistic) preferences, and a more narrow conception of welfare, which is the focus of 'self-interested preferences (Adler & Posner 2006). I use welfare to refer to this more narrowly defined concept.

[4] I treat welfare, wellbeing and quality of life as synonymous, and use welfare throughout.

[5] Economics, on the whole, does not concern itself with the origins of an individual's preferences: these lie within the remit of psychologists, sociologists and socio-biologists.

Relying on the economist's concept of utility, CBA estimates the monetary value of projects in a very specific sense. CBA measures the amount of money which would have to be taken from a person, following adoption of the project, such that they consider themselves to be as well off with the project as they would have been without it, i.e. their utility would remain unchanged. This sum of money is the compensating variation (hereafter CV) which will be positive if the person gains from the project. If the CV is made, the person is said to be indifferent between the project and the status quo.

CVs can sometimes be estimated using preferences which are revealed by the individual's observed behaviour. However, many project effects are difficult to value in this way, in which case stated preferences may be solicited, using contingent valuation methods (Bateman et al. 2002). Here, respondents are asked to choose between the status quo and a project plus a certain level of CV. Thus, their CVs are the maximum they would be willing to pay for the project, if they expected to gain from it, or the minimum they would be willing to accept as compensation if they expected to lose from the project (Adler & Posner 2006:167.[6] Of course, there are many technical difficulties associated with estimating CVs from either revealed or stated preferences (e.g. Johansson 1993, Hanley & Spash 1993). However, I am not primarily concerned with the techniques for estimating CVs in this chapter (issues relevant to the case study are discussed in Chapters 9 and 10). Instead, I assume that unbiased and tolerably precise estimates of true CVs can be obtained with sufficient effort, and the CV to which I refer is the true, rather than estimated, CV.

Incommensurability

A recurrent objection to CBA is that some things which CBA seeks to measure are in fact incommensurable, meaning that CBA is flawed (Ackerman & Heinzerling 2004, Spash 2007b). In its strongest form, incommensurable can mean that "two items cannot be compared quantitatively at all; the one is neither greater than, nor less than, and not equal to the other … 'incommensurable'

[6] The equivalent variation (EV) of a winner is their willingness to accept (WTA) the status quo, and of a loser, their willingness to pay (WTP) for the status quo. Thus, changing from measuring CVs to EVs in effect reclassifies the project as the status quo, and the status quo as the project. EVs often do not equal CVs, since WTA often differs from WTP (Adler & Posner 2006:167).

[means that something] cannot be fitted onto any scale of measurement" (Griffin 1986:77, emphasis in the original). Yet, in the context of CBA, incommensurability is taken to mean that society cannot (and economists should not) measure the value of something (e.g. nature, human life) in monetary terms. Discussion of whether this is true is best postponed to the next section. Here, it is simply useful to identify what non-commensurability means, in the context of individual-level estimates of CVs.

Incommensurability implies that no increase in income can compensate for a decrease in that which is incommensurable. If this is the case, the CV cannot be estimated. The CV might be thought of as being infinite, though, strictly, this would imply some commensurability. In fact, true non-commensurability is inconsistent with the existence a one dimensional quantity as utility. Any CBA in which a CV could not be estimated for at least one project consequence and one individual because of incommensurability, would have to address this issue.[7]

Compensating variations as monetary valuations

It cannot be over-emphasised that CVs monetise the consequences of projects only in a very specific sense. They are not necessarily cardinal, nor are they necessarily comparable between individuals, or between the same individual at different times. A CV measures only the quantity of money which the individual must be given or deducted in order to be rendered indifferent, in their own judgment, between the project and the status quo. This conception of CV will be retained throughout this section; only in the next section will I address the question of inter-personal comparability and cardinality of CVs.

[7] Note that true incommensurability cannot result simply from a *refusal* to state a CV, or even an *inability* to state a CV, but rather from the *non-existence* of a CV. In other words, under true incommensurability, a person's welfare after the project could not be brought back up to pre-project levels, regardless of the amount of money given to that person, or spent on that person's behalf. Thus, the existence of "protest responses" in contingent valuation surveys, where some individuals refuse to state their willingness to pay or accept (e.g. Dziegielewska & Mendelsohn 2007), may be evidence of incommensurables, or it may be evidence of poor survey design, informational or cognitive constraints, or strategic behaviour.

Some objections to compensating variations

Perhaps not surprisingly, numerous objections have been raised against the economist's approach to monetising costs and benefits using compensating variations, and here I review those I consider to be the most important.

Information

Because CBA defines utility according to the individual's own judgment, the individual is the principal measuring instrument used by the analyst. As with all instruments, the measurements thus obtained are subject to error. In particular, CBA may ask individuals to rank actions that may affect their utility, rather than states which they will experience. The individual may misjudge the effect that a particular action will have upon their utility. Any limits on the information available to the individual, or on their ability to process that information, may distort their preferences, and thus decrease the accuracy of the estimated CV (Adler & Posner 2006). In effect, we can define any discrepancy between preferences for certain actions revealed or stated at the time of the CBA, and preferences for certain states when they are experienced, as being due to failures of cognition or information. Of course, any CBA conducted with the aim of informing a decision must be conducted ex ante, and may concern consequences with which the individual is not familiar. A critical factor for the success of CBA must be to minimise the distortion of preferences, and thus maximise the accuracy with which CVs are estimated, subject to constraints (decision costs).

In order to improve the accuracy of CBA, it is desirable that the preferences which CBA reveals or elicits are as well informed as possible. In the case of contingent valuation studies, for example, this can be achieved by: providing survey respondents with information which would later become available to them;[8] using preferences for states not actions, e.g. for health, not specific drugs, where

[8] Information which would never become available to the individual, should not be provided during a contingent valuation exercise. This is clear if one remembers that in most circumstances the individual's responses are used to derive estimates for the population as a whole, most of whom will not have taken part in the survey. Thus, individuals who would never have found out about a particular consequence of the project, e.g. extinction of a species, could not be affected by its extinction, except indirectly. Informing them of this possible consequence *may* therefore impart an upward bias to their estimates (Adler & Posner 2006, pp 136-138).

individuals may poorly understand the links between the two; using ex post as well as ex ante preferences; and allowing iterative judgments (Price unpubl., Hanley & Spash 1993, Sen 2001, Bateman et al. 2002). The principle of respecting the individual's sovereignty in judging their own utility can nevertheless be broadly maintained, at least at the population level.

A particularly important requirement when estimating CVs is that the individual's conception of the project coincides with that of the analyst – that the project be correctly and sufficiently specified. For example, if a respondent's welfare depends on his relative as well as absolute income level (e.g. Clark et al. 2008), a respondent's CV for a deterioration in environmental quality may be contingent on the incomes of those around them remaining unchanged. If individuals state their CVs assuming that their peers' monetary incomes remain unchanged, and these CVs are paid, they may prove to be insufficient.[9] A second important example is the proper treatment of social choice options referred to by Sen (2001:112-14).

CBAs use estimates of true CVs, and some uncertainty is inevitable (although it should be minimised, subject to constraints on decision costs). Such uncertainty does not fatally undermine CBA, and it ought to be possible for CVs to be estimated in a value-free manner. However, the complexity of CV estimation means that there is considerable scope for the analyst's choice of method to bias the results, and therefore for 'observer bias' (Ackerman & Heinzerling 2004, Price unpubl.) This highlights the need for transparency and peer-review of valuations intended for use in CBAs, and any CVs estimated by those who carry out the final, often value-laden, stage of CBA (aggregation) should be treated with caution.[10]

Wealth: a caveat to be retained

One of the most consistent objections to CBA is that it weights the preferences of the rich more highly than the poor (Sen 2001, Ackerman & Heinzerling 2004). This is because there is evidence to suggest that the marginal utility of income to

[9] Similarly, Ng & Ng (2001) show how economic growth may decrease welfare, even if all individuals are welfare maximising.

[10] Including, of course, those in Chapters 9-10 of this thesis.

an individual declines as their income increases (e.g. Stern 1977, Bailey et al. 1980, Evans 2005). If true, and if the effect holds across individuals, this would mean that a rich person would be willing to trade a larger amount of money for a given increase in utility, than would a poorer person. Thus, CVs will tend to be larger, *ceteris paribus*, for rich people than poor people. CBAs which aggregated CVs without considering this would favour systematically favour the preferences of the rich over those of the poor. This problem occurs when CVs are compared between individuals, and will be discussed in Section III.

Misunderstandings: risks, freedom of choice and coercion

Some of the fiercest criticisms of CBA relate to the way it values human life in monetary terms (or at least appears to do so). For example, Hahn and Tetlock (1999) use $6.6m as the value of a statistical life, when monetising the costs of road accidents due to cell-phone use while driving. Ackerman and Heinzerling (2004) object that lives and phone calls are not commensurable, since there is no way that money can compensate for death.

Yet this misunderstands what CBA is actually valuing. It is valuing the welfare loss associated with a given risk of dying, not with death itself. Contrary to Ackerman and Heinzerling's assertion, people do voluntarily trade off mortal risks against monetary reward, which can be spent while still alive.[11] Workers in riskier occupations are known to demand a risk premium, in the form of higher wages, as a condition of accepting the job. It is this, and similar, observed behaviour which is used to derive the value of a statistical life (Viscusi & Aldy 2003).

Some critics (e.g. Ackerman & Heinzerling 2004) respond that poor people sell their lives cheaply because they have no alternative. This introduces another misunderstanding, over freedom of choice and coercion. Ackerman and Heinzerling (2004) believe that poverty undermines the use of CVs, because it compromises the freedom with which preferences are expressed.[12] Yet the fact

[11] Of course, this money can also be passed on to others in the event of death (bequest value), and I discuss such disinterested preferences below.

[12] Note that this goes beyond the systematic bias in CVs discussed above: the existence of diminishing marginal utility of income alone does not undermine the validity of a poor person's CV, if its true meaning is kept in mind.

that a poor man may accept a small amount of money in compensation for an increased risk of dying doesn't mean that he values his life less than a rich man, but rather that he values money more. The CV still accurately describes his preferences, when offered a free choice between two alternatives. The fact of his poverty may be lamentable and his choice set undesirable but, if the project in question does not affect his material poverty, but only his safety, this is morally irrelevant to CV estimation. The objective is to estimate his CV for the choices he actually faces (project vs. status quo). It may well be morally wrong that society does nothing about his poverty, but that is another question. The fact that the poor man faces an unenviable choice does not mean that he does not face a free choice, which is the requirement for his preferences to lead to an accurate representation of his CV.[13] Coercion is a different matter. If a man was to put his daughter's life at risk for a hundred dollars, this would tell us nothing about her CV for greater risk, since she did not make the decision to accept that risk (or did so under coercion).

Utility, objective welfare and disinterested preferences

Adler and Posner (2006) list five reasons why preferences might be "distorted". I have discussed the implications of imperfect information, which leads individual's to mis-measure utility changes; and wealth differences, which do not distort CVs as such, but impart a bias to the poor's CVs relative to those of the rich.[14] In the case of information, the distortion is judged on the basis of the individual's own judgment – the individual may later regret their choice if they were ill-informed. The remaining three distortions proposed by Adler and Posner are qualitatively different, and rely on there being an objective and useful distinction between *utility* (as defined above) and *welfare*, which Adler and Posner consider to be the proper basis for CBA. Two of these distortions, "adaptive preferences" and "objectively bad preferences" seem to me to be unhelpful, and I consider them first. I then discuss the third, "disinterested preferences" which Adler and Posner

[13] This might be considered one example of an "adaptive preference" argument (Adler & Posner 2006), which I discuss and dismiss below.

[14] The distortion only enters when CVs are aggregated, see next section.

distinguish from self-interested preferences. I argue that the distinction is difficult to make objectively, but that there may be pragmatic reasons for trying.

Objectively bad preferences and adaptive preferences

"Objectively bad preferences" are deemed to be objectively bad for the individual, while "adaptive preferences" result from adaptation by the individual to circumstances which are held to be objectively bad (Adler & Posner 2006). An example of objectively bad preferences might be the preferences of an addict for drug consumption. An example of adaptive preferences might be an overburdened housewife who has become adapted to her circumstances and comes to prefer the extra work associated with carrying water from a river, rather than from a new pump that the project would provide.[15] Note that in each case, the supposed distortion stems not from insufficient information or impaired cognition, which have been dealt with above. Instead, the distortion is due to the individual's preferences deviating from those of an observer. In other words, their self-defined *utility* deviates from the observer's definition of their *welfare*.

Adler and Posner (2006:188) suggest that CVs might be "laundered" removing these distortions, though they are troubled by the practicalities. I do not believe that these are useful categories of preference distortions, and believe instead that any valid concerns in this area are best addressed as disinterested preferences, below. The adaptive preference argument seems to me to be similar to the 'poverty as coercion' argument dismissed above. Proponents of correcting adaptive preferences apparently disapprove of the status quo (e.g. the overburdened housewife walking miles to a river). This is not unreasonable, but it is irrelevant to the CBA. If a project was proposed which did change the status quo (which in the example above would require not only the building of a pump but changing the preferences of the housewife), this might be preferred, ex post at least, by the individual in question. No correction is therefore necessary to the concept of preference-based CVs as described above.

The argument that some preferences are objectively bad can be understood in two ways. First they may represent judgments by an individual, about how other

[15] This example comes from Sen (1987), quoted in Adler & Posner (2006).

people should behave, even though the behaviour does not directly affect the individual. For example, if I believe that drug addiction is objectively bad this is best dealt with as a disinterested preference (or ethical concern) of mine, and therefore relevant to *my* CV for a project (see below). Second, like adaptive preferences, they may represent an attempt to distinguish an individual's welfare, as defined by an observer, from utility, as maximised by an individual (Adler & Posner 2006:31). Of course, it is proper that philosophers should speculate on what people should prefer, and on what constitutes the "good life" – this is, after all, a central concern of ethics (Deigh 1999) and they may even succeed in changing the preferences of others.[16] However, defining welfare as something that philosophers think that we ought to appreciate, is problematic for public policy, raising questions about elitism and authoritarianism. A CBA using preference-based CVs will inform a decision-maker of the likely effects of a project from the perspective of the individuals it affects. To me, this seems far more useful than informing them of the effect that the project would have, in the hypothetical case where everyone else shared the preferences of a particular philosopher, when in fact they do not.

Disinterested preferences

Adler and Posner (2006) argue that "disinterested preferences" distort CVs and should be excluded from CBA. The example they give is "existence value" when individuals value the mere existence of, for example, a wild species such as the tiger, quite apart from any direct or indirect use value they gain from it (e.g. Edwards-Jones et al. 2000:85). Disinterested preferences do pose an interesting issue for CBA, but existence values are probably a bad example, since they could easily be described as self-interested.[17] It is surely not impossible that someone

[16] It is also possible that the welfare of philosophers is negatively affected when others express "objectively bad" preferences, but this is simply a welfare-based preference like any other, and relates to the philosopher's CV, not to that of the drug addict.

[17] Adler & Posner (2006:136-8) make a good point about the information which analysts provide to individuals when soliciting stated preferences about existence values. Existence value CVs should not be solicited for entities that the individual did not know existed, and would never have known existed. Indeed, there are many difficult issues associated with estimating CVs for such values. But these are a question of proper survey design: the analyst must be careful not impose their conception of the project on that of others. This point is covered within the category of information, above, and does not merit a separate category for apparently altruistic or ethical preferences.

could suffer a loss of welfare as a result of knowing that the tiger had become extinct, and furthermore be prepared to sacrifice some portion of their income to avoid this occurring. Exactly this behaviour can be observed among a significant proportion of the population, who donate money to international conservation organisations in the hope of saving species that they will probably never see.

Better examples of disinterested preferences might be those that concern the rights of fellow humans who, unlike tigers, form part of the moral community normally considered by CBA. These might include rights to freedom of action, property, and equality of welfare. Conventionally, CBA has ignored the first two of these (Sen 2001), while some CBAs have addressed the latter (e.g. Tol 2001).[18] This apparent blindness of CBA to ethical concerns is a major source of criticism (Sen 2001, Ackerman & Heinzerling 2004).

However, even in relation to these preferences, Adler and Posner's (2006) contention that they be excluded from CBA is problematic. The problem is that it is difficult to decide on objective grounds which components of utility should not be included in a CBA. For example, psychological egoism argues that no preferences are truly altruistic (Russell 1999). Even if we do not accept this theory, it highlights the fact that many apparently altruistic acts may have at least some self-interested component. What proportion of charitable giving is accounted for by a selfish desire for esteem or social interactions (Mathur 1996) and does generosity make us happier (Konow & Earley 2008)? What proportion of an individual's lifetime earnings are motivated by bequest values (Kopczuk 2007)? How should "warm glow" values be treated (Nunes & Schokkaert 2003)? Adler and Posner (2006:49) admit that "no one has yet fully explained what the difference is between "self-interested" and "disinterested" preferences".

In principle, CVs might be estimated for the full range of preferences held by an individual, from the prosaic and self-interested to the intangible and altruistic. For

[18] Adler & Posner (2006) observe that CBAs are subject to ethical concerns, since the actions which can be considered within a CBA are constrained by laws. This is true, up to a point, but CBAs can also be used to assess the case for a change in the law (e.g. Hahn & Tetlock 1999) and since the cost-benefit principle is itself used to determine what is legal (Adler & Posner 2006). In addition, the force of law may be weaker in the context of global environmental issues, which cross national boundaries and may involve at least some countries where institutions are poorly developed.

example, Corneo and Fong (2008) estimate the willingness to pay for the redistribution of income in the US. These CVs would provide information on the preferences of each individual in a society, and the relative intensity of those preferences, in terms of the monetary adjustments necessary to render them indifferent between the project and status quo. Many will instinctively revolt at this suggestion: surely one's concerns for rights and justice cannot be treated in the same way as one's preferences for food and holidays? Yet we should surely guard against instinctive rejection, for several reasons.

First, as I will explain below, it should be appreciated that many CBAs already aggregate ethical preferences (over the distribution of income between and across generations) in this way (Price 1993, Portney & Weyant 1999a), and it seems arbitrary to include some ethical preferences and not others. Second, the alternatives; aggregating ethical preferences through the ballot box or public consultations; do not enjoy complete support either, because they do not measure the intensity of people's ethical preferences, and therefore do not compare the tangible sacrifices which people are willing to make with the material costs of observing such preferences (see e.g. Price 1989:48, Caplan 2007). Beyond this, some libertarians maintain that that an individual's rights cannot be determined by the preferences of others, whether through CBA or voting:

> "Individual rights are not subject to a public vote; a majority has no right to vote away the rights of a minority; the political function of rights is precisely to protect minorities from oppression by majorities (and the smallest minority on earth is the individual)." Rand (1964[1989]:104).

This, however, raises a chicken-and-egg question of how the rights of individuals are defined to begin with.

In principle it would be useful to include ethical concerns in CVs, in order to be able to compare the results with other approaches, where such ethical concerns are incorporated at a later stage, or not at all. Nevertheless, while I am sceptical that a truly satisfactory and objective distinction can be drawn between self-interested and disinterested preferences, it is important to note that, conventionally, CBA has tended, somewhat arbitrarily, to exclude most disinterested preferences from CVs

and it is possible that for practical rather than philosophical reasons, it might be desirable to exclude certain ethical preferences from individuals' CVs. For example, it might be hard to obtain monetary estimates for some preferences. Alternatively, decision-makers (if the procedure was not decisive) may not understand that such ethical concerns were already included in the CBA, and attempt to account for them again – which could lead to double accounting. Ultimately, CBA can only improve decision-making if it is comprehensible to society. For the time being, then, ethical preferences, like wealth biases, are caveats that I will hold over until the aggregation phase, which I discuss next.

III. Aggregating consequences across individuals

The purpose of this section is to discuss how CBA aggregates information, how decisive it is, and how helpful to the decision-maker. I start by introducing aggregation and decisiveness, before moving on to discuss two caveats held over from the previous section: inter-personal comparability of CVs and the treatment of ethical concerns in CBA. I then review four existing approaches to aggregation: the Pareto criterion; the potential compensation criteria (the sum-of-CVs); the sum-of-corrected-CVs; and social welfare functions. In each case, the aim is to investigate the meaning of the results, and what they leave to the decision-maker.

Aggregation and the decisiveness of CBA

To know whether to choose the project over the status quo (or which project to choose) society must somehow aggregate all CVs, along with any other ethical concerns, into a judgement of the project's social desirability compared with the status quo. There is no alternative to aggregation: howsoever the decision is made, it implies a weight for each individual's CV, and for each ethical concern. To avoid the decision is merely to accept the status quo, and carries the same implications.[19] This means that anything not satisfactorily incorporated into the CBA must be weighed against the results by the decision-maker. Similarly, if any value judgments are incorporated into the CBA that the decision-maker believes

[19] Avoiding or taking the decision also requires that a weight is implicitly put upon the decision costs, which may not be trivial.

to be unsatisfactory, they must be able to understand their effects, and to correct for them.

Since all information must be weighted and aggregated, either explicitly by the CBA or implicitly by the decision-maker, less aggregation within the CBA implies that more aggregation is left to the decision-maker. CBA approaches can therefore be conceptualised as lying along a spectrum, according to the amount of post-measurement aggregation. At one end of the spectrum, some welfare economists (e.g. Just et al. 2004:5), reject any form of aggregation, proposing that only disaggregated data should be presented to the decision-maker. Thus, they reject CBA altogether. Part-way along the spectrum, some authors advocate simple summation of costs and benefits, leaving many pertinent concerns to be weighed by the decision-maker (e.g. Kaldor 1939, Johansson 1993, Adler & Posner 2006). At the other extreme would be a CBA which claims to aggregate everything society considers pertinent to the decision, such that the results of the CBA were decisive, removing the need for a decision-maker. Such a "super-procedure" may seem implausible, yet economists sometimes appear to be surprised when decision-makers over-rule the recommendations resulting from CBAs (e.g. Hahn & Tetlock 2008).[20] When the results of multiple CBAs are discussed in a policy context, the headline results of CBAs are often aggregated as if they were decisive (e.g. Balmford et al. 2002).

Two problems for aggregation

Here I return to the two issues held over from the previous section: inter-personal comparability of CVs and the effect of income upon them, and the place of rights and ethical judgments in CBA. Each poses special difficulties for CBA, which will become apparent in discussing aggregation procedures, below.

The meaning and inter-personal comparability of CVs

Throughout the previous section, I kept to a very strict definition of CVs: the amount of money which must be taken from or paid to an individual to ensure that

[20] This attitude is not exclusive to economists. Goldston (2008) describes how natural scientists involved in making policy recommendations also frequently view their recommendations as being binding upon elected politicians and their appointees.

they are indifferent between the project and the status quo. However, what if the CV is not made? If CVs are not actually made when the project is adopted, then some individuals will be better off (prefer the project) and some worse off (prefer the status quo). But is it possible to say anything about how much better or worse off one individual is compared to another? Whatever approach is taken to aggregation, some answer must be found to this question, and this hinges on inter-personal utility comparisons: can CVs be compared between individuals? Can we add CVs together? If not, can economists summarise the results of their analyses, or must they present the decision-maker with an incomprehensible barrage of CVs, one for each person (or group of persons) affected by the project?

This question goes to the very heart of economic method, and its role in society. It has a long history (e.g. Harrod 1938, Robbins 1938, Kaldor 1939) but continues to the present day (e.g. Ng 1997, Adler & Posner 2006). Here, I present what seem to me to be the essentials of the debate. The practical implications of the debate will become clear below, when discussing aggregation approaches.

The sceptical position is well summed-up by Miller (1994:418-9, quoted in Ng 1997:1851):

> "There is no way you or I can measure the amount of utility that a consumer might be able to obtain from a particular good ... there can be no accurate scientific assessment of the utility that someone might receive by consuming a frozen dinner or movie relative to the utility that another person might receive from that same good ... Today no one really believes that we can actually measure utils."

Thus, in the case of a project, for which A has a CV of +$40, and B a CV of -$20, and no payments are made or deducted, we can say that A has gained 40 dollars' worth of utility and B has lost 20 dollars' worth. However, because utility is defined relative to the individual, the units are not comparable: a dollar given to A may bring more or less utility than a dollar does to B. Without assuming anything more about the meaning of these units, all we can say is that, as a result of the project, A's utility has increased as much as if they had been given $40, and B's has decreased by as much as if $20 had been taken away from them. We can say nothing about whether the project has increased A's utility more than it has

decreased B's (let alone by how much), nor whether society should undertake the project – that decision would depend on the decision-maker's judgment about numerous factors, including: the marginal utility of income to A and B; the rights of A or B to their gains and losses (see below); and the relative importance to society of each individual's rights and utility. Note that the complexity of the decision-maker's task increases geometrically with the number of individual's affected by the project. Note also, that if the decision-maker is simply told that the project has a net benefit of "+$20", they cannot make any of the judgments above, since the information is lost in the aggregation.

What, then, should CB analysts do? If one holds adamantly to the view that CVs are not inter-personally comparable measures of utility, there are two options:

The first is to eschew any form of aggregation, thereby abandoning CBA (recall Sen's third foundational principle), and to present the decision-maker with estimates of each individual's (or homogenous group of individuals) CVs, and leave aggregation to them. This is Just et al's (2004) preferred option, and leaves the analyst's objectivity intact, but in the context of complex projects, with multiple winners and losers, it is unlikely to be of great help to the decision-maker, who is forced to carry out the aggregation themselves.

The second is to arithmetically aggregate CVs, without assuming that they bear any systematic relationship to utility or welfare. This is the 'potential compensation criterion', which I discuss below.

A third possibility is to assume that CVs *do* represent interpersonally comparable measures of utility. This means that the marginal utility of money must be constant across individuals. There are three reasons why this might not be the case, and I outline them below. The first two can be accommodated within CBA. The third requires its complete abandonment.

First, marginal utilities might vary at random across individuals. In this case, CVs would still provide a probabilistic guide to welfare changes, and a rule which maximised the sum of CVs, would at least maximise expected net utility. Thus, CVs would not be expected to have an exact relationship to utility, but rather to be probabilistically related to it. In the context of global environmental issues, many

relationships are probabilistic, and the decision to accept CVs as a probabilistic guide to utility changes seems to present no special difficulties.

Second, and perhaps in addition to a random component to the variation, marginal utilities might vary systematically across individuals, on the basis of some observable characteristic such as income, gender, or race. The most important of these is income, because money is the numeraire (currency) used in generating CVs. Since money is unequally distributed in the population, the use of money as a numeraire could lead to systematic biases in CVs as a measure of utility changes. As discussed above, there is empirical evidence to suggest that the marginal utility of income declines as income increases. If this were the case, projects which increased the sum of CVs might actually reduce the sum of utility. This is a commonly recognised possibility, and sometimes corrected for within CBAs (see the review of climate change CBAs by Tol 2005). Although this would have serious consequences for the decisiveness of a procedure which maximised the sum of CVs, it need not undermine the use of CV-based welfare measurements, provided that appropriate corrections are made. Indeed, a CBA based on uncorrected CVs can simply be regarded as a special case of a more general, corrected-CV procedure, in which the correction factors have been set to unity (Price 1989).[21]

The third possible reason why marginal utility of income may not be constant is that individual utilities are in some fundamental respect non-comparable across individuals. Despite opposition to this view (e.g. Ng 1997, Adler & Posner 2006), most economists seem to have accepted it (Ng 1997). Note, however, that this is not the same as arguing that interpersonal comparisons are either difficult or impossible to make in practice, which is what Miller, appears to be arguing in the quote above. This can be demonstrated by a simple thought experiment. Imagine two individuals A and B, whose marginal utilities of income are unknown and immeasurable, yet nevertheless exist. We wish to allocate $100 between these two individuals, in order to maximise total utility. With no information about their

[21] I prefer to use the term correction factor, rather than weight, because the purpose is to correct a particular distortion which is in principle empirically estimable, rather than to apply a subjective weighting. However, correction and weighting are difficult to distinguish, as I discuss below.

marginal utilities, we would probably decide to divide the money equally between them. This might not maximise utility, and we might never know whether we had maximised utility (or how badly we had failed), yet it was, and would remain, the option *most likely* to maximise utility. In order to dispute this conclusion it is necessary for A and B's marginal utilities to be not simply immeasurable, but *nonexistent*. Yet for them to be non-existent, cardinal utility itself must surely be non-existent.[22] Perhaps this is possible, but if so, then there is no reason to favour any utility-based aggregation procedure over a simple coin toss, for any Pareto non-comparable cases (see also Ng 1997).

Thus it seems plausible that CVs (when corrected for known biases due to the numeraire used) can be treated as cardinal and interpersonally comparable probabilistic *proxies* for individual welfare.[23]

It seems to me that the assumption of inter-personal comparability of utilities may not represent a positive belief that the utility value of a dollar is, probabilistically comparable between individuals (after bias correction). Rather, it seems to imply a normative belief that each individual's utility should be scaled, such that no-one, no matter how efficient they may be at converting dollars into utility, should be able to dominate the right of another to utility.

Ethical concerns

> "The exponents of the mainstream need not face much questioning from the deontologists (who will not speak to them)." Sen (2001:116).

In Section II I noted that, conventionally, CBA has not included in an individual's CV the disinterested preferences they may have relating to the rights of other members of the moral community. Yet it is of these individual disinterested preferences that society's ethical concerns are made. Thus, while conventional CVs evaluate how an individual's welfare[24] changes as a result of a project, they

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[22] It is not sufficient for them to be zero (i.e. income has no relation to utility). In this case, the same would apply.

[23] This conclusion is similar to that reached, perhaps by different routes, by Ng (1997) and Adler & Posner (2006). Note that if there is any reason to believe that the decision-maker will have a better notion of the relationship between CVs and utility, this could be incorporated into the analysis.

[24] As noted above, I use welfare to refer to utility excluding ethical concerns.

convey no information about the strength of that individual's right to the welfare lost or gained (and therefore to the CV which might be made). Nevertheless, the strength of an individual's right to CVs must be determined by society in order to reach a judgment. If disinterested preferences are not included in an individual's CV they must be incorporated at some point in the decision-making process, in the form of society's ethical concerns. For the rest of this chapter, I will assume that disinterested preferences have not been included in the individual's CV, and therefore that ethical concerns over individual's rights will be ignored, unless specifically incorporated at the societal level.

In the context of evaluating projects, the claims of an individual to the welfare lost or gained due to the project might be determined by at least two broad categories of right. First, there are rights to the property or actions which produce the welfare gained or lost in the project case. Second, an individual may have rights to welfare on the basis of their level of welfare relative to others. Often, the former type of right will be in force in the status quo, while the latter will pertain to redistribution, but in some cases the project under consideration may be intended to strengthen property rights, possibly at the expense of equity.[25] The moral standing of an individual may strengthen or weaken the rights they are accorded by society, but this will not be considered further here.[26] It is important to remember that rights may not be absolute, but rather be *prima facie*; even important and apparently fundamental rights may be trumped by other rights (Wellman 1999). As discussed above, individuals may be prepared to exchange important rights, such as their right to life, in exchange for monetary compensation.

[25] Even a libertarian may recognise that complete enforcement of property rights is impossible, or at least requires the infringement of other rights (right to freedom of those wrongly convicted, property rights of those taxed to pay for law enforcement). A project may therefore aim to increase the protection of some rights, at the expense of others, and with a positive or negative effect on welfare.

[26] For example, a criminal's right to freedom might be taken away, not simply to prevent them from committing further crimes, but also because of (i.e. in punishment for) previous crimes.

Aggregation procedures

I now review conventional approaches to aggregation in CBA, from the perspective of the issues raised above. The aim is to show what the results of a particular aggregation method actually mean, and what it contributes to the decision-making process.

The Pareto criterion

According to the Pareto criterion, a project is to be preferred if it makes at least one person better off, and nobody worse off. Equivalently, either the status quo or project are said to be Pareto optimal if nobody can be made better off without making someone else worse off (Just et al. 2004). However, Pareto improvements can rarely be identified in practice, unless some redistribution between winners and losers is permitted (see below) and the criterion therefore fails to rank most projects in relation to the status quo: both status quo and project may be Pareto optimal. Nevertheless, Pareto improvements are generally regarded by economists as being desirable and the conventional criteria for judging the social desirability of projects are built on the Pareto criterion (below). Therefore, while the Pareto criterion has never played an important direct role in CBA, it is worth considering some issues it raises.

First, the Pareto criterion is not as value-free as it may appear (Hausman & McPherson 2006). For any given Pareto-inefficient status quo, there will normally be a range of Pareto-optimal worlds which can be reached through different Pareto-improving projects. However, choosing any individual Pareto-improvement will narrow down the range of attainable Pareto-optimal worlds. Since the Pareto criterion cannot be used to choose among different Pareto-optimums, and since society may not be indifferent between them, selecting any project which meets the Pareto criterion requires a value judgment as to which Pareto-optimum is preferred (Hausman & McPherson 2006:136-8). Second, the Pareto criterion highlights the need to correctly specify the individual's utility function. A Pareto improvement is one in which at least one individual's *utility* increases, while no other's is decreased. This is not the same as one individual's *income* increasing while others remain unchanged, unless their utility functions are insensitive to relative income - and there is empirical evidence that they may

not be (Clark et al. 2008). Third, if utility is defined so as to exclude ethical concerns, the Pareto criterion provides no basis for incorporating them.

The compensation criteria

Because of the limitations of the Pareto criterion listed above, a different, but related, approach was proposed independently by Kaldor (1939) and Hicks (1939), and is known collectively as the compensation criteria. The Kaldor criterion states that a project is desirable if, following its adoption, the winners would hypothetically be able to compensate the losers such that no-one was worse off and at least one person was better off. Similarly, Hicks proposed that a project be considered desirable if the would-be losers were unable, again hypothetically, to bribe the would-be winners not to undertake it (Johansson 1993:120). The relationship of these criteria to the measures introduced in Section II is that a positive non-zero sum of CVs ($\sum CV_i > 0$) is a necessary but not sufficient condition for the Kaldor test to be passed, while a positive non-zero sum of equivalent variations[27] ($\sum EV_i > 0$) is a sufficient condition for the Hicks criterion to be passed (Johansson 1993:120).[28] Unfortunately, the two tests may produce contradictory recommendations (Boadway & Bruce 1984). However, these distinctions are not important here, and for simplicity, in what follows I refer to the satisfaction of the compensation criterion as a positive non-zero sum of CVs. It is important to note that neither criterion requires that compensation actually be paid.

If CVs were an unbiased estimator of utility changes, the sum-of-CVs would provide the decision-maker with an indicator of the net change in utility. However, there are two problems with this. First, it is unlikely that CVs are an unbiased estimator of utility changes, because of the diminishing marginal utility of income. Second, even if they were an unbiased estimator, it is unlikely that the net change in utility is the only factor of interest to society. In addition to the sum of net utility changes, society may also have ethical concerns about each

[27] See Footnote 6, above.

[28] As we will see below, this assumes that the costs of compensation are zero, which is rarely, if ever, the case.

individual's rights to the utility lost or gained, yet the sum of CVs provides no information about this. A society which did not have such concerns would be indifferent between a world in which the rich stole effortlessly from the poor and one in which this did not happen.

The conventional defence of the sum-of-CVs is two pronged. First it is argued that, while ethical concerns about rights are legitimate, they do not fall within the economist's remit and should be incorporated by decision-makers, post-analysis. This is unconvincing as, although ethical decisions do lie outside the remit of economists, the sum of CVs is not value-free, nor is its meaning clear. How then is a decision-maker to weigh ethical concerns against the sum of CVs? Secondly, it is assumed that society has a costless and perfect means of redistributing income among individuals, which would allow it to correct for any distortions from the preferred state that result from choosing projects based on the sum of CVs. By assuming this, it is possible to avoid the issues raised above, restore the decisive role of CBA, and rescue some meaning for the sum of CVs – as the monetary measure of the gain to society, after having costlessly and perfectly forced all winners to compensate all losers – a sort of 'societal CV'.

Yet this is not plausible. A project with a positive sum-of-CVs that creates unjust distortions implicitly requires substantial redistributions, which are not costless or simple to ensure. Assuming that they are so is no more justifiable than arbitrarily assuming that the project's other costs are also zero.[29] It is worth remembering that the example used by Kaldor (1939) when expositing his criterion, was the repeal of the corn laws, which, he stated, had just a single group of losers (the landlords) and a single group of winners (consumers). Of course, even in this case compensation would not have been costless.

The weaker assertion that "this issue of income inequality is better tackled through the general tax/transfer system" (Ng 1997) may be true, or it may not, and

[29] In addition, I am not aware of any instances where an economist or politician has argued for the progressivity of the tax system on the grounds that it was necessary to compensate for the distortionary impacts of implementing decisions based on (conventional) CBA. Nor am I aware of any systematic attempt to monitor the distortionary impacts of implementation decisions based on conventional CBAs, which, on average, would be expected to transfer *utility* from poor to rich. If the use of CBA is increasing in some countries more than others (e.g. Hahn & Tetlock 2008) one would expect to see the progressivity of the tax systems increasing more quickly as well.

will depend on the facts of a particular case. The tax system may well be a better way to tackle the general problem of redistribution, but not the specific distortions caused by a particular project. Thus, Sen (2001) notes that: "The compensation tests are either redundant or unconvincing".

The sum-of-corrected-CVs

One response to the problems of the sum-of-CVs approach is to propose that CBA should calculate (and decision-makers should maximise) the sum-of-corrected-CVs: where CVs are corrected for systematic biases due to the diminishing marginal utility of income (see Price 1989, Johansson 1993, Pearce et al. 2006).

Since Adler and Posner (2006) advocate CBA as the procedure most likely to maximise "overall welfare" (the sum of individual utilities), they consider correcting CVs for the income bias. However, despite their enthusiasm for correcting other distortions, they offer only lukewarm support for correcting CVs for the systematic biases due to income. They note that, if CVs are corrected prior to aggregation, CBA will favour any project which simply redistributes income from rich to poor at low enough cost. Instead of taking this as an indictment of a procedure based on maximising overall welfare alone, they reject corrected-CVs, citing concerns over disincentive effects, and post-project trading between rich and poor. These concerns, while valid, should be included as part of the project's estimated costs and benefits; they are not a valid argument against the correction-of-CVs (Kuziemko 2007). Adler and Posner (2006) instead recommend a rule of thumb that CBA should not be used for projects in which wealth disparities are large. This renders it incapable of ranking a large proportion of possible projects, including those related to many global environmental issues.

Despite the reservations of Adler and Posner (2006), the sum-of-corrected-CVs is an improvement on the sum of CVs approach, though it remains relatively rare (Tol 2005). At least the product of the CBA now has some meaning: it does estimate (probabilistically) the effect of the project on overall welfare. However, like the sum-of-CVs, it contains no information on the implications of the project for ethical concerns other than for overall welfare, such as concerns about rights. It cannot therefore be decisive, and the decision-maker is left with several problems. First, it is unclear how they should weigh the relative importance of

welfare and other ethical concerns, since it provides no information on the latter. Second, if the decision-maker decided that the welfare gains did not justify the ethical costs, it is far from clear that complete abandonment of the project would be optimal – the introduction of compensation mechanisms may, depending on their cost, result in an improvement over the status quo.

Before proceeding, it is worth noting that many authors (e.g. Pearce et al. 2006) treat the correction of CVs for the diminishing marginal utility of income as synonymous with incorporating equity concerns into the CBA. This is not necessarily the case. It is possible that society might have preferences for the distribution of utility not just income. Correcting CVs for diminishing marginal utility is necessary for a utilitarian maximisation of overall utility. Ethical concerns over the distribution of utility are, conceptually at least, another matter (Johansson 1993:15), and I discuss them next.

Social welfare functions

A further modification is often considered in CBA textbooks (e.g. Johansson 1993, Dinwiddy & Teal 1996) though rarely explored in applied CBAs of global environmental issues (Tol 2001 gives a rare example in the context of climate change). This is the use of social welfare functions.

Social welfare functions generalise from the purely utilitarian objective of maximising the sum of utility (or welfare), to take into consideration other concerns which society may have. They perform an analogous role at the societal level to that performed by the individual's utility function. If ethical concerns are not captured within an individual's CV, it is here in the social welfare function that they may be addressed.

Thus, we can conceive a social welfare function W which aggregates each individual's utility level V_i. A purely utilitarian society concerned only with the sum of individuals' welfare simply maximises a function of the form (Johansson 1993:17):

Equation 2.1 $$W = \sum_{i=1}^{i} V_i$$

However, as discussed above, society may concern itself not simply with the sum of welfare, but with individuals' rights to portions of that welfare. One example

would be where an individual's claim to welfare is determined by (and only by) their welfare relative to the rest of society. In the extreme Rawlsian case (based on Rawls's 1971 *Theory of Justice*), society's welfare is determined solely by the welfare of the least well-off person (Johansson 1993:19):

Equation 2.2 $$W = \min\{V_1 ... V_i\}$$

Thus, whereas the utilitarian social welfare function (Equation 2.1), assumed by conventional CBA, treats utility losses and gains equally regardless of to whom they accrue, other forms of social welfare function assume limited or no substitutability between the utility of different individuals. More generally, the spectrum from utilitarian to Rawlsian functions is given by (Johansson 1993:121):

Equation 2.3 $$W = [\sum_{i=1}^{i} a_i (.V_i)^{1-\rho}](1-\rho)$$

Where ρ is the degree of inequality aversion[30] and a_i is an individual-specific weight. Where a_i=1 for all individuals, and ρ=0, the function reduces to the utilitarian function (Equation 2.1), and when ρ tends to ∞, the function approaches the Rawlsian function (Equation 2.2). In principle, the weights, a_i applied to an individual's welfare might be determined by any of the factors mentioned previously (moral standing, relative welfare), but in practice attention has focussed on relative welfare as determining relative claim strengths, through the parameter ρ (e.g. Johansson 1993, Azar 1999, Tol 2001). If, as seems plausible, rights weightings apply not to the individual, but rather to a particular piece of property or action, Equation 2.3 would have to be disaggregated, such that different weights could be applied to different components of an individual's CVs for the project.

CBAs based on the sum of corrected CVs provide decision-makers with information only on how the project affects the sum of welfare. In generalising away from perfect substitutability, social welfare functions allow priority to be

[30] It should be noted that, empirically, it may be difficult to distinguish between *e*, the marginal utility of income to an individual, and ρ, the marginal social value of welfare, or the social aversion to welfare inequality. This explains why many authors treat CV correction and equity concerns as one process. This is a simple matter in the case of Prioritarian social welfare functions, since Equation 2.4 has the same form as the function normally assumed to link income and utility at the level of the individual However, with a Rawlsian social welfare function, the two processes are not so easy to roll into one.

given to less well-off people in distributing welfare, thus incorporating one of the principal ethical considerations identified above. However, use of a social welfare function of the form given in Equation 2.3, and in which a_i=1 for all individuals, does not incorporate concerns over other rights, e.g. to property or freedom of action. Social welfare functions ignore these rights because they do not distinguish between welfare losses; welfare gains which are "paid for" by these losses (and therefore represent pure re-distribution); and welfare gains which represent "value added" by the project. Yet it is probable that society would view rights to these three categories of welfare differently.

The other problem with social welfare functions is that, like CBAs using corrected-CVs, a project which appears to reduce the value of the social welfare function relative to the status quo, might increase it if adequate compensation mechanisms are deployed.

Summarising of the problems of existing approaches

In reviewing existing approaches to aggregation in CBA I have shown that all of them are incomplete. Only the sum-of-corrected-CVs has a clear meaning,[31] and none are decisive. Since they are not decisive, society must collect other information or value judgments, and weigh aggregated information against disaggregated information. I do not believe that any of the current approaches adequately helps the decision-maker to do that. In fact, I think it is plausible that CBAs which present aggregated results, without incorporating all of society's relevant concerns, could in fact distort decision-making, by leading decision-makers to under-weight those factors excluded from the aggregation process (e.g. Heinzerling 1998).

In principle, a social welfare function which incorporated all ethical concerns, would be meaningful and decisive. However, it could not be constructed without consulting decision-makers, since the importance attached to various rights (to property etc) are not easily estimable as population parameters, or at least, this is

[31] Even this relies on the distinction made in Section II, between self-interested and disinterested preferences, being valid and meaningful.

not currently done.[32] This is a simple but important point: no value-free aggregation procedure should be considered decisive. Aggregation is not possible in isolation from society. We must therefore abandon the notion of CBA as a value-free advisory tool, prepared by economists and presented to decision-makers.

The second point to note is that for any social welfare function in which the marginal social value of income is not constant, the result depends not just on the "value-added" by the project, but the distribution of welfare gains and losses among individuals, which depends on the precise design of the project. However, CBAs have not generally considered compensation mechanisms as part of the project specification, but instead leave this to the decision-maker. A fully specified social welfare function, while potentially decisive, provides a largely arbitrary test of whether action is justified, unless the project specification includes possible compensatory mechanisms (which must be fully costed). A project might fail to increase social welfare over the status quo, despite generating sufficient value-added so that, with the addition of appropriate compensation mechanisms, it could do so. In Chapter 11 I provide an empirical illustration of these issues.

IV. Inter-temporal aggregation

In this section I consider whether aggregating CVs inter-temporally requires any special treatment, beyond that required intra-temporally. Most economists hold that it does, and that future CVs should be systematically discounted, usually exponentially, relative to those in the present (Solow 1999). Even with relatively low discount rates of, say, 4% per annum, this procedure reduces CVs which will occur a century from now to one fiftieth of their undiscounted value, apparently placing less weight on the preferences and welfare of future generations than on that of our own. Not surprisingly, discounting has accounted for much of the controversy surrounding CBA, and nowhere is this more true than in relation to

[32] The one exception is the marginal social utility of income. However, estimates of this may be confounded by other ethical issues. For example, a "pure" elasticity of the marginal social utility of income could not be estimated from observing tax regimes (e.g. Evans 2005) since these also incorporate ethical preferences over the strength of property rights.

environmental issues, which often require analyses spanning several centuries. For example, much of the controversy surrounding the Stern Review of climate change (Stern 2006) has centred on its treatment of time (e.g. Dasgupta 2007c, Nordhaus 2007b, Weitzman 2007, Spash 2008). Although Price (1993) has argued that discounting as currently practiced cannot be justified, it remains the case that the procedure is routinely applied in CBA (Basu's 1994 assertions notwithstanding).[33]

Drawing heavily on Price (1993), I briefly review the four most important rationales for discounting. None can be entirely ignored in CBA, but none justify routine use of uniform exponential discount functions to aggregate CVs through time. As in the previous section, the cases where such a practice may be an acceptable approximation to the truth turn out to be special rather than general, and particularly unlikely in the context of global environmental issues.

The opportunity cost of capital

The original rationale for discounting was the observation that benefits which occur early in time can offset, or compensate for, costs which occur later, because the benefits can be invested between-times, growing according to the rate of interest they attract. If a project requires early outlay for later reward, it is therefore legitimate to ask whether those same outlays might achieve a greater reward if invested elsewhere. Accounting for the opportunity cost of capital, and comparing the returns from different projects (including investing the money required in a bank) does not intrinsically discriminate against future generations (Portney & Weyant 1999b). The problem is not with this rationale per se, but rather with its application in CBA.

Conventionally, a uniform exponential discount rate is used to build this criterion into the CBA (Price 1993). In effect, all benefits are assumed to be invested at this rate, and all costs are assumed to be compensated out of funds so invested. Within the scope of a narrow financial CBA, where all benefits and costs are received or

[33] In reviewing Price (1993) Basu's main criticism is that "what he sets out to demolish is upheld by very few" (Basu 1994). However, Solow (1999) writes that "when asked to summarize and evaluate costs and benefits over time, it is second nature for any economist to reach for a discount rate, and very likely a market interest rate".

incurred in cash over a relatively short timescale, this may be appropriate. However, there are at least three problems with using a uniform discount rate for more complex projects.

The first is investibility. Using a discount rate based on market interest rates assumes that the benefits received early in the project are actually invested at that rate. In the case where the project would require cash to be borrowed on the open market, this might be true. But many costs and benefits of projects do not take the form of cash borrowed from or lent to banks (Price 1993). Strictly speaking, positive CVs represent a gain in some individual's income (or equivalent), not an impersonal cash payout. When individuals receive a boost to their income, they may save some of it, but the savings rate is rarely 100% and the marginal propensity to save may depend on the income level of the individual (e.g. Dynan 2004) and may differ between individuals and countries. The savings rate may also depend on the form that the benefit takes. Although positive CVs for non-monetary benefits are themselves non-investible (one cannot directly invest a nice view, Price 1993), they may lead to an increase in saving, since the person, by definition, feels as well off as if they had received the money. The proportion saved may be different from the case where the benefit was received in cash, however.

This brings us to the next problem. The rates of return available to individuals may vary and are linked to the per capita income growth rate (Ramsey 1928, see below) as well as local factors (Saleem 1987). Although interest rates and growth rates are commonly expressed in percentage terms over any given period, this does not imply that investments or incomes grow exponentially, as I will show in Chapter 3. Where projects affect individuals across a broad spatial and temporal range, it is unlikely that a single exponential discount rate will provide a sufficiently good approximation to real-life processes. In addition, the social rate of return on investment may be lower than the market rate, if investments yield negative externalities (Dasgupta et al. 1999).

The two preceding points highlight the fact that the opportunity-cost-of-capital rationale for discounting relies on a similar logic to the potential compensation criterion. In applying a discount rate to project costs and benefits, it is implicitly assumed that compensating variations are actually made, i.e. that money is

invested by beneficiaries and paid to losers in the future (Price 1993). Yet, in reality, positive CVs in the present will only be partially saved and there is, of course, no guarantee that the resulting capital will be distributed to future individuals with negative CVs. Thus, the opportunity cost of capital rationale for discounting suffers from the same disadvantage as the potential compensation tests – it relies on an assumption that society is indifferent as to who is affected by benefits and costs.

CBA cannot ignore the opportunity cost of capital, but to account for it using a single exponential discount rate is only likely to be appropriate under very specific conditions, which will be restricted to a narrow class of projects. Of course, these are exactly the kind of projects for which CBA, and discounting, were originally devised (e.g. Dupuit 1844 and Faustmann 1849, cited in Price 1993; see also Hanley & Spash 1993). CBA must now tackle a quite different set of problems if it is to assist society in making decisions about global environmental issues.

Expected income growth

Another rationale for discounting is based on the assumption that future generations will be richer than our own. If this is the case, future CVs should be corrected for the diminishing marginal utility of income just as they are intra-temporally (e.g. Sharma et al. 1991, Stern 2006). Similarly, if society has preferences over the distribution of utility, these should surely be applied in a consistent manner intra- and inter-temporally. Note that both of these, CV correction and equity weighting, may need to be accounted for separately from the opportunity cost of capital. However, valid though these concerns are, three important points must be considered before future CVs are discounted on the basis of these rationales.

First, for exactly the same reason that CVs must be corrected for the marginal utility of income, it is highly unlikely that CVs will be unchanging through time. The CV for a given mortal risk for example, would be expected to rise with rising incomes. If CVs for a given mortality risk are corrected for income, they would be expected to remain roughly constant over time (though they might also be weighted according to prioritarian concerns), and it would therefore be incorrect

to use the diminishing marginal utility of income as a reason for placing less value on a future life than on a present one (see Gravelle & Smith 2001).[34] Project consequences for which individuals have large positive income elasticities of demand may lead to CVs which rise over time even after correction for the marginal utility of income (Price 2000).

Second, as noted above, income growth takes place at very different rates in different groups of individuals, and in different time periods. Over the last thirty years, average per capita income in China has shown double digit annual growth rates, while those in sub-Saharan Africa have stagnated, or even declined (UNDP 2006). Similarly, Western Europe experienced very different long-run growth rates prior to and after the industrial revolution (Maddison 2006a,b).

Third, whether they are applied intra- or inter-temporally, social welfare functions based on prioritarian concerns represent an arbitrary foray into deontological territory, one which ignores ethical questions such as whether future generations have any property rights over the physical resources of the world.

It is, thus, undoubtedly important that income-based corrections of CVs, together with other ethical concerns, are applied in a consistent manner whether aggregating intra- or inter-temporally. However, they must be based on explicit income projections, with appropriate spatial and temporal resolution. Only in rare circumstances will these considerations justify a uniform exponential discount rate.[35]

Pure time preference

Pure time preference is a preference for utility experienced now rather than at some other time, independent of any other considerations. It is the component of time preference which remains after risk, uncertainty, mortality, bequest motives and changes in the marginal utility of income have been allowed for (HM

[34] If, however, life expectancies continue to rise, the value of a statistical life might well increase even after correction.

[35] One might also observe that support for discounting seems to be more widespread in CBAs than support for CV correction. This might be explained by the fact that society certainly cannot carry out post hoc redistribution of wealth from the future to the present, but the previous section already highlighted the problems with assuming that even intra-temporal redistribution mechanisms exist.

Treasury 2003). It is problematic to measure directly, because of the difficulty of controlling for other factors, which include the individual's own private expectations about the future. Nevertheless, there is evidence that people exhibit rates of time preference which are not fully explicable in other ways (Frederick et al. 2002).

Even if the existence of pure time preference is undisputed, however, its importance for CBA is not immediately clear. Price (1993) summarises psychological evidence that pure time preference in fact represents a preference for "nowness" rather than earliness, implying that pure time preferences are time inconsistent and possibly irrational. Irrationality is difficult to prove, and should not be assumed lightly. However, it is easy in this case to offer reasons why we might expect irrationality. Irrational pure time preference may result from a conflict between *Homo sapiens*' long evolutionary history as fitness maximisers, and *Homo economicus*'s economic role as utility maximisers. Many of our heuristics may still tend to maximise fitness not utility (see e.g. Warneryd 2007). In particular, in most countries, humans have experienced a dramatic reduction in mortality rates over the last century or so (Friedman 2005, Maddison 2006a,b). Whether pure time preference is a genetic fitness maximising heuristic or a culturally inherited utility maximising heuristic the rate may be too high for the lower mortality risk we now face.[36]

If pure time preference is irrational, it can be treated like other cognitive biases which CB analysts might correct for when using an individual's preferences to estimate CVs within their own lifetime. Indeed there is evidence that, given the opportunity, individuals often choose to correct their own myopic biases, for example adopting mechanisms to limit procrastination (Ariely & Wertenbroch 2002).

Whether irrational or not, how should society treat the existence of pure time preference when allocating utility across generations? Some economists argue that

[36] Since lower rates of pure time preference lead to better educational outcomes (e.g. Fersterer & Winter-Ebmer 2003), and since the latter reduce fertility (e.g. Basu 2002), there may not be any reason to expect irrational pure time preferences to decline in the population at large. It will depend on the relative success of genes coding for high pure time preference rates and memes coding for smoothed consumption and lifetime utility maximisation.

it is immoral for society to pay heed to individuals' impatience (Price 1993) and this "prescriptive" approach (Portney & Weyant 1999b) is the position taken by Stern (2006) who adopts a zero rate of pure time preference. Nordhaus (2007b) and Weitzman (2007) disagree, arguing that it is not for economists to overrule the preferences of individuals.[37] There are three questions here. First, to what extent does market behaviour informs us about individuals' moral beliefs? Second, how should we aggregate the moral beliefs of those who constitute the moral community (Kopp & Portney 1999)? And third, who constitutes the moral community? As Deaton (2007:4) says:

> "Whatever it is that is generating market behaviour, it is not the outcome of an infinitely lived and infinitely far-sighted representative agent whose market and moral behaviours are perfectly aligned, and who we can use as some sort of infallible guide to our own decisions and policies."

We must be careful about inferring an individual's normative position on the rights of future generations from their saving decisions. Even if it were rational, it is not at all obvious why an individual's refusal to save money at less than 4% interest implies a belief that, say, future generations do not have a right to certain natural resources – the individual may impose ethical limits on their own utilitarianism. As noted in the previous section with regard to inequality aversion, economists may be guilty of misinterpreting behaviour as revealing moral beliefs which the individuals themselves do not hold.

The question of how we should aggregate moral beliefs of the community and who represents the moral community are somewhat circular, since the definition of the moral community is a normative judgment (Shoemaker 2007). However, if we assume that future generations would show time preferences which are similarly symmetrical around "now" as those of the current generation, then the pure time preferences of each generation would cancel out when aggregated. A resulting question is, then, whether it is unacceptably paternalistic to include future generations in the moral community, *possibly* against the wishes of the

[37] Deaton (2007) argues that this illustrates a sharp divide between British and American economists.

present generation (Deaton 2007, Weitzman 2007). This is an important question, but not one that economists can easily answer. Of course, this does not mean that pure time preference can be ignored in other parts of the CBA. That it helps determine interest rates is a positive fact, not a normative judgment, and as described above, interest rates do matter for CBA.

Risk, uncertainty and extinction

The final rationale for discounting future costs and benefits is that they may not, in fact, occur (Price 1993, HM Treasury 2003, Stern 2006). Technologies may be developed which avert damages much more cheaply than is currently possible, or which render expected benefits obsolete and thus valueless. In the context of climate change, the probability that an efficient carbon storage technology will be developed is clearly important in determining the case for emission cuts now rather than later (Gilotte & Bosetti 2007).

Once again, this is a valid concern, but not one which should be subsumed into a blanket exponential discount rate. Technological progress is likely to be related (perhaps circularly) to economic growth (see Chapters 3-6), but more importantly, specific advances are likely to be in response to specific incentives. Carbon storage technology may be more likely under conditions where incentives for carbon efficiency apply. Therefore, the rate of technological progress may not be equal in the status quo and project worlds. Risk and uncertainty are best addressed through ascribing explicit probabilities to events.

Some CB analysts have argued for discounting the future at a low but positive rate which reflects the annual probability of human extinction, which would nullify all future costs and benefits in both project and status quo worlds (e.g. Ng 2005). For example, despite eschewing pure time preference, Stern (2006) included a constant annual risk of extinction of 0.01%. This appears to be the only plausible rationale for a project-wide discount rate, though even this rate might best be considered endogenous to the particular project, or risk becoming a self-fulfilling prophesy (Price 1993).

Must "non-discount rates" equal market interest rates?

Price (1993) argues compellingly that the various processes put forward as providing a rationale for exponential discounting, while often valid in specific circumstances, cannot be adequately represented by a homogenous exponential discount rate, assumed to equal market exchange rates. He therefore advocates the abandonment of conventional discounting, to be replaced by a piecemeal approach, which deals appropriately and explicitly with each of the issues above.

This position has been at least partially endorsed by Stern (2006) who explicitly justified his 'discount rate' by building it up from its component parts, the resulting rate of 1.4% being considerably lower than the market interest rates normally used (though see Weitzman 2007 for discussion of the correct market interest rate).

Dasgupta (2007c), Nordhaus (2007b) and Weitzman (2007) have all argued that the analysis of Stern (2006) is flawed because his chosen parameters are incompatible with observed savings and interest rates. The basis for these criticisms is Ramsey's (1928) equation which says that the interest rate r, is determined by:

Equation 2.4 $$r = \delta + \eta \cdot g$$

where δ is the rate of pure time preference; η is the elasticity of marginal utility of income; and g is the growth in per capita income.

Since Stern takes g = 1.3%, δ=0% (plus 0.01% extinction rate), and η=1%, this implies an interest rate, of 1.4%. Nordhaus (2007b) claims that this is too low compared with observable interest rates, which he puts at nearer 6%. Dasgupta (2007c) follows the same logic, observing that Stern's values would imply a savings rate of 97% at interest rates of 4%, and argues that Stern's value for η is too low.

Dasgupta may be correct that Stern's value for η is too low. However, it is not clear to me that Ramsey's equation can be used to determine the consistency of the parameter values used in a CBA. To argue that it can, is really just to restate that future CVs must be discounted at market interest rates. Dasgupta (2007c) appears to argue that, in matters of public policy, η must take up the slack left by

reducing δ to zero (something he endorses). But this begs the question: if a policy maker decides to include future generations in the moral community, choosing a zero pure time preference rate, is there any reason why the "discount rate" implied by these decisions should equal a market interest rate, given that the market interest rate is partly composed of a pure time preference rate, which the decision-maker has declared to be irrational, immoral or irrelevant? The fact that Ramsey's (1928) equation can be used in positive economics to estimate what interest rates will be, given observed values of δ, η and g, surely does not imply that it can be used to determine what 'discount rates' should be, in a CBA?[38] The values of δ and η may have different meanings in the two contexts. In positive economics, δ represents an individual's pure rate of time preference, in the absence of ethical concerns about the rights of future generations, while η represents their elasticity of marginal utility. However, in normative CBA, the former represents the risk of human extinction, while the latter incorporates both the elasticity of marginal utility and any additional inequality aversion.[39] If the value taken by each of the two parameters were necessarily identical in the two economic applications, Rawls' (1971) *Theory of Justice* could be disproved simply by observing market exchange rates.[40]

VI. A generalised CBA for global environmental issues

> "When all the requirements of ubiquitous market-centered evaluation have been incorporated into the procedures of cost-benefit analysis, it is not so much a discipline as a daydream." Sen (2001:116).
>
> "Do old fallacies ever die?" Friedman (1992).

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[38] DeLong (2006), responding to an earlier manuscript by Dasgupta, makes a similar argument.

[39] In addition, when determining interest rates, g represents the individual's *expectation* of their future income growth over the period of the investment, which may be short, while in CBA it represents society's projection of income growth over the period of the project, which may be long.

[40] Put another way, we could take the existence of non-zero murder rates in the real-world as evidence that, when deciding public policy, we should assume that some non-zero murder rate is morally desirable. The *point* of public policy is that it is, in some senses, paternalistic and coercive.

CBA has the potential to improve the accuracy and rationality of public choices when decision-makers are faced with highly complex issues. However, the preceding review has highlighted a number of flaws in conventional approaches to CBA which limit its ability to evaluate global environmental issues. These issues present particular challenges for CBA, as their effects cross different nations, levels of wealth and time periods. In order to meet these challenges, two major changes in the way CBA are currently carried out are required:

1. The pretence to a decisive, value-free CBA should be abandoned in favour of a two-stage procedure: relatively value-free projections of CVs, income growth rates, etc; followed by an aggregation stage driven by society's value judgments, but assisted by economists.

2. Generalised CBA procedures should be developed, free of arbitrary assumptions and omissions present in conventional CBA and able to be applied to the widest possible range of cases.

These shifts have been advocated in one form or another by many previous authors (e.g. Price 1993, Sen 2001, Azar & Sterner 1996) and they represent the logical conclusion of the many critiques of CBA. Nevertheless, CBAs of global environmental issues still use essentially the same methods as those developed for bridge building and tree planting - and their results may be treated as decisive. Therefore, to summarise the review, I outline in this section the form that I believe a generalised CBA should take.

Estimation of compensating variations

Few adjustments are necessary to the standard approach, which uses CVs to measure project consequences, except to stress that CVs should not be corrected or "idealized" at this stage, other than to correct for distortions due to imperfect information and cognition.[41] Ideally, such corrections should take the form of improved study design and project specification, rather than post hoc adjustments, in order to limit the potential for analyst bias. CVs should be as complete as possible; however, some components of a CV that relate to ethical concerns over

[41] Of course, considerable improvements might be made to the methods used to estimate CVs, but these lie outside the scope of this thesis.

the rights of the moral community might be omitted if they are difficult to estimate reliably. If so, these preferences must be explicitly incorporated into the aggregation stage, below.

Aggregation

In the case of a CBA used to advise decision-makers, this stage requires that the decision-maker's ethical preferences are solicited in order to reach a conclusion, which would then be decisive. The CBA is therefore shaped by decision-makers, and cannot be presented to them as a fait accompli. It proceeds through four steps, the process being iterative, as decision-makers explore the consequences of their ethical preferences. Of course, this does not preclude analysts from anticipating the likely preferences of decision-makers (perhaps through studying past judgments) in order to minimise iterations. Neither does this conceptualisation of CBA preclude analyses carried out for the purposes of research (as in Part III), but it does strictly limit the conclusions which can be drawn from them. For example, it is not possible to claim, as Balmford et al. (2002) do, that "the overall benefit:cost ratio of an effective global program for the conservation of remaining wild nature is at least 100:1". Instead, such analyses can explore the effect of certain ethical assumptions, in the manner of Tol (2001), but their results can only be illustrative, not decisive. The distinction between these two forms of CBA must explicitly been made.

Step 1: identification of winners and losers

First, the distribution of CVs (corrected and uncorrected) can be analysed, to identify winners and losers, and the reasons for their losses and gains. Next, separate aggregations of positive and negative CVs (uncorrected) give a rough indication of the monetary amounts which might be recouped from winners, or which might need to be distributed to losers, if it were deemed necessary. The results of this step allow the analyst to solicit, and probably anticipate, decision-maker preferences; they have no other significance.

Step 2: evaluation of compensation mechanisms

Where the decision-maker is likely to indicate that individuals have claims over utility losses, plausible compensation mechanisms must be designed. These should be evaluated in the same way as the original project: their costs must be

estimated, both in terms of the utility losses of those who pay for the mechanism, and the operating or deadweight costs. The efficacy of these mechanisms in compensating individuals who lost from the project should also be estimated, as well as any other side benefits. In effect, the compensation mechanisms should be treated as variations on the initial project. Therefore, CVs must be recalculated for each possible mechanism. Such project redesigns are not restricted to compensation mechanisms, but could apply to any aspect of the project, and might be prompted by expert opinion as well as decision-maker value judgments. Thus CBA has an important role in research and project design, as well as decision-making (as noted by Hahn & Tetlock 2008).

Step 3: incorporation of ethical concerns

The views of decision-makers must now be solicited on the strength of each individual's claim to the utility which would be lost or gained as a result of the project. This might take two forms:

A social welfare function, parameterised to reflect the strength of the decision-maker's pure aversion to utility inequality, ρ. This is society's inequality aversion in the absence of other ethical concerns over property rights etc. This can be thought of as applying to "surplus" utility, generated by the project and belonging a priori to no-one.

A judgment as to the strength of individuals' claims to the utility they lose or gain as a result of the project. This is an ethical parameter, e_{ij} representing the strength of individual i's claim to welfare loss (or gain) j. The value of this parameter will determine those to whom compensation should be paid, and from whom it should be extracted. Decision-maker judgments should be sought with regard to the property rights of individuals, who, while not directly affected by the project, might nevertheless be targeted for cost recovery, e.g. the payers of general taxation.

Step 4: aggregation

Aggregation is best explained first in the simple case, where complete compensation is made, and second in the case where it is not.

In the case where the compensation of negative CVs is projected to be completely achieved, the project (including compensation) can be said to be ethically neutral

with regard to rights to property or free action (note that it is ethically neutral relative only to the status quo, and not in any absolute sense). In this, the simplest case, the project and status quo can be compared on the basis of individual utility changes (corrected-CVs), aggregated according to the social welfare function specified in step 3.1. The sign of this function determines whether the project with compensation is more or less socially desirable than the status quo, and the value of the function can be used to determine the optimal compensation regime. In addition, the project with compensation can be compared to that without, providing an estimate of the opportunity cost (in social welfare terms) of insisting upon compensation.[42] Thus, through the explicit inclusion of compensatory mechanisms, ethical concerns can be incorporated into CBA, while still complying with Sen's (2001) principle of additive accounting. This is done by calculating the opportunity cost of rights in terms of social welfare foregone.

However, compensation may be imperfect, either because some losers go uncompensated, or because compensation is paid for by individuals who may not have positive CVs for the project (e.g. general tax payers). In this case, utility losses to which individuals have a claim enter into the social welfare function, but as the product of the individual's CV and the ethical parameter e_{ij} (step 3.2 above).[43] This parameter is subjective, and may be different for different components of the individual's CV. e_{ij} is therefore related, but not directly analogous to, the individual-specific weight a_i in the generalised social welfare function 2.4, repeated below (as 2.6).

Equation 2.5 $$W = [\sum_{i=1}^{i} a_i (.V_i)^{1-\rho}](1-\rho)$$

Since the parameter a_i applies to the entirety of an individual i's welfare function V_i, it represents the special case where the individual's claim to the whole of their utility is determined by a single factor. This might be the case when, for example,

[42] Note that in the case where the compensated individuals are poorer than the compensating individuals, this opportunity cost may actually be negative, if the deadweight costs of compensation are sufficiently low compared with the elasticity of the marginal utility of income and the inequality aversion parameter.

[43] This parameter is necessary, because the inequality aversion parameter supplied in step 3.1 relates to project surpluses over which no individual has a claim, and must therefore be moderated with respect to specific losses and gains.

a criminal is sentenced to death, as a result of society's judgment about their moral standing. Where society acts only to prevent certain criminal acts (e.g. speeding), but otherwise considers the would-be speeder a full member of the moral community, it might zero-weight their CV for speeding, leaving other preferences unaffected (i.e. $e_{i,speeding}$= 0, $e_{i,j}$ = 1). It is important to note that although it is subjective, a change in the value of the parameter $e_{i,j}$ can be priced in terms of its social welfare opportunity cost (as I demonstrate in Chapter 11).

An example

For simplicity, we assume that there are two individuals, A and B, where B is poorer than A; and B's marginal utility of income is consequently higher than A's. B requires compensation as a result of the project (CV_B<0), but this compensation can only be supplied by taxing A, who is otherwise unaffected by the project (CV_A=0). Because of its positive inequality aversion (ρ>0), and the positive elasticity of marginal utility of income (η>0), society prefers project surpluses to go to B. However, society also values A's claim to the welfare level A enjoys under the status quo, out of respect for A's property rights. Therefore, the decision society faces is whether or not to take money from A and give it to B. This depends on five things:

1) The relative marginal utilities of income of A and B (which determine the correction factors applied to their CVs).
2) The inequality aversion parameter, ρ (which measures the strength of B's relative claim on the income, in the absence of other rights, on the basis of B's lower welfare).
3) The strength of B's claims to the utility B lost as a result of the project, $e_{B,Project}$
4) The strength of A's claims to the utility they lost to the compensatory tax, $e_{A,Tax}$
5) The efficiency of the compensation mechanism, which measures the proportion of money lost to deadweight costs.

The parameters $e_{B,Project}$ and $e_{A,Tax}$ are subjective, and supplied by the decision-maker, but the opportunity costs of adopting particular values, in terms of social

welfare, can be calculated for a given value of ρ. The values can also be compared across decision-makers and projects, and this framework could therefore increase the transparency of decision-making, and provide a useful analytical tool for political economists investigating the consistency of decision-makers, and the relative support that they give to certain classes of rights (e.g. to property, free action, equality).

Inter-temporal considerations

Discount rates cannot simply be inferred from market interest rates, as claimed by Nordhaus (2007b). In fact, it can be argued that they cannot legitimately be used at all (Price 1993). Explicit treatment of relevant processes, such as income growth, and its effect on the marginal utility of income, is the best way to proceed, and only rarely will circumstances allow this process to be collapsed into a single discount rate, for the sake of minimising decision costs.

As discussed in Section V, the opportunity cost of capital, marginal utility of income, and human extinction risk must be considered within the CBA, on a case by case basis. Therefore, we can see that time itself requires few adjustments to the atemporal aggregation procedure outlined above. The requirement to correct CVs for the diminishing marginal utility of income should apply equally in the future as in the present (e.g. Schelling 1999). Of course, this requires explicit projections of income growth (which may in any case be necessary in order to predict the volume of benefits, see next chapter and Chapters 9-10) and a value for the inter-temporal elasticity of marginal utility, η, which might differ from the intra-temporal value if adaptation to higher income levels occurs. Market interest rates are relevant only to cash flows, which may be a relatively small part of many CBAs of global environmental issues, though they may be an important consideration in the design of compensation mechanisms. Inter-temporal compensation can be treated in the same explicit way as it is intra-temporally. Pure time preference is an ethical parameter, similar to those considered above for rights and inequality aversion. If future generations are included in the moral community, this should be given a value of zero, as done by Stern (2006). A positive value implies a successively weaker right to welfare for each subsequent generation. Human extinction risk can be accounted for by project-specific rates, which may change over time.

If compensation is incompletely or excessively made, this may affect the individual's welfare in future years, as well as in the year in question. This must be true if CVs are estimated independently over time, in other words, if an individual's CV for the project in year n is calculated assuming that their CVs were made in all years: 0,1... n-1. If this is the case, failure to compensate them for losses in earlier years as well as in year n, might result in a welfare loss in year n which is greater than that implied by their CV for that year, if they would have invested some proportion of the previously lost income. This will be determined by their marginal propensity to save, and the rate of return available to them. Strictly, if there are positive externalities to capital investment, the welfare of others may be affected as well. In practice, CVs may or may not be estimated independently, something I discuss in relation to the case study in Chapter 10.

The necessity of explicit income projections

Explicit income projections for each individual or group of individuals are an important requirement for any project spanning more than one time period. They are needed to correct future CVs for the diminishing marginal utility of income; to project the market interest rates to be applied to cash flows and compensation mechanisms; and to estimate the rates of return available to individuals and their propensity to save.

In addition, explicit income projections are also important for determining the volume, as well as value, of many costs and benefits. Income affects both the desirability of a good relative to other goods, and the amount an individual can afford to spend on it. Together with other factors such as taste, income therefore determines both an individual's willingness to pay for a good and the quantity of the good demanded by the individual (at any given price). While a higher income will always increase the ability to pay for a good, the effect of income on willingness to pay and quantity demanded is indeterminate.

A criticism anticipated

The most obvious criticism is that the generalised procedure outlined above is considerably more complex than the conventional aggregation approach, of summing CVs which have been discounted at market rates. However the procedure simply makes explicit the moral decisions which a decision-maker must

make. That the procedure appears complex shows that CB analysts should reconsider their assumption that decision-makers are capable of making such calculations, rationally and consistently, completely unaided. Second, CBA in its present form assumes that decision-makers are incapable of estimating the welfare implications of projects (CVs), yet capable of performing (presumably in their heads) the calculations necessary to weigh their importance against other ethical considerations. Third, if this generalised CBA were adopted as standard practice, it is likely that decision costs would quickly be reduced, as we accumulate information which is transferable from one CBA to another, such as income projections and information on decision-maker preferences. Finally, like any CBA, the procedure can of course be simplified on a case by case basis in order to constrain decision costs. The difference is that by starting from the general procedure outlined above, simplifications can be made on the basis of sensitivity analysis, rather than being the product of historical precedents.

VII. Conclusions

I have reviewed many of the critiques of CBA, highlighting the deficiencies in existing approaches, most importantly the arbitrary and simplistic approach to aggregation. These deficiencies are likely to be of great significance in CBAs of global environmental issues. To summarise these deficiencies, I have outlined the generalised form that CBA must take to address them while still fulfilling Sen's three basic principles (Sen 2001). In Part II (Chapters 3-6) of the thesis I consider the explicit income projections required by a generalised CBA, and in Part III (Chapters 7-11), I investigate the social desirability of nature conservation in developing countries, exploring the issues of aggregation and compensation using a case study from Madagascar.

3. 21st century economic growth: comparing IPCC projections with historical data

Abstract

Long-term income projections are rare and controversial, yet vital in applied economics. The best known have been developed by the Inter-governmental Panel on Climate Change and, despite criticism by some economists, underpin its most recent assessments. I present the first rigorous comparison of these projections with the historical data. I show that the results depend heavily on the models considered and that convenient comparisons of growth and convergence rates are not meaningful. I make recommendations for the development of future projections and also for the way that economists statistically compare growth projections.

I. Introduction

Where will per-capita income levels stand in 2100? Long-run projections of income are vital in applied economics: they determine discount rates in cost-benefit analyses of public policy and help predict important variables such as greenhouse gas emissions. Yet mainstream economics is largely devoid of formal long-run projections (Lucas 2000 provides a rare example). Informal predictions are more common but often involve cursory empirical analysis and no explicit justification of the model used (e.g. Beckerman 2003:16-17[44]). In cost-benefit analysis, growth rates are implicitly predicted whenever discount rates are chosen.[45] The choice of discount rate often determines the conclusion (see e.g. Nordhaus 2007b), yet only rarely does this result from explicit economic modelling (see Nordhaus 1993 for one example).

Perhaps this is unsurprising. Temple (1999) observed that, despite the undoubted importance of the subject, "the study of growth at the aggregate level has often been something of a backwater", due to a lack of data and the perceived exogeneity of the primary driver of growth; technology. More recently, Easterly (2007) concludes that the explosion of empirical research into the causes of growth has "collapsed from a surplus of answers".[46] Away from academic research, the long-range projections of most economic and finance organisations span years rather than centuries.[47]

Regardless of the predictive power of growth theory, strong demand for long-range projections of economic growth has encouraged their supply, particularly in the domain of global climate change. The best known projections are those

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[44] Beckerman, like Lucas, uses an exponential function to extrapolate past growth rates. Neither author provides any theoretical justification for their choice, or any measure of goodness of fit to the historical data.

[45] Whether discounting is justified using the opportunity cost of capital or from diminishing marginal utility of income to richer, future generations, they ultimately derive from economic growth.

[46] Easterly quotes Durlauf et al. (2005) who note that "approximately as many growth determinants have been proposed as there are countries for which data are available".

[47] The World Bank's "Global Economic Prospects" includes "long-term global scenarios which look ten years into the future" (World Bank 2007). The International Monetary Fund's World Economic Outlook, published in October 2007, forecasts output to 2012 (IMF 2007)

published in the Special Report on Emissions Scenarios (hereafter SRES, Nakicenovic & Swart 2000), and which underpin the most recent assessments by the Inter-governmental Panel on Climate Change (IPCC). Critics (e.g. Castles & Henderson 2003a,b) compared the projections to the historical data concluding that they are implausible, which defenders of the SRES (Nakicenovic et al. 2003, Grübler et al. 2004) hotly denied.

However, as I describe below, these comparisons had neither an explicit economic basis nor rigorous statistical methodology. In this chapter, I attempt to address this by carrying out the first comparison of the scenarios with the historical data, something requested by the SRES team itself:

> "We feel that [Castles and Henderson] have quoted selectively from the literature and that much greater clarity, precision, and comprehensiveness is required in the presentation and criticism of SRES. A peer-reviewed evaluation of their criticisms could resolve some of these issues and help inform future emissions assessments." Grübler et al. (2004 p13).

This study is important in three respects. First, the SRES projections underpin the IPCC's latest assessment,[48] which will not be superseded before 2013 and is likely to have significant influence on public policy. Understanding these scenarios is vital to understanding the assessment itself. Second, the IPCC has called for the development of new scenarios to underpin the next assessment,[49] and the present study aims to inform both the development and criticism of future income projections. As the focus shifts from climate science, to the economics of climate-change policies and adaptation (Hopkins 2007), income projections will assume even greater importance. In particular, there will be a greater emphasis on the relative as well as absolute income levels of nations and therefore on predictions of convergence as well as growth. Whereas previous critiques have focussed on growth rates at the expense of convergence, I try to redress the balance. In addition, much attention has focussed on the quantification and presentation of

[48] http: www.ipcc.ch

[49] In an open letter from R.K Pauchari, Chairman of the IPCC, addressed to the scientific community, dated 12 September 2006.

uncertainty in the IPCC process (see Schneider 2001, Pittock et al. 2001), yet this issue has received little attention in the debate over the SRES income projections. I provide an analysis of the different kinds of uncertainty in projecting income, and make recommendations as to how they should be presented. Finally, the analysis presented here has wider implications for the way that economists instinctively conceptualise economic growth, and the way that income projections are parameterised and evaluated. In particular, I show that the historical data provides little support for representing and extrapolating growth as an exponential function. I also show that convergence must be seen as non-linear, non-monotonic, and above all potentially cyclical; a product of at least three different processes. This implies that, like annualised percentage growth rates, convergence rates estimated using linear, monotonic models cannot be usefully extrapolated from past to future.

In Section II of this chapter I describe the SRES and the controversy surrounding it. In Section III, I outline the scope of the study and my analytical approach. The candidate set of models are identified in Section IV and I explain my treatment of the data and statistical methods in Section V. In Section VI, I present and discuss the results for the richest region, the OECD, and in Section VII for the two developing regions: Asia; and Africa, Latin America and the Middle East (ALM). Finally, I conclude in Section VIII with a brief discussion of some points general to the study as a whole, summarise my conclusions and make recommendations for the development and evaluation of future income projections.

II. The SRES and the controversy

In the late 1990s, the IPCC published the SRES to provide the basis for its Third Assessment Report. One objective of the scenarios was to provide projections of greenhouse gas emissions to be used in the climate models but the IPCC also made clear that the scenarios were to be used for “assessing alternative mitigation adaptation strategies” (Nakicenovic & Swart 2000). Therefore, they included projections of income (GDP per capita), which is both an input variable in constructing emissions projections, and an important output of the scenario process in its own right.

The controversy over the SRES scenarios began with a critique by Ian Castles and David Henderson.[50] This took the form of seven separate letters and documents they had sent to the IPCC, and which were then published as a single paper in *Energy and Environment* (Castles & Henderson 2003a). This was directly followed by a reply from the SRES authors (Nakicenovic et al. 2003) in the same issue. Castles and Henderson responded to this reply (Castles & Henderson 2003b), and again the SRES team replied (Grübler et al. 2003).

Castles and Henderson's most relevant criticisms of the SRES projections were that they predominantly used GDP converted to international dollars using market exchange rates (GDP_{MER}) rather than purchasing power parity rates (GDP_{PPP}), and that they made over-optimistic assumptions about convergence between developing countries and the west.[51] In their view, using GDP_{MER} 'exaggerated' the initial gap in income, and combined with the convergence assumptions led to developing country growth rates that were historically implausible. In addition, they asserted that where the SRES did project GDP_{PPP}, the projections were unsound as they do not display the characteristics expected of them.[52] The SRES team, for their part, robustly defended the scenarios (Nakicenovic et al. 2003, Grübler et al. 2004). Earlier IPCC scenarios have been criticised for their assumption that per-capita income growth rates will slow over time, something also assumed by the SRES scenarios (Nakicenovic and Swart 2000, and see below) but for which "there is no historical basis" according Nordhaus (1994).

The debate was covered in the popular press (see e.g. The Economist 2003) and sowed serious doubt (in some quarters at least) about the validity of the SRES and therefore the wider IPCC process. None of the four papers were peer-reviewed, however, and the debate ended with neither side having convinced the other:

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[50] Respectively, former head of Australian National Statistics and former chief economist at the OECD.

[51] The critics ascribe these failings to their development by "an unrepresentative professional milieu", from which economists and economic historians were absent. They do concede, however, that the IPCC should "have no illusions about what economists can say hope to say with confidence about the future".

[52] I explore and largely reject this criticism of the PPP projections in Appendix B.

> "For the reasons that we have outlined in this article and its predecessor, we do not think that the SRES should be taken as the accepted point of departure for this coming Fourth Review." Castles and Henderson (2003b:430).

> "We feel that Castles and Henderson have quoted selectively from the literature and that much greater clarity, precision, and comprehensiveness is required in the presentation and criticism of SRES. A peer-reviewed evaluation of their criticisms could resolve some of these issues and help inform future emissions assessments." Grübler et al. (2004:13).

This debate prompted further criticisms by others, notably by McKibbin et al. (2004) and Ryten (2004). However a peer-reviewed evaluation of the SRES concluded that the scenarios were adequate and did not need updating before the fourth assessment report (van Vuuren & O'Neill 2006), though in an editorial Stegman (2006) highlighted the narrow scope of this review. Finally, several studies have been published that, like McKibbin et al. (2004) focus on assessing the effect on emissions of the alleged deficiencies. Manne et al. (2005), Tol (2006) and van Vuuren and Alfsen (2006) all found only a small effect in contrast with McKibbin et al.'s analysis.

To date, reviews and critiques of the SRES have been selective and limited in scope. For example, Castles and Henderson's critique was hampered by their confusion of low-growth scenarios with low-emissions scenarios, and most of the historical comparisons they provide relate only to the latter, which in fact assumed relatively high economic growth rates. Surprisingly, they mix GDP_{PPP} and GDP_{MER}, comparing historical growth rates of the former with projected growth rates of the latter. They are also selective in the periods and countries with which they compare the SRES projections. Despite asserting that the convergence assumptions adopted by the IPCC are implausible, they focus only on developing country growth rates without directly comparing convergence rates. Finally, they do not explicitly define their economic models, nor provide a clear statistical analysis, both of which I will demonstrate to be crucial. The SRES team's response (Nakicenovic et al. 2003, Grübler et al. 2004), because it focussed on directly refuting the points raised by Castles and Henderson, suffers from some of

the same limitations as the original critique, as do other papers criticising the scenarios (e.g. McKibbin et al. 2004). In the only quantitative evaluation of the SRES to be peer-reviewed (van Vuuren & O'Neil 2006), the only comparison with historical data compared the SRES projections with the data observed in the decade *since* their development (1990-2000). Thus, the plausibility of the SRES scenarios compared to the historical data has yet to be rigorously tested. Appendix A gives a more detailed review of the SRES controversy.

III. Analytical approach and historical scope

In this study, I do not assume a particular model of economic growth. Instead, I explicitly define, a priori, a candidate set of economically plausible models (see Section IV) which are then fitted to the historical data. In contrast, the SRES together with previous critiques, reviews and responses, simply compared projections with the past using annualised percentage growth rates, the implication being that if they differed, the projections were historically implausible. There are two problems with this approach. First, it does not provide a very informative comparison unless growth has been perfectly exponential in the past. Second, in the context of developing countries, it makes little economic sense to compare their growth rates in isolation from their contemporaries at the productivity frontier. Below I outline how my approach addresses these issues before discussing the data I use and the historical scope of the analysis.

Analytical approach

Once annualised percentage growth rates are abandoned, inter-temporal comparisons become more complex. The only way to properly compare past and future is to represent the past using an appropriate model or models and then to extrapolate it into the future. This extrapolation can then be directly compared with the projection, and the degree of overlap determined. This allows a measure of confidence to be assigned to any conclusion of similarity or difference that takes account of both model selection uncertainty and model fitting error. Of course, the result is dependent on the set of models initially considered, and assumes that growth is a smooth function and that the function remains constant over time.

After identifying the candidate set of models (Section III), I evaluate this set using an information theoretic approach; the Akaike Information Criterion (AIC). This combines an estimate of the relative distance of each model from the 'true' model (the Kulback-Leibler distance), and a penalty for each parameter included in the model, to prevent over-fitting (Burnham & Anderson 2002). I then use weights based on the AIC scores to produce a model-averaged extrapolation of the past (Burnham & Anderson 2002:75). Finally, I compare the probability distribution of this extrapolation with the full range of SRES scenarios, to estimate the degree to which the scenario range encompasses the extrapolation of the historical data. This method incorporates both model selection uncertainty and prediction error, given a model.

Extrapolating the future from the past implies the 'assumption of continuity': that future trends are discernible from past data, or that the past and future form part of the same phase or population (Makridakis et al. 1998). To argue that projections should be confined to extrapolating past trends is to make a strong assumption of continuity. I stress that I do not suggest that the SRES scenarios should be limited to such an "inappropriate historical determinism" (Nakicenovic et al. 2003:200), although I do argue in Section VII that they should encompass, and be centred upon, such an extrapolation. However, whether or not this latter point is accepted, it remains true that the only way to compare scenarios with the past is to extrapolate the past using explicitly defined and objectively evaluated models.

Inspired by Solow (1956),[53] economic theory usually treats the growth of developing countries (which are capital-poor) differently from those in the vanguard of economic growth, which operate at the productivity frontier; different processes are assumed to be operating in each case. For example, in neoclassical growth theory, technology is pre-eminent in driving the long-run rate of growth in vanguard economies, while in poorer countries, their growth rates are determined in part by their position relative to the vanguard economies (Barro & Sala-i-

[53] As Mankiw et al. (1992) make clear, the Solow (1956) model actually referred to the behaviour of a single economy around its own steady state, rather than the behaviour of poorer economies relative to richer ones. Nevertheless, the convergence hypothesis took root, and a great deal of empirical work followed. The strong assumption of convergence has been tempered by this empirical work, and the consensus now emphasises convergence of poor countries on the rich, conditional on several, somewhat poorly understood factors (see Islam 2003 for a review).

Martin 2004).[54] For this reason, it makes little economic sense to treat developing country growth in isolation from that of the vanguard, as previous critiques have done. In this study, I analyse trends in income level for the vanguard region (the OECD) and in relative income for the developing regions. This allows the causes of any disparities between the SRES scenarios and the historical data to be identified. For example, developing country growth may be too fast for two main reasons. Either the rate of convergence between them and the vanguard is too fast, or the rate of growth in vanguard regions is too high. The framework adopted here allows these two possibilities to be clearly separated.

The SRES projections

Despite the weakness of Castles and Henderson's critique, they were correct on at least one major point: the desirability of projecting GDP_{PPP} (see Nordhaus 2007a).[55] The SRES team have robustly defended their GDP_{PPP} projections as valid. In Appendix B, I show that the relationship with their sister GDP_{MER} projections is plausible, therefore, I confine my analysis to the projections of GDP_{PPP} (produced by the MESSAGE modelling group, one of five groups that produced emissions scenarios for the SRES). Since the assumptions underlying the economic projections were common to all groups, these are representative of the SRES as a whole.

The SRES projections of population and GDP are available for ten-year intervals (1990-2100) from the IPCC emissions scenarios database (Morita & Lee 1998[56]). The projections were made at the level of four large regions (Table 3.1). Since disaggregated data is not available, my analysis is carried out at the level of these regions. Because of the lack of reliable historical data for the REF region (see Section IV and Appendix C) I exclude it from the study.

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[54]This point was recognised in the SRES (Nakicenovic & Swart 2000). However, the SRES team do not discuss convergence rates, only growth rates, in their debate with Castles and Henderson.

[55] As Ryten (2004) notes, "the only viable alternative to the use of inadequate PPP-based estimates is better PPP-based estimates", and as far as predicting welfare is concerned, this is surely true.

[56] Version 3, downloaded from: http://www-cger.nies.go.jp/scenario/index.html, 23rd August 2007.

Table 3.1. Regions used in the SRES[57].

Region	Definition
OECD90	Member countries of the Organisation for Economic Cooperation and Development in 1990: North America, Western Europe, Japan, Australia and New Zealand.
REF	Countries undergoing economic reform: Eastern Europe and the former USSR
ASIA	Asia, including the pacific.
ALM	Africa, Latin America and the Middle East.

Historical data and scope

When comparing projections with the past, the historical scope of the analysis will determine the result. Analysing a longer sample period will potentially increase the number of trends discernible in the data, leading to different, possibly more accurate predictions. On the other hand, it will likely increase the number of distinct phases represented in the data, increasing the within-sample complexity, and reducing the chance that the data can be adequately described by a simple, well-understood function. Therefore, it is helpful to divide the past into a number of phases, each of which can be approximated by relatively simple functions, and to distinguish between, on the one hand, trends within a phase and on the other, phase shifts, when the data show a marked transition to another trend. If these phase shifts themselves appear to follow a consistent pattern, they can be characterised as “mega-trends” and subjected to statistical analysis allowing them to be extrapolated (Makridakis et al. 1998). This is the approach adopted here, and I proceed as follows. First, a formal statistical analysis is used to compare the SRES with the modern data from the most recent phase of growth and this comparison therefore makes a strong assumption of continuity. Second, this analysis is placed within the context of the broader sweep of human history to identify pertinent phase shifts and mega-trends, in order to consider the plausibility of this assumption.

In the case of income, another consideration in determining the historical scope of the analysis is the availability of data. Modern national accounts were developed in the 1920s and 30s, and began to be published in developed countries after the

[57] For full definitions of the regions see Appendix III of IPCC (2001): http://www.grida.no/climate/ipcc/emission/149.htm

2nd World War (Lequiller & Blades 2007:398-399). Although modern national accounts are themselves complex and imperfect proxies, any estimates of income that predate this are inevitably the results of considerable supposition and inference, the more so the further back they extend (Maddison 2006a,b). Of particular concern for this study is not only the decrease in the reliability of the estimates moving backwards in time, but the possibility that certain models have been assumed in estimating the data. For example, if annualised percentage growth rates are interpolated or extrapolated backwards to fill in gaps in the data, growth over the period will of necessity perfectly fit an exponential function, whether or not growth was in fact exponential.

Thus, for analytical reasons, I draw a distinction between the modern phase of economic growth (1950-2003)[58] for which national accounts are more or less directly available and the longer sweep of history for which income has been estimated by economic historians. The data on the former is taken from the Penn World Table 6.2 produced by Heston et al. 2006 (see also Summers & Heston 1991). This is the dataset used almost ubiquitously in studies of growth covering the post war era. Data on the latter comes from the prodigious research efforts of Maddison (2006a,b).

III. Candidate models

Vanguard economic growth

Economic growth is usually conceptualised as an exponential process (e.g. Lucas 2000, Nakicenovic & Swart 2000, Beckerman 2003), and all sides of the SRES debate have directly compared annualised percentage growth rates from different periods, thus implying the assumption of an exponential function. However, the appropriateness of assuming an exponential function for growth has been questioned by Wibe and Carlén (2006) who advocate the use of a linear function, while Maddison (2006a) suggests that the long-run rate of technological progress is slowing down, implying that growth may at present be sub-exponential. On the

[58] Maddison (2006) identifies a number of internally consistent phases of economic growth, the latest being 1950 to the present, although he does also subdivide this into two phases, broken at 1973.

other hand, Nordhaus (1994) commenting on the previous set of IPCC scenarios, which showed just such a trend, asserted that there is no evidence that per capita income growth slows over time.[59] Indeed, 'new economy' advocates point to steadily increasing growth rates since the start of the industrial revolution and exponential improvements in technology, and predict rapid exponential or super-exponential growth for the 21st century (e.g Kurzweil 2001, Hanson 2000). Therefore, even without allowing for possible limits to growth (which I consider in the next chapter) there is clearly a diversity of opinion about suitable growth functions. This is in line with growth theory. If growth in vanguard economies is at least partly determined by technology, and not simply by capital accumulation (see Barro & Sala-i-Martin 2004 for a review of modern growth theories and evidence), there seems to be no reason why the economy as a whole should be conceptualised as growing in the manner of compound interest. Indeed, there is perhaps no reason why the economy should consistently grow according to any particular mathematical function. Although there is evidence that many physical measures of technology increase exponentially (e.g. Kurzweil 2001, Nordhaus 2007), economic growth results from the interaction between technology and society, and there is no reason why exponential improvements in 'physical' measures of technologies will necessarily result in exponential growth.[60] New Growth Theory provides no consensus on the functional form of income growth. Conceptualising growth as inherently exponential appears to be a throw back to pre-Solow ideas about capital accumulation.

To capture this diversity of opinion (and theoretical agnosticism) about possible growth functions, I included three monotonic models in the a priori candidate set:[61] the conventional exponential function; the linear model suggested by Wibe

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[59] It is important not to confuse global growth rates with growth rates at the technological frontier, i.e. in the richest economies. Global growth rates may often be higher, if poorer countries are catching up with the richest countries.

[60] Diminishing marginal returns to any particular technological advance (e.g. increased computer processing power) may mean that growth is driven more by the rate of *new* innovations, rather than the rate of improvement in existing innovations. It is harder to measure the rate of the former, except through measuring productivity itself.

[61] A super-exponential model was initially included, which allowed the annual growth rate to increase as a function of time, i.e.: $Y = e^{(a+bt)\cdot t)}$, however, the coefficient *b* was found to be negative, meaning that the growth rate *decreased* over time, and the function was discarded

and Carlén; and a power function. This latter represents an intermediate case between exponential and linear growth where annual increments are ever-increasing in absolute terms but ever-decreasing in percentage terms (see Ghiglino 2007 for an argument that productivity follows a power law).

Exponential:

Equation 3.1 $$Y = e^{(a+bt)}$$

Linear:

Equation 3.2 $$Y = a + bt$$

Power:

Equation 3.3 $$Y = a + b \cdot t^{c}$$

Where Y is per capita income, and t is time in years post 1951

Developing region convergence

Castles and Henderson (2003a,b) criticise the SRES scenarios for being overly optimistic about developing country growth, both in absolute terms and relative to the OECD region. They argue that the SRES scenarios have assumed universal and unconditional convergence of developing countries on the OECD, and that this is not justified by the literature. Research, they say, has failed to find evidence for unconditional convergence on a global level, and where it has found convergence, the rates estimated are quite slow[62] (McKibbin et al. 2004 make the same point). The SRES team openly admits that the convergence of developing regions on the OECD was an assumption of the SRES process, but contend that this is in line with the literature (Nakicenovic & Swart 2000, Nakicenovic et al. 2003).[63] The critics are correct that research to date has not shown convergence to be ubiquitous, but rather to be conditional, with certain types of countries converging, while others do not (see Islam 2003 and Abreu et al. 2005 for

[62] An apparent empirical consensus around 2% annual reduction in the relative gap between rich and poor led some to claim a "natural law" but this consensus is questioned by Abreu et al. (2005).

[63] Castles & Henderson 2003a even suggest that the SRES adopted convergence for political reasons. However, it seems strange that developing countries would press for income scenarios which predicted high growth for them, since this would have the effect of increasing pressure on them to take part in emissions curbs, and also reduce the case for helping them cope with future climate change.

thorough reviews of the subject). Are the SRES scenarios implausible then, compared to the historical data? I investigate this by fitting three models to data on the relative gap[64] between the developing region (ASIA or ALM) and the OECD.

The first is an exponential decay function, which reflects the conventional way in which convergence rates are estimated and presented in the literature[65]. When applied to the time series data, this function will estimate a rate of convergence which is analogous to those estimated through 'growth initial regressions' Islam (2003). It was to the results of these analyses that the SRES team and critics refer, and which they implicitly extrapolate into the future when debating the plausibility of the SRES projections:

Exponential:

Equation 3.4 $$G = e^{(a-bt)}$$

Where G is the relative gap in income, t is time in years post 1951, and the constant b >0.

The second model represents the critics' hypothesis that the developing region will not converge, but rather continue to diverge. Since the size of the relative gap is bounded at unity, I use a function that converges asymptotically upon one:

Asymptotic Exponential:

Equation 3.5 $$G = 1 - a \cdot e^{-bt}$$

where b >0 and a >0.

One reason why previous analyses such as growth-initial regressions have found little evidence of convergence may be that they have focussed on monotonic convergence, not allowing for the possibility of both divergence and convergence

[64] Where the relative gap is the gap in per capita income between the developing region and the OECD as a percentage of the OECD's income. This is the natural metric to use because it is referred to ubiquitously in the growth and convergence literature (Islam 2003).

[65] The precise shape of the convergence function depends on the underlying growth model assumed, and the convergence rates estimated are in fact approximations. However, results are normally presented in terms of a parameter λ, which is the annual percentage reduction in the relative gap between the poor country and the vanguard. This implies an exponential decay function, which is assumed here in order to be agnostic about the underlying model.

during the period studied (see Durlauf & Quah 1999). For example, in growth-initial regressions, growth rates over a period are regressed on initial income levels (Islam 2003). A negative slope indicates that poorer countries grew faster than rich countries, thus indicating convergence.[66] However, divergence is a precondition for convergence, since we know from Maddison (2006a,b) that all countries and regions enjoyed roughly the same low level of income a few centuries ago. It is therefore possible that during the period studied, developing countries and regions will have experienced divergence, followed by convergence. If this is the case, convergence may be masked when using 'blunt' methods such as growth-initial regressions, which implicitly assume convergence to be a monotonic, fundamentally linear process.[67] I therefore include a third function, a quadratic exponential function, which allows for a non-monotonic relationship. This is not the only possible function, but this form is strongly suggested by economic theory and history, and has the virtue of being simple. It includes a period of exponential divergence from zero, such as would be seen if vanguard economies 'took off' exponentially during the industrial revolution (see Maddison 2006a,b). This is followed by a period in which divergence slows, as the developing economy begins to fulfil the conditions necessary for convergence. During this period, two opposing forces are acting upon the developing economy. On the one hand, and to the extent it has fulfilled certain conditions, it provides higher returns to capital (due to a lower capital to labour ratio than vanguard economies) meaning that it will tend to attract capital, increasing labour productivity and thus income (as per Solow 1956). On the other hand, unfulfilled conditions, such as poor institutions or low human capital prevent the developing

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[66] In fact, as Friedman (1992) and Quah (1993) have pointed out, evidence of beta-convergence, as this is known, does not provide conclusive evidence for convergence but may simply represent regression to the mean. Beta-convergence is a necessary but not sufficient condition for convergence, since random perturbations may counter the general tendency to convergence. For this reason Quah (1993) recommends analyses of income dispersion through time. Such studies look for 'sigma-convergence' or a reduction in the dispersion of income levels over time. However, in the context of this analysis, involving regionally aggregated data, stochasticity becomes less important, and since comparisons are made between just two entities (the developing region and the OECD), "beta convergence" would imply sigma convergence.

[67] For example, the study quoted in the SRES and reproduced in Figure 3.10 of Nakicenovic & Swart 2000), is Barro (1997), which uses the annualised percentage growth rate over 20 years (1965-1985) as the dependent variable. This study does find a negative correlation between growth rates and initial income, but only after other factors have been taken into account, which can be interpreted as conditions, and perhaps as proxies for the country's "stage of development".

economy from fully capitalising on its situation, and reduce the ability for it to benefit from any positive externalities from vanguard economies' innovation. As more and more conditions are met, a turning point is reached, and convergence begins, albeit slowly. Once all the conditions have been met, the only force acting on the economy is advantageous – that of higher returns to capital - and it narrows the gap exponentially. Convergence in this final phase proceeds as with the exponential decay function above, with developing country growth rates converging asymptotically on those of the vanguard as income levels converge.

Quadratic Exponential:

Equation 3.6 $$G = e(a + b \cdot t - c \cdot t^2)$$

where $c > 0$.

IV. Data and methods

The SRES projections show small differences in values for the initial year, 1990.[68] I therefore divided through by a common factor all of the values in each projection, in order to equalise the initial values with that of the average for all scenarios. This leaves rates of change and the shape of trends unaltered.

I aggregated the Penn World Table income series to the level of the regions used in the SRES by calculating a weighted average of per capita income over all countries in the region that are included in the Penn table.[69] The Penn World Table runs from 1950 to 2004, but coverage (of variables other than population[70]) is incomplete for many countries. In order to ensure that trends are genuine and not the effect of changing the countries included in the analysis, I used a constant sample of countries starting in 1952. This sample provided the best compromise

[68] Since this year was not forecast by the scenarios, and was already ten years past by the time the SRES was published, the reasons for these differences are not clear.

[69] Data for Bhutan and Cambodia were excluded because the population data, and hence the GDP per capita data, appeared to be seriously anomalous, and this was confirmed by the Centre for International Comparisons (Ye Wang, pers. com. 31st July 2007). With the exception of Bhutan and Cambodia all other countries listed by the SRES and not included in the Penn tables are microstates, dependencies and overseas territories.

[70] The population estimates come from the US Census Bureau. Because these represent the best guess of the true population figure, they may differ from official population statistics published by country governments and collated by the UN.

between longitudinal and cross-sectional coverage (see Appendix C for more details). The objective is to determine the historical plausibility of the SRES projections, given the information available at the time they were developed, so the sample ends in 1990. The observed data from 1991-2003 is also presented. Once again, all points in each time series were divided through by a common factor to equalise 1990 levels with the SRES projections.

All analyses were carried out using the nls function in R (R Core Development Team 2007). Probability distributions for each extrapolated function were estimated by fitting the function to 10,000 bootstrapped samples; and for the model-averaged projections by taking w_i 10,000 samples without replacement from each function's set of bootstrapped samples (where w_i is the Akaike weight of function i in the set). 95% confidence intervals for each function or model-averaged projection were estimated by selecting the 250th and 9750th highest prediction for each year from the probability distribution.

IV. Vanguard economy growth

Model fitting and selection

Comparing AIC scores (Table 3.2), the power function receives most support from the data, the linear function considerably less, and the exponential function essentially none (ΔAIC>10, Burnham & Anderson 2002).

Table 3.2. AIC scores and weights for models of vanguard economic growth.

	Parameters	**AIC Score**	**Delta AIC**	**Akaike Weight**
Power	3	565.59	0.00	0.87
Linear	2	569.35	3.75	0.13
Exponential	2	597.48	31.89	1.03x10-07

Figure 3.1 shows the functions fitted to the 1952-1990 data, and extrapolated to 2003, as well as the observed data from 1991-2003. The best performing function as fitted to the data 1952-1990 (power), also best predicts income growth from 1990-2003. Thus, there is no evidence that economic growth in the vanguard region has been exponential over the only period for which national accounts have been published. The relatively strong performance of the linear model, and the fact that the exponent c of the power function is only slightly above one (Table

3.3), shows that growth has been much closer to linear (i.e. constant absolute annual increments) than exponential (constant percentage increments).

Table 3.3. Model parameters for OECD growth.

Model	Parameter	Estimate	Std. Error
Exponential	a	8.663	0.020
	b	0.027	0.001
Linear	a	4672	111
	b	289	4.841
Power	a	5096	198
	b	183	37.2
	c	1.119	0.053

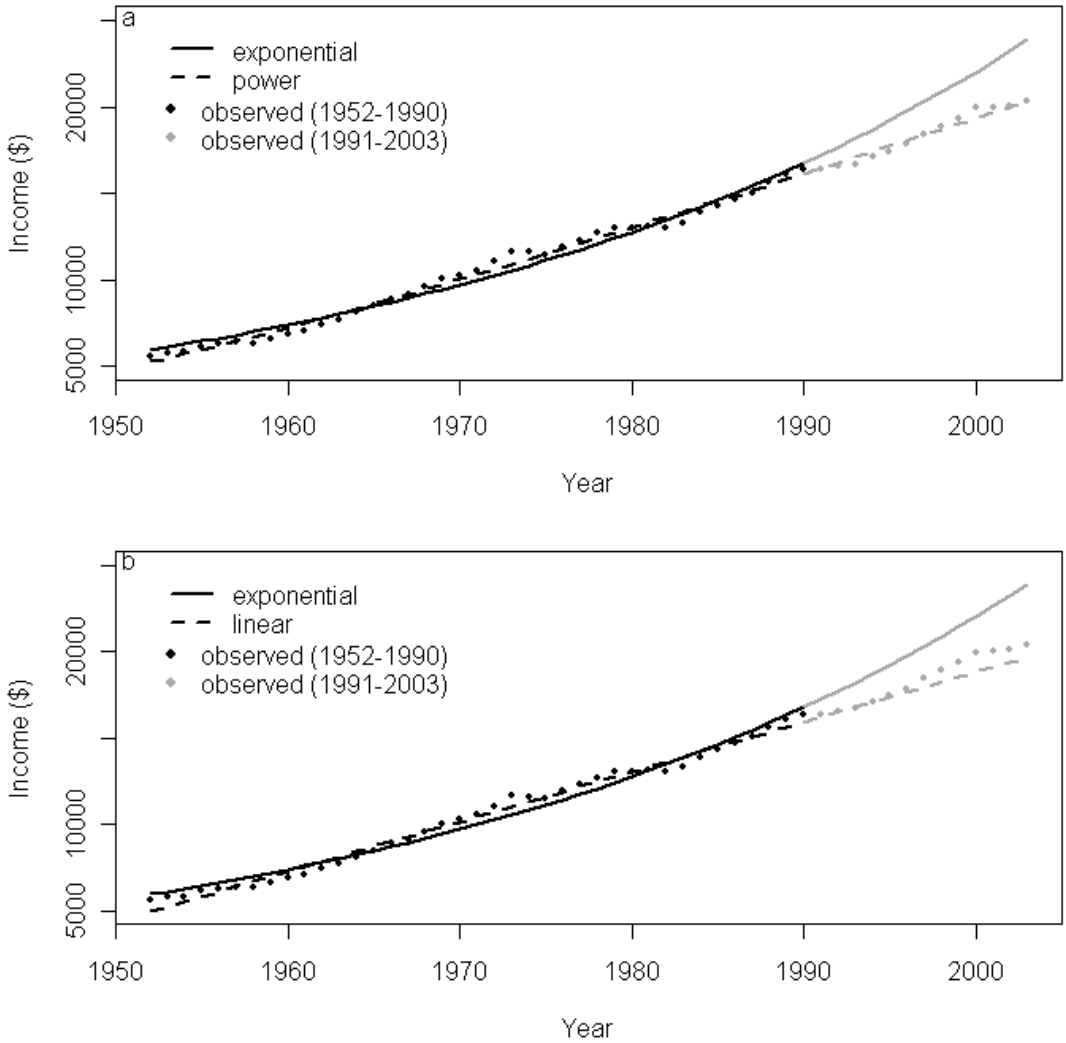

Figure 3.1. Near term income projections using models fitted to 1952-1990, a) exponential and power, b) exponential and linear.

Comparison of the SRES with the recent historical data

The SRES scenarios completely encompass the model-averaged projection, as well as its 95% confidence intervals (Figure 3.2). The exponential function, on the other hand, lies well outside the SRES scenario range.

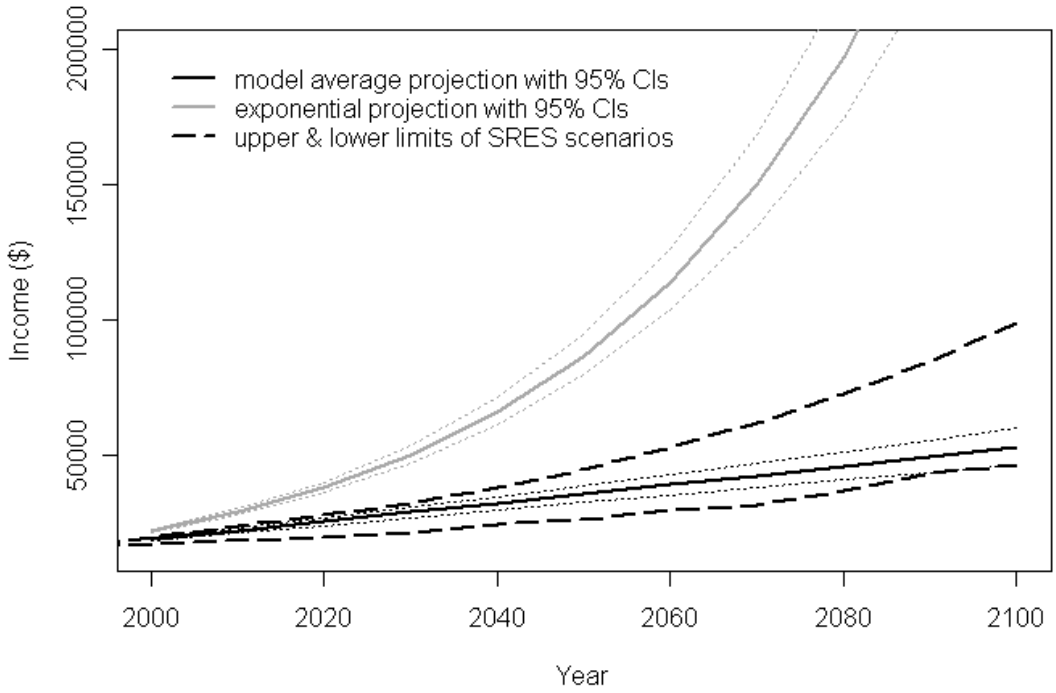

Figure 3.2. The upper and lower limits of the SRES income projections, together with the model-averaged extrapolation of the past, and of the exponential function alone (each with 95% confidence intervals).

Compared to the model averaged function, the SRES scenarios are on the low side early on, and on the high side later, but the proportion of the probability density of the extrapolation falling within the scenario range never falls below 98 per cent. The model-averaged projection from this candidate set therefore leads to the conclusion that the SRES is comparable with the past (though tending to the high side). Using the conventional model assumed by both sides of the debate – the poorly performing exponential model - would lead to the opposite conclusion. Annualised percentage growth rates in the SRES are well below those that would be calculated if an exponential function were assumed for the historical data.[71] This illustrates an important message of this chapter – whenever projections are

[71] This makes their use of annualised percentage growth rates to compare the SRES scenarios with historical data (Nakicenovic et al. 2003, Grübler et al. 2004) puzzling. As I explain further in the discussion, unless one assumes an exponential function, such a comparison is meaningless.

compared with the past, the models used to compare them must be explicitly defined, justified by theory and then tested empirically on the past data. Blind use of the exponential function is not an appropriate way to compare growth between different periods, or to extrapolate the past.

Implications for conceptualising growth

The superior performance of the power function compared to the exponential function in predicting growth 1991-2003 demonstrates the importance of careful model definition, justification and selection, rather than simply relying on conventional wisdom and convenience. When comparing income projections with historical data, using annualised percentage growth rates is convenient, but meaningless, if growth is not exponential. For example, Beckerman (2003) writes:

> "…. this growth of output per head has been 2.1 percent [per annum]….. The power of compound interest being what it is, world average real incomes per head in the year 2100 should be 4.43 times as high as they are now!"

'Compounding' is indeed powerful, but Beckerman provides no evidence that past growth has been compounded. When Beckerman says growth has been 2.1% per annum, he does not mean that he has fitted an exponential function to the data, and found it to have an good fit, better than any other function. Instead he has assumed the exponential nature of past growth, calculated an annualised percentage growth rate from the initial and final values[72] and then extrapolated his untested function into the future.[73]

The only tenable position is to assume that no single, simple, function can be expected to accurately represent growth in all phases, since even the simplest growth model combines several different factors (capital accumulation, human capital, technology) all of which may increase in different, perhaps non-linear ways. Any growth function can only approximate such a process, and will likely only be valid for a limited period. Another important implication is that if growth

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[72] The annualised interest or growth rate is found by dividing the final value by the initial value, and then exponentiating the result by the reciprocal of the number of years.

[73] I acknowledge that Beckerman is referring to global output here. However global output is not exponential either (see Chapter 6).

is not exponential, there is no rationale for using an exponential function when discounting future costs and benefits.

Long-run mega-trends and phase shifts

Figure 3.3 shows three important mega-trends in economic growth, for Western Europe.[74] The aim is to examine the claim made by some (e.g. Nordhaus 1994) that annualised percentage growth rates tend to increase over the long-run, and *therefore* that ever faster exponential growth is likely. The figure plots three variables, each calculated for successive time periods identified by Maddison (2006a,b) as being distinct phases. First, the annualised percentage growth (solid line), estimated by fitting an exponential function. Second, the ΔAIC score of the exponential function compared to a power function (dashed line), which when negative indicates that the exponential function receives greatest support from the data, and when positive the reverse. ΔAIC of less than two indicate that both functions receive considerable support. Finally, the dotted line shows the exponent c of the power function, where c=1 indicates linearity.

First, it does appear that annualised % growth rates have been increasing, albeit rather erratically. Interestingly, if the increase in growth rates is viewed as linear, it is the high growth rates of the early post-war period that appear anomalously high, rather than the more recent lower growth rates appearing to be low, which calls into question the concept of a productivity "slow down" in recent years (see e.g. Nordhaus 2002).[75] Meanwhile, the dashed line shows another important trend, the ΔAIC score of the exponential function. Never more negative than minus one, it turns strongly positive as soon as we enter the modern era. This suggests one of two possibilities. Either growth has become less exponential in the modern era, or the exponential nature of earlier periods is an artefact of Maddison (2006a,b) assuming an exponential function when reconstructing past income estimates. Either way, it undermines any interpretation of historical growth rates that relies on assuming an exponential function, and comparing annualised percentage

[74] The trends for Japan, and the 'western offshoots' USA, Canada, Australia and New Zealand are qualitatively similar. However, Western Europe is shown here as it has been at or near the productivity frontier for longest.

[75] These very high growth rates could be attributed to a Solowian bounce back to each economy's steady state after the second world war.

growth rates. Finally, the dotted line shows the exponent of the power function. If the long run trend appears to be for increasing percentage growth rates, it also appears to be for more linear growth, which presents a contradiction: the exponent falls from 1.6 before the industrial revolution to close to unity in the present day. The contradiction is resolved by recognising that while the gradient of the income time series is increasing (whether measured linearly or using an exponential function) the form of growth is not inherently exponential.

There are many different ways such mega-trends could be extrapolated, depending on the assumptions one makes about the data and the underlying economic processes, and further analysis is warranted. However, the message of the paper is that it is unnecessarily selective to naively extrapolate increasing annualised percentage growth rates, without also considering the (equally robust) trend for growth to become more linear. Exponential growth at ever-higher rates can never be ruled out for the future, but it is not necessarily implied by the long-run data, as some authors have stated (e.g. Nordhaus 1994, Hanson 2000). The upper limit of the SRES scenarios encompasses an exponential growth rate of around 1.7% per annum, over the next 110 years. This is considerably lower than that which would be obtained by linearly extrapolating the trend in increasing annualised percentage growth rates (around 3.75%) and assuming a phase shift back to exponential growth (see Figure 3.4). Estimating the probability of such a shift probably requires subjective judgement and certainly lies outside the scope of this study. Given that phase shift uncertainty is, by definition, infinite, it is a point for debate as to whether the SRES should have included this possibility within the scenario range or not.[76]

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[76] One interesting line of argument in favour of much faster growth in the 21st century, is based on increases in computing power. I noted above that if computers are viewed as tools that increase the productivity of people, one might expect diminishing marginal returns to increased processing power. However, if one believes the predictions of artificial intelligence advocates (e.g. Kurzweil 2001) one can view computers as economically productive agents in their own right. In this case, substantial increases in processing power would increase not only the productivity of workers, but the number of workers per (human) capita. Heroic extrapolation of current apparently exponential increases in processing power would predict one million human brain equivalents in 2017, and one billion in 2027 (M. Bahner pers. com.) If we assume that humans could appropriate all of that productive potential, this would have the effect of increasing the number of productive workers by roughly 1/8th of the human population. This is of course highly speculative, but interesting nonetheless.

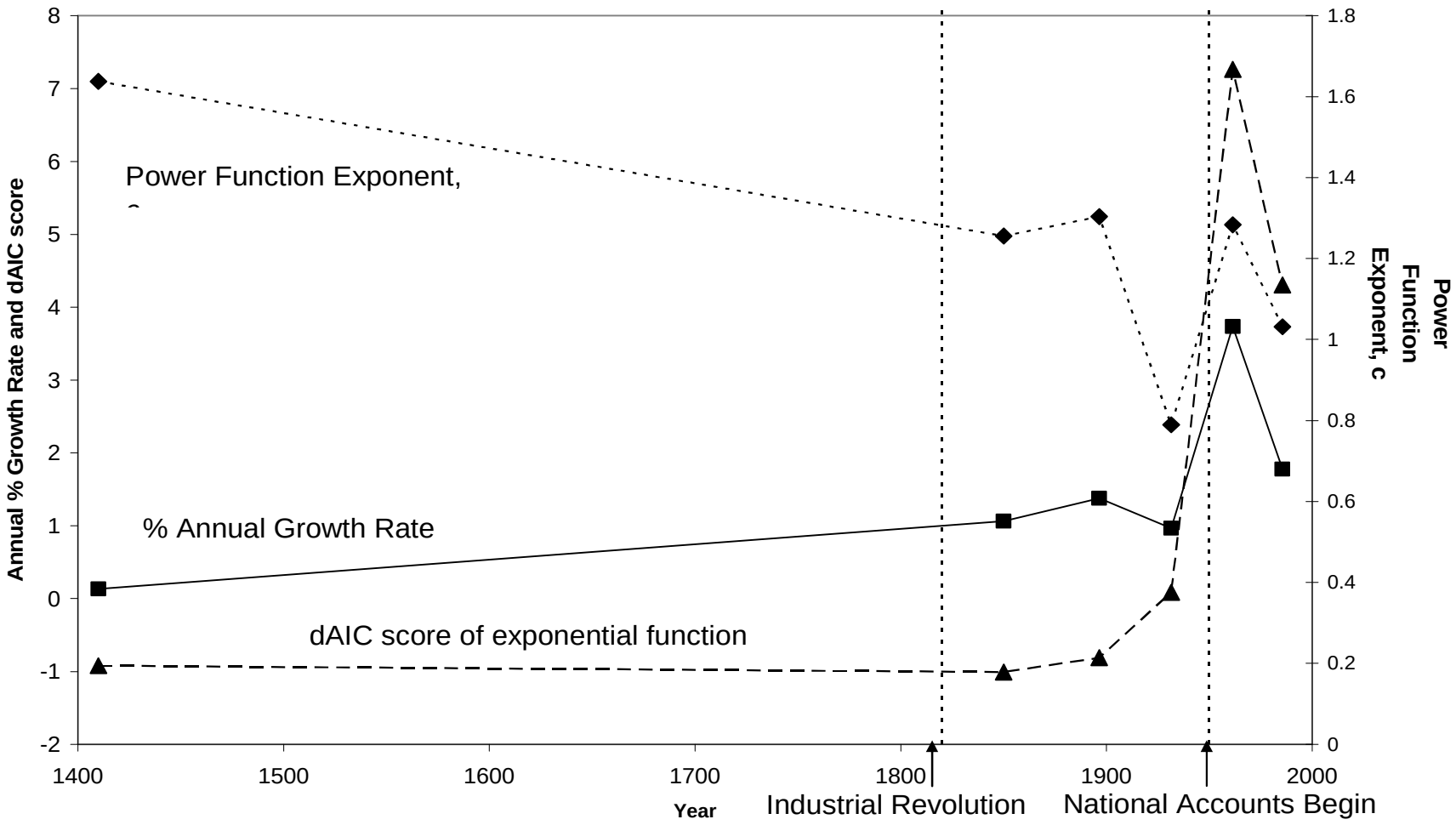

Figure 3.3. Mega-trends in economic growth in Western Europe based on 12 countries, using data from Maddison 2006b. Statistics are calculated over the following periods, as per the phases identified by Maddison (2006a,b): 1000-1820, 1820-1880, 1990-1913, 1913-1950, 1950-1973, 1973-1998. Data points are plotted at the mid year of the phase.

Economic model specification uncertainty

This study has used univariate regression to extrapolate historical trends in income growth. This approach was chosen because it was in these terms that the original debate over the SRES has been conducted. There are of course other ways to extrapolate trends in income, involving other predictor variables, such as the savings rate. However, when making long-run projections, one also needs to project these predictor variables (leading to a 'chicken-and-egg' situation), unless they enter the regression lagged by many decades. For most variables of economic interest, this is implausible.

One class of variables which do show promise in this regard are demographic indices, like the age structure of a population (see Ahlburg & Lindh 2007, and other papers in the same issue). Given the relatively long life span of humans in comparison with the time horizon of the projections (c70 years compared to 110 years), many demographic variables can be predicted with some certainty over at least a portion of the projection period. There are many interesting theories that link population and economic growth, some of which may result in predictions of exponential growth. For example, Beckerman (2003) argues that it is the number of people worldwide who posses high levels of human capital that drives growth. Under this view, growth may accelerate as more countries converge on western income levels. Such theories deserve further attention, but lie beyond the scope of the original debate and therefore this study: it is sufficient to note that the results of this study in terms of the plausibility of the SRES projections, are conditional on the approach taken.

Another way in which the results presented here are conditional on the economic model assumed and thus the analytical approach taken, relate to the use of aggregate level data. I used this for simplicity's sake and to directly mirror the approach taken in the SRES projections. In respect of the OECD region, the implication of this is that I have assumed that the OECD shows relatively homogenous growth rates over the long term. If this assumption is relaxed, and a new period of divergence and takeoff assumed, one can develop very different extrapolations of the past. For example, we might define the US, rather than the OECD, as representing the productivity frontier. Although the US has not grown exponentially in the modern era, its growth has been more exponential (ΔAIC

score of the exponential function compared to the power function is 1.98).[77] A model averaged function can then be extrapolated to predict US income in 2100. However, to predict OECD income requires an assumption about what will happen to the rest of the OECD. If we assume that this new US 'take-off' (sensu Lucas 2000) would be similar to that of Great Britain at the start of the industrial revolution, we would expect the rest of the OECD, and possibly some developing countries to quickly catch up, as they caught up with Great Britain two centuries ago (Maddison 2006a,b). Assuming a convergence rate of 2% from 1990 onwards for illustrative purposes,[78] would give an OECD per capita income in 2100 of around 140,000, well above the upper limit of the SRES scenario range. Using country-level data may therefore allow us to pick up new trends before they appear in the aggregate data, but requires us to assume other parameters, like convergence rates. On the other hand, we run a greater risk of confounding mere stochastic variation with durable trends.

Figure 3.4 shows the implications of assuming this model, compared to the SRES scenario range and the previous model-averaged prediction shown above. For illustrative purposes, I also show the effects of including a logistic model in the a priori candidate set,[79] as well as the phase shift to ever increasing exponential growth described in the previous section. The main message of the figure is that the uncertainty surrounding plausible phase shifts and alternative model specifications greatly exceeds that associated with model selection uncertainty and prediction error, and also greatly exceeds the range covered by the SRES scenarios. These uncertainties are difficult to assess probabilistically, which demonstrates the need for scenario exercises that are not confined to an "inappropriate historical determinism". It is a matter for debate as to how far the IPCC should go to encompass plausible phase shifts and alternative models, one

[77] See Temple (2002) for a review of US performance in the 1990s.

[78] This appears to be empirically plausible based on convergence studies of OECD countries (see Islam 2003).

[79] A logistic function could represent limits to growth, whether environmental or social. Taking the narrow definition of GDP currently used, GDP per capita might be expected to flatten out as people increasingly trade in marketed production (included in GDP calculations) for increased leisure time (not included in GDP). This raises the question of the durability of the GDP concept, discussed in Section VII below.

that will be hard to resolve unless an effort is made to put subjective probabilities on them, for example by polling expert opinion, a point I discuss further below.

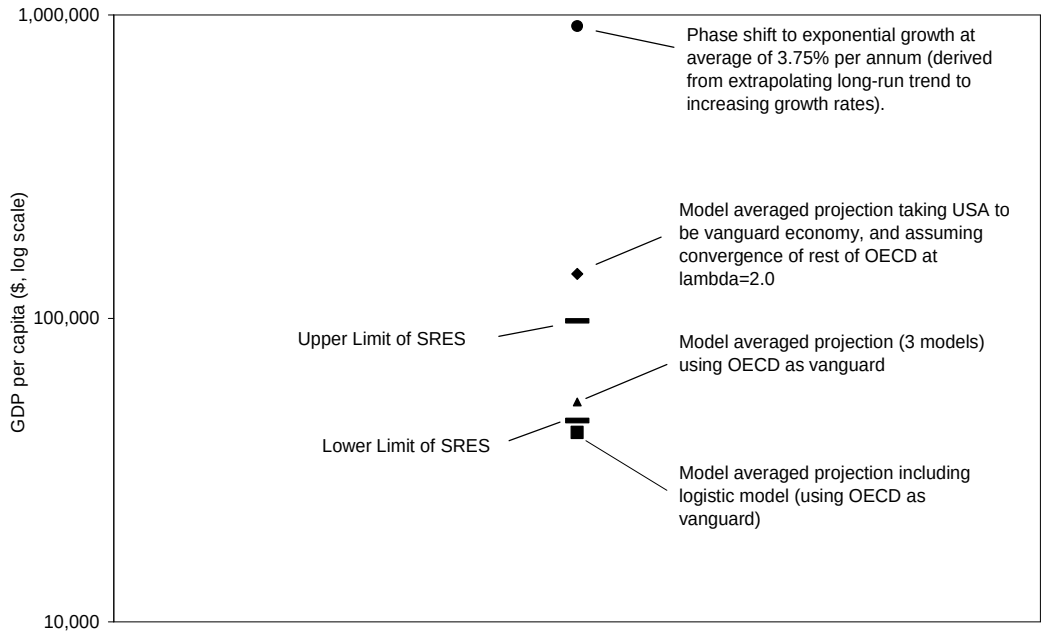

Figure 3.4. Alternative predictions of OECD income in 2100, compared to the SRES range (log scale).

V. Developing region convergence

Model fitting and selection

ASIA

Table 3.4 shows that the estimate of parameter b from the asymptotic exponential function is negative, when it must be positive for the function to have the property of asymptotic increase and this function is therefore discarded[80] - there is no evidence of monotonic divergence in the data, given this model. Table 3.4 shows that the quadratic exponential function receives vastly more support from the data than the exponential decay function. Figure 3.5 shows the fit of the quadratic exponential and exponential decay functions to the historical data. Both divergence and convergence are clearly present in the period analysed, and the quadratic exponential function has a good fit to the data. The exponential decay

[80] The function as estimated is no longer an asymptotic function, but is decreasing in y.

function, by contrast, has a very poor fit, and gives a very low estimate of convergence speed, equivalent to that which would be estimated using a growth initial regression over the period 1952-1990.

This figure also shows near term extrapolations of the two functions, together with data observed 1991-2003. This more recent data confirms a more rapid convergence trend than that which would be estimated from an analysis assuming a monotonic function, though it suggests an even steeper rate of convergence than that predicted by the quadratic exponential model. This is in part because the quadratic exponential function is constrained to be symmetrical. Although this helps to simplify the function, there is no economic reason why it should be so – very different forces are acting during the divergence and convergence phases.[81] It would be interesting to experiment with similar, but asymmetrical functions in order to predict future convergence, and they would likely predict a faster rate of convergence in the near term.

Table 3.4. Parameter estimates for the ASIA region.

Model	Parameter	Estimate	Std._Error
Exponential Decay	a	-8.90×10^{-02}	3.67×10^{-03}
	b	2.84×10^{-04}	1.61×10^{-04}
Asymptotic Exponential	a	8.47×10^{-02}	3.23×10^{-03}
	b	-3.18×10^{-03}	1.61×10^{-03}
Quadratic Exponential	a	-1.13×10^{-01}	2.33×10^{-03}
	b	3.29×10^{-03}	2.68×10^{-04}
	c	8.94×10^{-05}	6.51×10^{-06}

Table 3.5. Akaike Information Criterion scores and weights for the ASIA region.

Model	Parameters	AIC_Score	Delta_AIC	Akaike Weight
Quadratic exponential	3	-312.03	0	≈1
Exponential decay	2	-242.56	69.48	8.19×10^{-16}

[81] The speed of divergence is initially driven by the growth rate of the vanguard economy once it has 'taken off'. The convergence rate is a product of the way in which capital and technology flow between regions. Any tendency for the vanguard economy to slow as it 'matures' (which may be evident in the OECD data) may speed convergence if this maturing phase in the vanguard economy happened to coincide with the convergent phase in the developing economy. This will depend on the extent to which the developing economies are reliant on present-day, or past technological progress in the vanguard economies.

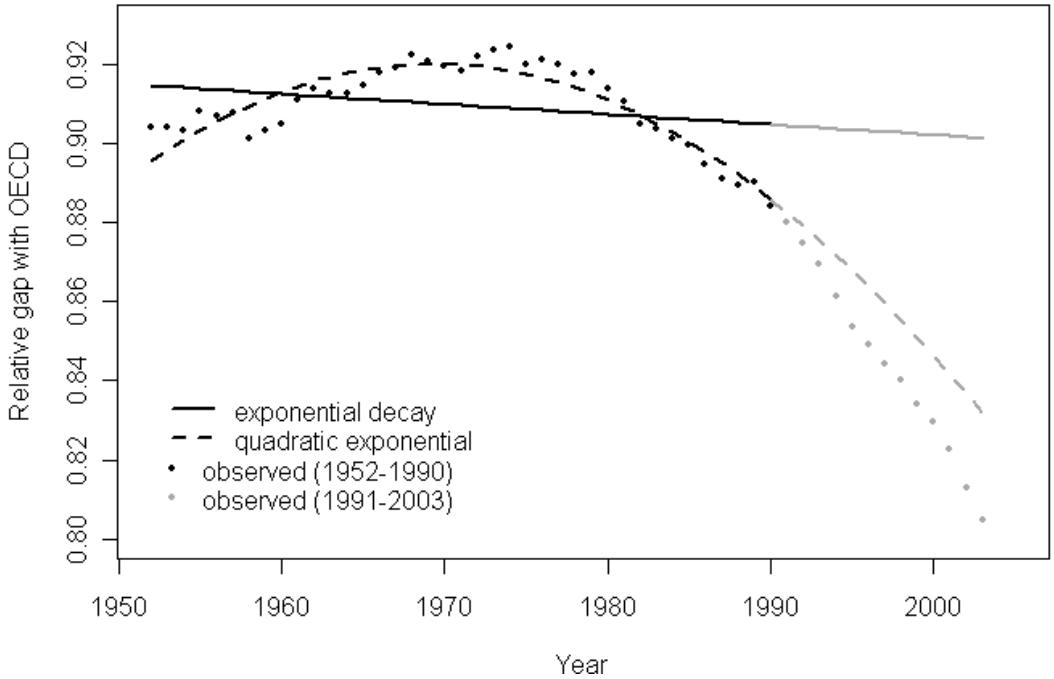

Figure 3.5. The fit of the quadratic exponential and exponential decay functions to historical data on the relative gap between OECD GDP per capita and ASIA GDP per capita.

ALM

Parameter b of the exponential decay function and c of the quadratic exponential function are negative (Table 3.6). Thus, there is no evidence for any decreasing trend in the data, given these models, and no evidence for a turning point. The region shows only divergence from the OECD, over the period analysed. Both of these convergence functions are therefore discarded, leaving only the asymptotic exponential function, representing continued divergence, approaching the mathematical maximum at unity. No AIC scores are presented for the ALM region, since only one function could be estimated from the data.

Figure 3.6 shows the asymptotic exponential function fitted to the ALM data 1952-1990, and extrapolated to 2003. There is considerable variation around the trend that appears to be somewhat cyclical. However the post 1990 data shows no sign of returning to the trend, suggesting that the trend may have been shocked to a new level in the late eighties. Therefore, I also show the same function recalibrated to pass through the 1990 data (by adjusting the value of the intercept parameter a), and this appears to fit the post 1990 data well.

Table 3.6. Parameter estimates for the ALM region.

Model	Parameter	Estimate	Std._Error
Exponential Decay	a	-3.41×10^{-1}	4.70×10^{-3}
	b	-2.52×10^{-3}	2.00×10^{-4}
Asymptotic Exponential	a	2.91×10^{-1}	3.81×10^{-3}
	b	7.39×10^{-3}	6.15×10^{-4}
Quadratic Exponential	a	-3.32×10^{-1}	7.13×10^{-3}
	b	1.19×10^{-3}	8.08×10^{-4}
	c	-3.31×10^{-5}	1.94×10^{-5}

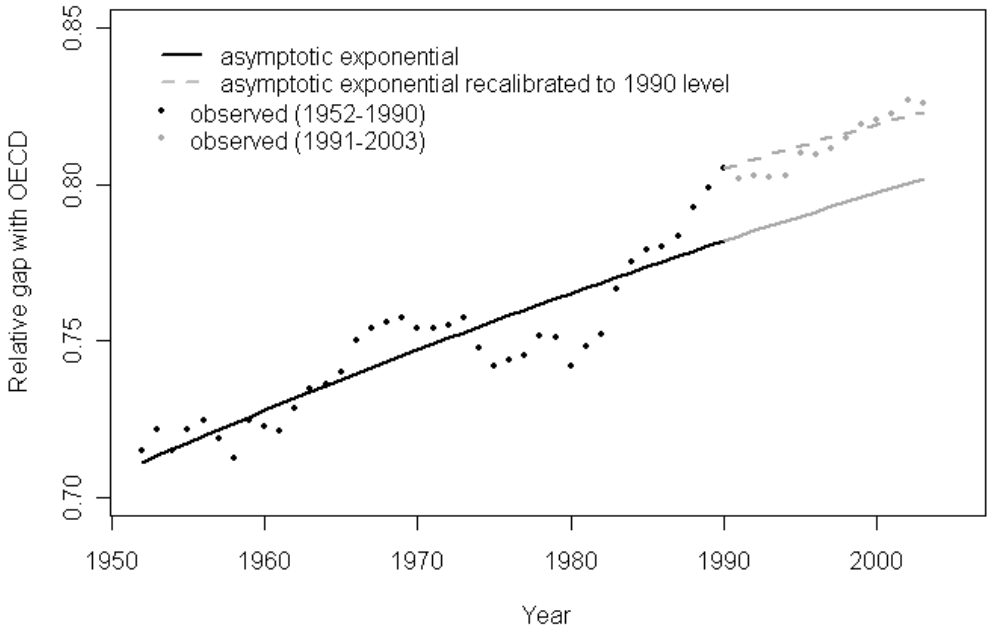

Figure 3.6 The asymptotic exponential function fitted to the ALM data 1952-1990, and extrapolated to 2003 (and recalibrated to pass through the 1990 data).

Comparing the SRES scenarios with the recent past

Figure 3.7 and Figure 3.8 show the upper and lower limits of the SRES scenarios, together with an extrapolation of the model-averaged function (with 95% CIs) for ASIA and ALM respectively. It shows that, contrary to Castles and Henderson's claims, the SRES appears to have been pessimistic with regard to Asian convergence[82] and fails to encompass more than a few percent of the probability density of the extrapolation. This conclusion should be treated with caution, since only one non-monotonic function was considered, although the data since 1991

[82] This may, in part, be due to their having been somewhat optimistic on OECD growth. On balance then, they may have got Asian growth roughly right. If anything has contributed to excessive ASIA growth projections relative to the historical data, it is over-optimism on OECD growth, not ASIA convergence

appear to confirm it. Note that here again, the comparison of the SRES with the past depends very heavily on the models included in the a priori defined candidate set. If only monotonic functions like the exponential decay function had been considered (as implied by many convergence analyses) this would have led to the conclusion that the SRES had been hopelessly overoptimistic in comparison with the past. Including a non-monotonic function in the candidate set leads to the opposite conclusion.

There appears to be no evidence of convergence for the ALM region, from the aggregated historical data. Figure 3.8 compares the re-calibrated projection with the SRES scenarios. Although the scenarios have taken a less optimistic position on ALM convergence compared with ASIA, they nevertheless assume that a turning point is reached at or before 2010. The SRES does not include a scenario compatible with continued divergence past 2010, and the scenario range does not encompass any of the probability density of the projection from 2040 onwards.[83]

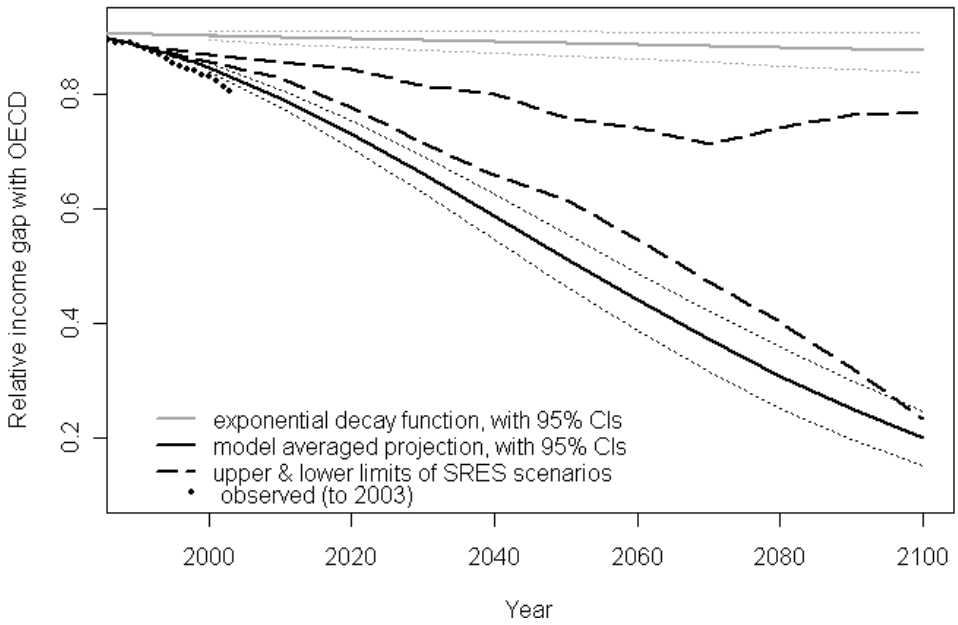

Figure 3.7. The upper and lower limits of the SRES scenarios, together with an extrapolation of the model-averaged function (with 95% CIs) for ASIA.

[83] It should be remembered that the variable on the *y*-axis is income gap *relative* to the OECD. Thus, near the maxima on this axis, small changes in *y* represent huge changes in ALM income level. Holding OECD income constant, a reduction in the relative gap from 90% to 80% means that ALM income level has doubled. The difference between the extrapolation line and the upper limit of the SRES scenarios is therefore very large in terms of the income level of the developing region.

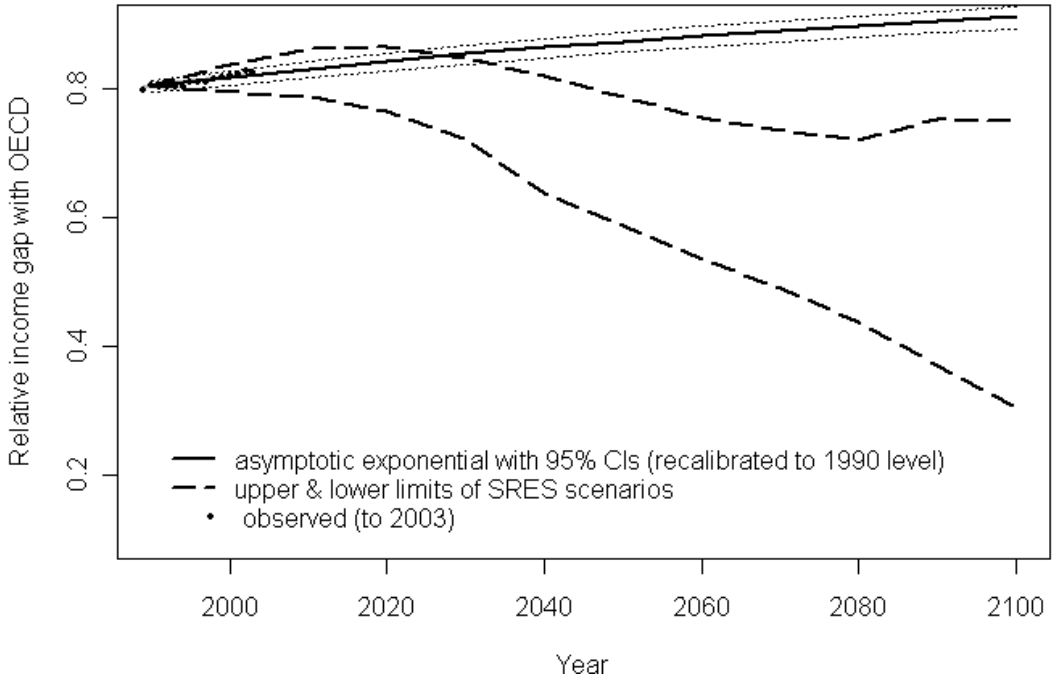

Figure 3.8. The upper and lower limits of the SRES scenarios, together with an extrapolation of the model-averaged function (with 95% CIs) for ALM.

Implications for Conceptualising Developing Country Growth

The quadratic exponential function is only one of many possible functions, but it serves to illustrate an important point. That the evolution of relative incomes of countries behind the productivity frontier is non-linear, and despite their convenience, convergence rates estimated from growth initial regressions cannot be meaningfully extrapolated. The results show that the relative performance of developing countries must be seen as a non-monotonic function of at least three processes: take-off in other regions (at the productivity frontier), the gradual attainment of the conditions leading to convergence, then convergence due to diminishing marginal returns to capital in vanguard countries.

The analysis of aggregated data here presents a simple picture, of one region having turned the corner and now converging, another still diverging. In reality, the situation will be more complex. Country level data would show that this aggregation contains individual nations at various stages of the cycle. In particular, the long historical perspective, as well as evidence from ALM shows that this process is best understood as cyclical or iterative (see below). Convergence and divergence may follow each other, as major or minor take-offs

occur in the vanguard, or when new conditions, not yet satisfied, suddenly become more important.[84]

D. Long-run mega-trends and phase shifts

History shows that several successive periods of divergence and convergence can follow one another. Figure 3.9 shows data for the areas currently known as Italy and the Netherlands, from AD1 until 1900 (from Maddison 2006a,b). It illustrates two points. First, divergence-convergence is not necessarily a one-off process. Second, convergence may not necessarily be completed before another phase of divergence sets in (1820), and third, what was previously the vanguard economy, may become the laggard (1600). Viewed in this context, the SRES scenarios have not explored historically plausible phase shifts for the ASIA region, such as the reversal of convergence (as the OECD pulls away once more). The simplest way to conceptualise this for ASIA would be as a sudden shift from a quadratic exponential function to the asymptotic exponential function, the shift having a certain (subjective) probability in any given year. For ALM, the reverse is the case, and the SRES appears to have assumed a 100% probability of such a phase shift occurring before 2020, which seems unreasonable. Explicitly stating these subjective phase shift probabilities would greatly increase the transparency of the scenarios and aid constructive debate.

[84] An example of the changing importance of conditions might include the IT literacy of a developing country's population, which may be more different from that of vanguard economies than literacy in general. If this suddenly replaced general literacy as an important condition for capital inflows, convergence might slow or cease. Thus, convergence can be seen as requiring the developing country to fulfil an ever-changing set of conditions, in order to stand on the escalator of capital inflows.

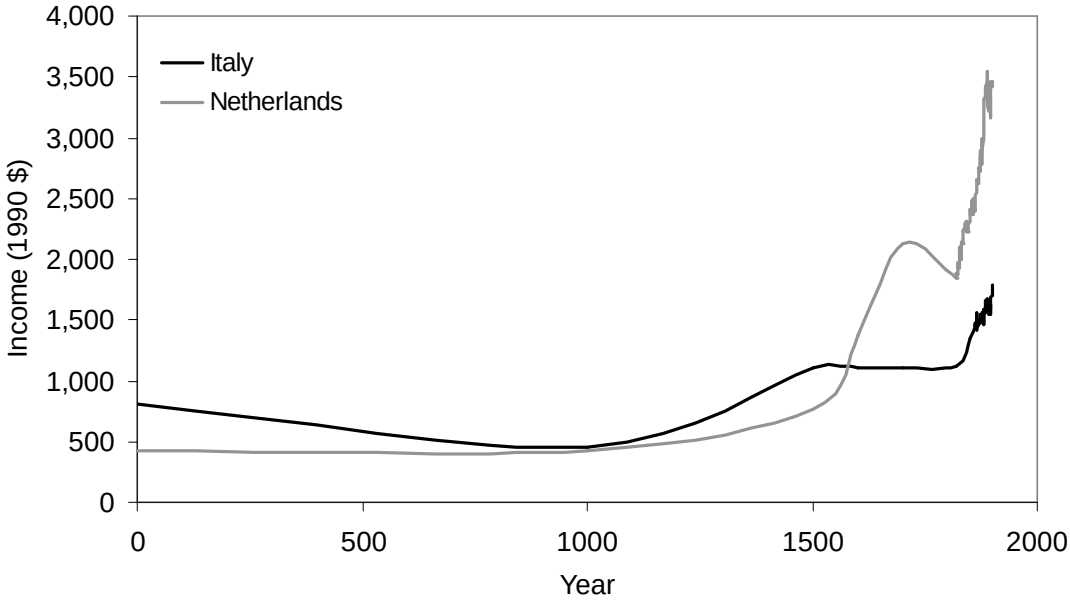

Figure 3.9. Estimated income for the areas currently known as Italy and the Netherlands, from AD1 until 1900 (from Maddison 2006b).

Economic Model Specification Uncertainty

The conclusion that there is no information in the data about an ALM turning point is tentative. It could be overturned by a country level application of a model such as the quadratic exponential model, possibly modelling turning points by a hazard function in a similar way to Lucas (2000). However, there is no evidence of any slowdown in ALM divergence, which would be expected if some countries had begun to converge.

VI. Discussion, Conclusions and Recommendations

The durability of GDP

In Section V I called into question the durability of the GDP concept using the example of leisure time. It seems possible that GDP as currently measured, will not continue to be a useful statistic in a century's time, especially if the more optimistic growth scenarios come to pass. GDP may become obsolete in two conflicting ways, depending on the purpose of the income projections. If they are to be used as a proxy for welfare then leisure time, social cohesion, environmental quality and the diminishing marginal utility of income, all of which are ignored at present in GDP calculations are likely to become increasingly important

determinants of welfare in the future, leading to an ever-increasing divergence between GDP and welfare.

If, on the other hand, the purpose of the projections is to predict emissions, GDP may become obsolete for almost the opposite reason. The fact that GDP treats a dollar of production equally, whether it stems from heavy industry or services, means that correlations between GDP and emissions established over the recent past may cease to be valid in a future economy, requiring subjective adjustments to the observed relationship between GDP and emissions.[85] If a radically different version of GDP will be required in projections, this version will of course have to be constructed for the past, in order to be able to parameterise projections using past data. This would require an effort on a similar scale to that of Maddison's (2006a,b) work to provide modern GDP estimates for the two-thousand years prior to 1950.

Conclusions

Critics have separately alleged that in comparison with the past, the SRES income projections are unfeasibly high (in developing regions), and unfeasibly low (for the world as a whole). However, these comparisons, and those provided by the SRES team themselves, have been selective and statistically unsound. This study provides the first rigorous comparison of the SRES projections with the historical data, explicitly defining, justifying and empirically testing the models used. These models were chosen to represent the terms on which the debate has taken place to date: that is to say univariate comparisons of the SRES projections with the historical data, using annualised percentage rates of change.[86] Given these models, I conclude that the SRES projections of OECD growth fully encompass recent trends, and that there is not unambiguous evidence for predicting exponential growth much above that encompassed by the SRES. For the ASIA region, I find, contrary to the critics' claims, that the SRES projections of relative income appear

[85] Although the trend towards decreasing carbon intensity of economic activity with economic growth has been observed and incorporated into the SRES scenarios (Nakicenovic & Swart 2000), this relationship, estimated over a comparatively narrow range of GDP, may not hold if GDP increases substantially in the future.

[86] Although neither critics nor developers actually quoted convergence rates, they implicitly did so in referring to the findings of the convergence literature, which had themselves been largely based on linear analyses producing annualised percentage rates of change.

to be somewhat pessimistic, though qualitatively reasonable, when compared to the historical evidence. Only for the ALM region do I find that the critics' concerns are justified. Here, the SRES appears to have made a strong assumption about when this region will start to converge on the OECD, which is not justified by the data observed before or since the SRES were developed.

That said, I have also explored the implications of phase shift uncertainty and economic model specification uncertainty. I have demonstrated that the uncertainty associated with historically plausible phase shifts, and economically reasonable alternative model specifications is large relative to the SRES scenarios. Thus, the SRES has not performed well in encompassing these two types of uncertainty. This is ironic, given the SRES team's insistence that they should not be bound to "simple extrapolations of the past" (Nakicenovic et al. 2003). However, in their defence, the difficulty of encompassing such uncertainty, given the SRES's terms of reference, was anticipated by the team (Nakicenovic & Swart 2000).

The principal message of this paper is that convenient metrics based on annualised percentage rates of change, whether growth rates or convergence rates, are not useful for the development, parameterisation or evaluation of income projections, and that the business of comparing projections with the past is considerably more complex than has hitherto been acknowledged by either side in the debate. I detail my recommendations for the development and criticism of future economic projections below.

Recommendations

The primary objective of the chapter has been to demonstrate the importance of explicitly justified models and rigorous statistical analysis, when comparing projections of income with historical data. I recommend that in future, scenario authors, and their critics apply the following guidelines when developing income projections or when challenging those developed by others – the two processes are fundamentally the same, after all, since one cannot challenge a projection without proposing an alternative.

First, the analytical approach and models used must be explicitly set out and justified. Although the SRES discussed relevant economic theories and empirical

evidence, the link between this discussion and the income projections themselves was not very clear. Certainly, one could not have repeated the projections based on the information given in the SRES.

These models should then be fitted to the historical data, and the best models selected and averaged using objective techniques, such as those based on likelihood. Where this is difficult, e.g. where phase shifts may occur, subjective probabilities should be attached to these phase shifts. The models should then be extrapolated into the future, together with a probability distribution. Scenario developers should develop their scenarios with reference to this model-averaged extrapolation.

Although there is no reason for scenarios to be constrained by a 'deterministic extrapolation of the past' (Grübler et al. 2004), reference should be made to the past, and divergences identified and explained. The range of scenarios should encompass prediction and model selection uncertainty, as well as 'reasonable' phase shifts and alternative model specifications. The only way to be transparent about what constitutes reasonable, is to assign subjective probabilities, and to agree in advance some overall percentage of the probability distribution that should be encompassed. Similarly, the range of projections should be centred on the model-averaged projection, and be symmetrical with respect to the (partly subjective) probability distribution. There is understandable demand from the end users of scenarios for probabilities to be assigned to different outcomes (Schneider 2001, Pittocket al. 2001). Although the SRES team are right to point out that this cannot be done objectively (Grübler & Nakicenovic 2001, Grübler et al. 2006), it is essential if the projections are to have any useful role in planning and policy analysis, where tradeoffs must be made. The only solution therefore, is to assign subjective probabilities, possibly based on polls of experts, to every assumption not based on data, e.g. the probabilities of phase shifts occurring. This does not preclude, and in fact improves, the representation of uncertainty on which the IPCC insists (Grübler et al. 2006). This will help to ensure transparency and constructive debate.

Attention should be paid to long-run mega-trends, though I recognised that objectively comparing scenarios with such trends is more difficult, because of the nature of the data, and the greater variety of methods that might be applied to their

extrapolation. However, in principle, the same approaches that have been applied in this study to the recent past, could be applied to longer-run data. I suggest that adherence to these guidelines on the part of both developers and critics would make for considerably more constructive debate when the next generation of long-range income scenarios is published.

4. The environmental limits to economic growth: a review using the Ecological Footprint

"Anyone who believes exponential growth can go on forever in a finite world is either a madman or an economist." Kenneth Boulding

"Although life on this Earth is very far from perfect there is no reason to think that continued economic growth will make it any worse." Beckerman (1972).

"….but there are also unknown unknowns." Donald Rumsfeld.

Abstract

I review the debate over environmental limits to economic growth, considering both mainstream and ecological economic perspectives. I introduce the environmental and economic scales of the human economy. I argue that while there are binding constraints on environmental scale, technological progress can ensure that there are no such constraints on economic scale implying that economic scale can be decoupled from environmental scale, resulting in an environmental Kuznets curve. I consider the characteristics of technology, noting that it is a form of manmade capital, and therefore that its development is dependent on income levels. Furthermore, because of the non-rival nature of technology, there may be significant spill-over to poorer countries. I argue that empirical analyses of decoupling that separate technological change from economic growth provide a biased assessment of the effects of economic growth on the environment. I outline a hierarchy of analytical frameworks suitable for investigating decoupling, ranked according to their treatment of technology. I introduce the Ecological Footprint as a measure of environmental scale and review analyses of the Footprint-income relationship, showing that the literature is biased towards over-pessimistic conclusions about decoupling. The most

pervasive source of bias is the use of analytical frameworks which ignore technological progress. Finally, I demonstrate the quantitative effect of ignoring technology when predicting future Ecological Footprints.

I. Introduction

In line with the mainstream view of economic growth, environmental constraints on the growth of the human economy were not explicitly considered in the previous chapter.[87] However, a dissenting school of thought argues that, since the human economy is a subsystem of a finite natural environment, dependent upon it for all its material inputs, continued economic growth must be subject to constraints. This issue must be addressed in any coherent cost-benefit analysis of global environmental issues. It makes little sense to assume the existence of important environmental concerns in calculating the expected net benefits of a project, while ignoring them when estimating long run economic growth (and therefore the discount rate).[88] The aim of this, and the following two chapters is to ecologically parameterise the economic predictions of the previous chapter so that the resultant model can represent both mainstream and ecological economic thought.

In Section II, I review the limits to growth debate, introduce the key concepts of environmental and economic scale, and the central importance of technology. In Section III I outline the unique characteristics of technology and discuss how technology should be treated in analyses of the relationship between environmental and economic scale, formalising this as a hierarchy of analytical frameworks. Section IV introduces GDP and the Ecological Footprint as proxies for economic and environmental scale respectively, which allow empirical analyses of the issue. In Section V I conduct a systematic review of Footprint-income analyses, and demonstrate that the literature to date has made disproportionate use of approaches which ignore technology, leading to a bias

[87] For example, in over 650 pages the leading graduate-level textbook on mainstream growth theory (Barro & Sala-i-Martin 2004) makes only one explicit mention of environmental limits (pp 407-408) where the ideas of Malthus are briefly introduced and dismissed in a discussion of fertility rates.

[88] Dasgupta (2008: 3) dissects one example of this incoherence.

towards overly pessimistic conclusions about decoupling and the existence of environmental Kuznets curves. Section VI goes on to highlight several further flaws in these analyses, which further bias the results towards pessimism. In Section VII I estimate the quantitative implications of ignoring technology, and in Section VIII I conclude.

II. Limits to the Scale of the Human Economy

The ecological economic approach to growth focuses on the scale of the human economy relative to the finite natural environment on which it depends (e.g. Daly & Townsend 1993:1-2). According to this view, the issue of scale has been neglected in mainstream economics, the result being that under the mainstream paradigm, the human economy will tend to reach a sub-optimally large scale, relative to the finite environment. This cornerstone of ecological economics has been the subject of vigorous debate and disagreement with mainstream economists, which has lasted several decades and continues to this day (e.g. Beckerman 1972, 2003, Georgescu-Roegen 1975, Daly 1997a,b, Solow 1997, Dasgupta 2008).

One major disagreement is over the desirability of using resources sustainably. The mainstream economic approach (e.g. Weitzmann 1997) emphasises welfare or utility as the focus of sustainability.[89] Thus, sustainable development is that which ensures non-declining welfare over time, and sustainable resource use is but one means towards that end. Many ecological economists, on the other hand, focus on sustainable resource use, implying that sustainable use of natural resources is an end in itself:

> "I adopt the throughput definition [of sustainable development] and reject the utility definition, for two reasons. First, utility is non-measurable. Second, and more importantly, even if utility were

[89] Dasgupta (2008) talks of non-declining productive base, but this productive base is valued from a utilitarian perspective.

> measurable it is still not something that we can bequeath to the future. Utility is an experience, not a thing."[90] Daly (2002:2).

These conflicting views can be analysed in two different ways, firstly as a disagreement over ethics, second as disagreement over facts. Ethically speaking, the 'utility definition' could be seen as consequentalist, while the 'throughput definition' is based on the property rights of future generations, and therefore deontological. Thus, if one argues deontologically, requiring sustainable resource use does not imply that it is necessary for sustainable welfare, but rather that future generations have a right to the same resource flows or stocks as presently enjoyed, regardless of the consequences for present or future welfare (see e.g Howarth 1995). If the disagreement is empirical, on the other hand, the differences in sustainability definitions imply positive differences of opinion over whether sustainable use of resources is a necessary condition for non-declining welfare. Although the ethical argument is clearly valid, it will not be considered further here, since the objective of the chapter is to consider the impact of resource-use patterns on future welfare – and therefore requires a consequentialist approach.

Before proceeding, I should note that in most of the following discussion, I focus on per capita economic growth in a broadly stable human population, whose present anf future size is more or less given. This is in contrast to much of the limits to growth debate, which took place in the context of apparently relentless population growth, and which often confounded the two issues. It now appears more likely that, irrespective of environmental constraints, the human population will stabilise during this century, and possibly decline thereafter, as a result of individual reproductive choices (Lutz et al. 2001).

Nevertheless, sustainability and the limits to growth cannot be considered without considering variable populations (where population is not exogenous to the scenarios under consideration), since some viewpoints (e.g. Georgescu-Roegen 1977b) imply a trade-off, even at stable population levels, between the existence of a human life now, and the existence of human lives in the future. Adler and

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[90] He continues, intriguingly: "I hasten to add that I do not think economic theory can get along without the concept of utility. I just think that throughput is a better concept by which to define sustainability"

Posner (2006:176) refer to variable populations as a "foundational problem" for cost-benefit analysis of the environment. The question of whether the existence of an additional person is good or bad is partly scientific (the effects of that person's existence on the welfare of others) and partly philosophical (whether the moral community includes those who will never be born under one or more scenarios).

The mainstream view of the limits to growth

Mainstream economists have not generally viewed unsustainable use of particular resources, at a particular moment in time, as a barrier to ever-increasing welfare. This is not because they view natural resources as infinite.[91] Instead, they recognise that it is not just depleted stocks of resources (natural capital) which we bequeath to our descendents, but also increased stocks of physical capital and technology. Thus, Wilfred Beckerman (1972, 1974, 2003) dismisses claims that increasing scarcity of natural resources would endanger the welfare of future generations. Instead, he argues that scarcity would stimulate the search for new deposits and substitute materials, and especially, the development of new technologies, which would increase the productivity of those resources that remained by reducing extraction costs and increasing resource use efficiency. Continuous technological improvement would allow endless substitution and recycling of resources and solve environmental problems, keeping living standards rising, even as natural resources were depleted (see also Barnett & Morse 1963). The ecological-economic response to this argument has been framed in terms of two key concepts, entropy and capital, which I discuss in turn below.

Entropy

Georgescu-Roegen (1971) argued that since the economic system, like any other, must be subject to the laws of thermodynamics, continual economic growth was impossible: "in thermodynamic terms, the economic process converts matter-energy from a state of low entropy to a state of high entropy" (Georgescu-Roegen 1995:177). In a closed system, endless recycling and substitution is

[91] Beckerman (2003:9) says: "Either resources are finite in some relevant sense, in which case even zero growth will fail to save us in the long run, or resources are not really finite in any relevant sense".

impossible, since any reduction in entropy can only be had at the expense of an even greater increase elsewhere. According to Georgescu-Roegen then, the arguments of Beckerman are flawed (Georgescu-Roegen 1975). However, while the laws of thermodynamics are not in doubt, three criticisms have been levelled at Georgescu-Roegen's assertion that they necessarily imply that continued economic growth is impossible and undesirable.

First, if the earth were a closed system, entropy would continually increase even if humans returned to the hunter-gatherer state, or ceased to exist altogether. Georgescu-Roegen (1977) argues that even a steady state or declining economy will "converge toward annihilation in a finite environment". In this case, drastic steps might postpone extinction, but at what cost in the meantime? Will our societal welfare function regard this as worthwhile? As Pournelle (1977) observes: "To recommend negative growth on the basis of billion-year thermodynamic analyses is to take a very long view indeed."[92]

Second, and more importantly, the earth is not a closed system, but receives a continual flow of solar radiation (low entropy energy) from the sun and radiates higher entropy energy away as heat (Young 1995). Indeed, according to Georgescu-Roegen himself (1971[1999]) "surprising as it may seem, the entire stock of natural resources is not worth more than a few days sunlight!" Thus, given a constant flow of low entropy energy, and sufficient technological progress, complete recycling may theoretically be possible – at least until the sun ceases to radiate energy, in five billion years time.[93] Pournelle (1977) comments thus:

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[92] Note that this point does not directly contradict the assertion that sustainable resource use is necessary for non-declining welfare, it merely points out that Georgescu-Roegen appears to argue that sustainable resource use is impossible, and therefore that non-declining welfare is also impossible. This is a challenge to Daly's steady-state argument, and presents a seemingly insurmountable problem, one which Georgescu-Roegen apparently tries to solve by demanding that population must not remain steady, but rather decline (1977). Of course, an ever declining population must eventually reach zero, also implying "premature" extinction of the human race.

[93] Georgescu-Roegen's (1977) insistence that complete recycling is not possible because "it suffices to recall the impossibility of completely purifying a mixture (Planck 1945) in order to see why, in addition, no single substance can be recycled completely" seems unconvincing. Since very few if any materials were completely pure when humans first used them, there does not seem to be any reason why they should be completely pure second time around.

> "If Georgescu-Roegen's analysis merely indicates that the universe will someday suffer heat death," he has said little that is new and nothing of utility for policy planning." Pournelle (1977).

In Georgescu-Roegen (1977a), he is more specific in his definition of the earth as a closed system: although it is an open system with regard to energy, it is closed with regard to matter. Georgescu-Roegen argues that matter can be viewed entropically, and thus the entropy of the earth's matter must forever increase. However, Young (1991) argues that the entropy law can only be applied to matter by way of analogy, and that "materials entropy cannot be defined independently of technology" (Young 1995). Thus, technological progress, which changes a stock of matter from unavailable to available, can reorder a system, such that it has lower entropy than the system had before. Although Townsend (1992) and Daly (1992) argue that Young is wrong, in my view their arguments do not refute his central point. For example, Townsend disputes Young's contention that technological progress can reduce the matter-entropy of a system, arguing instead that while technology can improve the efficiency with which we derive services from entropic degradation, it cannot change the direction of flow. In my view this is incorrect. However, even if we accept this as true, it is not at all clear that his conclusion is correct, i.e. that "economic growth ...reduces the potential for generation of services from stocks for future generations". If technological progress increases the potential for generation of services from given stocks, and if technological progress results from, or occurs in parallel with economic growth, the effect of economic growth on the "potential for generation of services from stocks" is indeterminate, not necessarily negative. Similarly, Daly (1992) appears to admit the truth of Young's argument when he writes that "new knowledge may expand available matter faster than economic activity will convert it into unavailable matter (and energy)". Although he goes on to argue that "new knowledge may also reveal new limits and reduce available matter-energy (e.g. discovery of greenhouse effect lowers the effective availability of fossil fuels..." While the second point is no doubt correct, it still points to an indeterminate, rather than necessarily negative, effect of economic growth (with attendant technological progress) on the potential for generation of services for future generations. In summary, Young (1991) does not argue that the entropy laws do not hold, but rather that they are irrelevant to economics:

> "In principle economic models of resource prices which signal relative resource scarcities are sufficient. There is no need to add anything based on the idea that entropy inevitably causes increasing absolute scarcity ... entropy considerations are redundant in an economy which generates a "correct" relative price structure". (Young 1995).

However, despite the reservations of Young (1991, 1994) and Pournelle (1977), entropy does offer a useful framework to analyse sustainability, since it makes clear that mankind does face some constraints, and also because the real-world economy does not, at present, "generate a correct price structure". In addition, Young (1994:213) admits that his arguments do not amount to a deterministic assurance that there are no environmental limits to growth, since the future development of technology is inherently uncertain:

> "This point in no way implies a belief that technology will solve all problems or that economic growth is not environmentally limited. To say that technology changes system boundaries in beneficial ways is not to predict that relevant technologies, e.g. cheap solar energy, will be forthcoming." Young (1994).

While endless recycling and substitution might theoretically stave off scarcity forever, they are bounded by the supply of low entropy energy that we receive from the sun and by our technological ability. This highlights the central role which technology must play in future economic growth, and offers a potential route towards the quantification of limits, which I discuss below.

This highlights a third criticism which can be made of the arguments of Georgescu-Roegen and Daly. Namely that their arguments appear to rely on a presumption that continuing technological progress is impossible. For example, when arguing that even a stationary population must suffer an ever-declining standard of living, Georgescu-Roegen (1977b:771) writes:

> "Let y_1, y_2,. . . ,y_n, be the amounts of mineral resources ranked in the order of the real unitary costs of bringing them to the surface of the earth: $c_1<c_2<c_3<...<c_n<....$ After a stationary population consumes y_1, it must turn to y2. Since the latter is harder to mine, something must happen: Either an invention miraculously comes up to decrease the unitary cost from c2 to c1, or the population decreases, or life

> becomes less "good." The Fourth Law of Thermodynamics proves that there is no way to avoid this impasse forever." Georgescu-Roegen (1977b), emphasis added.

Yet, the highlighted sentence exactly describes the process that has occurred since man began to mine minerals (Beckerman 2003). Extrapolation is not proof, but it seems unnecessarily pessimistic to characterise a historically observed process as miraculous, and therefore impossible. It is ironic that Georgescu-Roegen's (1977a) assertion that a flaw of neoclassical economics is that it does not recognise the evolutionary, and irreversible nature of the environmental-economic system, could equally be levelled at Georgescu-Roegen himself: "knowledge itself is not entropic, because it is not conserved when it is used...knowledge can be created" (Young 1994:212). Thus knowledge can accumulate, implying an optimistic aspect of irreversibility.

Thus, the entropy approach to economic activity developed by Georgescu-Roegen is useful, but his own interpretation of it appears too simplistic and pessimistic to convince mainstream economists (e.g. Young 1995, Solow 1997, Stiglitz 1997) or even some ecological economists (e.g. Ayres 1998). Given continual technological innovation and solar radiation, it might therefore be possible for economic growth to continue indefinitely. However, the above analysis has focussed on flows, and would tend to imply that the actual stock levels of natural resources are unimportant. This is partly because, until relatively recently, the focus of the limits to growth debate was on non-renewable minerals (Barnett & Morse 1963).[94] The actual stock level of these materials may be relatively unimportant to the functioning of the earth. Since then, attention has increasingly turned to the "new scarcity": firstly of goods and services supplied to humankind by dynamic living processes, and secondly the harmful effects of waste products (Simpson et al. 2005). The latter indicates that levels of high-entropy may be as important as levels of low-entropy.[95] Analysis of the former requires attention to capital levels.

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[94] Note however that Beckerman (1972, 1974) treats the issue of pollution at length.

[95] Atmospheric concentrations of CO_2 are one example of damaging high-entropy matter. As Young (1991) pointed out, the stocks of high entropy matter can be reduced by technology, in this

Capital

Ecological economics recognises at least four generalised, but non-homogenous types of capital: natural capital; physical capital; human capital; social capital; and cultural capital or technology (Edwards-Jones et al. 1999[96]). The level of current consumption is determined by the level and composition of the overall capital stock. Capital of any type can be consumed in the short run to boost consumption levels but a permanent reduction in the level of any type of capital will, holding all else equal, reduce current and future consumption possibilities (see e.g. Arrow et al. 2004).

Natural capital predated humans, while the other capital types result from investments made by humans out of current consumption – they could be included within a broadly defined "man-made capital". However, while this emphasises the point that human and social capital and technology result from human actions and require savings (and therefore income) as well as human brainpower (Simon 1981), they differ from physical capital in that they do not directly contain natural resources (although natural resources may be used in their production). Viewed from a capital perspective, the history of the world since the emergence of humans has been one of decreasing natural capital, and increasing man-made capital (including technology and human capital). I discuss the particular characteristics of technology, and the implications for empirical analyses of sustainability, in Section II.

In principle at least, we can envisage substitutability between natural capital and manmade capital. Depending on the level of each, an efficient solution is to invest foregone consumption in the capital type offering greatest marginal returns. However, the degree to which the two types of capital are substitutable is the source of considerable debate (e.g. Daly 1997a,b, Dasgupta 2008). Ecological

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case, technologies which either return atmospheric CO_2 to a less damaging form (sequestration) or which protect against the negative impacts of climate change (mitigation).

[96] Human capital includes the abilities and knowledge possessed by individuals, which dies with those individuals. It is commonly separated from raw labour (e.g. Mankiw et al. 1992). Cultural capital or technology includes knowledge, policies and institutions, which can be passed on. However, it is common in all strands of economics to treat technology as a residual – whatever is not specified in a particular analysis. It is in this sense that I will refer to technology throughout much of this chapter, but the point that technology is a form of capital, which requires investment, but does not contain either matter or entropy, should always be retained.

economists argue that there are significant constraints on the substitutability of the two forms of capital, and that there may be thresholds beyond which depletion of natural capital leads to non-linear and devastating consequences (Arrow et al. 1995).[97] This leads to the concept of critical natural capital, destruction of which would be irreversible and disastrous. Almost by definition, the point at which such non-linearities begin is highly uncertain (Arrow et al. 2004). This argument presents significant difficulties for conventional economic analysis (Dasgupta 2007a), though neoclassical economists have begun to respond (e.g. Weitzman 2008). Entropy and capital are linked because the reduction of entropy levels requires that a greater part of solar radiation must be appropriated by humans (for a given level of technology), implying a reduction in the natural capital that is sustained by the remaining solar radiation.

Thus, aside from the simple fact that declines in low-entropy stocks or natural capital cannot continue indefinitely, future consumption can be jeopardised in at least two ways. First, elevated entropy levels, even at equilibrium, may be dangerous: for example, elevated atmospheric CO_2 concentrations, which lead to climate change. Second, levels of natural capital may be reduced beyond critical thresholds. The human economy therefore faces both entropic and natural-capital constraints. As a result the scale of the economy, measured in entropic and natural capital terms (what I will term the environmental scale[98]) is constrained. However, this does not determine whether future growth in welfare (the economic scale of the economy) is possible as this depends on whether the environmental scale of human society is constantly proportional to the economic scale.

Economic scale and environmental scale

In refuting what he saw as ecological pessimism about the possibility for growth in the economic scale of human society, Beckerman (1972) commented thus:

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[97] In effect, the marginal rate of substitution between manmade capital and natural capital becomes suddenly infinite at these thresholds.

[98] Environmental scale is used throughout this thesis to mean the size of the human economy relative to binding environmental constraints. It is therefore measured in physical units, not economic ones (except for a given level of technology). It is multi-dimensional, since constraints are multi-dimensional. Informally, it will be used interchangeably with “environmental impact”, e.g. when discussing the “IPAT” formula.

> "Hence, when the scientists ... decide that [ecological demand is a function "*f*" of GDP]: it seems obvious to them that the only way to stop an indefinite rise in [ecological demand] is to stop the rise in GDP. But, of course, to the social scientist, it is equally obvious that one must first ask whether it would not be preferable to use some policy instrument to change '*f*'." Beckerman (1972).

This can be restated as:

Equation 4.1 Environmental Scale = *f*(Economic Scale)

In considering that *f* might need to be changed, Beckerman implicitly acknowledges that there may be limits to the environmental scale of human society. In assuming that *f* can be changed he denies that limits to growth can meaningfully be measured in economic terms. Thus, environmental scale is measured in physical units (e.g. entropy and capital) while economic scale is measured in terms of utility (perhaps approximated by dollars of GDP per capita). To Beckerman at least, it is clear that the economic growth that many ecological economists believe must stop, is the same economic growth for which GDP acts as a proxy: the value of goods and services consumed by the population. This is, after all, the conventional and almost ubiquitous definition of economic growth. However, it is not at all clear what many prominent ecological economists mean, when they refer to an "economic growth" which must stop.[99] For example, Daly and Townsend (1993) state baldly that: "economic growth is both physically and economically unsustainable, as well as morally undesirable". Later in the same book (p325), Daly (1993) equates economic growth with growth in physical capital and throughput of matter-energy, since it is these (along with population) which he proposes be held constant in a steady-state economy. He then (p330) introduces a distinction between economic growth and economic development, the former representing growth in services attributable to growth in throughput, while economic development represents changes in the efficiency with which

[99] Boulding's famous statement, which began the chapter, refers only to "growth" and does not mention economic growth. It is unclear whether Boulding meant "economic growth", or whether he believed that nothing, not even non-physical quantities like utility or happiness, could grow indefinitely in a finite universe.

throughput renders services. Yet no mainstream definition of economic growth makes this distinction, a point Daly (1993) acknowledges (p330).

Has the limits to growth debate therefore been the result of semantic differences over the meaning of economic growth? Would Daly and Boulding be reconciled with Beckerman and Solow if they all agreed to a definition of economic growth that was not directly related to physical stocks and flows? I find it difficult to say. Of course, the former were arguing that mankind was not making sufficient effort to change Beckerman's *f*: i.e. that our environmental policy was sub-optimal. But this is really a question of degrees: how much effort should we make to change *f* at any given time? The greatest differences have been the emphasis by ecological economists on non-linearities in the substitutability of natural capital and manmade capital (e.g. Dasgupta & Maler 2003), their focus on the conditions necessary to maximise intergenerational welfare,[100] and also their repeated reminder that the real-world economy does not resemble the perfect price generating economy assumed in standard economic models. In addition, many of their criticisms of the status quo have been directed at policies which seek to maximise crude statistical proxies (such as GDP) rather than welfare itself (of which more in Section III). These are valid contributions and correct important flaws in the neoclassical approach. However, none of them necessarily dictates that continued growth in per capita welfare is impossible if appropriate attention is paid to the environment.

Technology, decoupling and the environmental Kuznets curve

If we adopt a utility-based definition of economic growth, it is clear that continued growth in economic scale may be possible, if it can be sufficiently decoupled from environmental scale.[101] As Ayres (1998) argues:

> "It is possible to have economic growth - in the sense of providing better and more valuable services to ultimate consumers - without necessarily consuming more physical resources." Ayres (1998).

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[100] For example Weitzman's (1997) approach relies on perfectly functioning markets and perfect information.

[101] This decoupling may be urgent, if mankind is presently close to critical thresholds in high-entropy levels (atmospheric CO_2) or natural capital (e.g. biodiversity).

Of course, the key determinant of decoupling is technology. If we obtain greater utility from smaller throughput, that is because technology, broadly defined, has changed.[102] Technology, broadly defined, can be thought of as the *f* in Beckerman's equation. Yet some ecological economists have stated that it is reckless to put too much trust in technology:

> "This blind and total optimism about the ability of technology to solve all our problems and allow economic and population growth to continue un-abated forever is certainly not a position held by many reputable economists." (Costanza 1995 p89).

There are two problems with this view. First, it seems to ignore that, under most definitions, Daly's prescription for a steady-state economy (about which Costanza speaks highly) is a technology.[103] The technologies that could bring us ever-lasting economic growth, in the face of fundamental scarcities, include policies which act to increase the efficiency with which physical throughput is turned into utility. To the confusion over definitions of growth, we can add confusion over the definition of technology.[104] Second, this seems to ignore issues of causation. Technology is the cause of economic growth, if factor endowments are held constant. To argue that economic growth should not be "allowed" to continue (even if resource depletion is constrained) is to argue that technological progress should actually be banned, not simply that it should not be relied upon.

A specific formulation of the belief that economic and environmental scales can be decoupled is the Environmental Kuznets Curve (EKC) hypothesis. This posits that environmental scale[105] increases initially with rising incomes, and later decreases (see e.g. Dasgupta et al. 2002). If f can be changed, particularly as greater stocks of technology are accrued, then an EKC may exist, and it becomes

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[102] This may also include preferences, since the final stage in the process of converting services into utility occurs within the human brain.

[103] Both sides of the debate define technology broadly. For example, the IPAT formulation (Impact=Population*Affluence*Technology) favoured by many environmentalists, e.g. York et al. 2003 define technology thus: "In the STIRPAT model T represents everything that is not population and affluence" (p 354). In neoclassical economics (e.g. Solow 1956) technology is everything apart from reproducible capital and population.

[104] Of course, Costanza may mean that we should not deplete resources in the hope that technology will enable them to be recycled or replaced, within entropic restraints, but this is not clear.

[105] Scale is multidimensional, since constraints are also multi-dimensional.

possible to reconcile an ecological economic view of the human economy – one which sees it as embedded within an ecological earth system, bounded within an entropic-capital framework – with continued economic growth. Of course, as Arrow et al. (1995) point out, an EKC is not a sufficient condition for long-term sustainability. For example, if there are thresholds on the extent to which natural capital can be depleted, if raised entropic levels carry their own dangers, and if both of these risks may manifest themselves without sufficient warning, human society might still be doomed even as it describes a perfect EKC. This issue is more complex, contains deep uncertainty, and will be discussed further at the end of the next chapter.[106] The remainder of this chapter, and most of the next, is concerned with empirical analysis of the changeability of *f*, and the existence of an EKC.

Conclusions

This section has described how the limits to growth can be conceptualised in terms of constraints on the environmental scale of the human economy and that the two major dimensions of these constraints are natural capital and entropy. Although these constraints are binding, they do not imply a binding constraint on the economic scale since this is not directly proportional to environmental scale, the relationship between the two being determined by technology and therefore potentially mutable with technological progress. I have argued that technology is a form of man-made capital resulting from human investments, and therefore cannot be considered as independent from income levels: economic growth may increase the capacity for technological progress. However, given the highly uncertain consequences of natural capital depletion and entropy increase, and the unpredictability of technological progress itself, economic growth may endanger the wellbeing and even existence of future generations, and the issue of limits to growth is inextricably linked to questions of the treatment of as yet unborn, and never born generations.

[106] However, note that if *f* is changeable, and if an EKC exists for environmental scale, the question becomes one of trading off (potentially unknown) risks against (potentially immeasurable) welfare gains, and the answer will be far from clear-cut: see Weitzman (2008).

III. The treatment of technology in analyses of decoupling and the environmental Kuznets curve

The previous section highlighted the importance of understanding how environmental scale is linked to economic scale (*f* in Equation 4.1 from the previous section), and of technological progress in changing this relationship.

Chapter 3 also made clear the central role of technology in driving economic growth. Neoclassical economists recognise that, for a given level of technology (broadly defined) an ever increasing physical capital stock has diminishing returns, in terms of per capita income (Solow 1956). To continue to expand income requires continual improvements in technology. This perspective is shared by ecological economists, who would also add that physical capital comes at the expense of natural capital and also there is limited and highly uncertain substitutability between the two (though this is technology dependent). In addition, given entropic constraints, maintaining the world at steady entropic equilibrium, while maintaining natural capital, will require steadily increasing stocks of technology.

Technological progress therefore drives economic growth and it is clear that we cannot ignore it. However, can we rely upon it? Will it continue? If technology drives growth, what drives technology?

In this section I consider the implications of technology for empirical analysis of the relationship between environmental scale and economic scale, and of decoupling between the two (an environmental Kuznets curve EKC). I propose a hierarchy of preferred analytical frameworks for these analyses (which will be used in Section IV to evaluate the existing literature).

The Drivers and Characteristics of Technology

I show in Section IV that the special characteristics of technology are often ignored in empirical analyses. In order to determine how analyses of economic and environmental scale should treat technological progress, it is helpful to first consider these characteristics and the factors which determine the rate of technological progress.

Unlike physical and human capital, technology is partly non-rivalrous. Thus, there is likely to be overspill from individuals or nations who invest in technology to

those who do not. For example, China is adopting European standards of vehicle pollution controls with a lag of only 8-10 years (Stern 2004).[107] Technology is also potentially immortal, leading to considerable inter-temporal overspill.[108] Thus, while future generations are likely to bear some negative externalities as a result of the present generation's resource use, they are also likely to benefit from the positive externalities of its innovation and accumulation of physical capital. This discussion should make it clear that technological progress cannot be considered at the level of a single country.

In addition, the rate of global technological progress is likely to be related to the other variables of interest in the analysis, including:

1) Expected returns on investment, which will be related to the perceived proximity of environmental limits or thresholds (i.e. proximity to binding environmental constraints will stimulate investments in technological progress).
2) Income. Technology results from investing foregone present consumption (income or welfare) and rates of investments are likely to increase with income levels.[109]
3) Human capital. Simon (1981) argued that the source of all wealth was the human brain. The level of human capital is related to a population's size but also its health, age, education and economic and social freedom. Human capital is itself an investment, and is correlated with income (e.g. UNDP 2006).

The driving forces of technology, together with its non-rival nature, make analyses of the relationship between environmental and economic scales difficult.

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[107] Friedman (2005) provides another striking example of technological overspill. Life expectancy in China is 71 years, despite its relatively low per capita income of $5,000. In 1880, when the US had a similar level of per capita income, life expectancy was only 41 years.

[108] The dispute between ecological and mainstream economists could be viewed as differences of opinion over the relative magnitudes of positive and negative externalities of economic growth. The former believe that growth has a net negative externality for future generations, because of resource depletion. The latter believe it has a positive externality because of intergenerational overspill of technology. According to conventional measures like GDP, the latter has apparently been the case to date (Maddison 2006).

[109] This would be true even if the marginal propensity to save were constant or to diminish with increasing income. In fact, it is probable that it increases (e.g. Dynan et al. 2004).

This is clear if we consider a regression analysis of environmental scale and economic scale (Equation 4.1), based on cross-sectional data. In such analyses, it is common to use the "IPAT" formulation, whereby Impact is proportional to Population, Affluence and Technology (e.g. York et al. 2003a, Dietz et al. 2007). In general, increasing population and per capita affluence are expected to increase the environmental impact (scale) of society, while technology is often the residual in the regression. The problem is that while it is relatively easy to attribute the direct consumption-related impacts to the country responsible, and therefore to income levels, it is very difficult to attribute the technological advances that result from increased income levels.[110] It is likely that the environmental impacts of poorer countries are deflated by technological overspill from past economic growth in rich countries. Consequently, analyses which ignore the role of one country's economic growth in driving technological progress in other countries, will lead to pessimistic estimates of the relationship between economic growth and environmental impact. I show in Section V that many studies commit this error, and in Section VII I quantify the effect.

A hierarchy of analytical frameworks

It is possible to identify a rough hierarchy of preferred analytical frameworks for modelling the relationship between economic and environmental scales, and for testing for the existence of an EKC.

The best analysis would be a detailed panel analysis of data covering many years and a large sample of countries. It would include measures of capital investment (including human capital and spending on research and development), technological innovation and overspill.[111] The environmental impacts of goods and services would be attributed to their consumption rather than their production,[112] and technological innovations to the country and year of origin. Note that this

[110] Castles and Henderson (2003a) criticise this separation of technology from affluence in the IPAT formula.

[111] Data on patent citations might offer one way of looking at overspills between countries, but would not extend to less developed countries. Griffith et al. (2007) use patent citations to demonstrate increasingly rapid overspill between countries.

[112] This controls for the "off-shoring" of environmental impacts, see e.g. Nahman & Antrobus (2005).

analysis would not be expected to yield a simple EKC, since the consumption and technology effects of income are separated (see next chapter).

The next best alternatives are to use simpler panel data (where technological progress is analysed at the world level and consumption is analysed at the national level) and global time-series (where both technological progress and consumption are analysed at the world level). Although apparently more informative, simple panel analyses are potentially misleading: the true sample size of a simple panel analysis in any given year is still one,[113] and if technological progress is modelled as a function of time, it risks the impression that the process of technological progress is independent of income levels.[114] Global time-series analyses, while simplistic, may be more analytically honest, since they do not disguise that the true sample size is one, and they fully incorporate technological progress into economic growth. They provide the best simple test of an EKC.

Next, time-series analyses of individual countries have limited generalisability, and do not account for the origin of technological progress. They can be used to test the EKC hypothesis, if impacts are correctly attributed to consumption rather than production. However, particularly for developing countries, they may fail to find an EKC when one does exist, because they fail to account for technological overspill.

Finally, cross-sectional studies can only be justified if all other options are closed, and even then their usefulness seems doubtful. Because of technological overspills, they are extremely unsafe for projecting future trends and they cannot reasonably be used to test for an EKC, since they exclude the technology effects of income growth. The EKC is a fundamentally longitudinal hypothesis.

Conclusions

In contrast to some ecological economists, I take the view that technology is best considered as being endogenous, and positively related to income, which is in turn positively related to technology. Therefore, the best analysis is one which uses

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[113] Again, such an analysis would not be expected to display a simple EKC, a point apparently ignored by Stern (2004).

[114] I discuss alternative specifications that may be more appropriate than time, including Gross World Product, in Section VI.

detailed data to explore the drivers of technology, and to explore the exact nature of the relationship between technological progress and economic growth. However, if this data is unavailable, it is more justifiable to subsume technological progress into economic growth than to separate it out.

IV. Proxies for economic and environmental scale

Since the economic and environmental scale variables are difficult, if not impossible to measure exactly, any empirical analysis must use imperfect proxies. Below I introduce two common proxies and discuss their advantages and limitations.

GDP as a Proxy for Economic Scale

The most common measure of economic scale is Gross Domestic Product (GDP[115]). Gross National Income (GNI) would be preferable, since it more closely relates to the income of citizens, but is less commonly available. When aggregated to the world level (as in the next chapter), the two measures are in any case equivalent (i.e. Gross World Product ≡ Gross World Income). Net National Income and Net National Product are less preferred, since they are not solely concerned with current income or utility, but rather with maintenance of the productive base.[116]

Per capita GDP or GNI are at best proxies for utility and may not even be monotonically related to it. Two potential causes of discrepancy are the exclusion of non-traded goods and services (including those provided by the environment and by leisure time), and the diminishing marginal utility of monetary income. Several attempts have been made to correct for these biases.[117] However, many of

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[115] At constant prices converted using purchasing power parity.

[116] A measure based purely on capital, rather than income could be used to measure economic scale, but this would pose a slightly different question. Arrow et al. (2004) propose such a measure (Genuine Wealth), which includes natural capital. Although they do not present this finding, analysis of data from their Table 2 shows a strong positive correlation between growth rates in GDP per capita and Genuine Wealth (R^2=89%, 69% if China is excluded, indicating that if such a measure was used instead of GDP, similar results might be obtained).

[117] Tobin and Nordhaus (1973) showed that their Measure of Economic Welfare, which accounted for leisure time and natural capital depletion, were well correlated with GDP. Daly & Cobb (1989) propose the Index of Sustainable Economic Welfare, which developed into the Genuine Progress Indicator (Talberth et al. 2007). These indicators apparently show a levelling off for the USA,

these have also accounted for the depletion of natural capital, as well as or instead of unmarketed ecosystem services. Therefore they are corrections to Net rather than Gross Income.

While GDP is imperfect, no available alternative includes utility-flows from natural and social capital and leisure, while controlling for the diminishing marginal utility of income. However, OECD (2006) shows that, cross-sectionally, adjusting GDP for the value of leisure time and for income inequality does not greatly change the ranking of countries.[118] More importantly, it should be remembered that all of these omissions from GDP are more likely to weaken evidence for decoupling than to strengthen it, since utility derived from non-physical capital and leisure time ought to be even more decoupled from physical throughput than GDP is.[119] Thus, if decoupling of GDP from environmental scale is found, this should provide grounds for optimism. However, this does not in any way justify the use of GDP in public policy. Indeed the adoption by governments of improved accounting measures, which improve policymaking by governments, may be a necessary condition for further decoupling of income from environmental scale.

A Proxy for Environmental Scale: The Ecological Footprint

The last decade or so has seen a proliferation of indicators designed to measure the impact of human society on the environment, and therefore its sustainability. Böhringer and Jochem (2007) review ten measures with an explicitly environmental component. Of these, the Ecological Footprint (Wackernagel & Rees 1995) has been more influential than any other indicator in both academic

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since the late 1970s, in contrast to a steady rise in GDP. However, Fig 7, p24 of Talberth et al. (2007) shows that the main cause of divergence between Genuine Progress Indicator and GDP has been depletion of natural capital – which is accounted for on the left hand side of the analysis under discussion. Dietz & Neumayer (2007) argue that this threshold effect is a methodological artefact.

[118] Longitudinally, the change in adjusted GDP over time might be different. However, the effect of leisure time and diminishing marginal utility of income would be expected to act in opposite directions, partly off-setting each other.

[119] An interesting question, which illustrates this issue, would be: "is per capita GDP at present a better measure of quality of life or of environmental scale, and how is that likely to change in the future?" It is clearly an imperfect proxy for either. My suspicion is that GDP, *as currently measured*, will become a less good measure of both scale variables, but that the GDP methodology will be adapted, such that it will remain a passable measure of the former, and a much worse measure of the latter.

and popular discourse (Figure 4.1). The Ecological Footprint compares the environmental impact of nations and the world with the available biocapacity. Its units are the Global Hectare (gha), where one gha has the bioproductivity of the world's average hectare. Eight components are included in the Ecological Footprint: cropland, livestock, forests, fisheries, built-up land, fuelwood, nuclear power and CO_2 emissions, the latter three making the energy component. Redefining Progress and the Global Footprint Network[120] provide Ecological Footprint data covering most nations of the world. The latter contributes to the WWF's biennial "Living Planet Report" which publishes the latest cross-sectional data for most countries, as well as time-series data for the world since 1961. Ecological Footprints have also been calculated for some nations by independent researchers (see Section IV).

The Ecological Footprint can be interpreted in an entropic-capital framework. The non-energy Footprint, as a proportion of available biocapacity, represents the proportion of natural capital displaced by human activity, and therefore the space remaining for wild nature (Noon & Dickson 2004). The energy Footprint represents the annual net change in high entropy stocks (of CO_2). The conversion rate between these two measures identifies what depletion of natural capital (increase in human appropriation of low-entropy solar radiation) would be necessary to render zero the net change in high-entropy stocks. CO_2 emissions are converted into global hectares based on the area of land that would need to be planted with trees to sequester the excess emissions.[121] The Ecological Footprint does not yet measure stocks of low-entropy energy-matter (e.g. non-renewable resources), though Nguyena and Yamamoto (2007) attempt this. Nor does it measure the human appropriation of low-energy flows relative to the maximum theoretically available from solar radiation.

The Ecological Footprint has several features that make it attractive for analyses investigating the relationship between economic and environmental scale and it has been widely used for this purpose (see Section IV). First, it covers a relatively large spectrum of human resource use but converts them into comparable units

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[120] http://www.rprogress.org/ and http://www.footprintnetwork.org/ respectively.

[121] The area of land which would need to be given over to biofuels to eliminate carbon emissions gives similar results (Loh & Wackernagel 2004).

(global hectares). Second, data are publicly available for a large number of countries in multiple years. Third, it attributes resource use to consumption rather than production (avoiding "off shoring" of environmental impacts, which could lead to the false discovery of EKCs in individual countries (Stern 2004). However, because the Ecological Footprint converts all impacts into gha, using global average yields, country-specific yield factors would be necessary to separate consumption and technology effects of income. These yield factors are not publicly available.

The Ecological Footprint has been the subject of several reviews.[122] Some criticisms of Ecological Footprint represent simple misunderstandings,[123] while others have criticised the ways that the data have been presented, rather than the underlying concepts.[124] A third type of criticism relates to impacts which are omitted from the Ecological Footprint, for example greenhouse gasses other than CO_2. In principle these could be included within the existing framework were data available.

The most important criticisms of the Ecological Footprint are those that cannot be met within the framework of an accounting measure. For example, van den Berg and Verbruggen (1999) criticise the calculation of the energy Footprint in gha, using only currently available technologies. It is unacceptable, they argue, to imply that the CO_2 emissions of the world would ever be tackled solely through the planting of trees and use of biofuels. Another criticism is that by summing impacts across different components and ecosystems, the Ecological Footprint

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[122] It was reviewed by van den Bergh and Verbruggen (1999) with responses by several contributors in the same issue of *Ecological Economics* (29:1) and again in volume 31:3. It was the subject of a discussion in the same journal in volume 32:3. Other critiques include Levett (1998), Ferguson (2002), and Jorgenson (2003, see also McDowell 2002) and Grazi et al. (2007).

[123] E.g. van den Berg and Verbruggen (1999) criticise the equal equivalence factors given to both built land and cropland – despite the former having greater environmental impact. This criticism is incorrect. Built up land, uniquely amongst the components, is added to both Footprint and Biocapacity (Loh & Wackernagel 2004). Thus, it reduces the biocapacity available for other uses. The equivalence factor is therefore irrelevant, but is set to be equal to that of arable land, since most built-land comes at the expense of arable land (Loh & Wackenagel 2004).

[124] van den Berg & Verbruggen (1999) have criticised the publication of "Ecological Deficit" figures for individual nations, because this displays an anti-trade bias. They argue that small, densely populated countries, like cities, will always run an ecological deficit, and yet such aggregations may be the key to ensuring sustainability (for example they can reduce per capita Footprints by reducing transport needs). This is a valid criticism: national Ecological Deficits themselves are not very informative (though trends might be), but they are not fundamental to the Ecological Footprint concept.

assumes substitutability of natural capital types, at least at the margin. These problems are inherent in static accounting measures (Wackernagel 1999). They highlight the need for: analysis of trends rather than static values in order to monitor technological progress; and modelling of plausible scenarios of technological progress and non-linearities in natural capital.

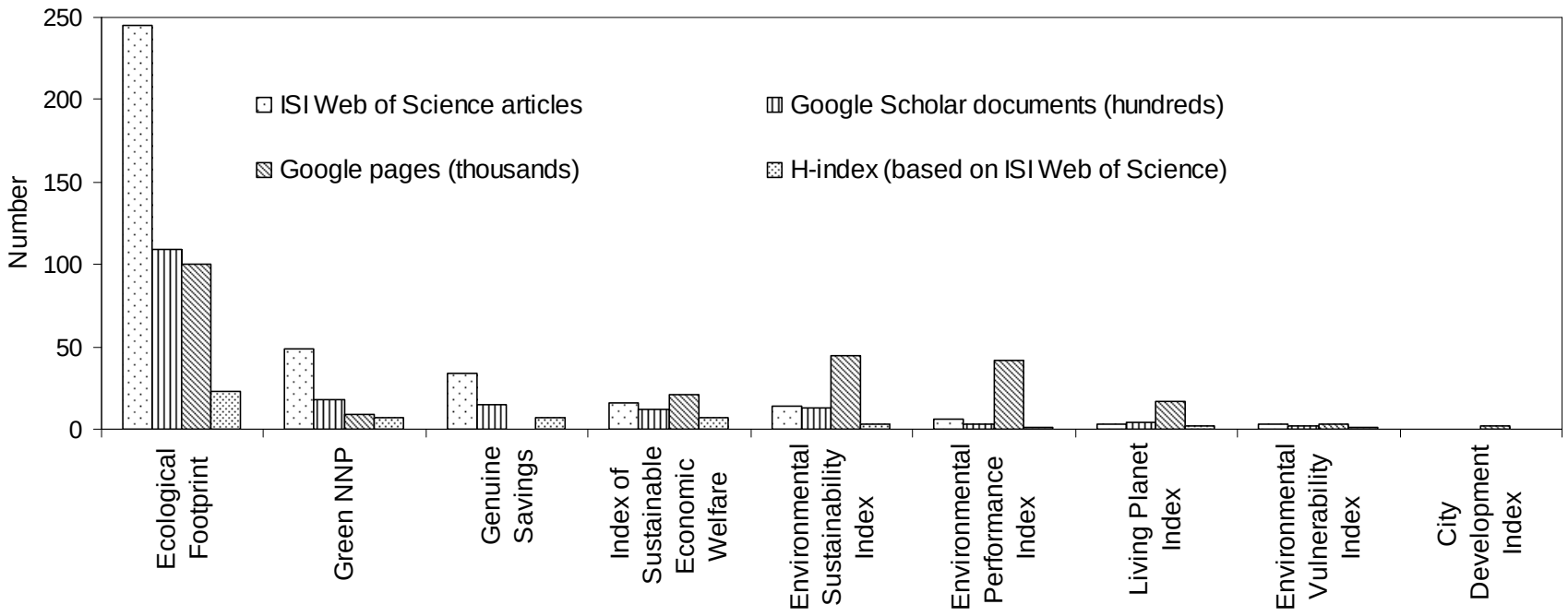

Figure 4.1. The relative influence of nine environmental sustainability indicators listed in Bohringer and Jochem (2007). Some of these measures (e.g. Green NNP) combine environmental and economic scale. The Human Development Index was excluded since it focuses on purely social aspects (eg GDP per capita). The "Well-Being Assessment" combines the "Well-Being Index", which is not environmentally based, with the "Ecosystem Well-Being Index" automated searches do not distinguish between the very large number of papers in the medical literature on the "Well-Being Index" and those on the broader index so this was excluded. However, the combined total of all of these papers (303) is comparable with those of the Ecological Footprint (245) meaning that the Ecological Footprint ranks higher than the environmentally based "Well-Being Assessment". The Google search was not possible for "Genuine Savings" because of the large number of commercial sites offering "genuine savings". The H-index was originally designed to quantify the productivity of an individual scientists. The H-index in this context is calculated from the set of articles that mention the indicator. It is the highest number (h) such that there are h articles, which have themselves been cited at least h times (Hirsch 2005). Thus, for the Ecological Footprint, there are 23 articles mentioning the Ecological Footprint which have themselves been cited at least 23 times. Searches were made on 27 January 2007.

V. A Critical Review of Footprint-Income Analyses

In this section I identify and describe studies that have used Ecological Footprint and GDP (or closely related measures) to investigate the relationship between environmental and economic scale. I review their conclusions, and classify them according the hierarchy set out in Section II.

Identification of studies

I reviewed all papers on the ISI Web of Science database containing the term "Ecological Footprint*" in the title, abstract or keywords (245 papers on 27th January 2007). Through examination of the abstracts of these papers, I identified all studies that reported inter-country or inter-temporal analyses of Ecological Footprint. I did not include studies that calculated Ecological Footprint for a single country or region in a single year. I found 23 such studies.[125]

Classification of studies and summary of results

Cross-sectional studies

Section II argued that cross-sectional studies are unsuitable for investigating the relationship between economic scale and environmental scale, because they ignore the effect of technological progress, which is positively related to economic scale. Because of this, cross-sectional studies are likely to provide a too-pessimistic representation of the true situation. Cross-sectional studies are also likely to reject the EKC hypothesis, when in fact it holds, or to find turning points which are higher than for the world as a whole. Despite these inherent flaws, cross-sectional analyses were the most common in the review, with eight studies investigating the influence of GDP per capita.[126] Of these studies, six explicitly test the EKC hypothesis.[127]

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[125] One other study (Bagliani et al. 2007) was identified by using Google Scholar to search for "Ecological Footprint" since 2005, and included in the review.

[126] Jorgenson (2003), York et al. (2003a&b), Rosa et al. (2004) Jorgenson & Rice (2005), York et al. (2005), Hammond (2006), Dietz et al. (2007). Two other studies (Jorgenson 2003 & 2004) use closely correlated variables (World Systems Position and urbanization).

[127] The exceptions being Jorgenson and Rice (2005) and Hammond (2006).

The eight studies used 3 different analytical specifications. Of those that test for the EKC, all used a quadratic income term. One group (York et al. 2003a&b, 2005, Rosa et al. 2004 and Dietz et al. 2007) used logged variables, aggregate Ecological Footprint and included population as an explanatory variable, while Bagliani et al. (2007) used untransformed variables and per capita Ecological Footprint. While the former group all find positive coefficients on the quadratic income term (implying the opposite of an EKC), Bagliani et al. (2007) report a negative coefficient. Nevertheless, Bagliani et al. (2007) argue that there is no evidence for an EKC because the quadratic function is outperformed by a strictly increasing concave power function, and because the turning point is at a relatively high income level, although within the range of the data. A final study, Hammond (2006) did not test for the existence of an EKC and used a power function. He finds the exponent on GNI per capita to be 2/3, implying a concave, though strictly increasing relationship.

As expected, Footprint-income studies that use cross-sectional specifications have reached pessimistic conclusions about the existence of decoupling between Ecological Footprint and income. However, in the next section I show that each of these approaches has flaws beyond those inherent in the cross-sectional approach, and that even the cross-sectional data are consistent with more optimistic conclusions.

National time-series studies

The primary aim of most studies reporting national time-series was to assess the trend in sustainability for the country in question.[128] Nevertheless, several studies discuss the role of economic growth in determining Ecological Footprint (e.g. Lammers et al. 2008, B. Chen & GQ. Chen 2006, 2007), and some (e.g. Lammers et al. 2008) appear to have this as their primary objective. However, only D. Chen et al. (2004) carry out any explicit analysis of the relationship between income per capita and Ecological Footprint, the rest reported trends in each variable

[128] Wackernagel et al. (2002) is a similar analysis at the global level. As with studies at the national level, no attempt is made to explicitly model the relationship between economic growth and Footprint.

separately. Those results which are reported are described below, arranged by country and study.

Lammers et al. (2008) “examines how Ireland’s economic growth has affected environmental pressure by calculating a time series of Ireland’s Ecological Footprint”. They plot indices of GDP per capita and Ecological Footprint from 1983 to 2001 for Ireland but strangely, given their objective, do not explicitly model one as a function of the other. Over this period, income increased by a factor of 6 and Ecological Footprint by a factor of 1.5. These results imply a steady reduction in the Ecological Footprint intensity of the economy, though the authors conclude only that “the growing economy has intensified human’s pressure on the Irish environment”. Ecological Footprint per capita increased from roughly 4 gha/capita to 6 gha per capita over the period, but information on absolute levels of GDP per capita are not given. Assuming a GDP per capita of US$30,000 in 2001 this would imply an intensity of 5000 US$/gha in 2001 compared with 1875 US$/gha in 1983. The energy Footprint was responsible for most of the rise in total Ecological Footprint. Interestingly, although the period 1995-2001 had the fastest growth in per capita income (>9% per year) the growth rate in per capita energy Footprint was slower for this period than for the period 1993-1995 which, the authors report, had lower, though still strong growth, implying that at least partial decoupling has taken place in this component.

For China, Chen and Chen (2006, 2007) provide time series of Ecological Footprint from 1981-2001. They plot the ratio of GDP to Ecological Footprint over the period, and this appears to rise fairly steadily, but, as with Lammers et al. (2008) no explicit analysis is made of the relationship. M. Chen et al. (2006) report very similar trends. D. Chen et al. (2004) is the only time-series study to explicitly analyse Ecological Footprint as a function of income. However, like Hammond (2006) they fit only a power function to data from 1981 to 2000, and find an elasticity of less than unity, implying some decoupling of Ecological Footprint from income. Their results are restricted by the choice of model, and I demonstrate in the next section that the data are consistent with more optimistic conclusions, and provide some evidence for an EKC.

Hanley et al. (1999) calculate a time series of Ecological Footprint for Scotland, but do not analyse it as a function of income, since this is not the purpose of their

study. However, they do report that Ecological Footprint per capita and population stayed roughly constant over the period considered, while GDP per capita rose. This indicates reduced Ecological Footprint intensity of the economy, and therefore at least partial decoupling.

van Vuuren and Smeets (2000) tabulate changes in population, GDP per capita, carbon intensity of GDP and energy Footprint, for four countries and three years (1980, 1987, 1994). Based on these patchy data, the relationship between GDP per capita and Footprint appears indeterminate. They also report an analysis of the effect of using local rather than global average yields when converting consumption impacts to Footprint. Their Figure 5 shows that using local yields makes the per capita Ecological Footprints of the four countries appear much more equal, while using global average yields tends to increase the Ecological Footprint of higher income countries relative to poorer ones. This demonstrates one way in which investments in richer countries underwrite the Ecological Footprint of poorer countries (when expressed in gha), by raising global average yields. In their Figure 6 they reproduce a cross-sectional regression using data from Wackernagel et al. (1997) showing a convex relationship between GDP per capita and energy Footprint per capita, while for land use Footprint it is concave, demonstrating opposing trends in natural capital depletion and entropy increase. Similarly, Haberl et al. (2001) and Erb (2004) report interesting analyses for Austria. Although they do not explicitly investigate the effect of economic growth, they demonstrate the effect of the choice of yield factors on Footprint, in a similar manner to that of van Vuuren and Smeets (2000).

All national level time series appear to show at least partial decoupling of per capita Footprint from income. However, despite many aiming to investigate the effect of economic growth on Footprint, none except Chen et al. (2004) explicitly model the relationship, and then only with functions which exclude the possibility of finding decoupling. I show in the next section that in this case at least, the data are again consistent with more optimistic conclusions.

Panel studies

Only one study (Jorgenson & Burns 2007) carries out a panel analysis of Footprint and income, concluding that economic growth drives Ecological

Footprint. However, they regress the change in Footprint per capita (1991-2001, not transformed) on GDP per capita (GDP_{pc}) in 1991 (logged), without any information on the change in GDP per capita 1991-2001. Therefore, they do not provide any information on the effect of marginal changes in income on the Ecological Footprint.

Global time-series studies

As well as their cross-sectional analysis, Bagliani et al. (2007) carry out the only global time-series analysis of Ecological Footprint and income. They find a concave relationship, but report that the turning point lies beyond the range of the data. However, for reasons they do not explain, they use aggregate global Ecological Footprint and Gross World Product when testing for a Kuznet's curve at the world level, without dividing either measure by population, thus confounding the influence of income and population growth over the period.[129] In the next section I demonstrate some other flaws in this analysis, and in the next chapter, show that the use of aggregate rather than per capita data masks the presence of decoupling.[130]

Results

In all, 11 studies reported time series for a single country (or in two cases 3-4 countries) covering 11 countries in total. Eight studies reported cross-sectional analyses of a large sample (>100) of countries in a single year (of these, six tested for the existence of an EKC). Only one study reported a panel analysis (Jorgenson & Burns 2007) and only one study reported a time-series analysis of the global Ecological Footprint (Bagliani et al. 2007)[131] – both of these tested for an EKC.

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[129] As I show in the next chapter, this masks a clear EKC in the data.

[130] This working paper has recently been published in Ecological Economics (Bagliani et al. in press). Interestingly, the authors have dropped the global time-series analysis (which is consistent with a relatively optimistic view of decoupling) and retained only the cross-sectional analysis.

[131] One other study (Wackernagel et al. 2002) provided a time series of global Footprint, but did not make any analysis of the relationship between Ecological Footprint and income, while van Vuuren & Bouwman (2005) carried out an analysis of scenarios for multiple world regions. Bagliani et al. (2007) provided both cross-sectional and global time-series analyses and this is therefore counted amongst the 10 cross-sectional studies and one global time-series study in my review.

Research effort has therefore been directed towards the lower end of the hierarchy I proposed at the end of Section III above (Table 4.1). As expected, cross-sectional analyses have on balance reached more pessimistic conclusions about the shape of the relationship between Footprint and income, and therefore about decoupling, than have higher ranked studies. Unlike cross-sectional studies, all but one of those reporting time-series at the national level did not explicitly analyse the relationship, despite being better placed to do so, and despite in several cases including this among their objectives.

In the next section I show that in addition to this tendency to use analytical frameworks which are inherently pessimistic about decoupling, I find that most of the studies contain further flaws which lead to results which are more pessimistic than are supported by the data.

Table 4.1. Classification (see Section III) of Footprint-income analyses.

Rank	**Study Type**	**Shape of Relationship Found**	**Number of Studies Explicitly Investigating:**	
			Economic Development	**EKC**
Highest	Detailed Panel	N/A	0	0
≈2nd	Global Time-series	Concave	0-1[1]	0-1[1]
≈2nd	Panel	N/A	0-1[2]	0-1[2]
4th	National Time-series	5 Concave 1 indeterminate	6[3]	0
Lowest	Cross-sectional	2 Concave 5 Convex	8[4]	5

[1] One study, Bagliani et al. (2007) does carry out an analysis at the global level, but I argue below that it is flawed. See Chapter 5 for a global time-series analysis.

[2] One study (Jorgenson & Burns 2007) does attempt a panel analysis, but I argue below that it is flawed. See Section V for a panel study.

3 This was rarely the primary objective of the paper.

4 Includes York et al. (2003b) which analyses drivers of only the energy part of Footprint. In addition, Jorgenson (2003) uses 'World Systems Position' and Jorgenson (2004) uses urbanization, rather than GDP per capita. However, the two measures appear to be strongly correlated with GDP per capita.

What has caused the bias towards low-ranked analysis types? Table 4.2 shows that the bias is partly explained by the way in which Footprint data has been made publicly available. With the exception of studies reporting national time-series, all the analyses have made use of publicly available data, and the data allowing cross sectional studies was published earlier (2000) than data which allowed simple panel analyses (2004) or global time-series analyses (2002). However, the table

also shows that author choice has played a role. Of the seven studies exclusively reporting cross-sectional analyses, five could have carried out global time-series analysis and two of these could also have carried out simple panel analyses, which have not yet been carried out for marginal changes. Without these deliberate choices on the part of authors, the literature would be much less biased towards relatively pessimistic specifications. No authors explicitly justify their use of cross-sectional data rather than other formulations.[132]

Table 4.2. Classification, and ranking (see Section III) of types of analyses of Footprint and income.

Rank	Study Type	Year Data Available	Number of Studies using approach	Number of studies which could have used this approach (and none better)
Highest	Detailed Panel	N/A	0	N/A
≈2nd	Global Time-series	2002	0-1	5
≈2nd	Simple Panel	2004	0-1	4
Lowest	Cross-sectional	2000	7	2

Discussion and conclusions

To date, the literature on the Footprint-income relationship has tended to use analytical specifications which are inherently biased towards relatively pessimistic conclusions about the existence and potential of decoupling. This bias stems first from the way data has been published, and second from the choices made by authors, given the available data. Although several studies acknowledge the problems with a cross-sectional approach (e.g. York et al. 2003a, Rosa et al. 2004, Dietz et al. 2007, Bagliani et al. 2007), they still draw confident conclusions that are not supported by the data. The best example of this being Rosa et al. (2004) who claim to have “embarrassed” the EKC hypothesis, and Dietz et al. (2007) who use their results to make an overly pessimistic projection of Ecological Footprint (see Section VI). In the next section, and the next chapter, I show that,

[132] Note that while Bagliani et al. (2007) report a global time-series analysis as well as cross-sectional analyses, and although they acknowledge the limitations of the cross-sectional approach, they omitted the time-series analysis when republishing their working paper (Bagliani et al. in press).

even given the analytical framework chosen by the authors, a series of flaws has led them to conclusions which are more pessimistic than those supported by the data. As a result, I develop guidelines for more robust analyses of the Footprint-income relationship, which are also applicable to any analyses of the decoupling of economic from environmental scale.

VI. Methodological issues in Footprint-Income analyses

In this section I discuss flaws in previous Footprint-income analyses which led studies to overly pessimistic conclusions of economic growth.

Testing for an EKC using skewed or log-transformed cross-sectional data (Dietz et al. 2007)

In addition to ignoring technological progress, cross-sectional Footprint-income analyses pose other challenges and ambiguities. For example, Bagliani et al.'s (2007) finding of a negative quadratic term contrasts with other cross-sectional studies which found a positive term (York et al. 2003a,b 2005, Rosa et al. 2004, Dietz et al. 2007). There are two differences between the approaches used, which may explain the conflicting results, and I investigate these by re-analysing the data from Dietz et al. (2007). The first difference is that Bagliani et al. (2007) regress Ecological Footprint per capita, on GDP per capita, while the others regress total national Ecological Footprint on population and GDP per capita, a formulation implied by their "IPAT" framework, which sees impact (*I*) driven by population (*P*), affluence (Y: GDP per capita) and technology (*T*; Equation 4.2).

Equation 4.2 $$I = a \cdot P^b \cdot Y^c \cdot T$$

where technology is the residual. They use this formulation, despite finding impact elasticities of population which are not significantly different from unity, implying that a per capita specification would be possible (see below).

The second difference, which also stems from the use of IPAT, is that Dietz and colleagues transform all variables using natural logs, to allow them to use the

IPAT formulation in a linear regression.[133] However, in order to test the EKC, a quadratic of the logged affluence term is used,[134] either:

Equation 4.3 $$[\ln(Y) - mean(Y)]^2$$

(York et al. 2003a,b, Rosa et al. 2004).

or:

Equation 4.2 $$[\ln(Y)]^2$$

(Dietz et al. 2007).

All four papers interpret a positive coefficient on this term as implying the opposite of an EKC. However, I demonstrate below that the positive coefficient on this term does not strictly imply a concave form in the untransformed data.

Methods

I fitted a simplified version of Dietz et al's (2007) model to their dataset of 135 countries:[135]

Equation 4.5 $$\ln(EF\ \ per\ capita) = \ln(A) + [\ln(A)]^2$$

Since all studies have found the coefficient on population not to be significantly different from unity, I eliminate population, and use Ecological Footprint per capita as the response variable, allowing the results to be represented in two dimensions. The model was fitted using the lm function in R (R Core Development Team 2007). I then plot this model, estimated from the transformed data, on untransformed axes.

Results

The coefficient on the quadratic term is positive, as in the original studies (Figure 4.2a). However, when the model is plotted on untransformed axes it is clear that it

[133] They also justify this on the grounds that the Ecological Footprint and GDP per capita data show excessive skew (non-homogeneity of variance), and this does appear to be the case (Figure 4.2b).

[134] Squaring affluence prior to logging would be simply twice ln(A), and perfectly co-correlated.

[135] Dietz et al's model included latitude dummies and the ln(land area). This simplified formulation allows the model to be visualized in two dimensions.

is almost completely linear (Figure 4.2b). Closer scrutiny reveals that the model is actually slightly concave to the origin (Figure 4.2c).

One reason for using log-transformed variables is that models fitted to the untransformed data show non-homogeneity of variance, meaning that too much weight is placed on higher income data points. However, if the aim is to investigate the effect of income growth on Footprint, it is this less well populated part of the data which is of greatest interest.

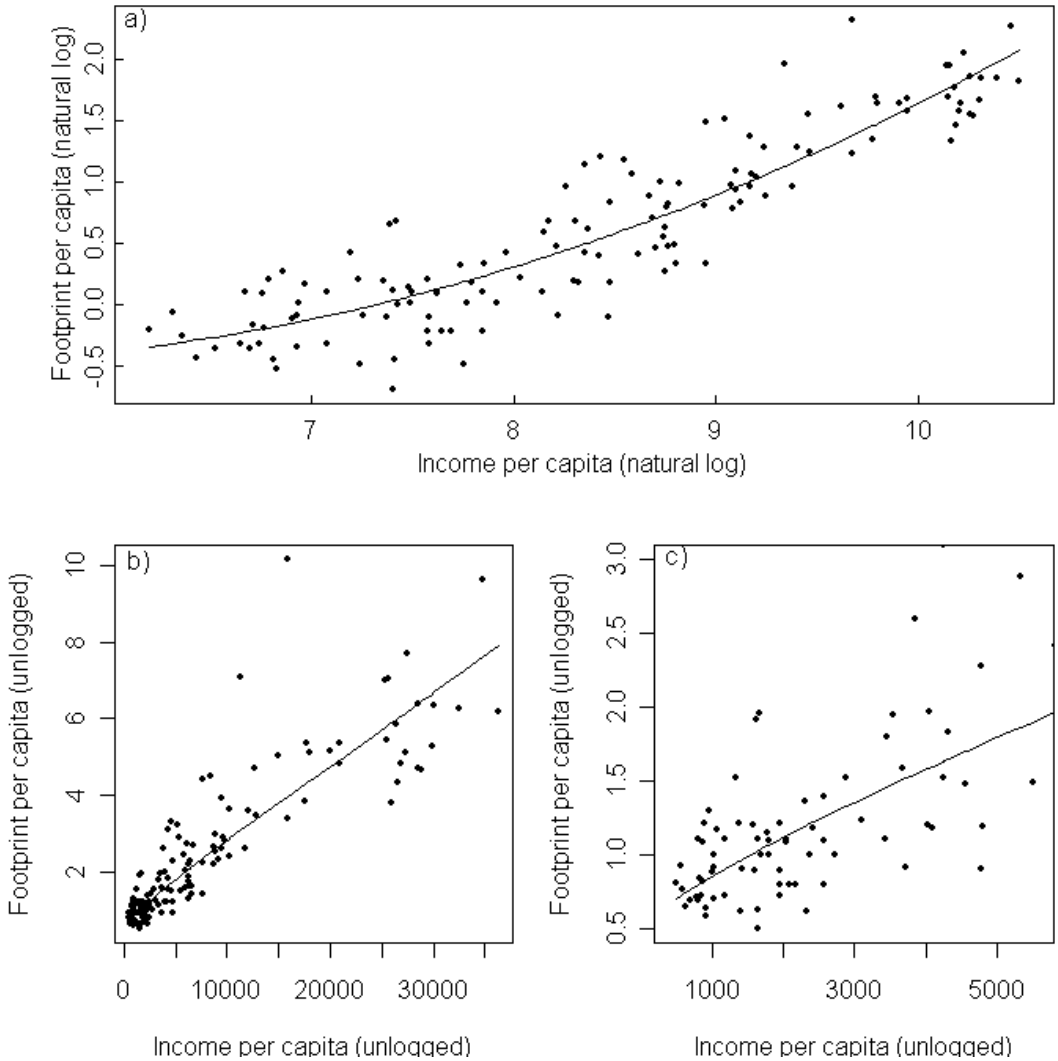

Figure 4.2: The misinterpretation of quadratic logged models based on IPAT framework. a) the logged data, together with the fitted line obtained using the simplified version of Dietz et al.'s (2007) model. b) the simplified Dietz et al's (2007) model on an un-logged scale. The positive quadratic shape disappears – in fact the trend is slightly concave. c) a close up of Figure 4.2b, near the origin showing the slightly concave nature of the model fit.

Discussion

It is clear that the positive quadratic found by Dietz et al. (2007) in the log-transformed data does not imply a convex relationship in the untransformed data. In fact, it is concave (albeit only slightly). Contrary to claims in York et al. (2003b) it is not at all clear that using the IPAT formula with a quadratic term is an appropriate method of investigating the relationship between income and Footprint. Although the model with the quadratic does outperform the same model without the quadratic, the authors do not provide any a priori justification for believing this to be an appropriate representation. Instead, they justify the inclusion of the quadratic term specifically in order to test for the existence of an EKC. Yet the quadratic term is somewhat unnecessary for this purpose, since any exponent of less than one on the affluence term implies a concave (albeit strictly increasing) relationship in the unlogged variables, which is as much as is implied by a negative sign on a quadratic term in untransformed data.

This analysis suggests that the authors' conclusions of convex relationships between economic and environmental scale (the opposite of an EKC) are unsafe. According to their own model, it appears to be slightly concave when plotted on untransformed axes. The analysis also illustrates the problem that in cross-sectional studies, the results are dominated by low-income countries in a skewed data set, yet the interest is generally in what will happen if incomes increase above this very low level.

Aggregate data (Bagliani et al. 2007, Dietz et al. 2007)

I noted above that York et al. (2003a,b, 2005), Rosa et al. (2004) and Dietz et al. (2007) used aggregate rather than per capita Ecological Footprint as the dependent variable, including population as an independent variable. They do this despite finding repeatedly that the impact elasticity of population is very close to unity. At best this clouds interpretation of the relationship between income and per capita Footprint, particularly given that, longitudinally, population is related to income per capita, higher levels of the latter tending to be associated with lower or zero growth in the former, implying that there may be a "population Kuznets curve". It would be better to use per capita values, and include population density as an independent variable (as Hammond 2006 does).

More seriously, Bagliani et al. (2007) also use aggregate Ecological Footprint when analysing a global time-series (despite using per capita Footprint in their cross-sectional analysis), and use aggregate, rather than per capita, Gross World Product. This is problematic since, even if a population Kuznets curve exists, population growth over the study period may obscure the decoupling of Footprint from Income, if the population Kuznets curve peaks at a higher income levels. Indeed, I show in the next chapter, that this does seem to have been the case.

Marginal changes and panel analysis (Jorgenson & Burns 2007)

Jorgenson and Burns (2007) state that their objective is to: "[test] a series of hypotheses concerning the political-economic causes of change in per capita consumption-based environmental impacts". One of the primary causes of change which they are interested in is "level of economic development", for which they use GDP per capita as a proxy.

However, their analysis actually investigates the relationship between absolute changes in Ecological Footprint per capita (EF_{pc}) over the period 1991-2001 and percentage differences in initial income level. In the simplest model without GDP per capita, they find:

Equation 4.6 $$EF_{2001} = 0.830 \cdot EF_{1991}$$

where EF is Ecological Footprint per capita. Thus, on average, per capita Footprints were 17% lower in 2001 than in 1991, despite the world economy having grown over that period. The authors make no mention of technological progress, or the significance of this term. Adding GDP per capita (in 1991) to their model, they find:

Equation 4.7 $$EF_{2001} = 0.417 \cdot EF_{1991} + 0.517 \cdot \ln[Y_{1991}]$$

where Y is GDP per capita. The interpretation of this is extremely difficult. It is equivalent to:

Equation 4.8 $$\Delta EF_{91-01} = -0.583 \cdot EF_{1991} + 0.517 \cdot \ln[Y_{1991}]$$

i.e. absolute change in per capita Footprint is regressed on the logarithm of initial income level. What is missing is any measure of change in income over the period. Thus, the analysis mixes absolute increments on the left-hand side and

percentage increments on the right-hand side, and also compares changes over time on the left-hand side with initial levels on the right-hand side.

When controlled for initial income, the larger a country's per capita Footprint in 1991, the smaller the absolute increase in per capita Footprint over the following 10 years, implying that Footprints were levelling off. On the other hand, there is a positive relationship between the logarithm of per capita income in 1991, and the absolute change in Footprint over the period. Thus, richer countries (which would tend to have larger per capita Footprints) experienced larger absolute increases in Footprint. However, without any measure of economic growth over the period, it is impossible to come to any conclusions about the effect of economic growth on Ecological Footprint. It is true that Jorgenson and Burns (2007) state that they wish to evaluate income level as a driver of Footprint change, but unless they believe that Footprints are completely decoupled from economic growth at the margin (which apparently they do not) or they believe that economic growth is unrelated to initial income (which it is not, see Chapter 3) this analysis does not address the question they pose. Jorgenson and Burns (2007) claim that their results show that continued economic growth is incompatible with stable or declining environmental impact, yet it is difficult to see how their unusual specification can be used to determine this.

As I noted in Section II, a simple panel analysis (like that in the next section) cannot be used to determine the effects of income growth per se, since the source of technological development is not accounted for. However, for the results to be in any way informative, measures of initial levels and changes over time must be available for both dependent and independent variables (as in Section VII, below) Otherwise, the analysis cannot properly be considered a panel study.

The importance of considering alternative models: time-series evidence for an EKC in China (Chen et al. 2004)

Chen et al. (2004) is the only time-series study to provide a regression of Ecological Footprint per capita on GDP per capita. However, the authors fit a power function (which is strictly increasing), and do not consider alternative functions, despite their power function showing a relative poor fit to the data (D. Chen et al. 2004, Figure 2). Thus, they are constrained to find only a partial

decoupling since no model of complete decoupling is tested. Similarly, Hammond (2006) uses only a power function when analysing cross-sectional data. I tested for the existence of an EKC in D. Chen et al's (2004) data, by comparing a strictly increasing, an asymptotic and a humped function.

Methods

I re-analysed the data from D. Chen et al. (2004). I fitted a humped function (a biexponential), an asymptotic function (asymptotic exponential) and an exponential quadratic function as well as the original authors' strictly increasing power function:

Power (strictly increasing):

Equation 4.9 $$F = a \cdot Y^{b}$$

3 Parameter Asymptotic Exponential:

Equation 4.10 $$F = a - b \cdot e(-c \cdot Y)$$

Exponential Quadratic:

Equation 4.11 $$F = \exp(a + bY + cY^{2})$$

Bixponential (strictly humped):

Equation 4.12 $$F = a \cdot e^{bY} + c \cdot e^{dY}$$

All models were fitted using the nls function in R (R Core Development Team 2007) and compared on the basis of AIC scores (Burnham & Anderson 2002).

Results and Discussion

Based on the AIC score, both the asymptotic and the quadratic exponential outperformed the authors' original function, while the strictly humped biexponential function did not converge (Table 4.3). Figure 4.3 shows that in this case, the quadratic exponential function is functionally asymptotic, since the maximum lies near the upper limit of the data. The data are therefore consistent with full decoupling of economic and environmental scales at the margin in the world's most populous and fastest growing economy. However, this receives little attention from the authors, since their restricted choice of function permits them

only to find an increase in the environmental efficiency of the economy, rather than a potential decoupling.

Table 4.3. AIC comparison of alternative models.

Type	Function	Parameters	AIC	ΔAIC over power function
Strictly Increasing	Power	2	-62.9	n/a
Asymptotic	3 Param. Asymptotic	3	-67.3	4.4
	Quadratic exponential	3	-74.1	21.2
Humped	Biexponential	4	Did not converge	

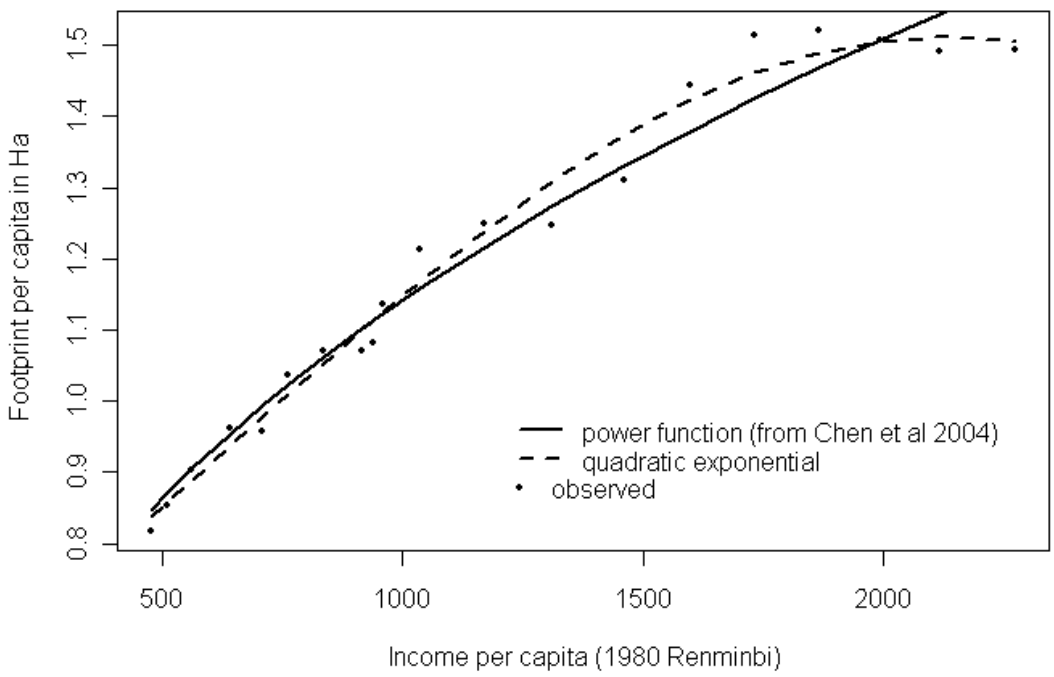

Figure 4.3. Ecological Footprint per capita vs income per capita for China (1981-2000). Showing Chen et al's (2004) power function (solid line) and a humped function (quadratic exponential) which outperforms it, strongly implying a turning point or asymptote within the range of the data. Redrawn using data from Table 1 in Chen et al. (2004).

Bagliani et al. (2007): Model comparison using AIC

Bagliani et al. (2007) use the Akaike Information Criterion (AIC) to compare competing models, including a strictly increasing concave power function and a quadratic function which is humped within the range of the data. The convention is that models with ΔAIC <2 are considered to receive considerable support from

the data (Burnham and Anderson 2002).[136] Yet the ΔAIC of the power function over the quadratic function in Bagliani et al. (2007) is only 0.03, implying that the two models receive virtually equal support from the data.[137] In addition, Bagliani et al. (2007) do not estimate all of the power function's coefficients using the regression analysis, but specify a fixed exponent, the value of which is separately optimised on the basis of the R^2. Since this parameter is not estimated in the regression on which the AIC is calculated, the power function is inadequately penalised, by 2 AIC points.[138] If the correct penalty were applied, the quadratic function would now outperform the power function by 1.97 AIC points, and the model averaged turning point would lie within the range of the data. These errors of interpretation lead Bagliani et al. (2007) to unnecessarily pessimistic conclusions. They repeat the mistake when analysing time-series at the global level, leading them to erroneously conclude that the power function, rather than the (negative) quadratic function is best supported by the data. Although they correctly note that the turning point of the quadratic lies outside the range of the data, it does not lie far beyond, and the model averaged turning point (with recalculated AIC scores) would be similarly close. Thus, even when using aggregate, rather than per capita figures, the evidence against the EKC is weaker than their conclusions would suggest.

In addition, they note that the turning point implied by the quadratic model is high relative to the range of the data. Yet, because of the flaws noted above, cross-sectional models will tend to provide turning point estimates which are biased upwards: the authors do not acknowledge this. Certainly, it is vitally important that the position of a quadratic's turning point is calculated, and compared to the range of the data, since a quadratic whose turning point lies at or beyond the upper limit of the data is functionally indistinguishable from asymptotic or strictly increasing functions respectively. However, in effect, cross-sectional analyses

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[136] AIC scores are on a log scale, and measure the relative distance of a model from the "true" model (Burnham and Anderson 2002). Thus, in if two models are compared, a ΔAIC of 2 implies an evidence ratio of approximately 3:1, and Akaike weights of 73% and 27% (Burnham & Anderson pp 75-77).

[137] This implies an evidence ratio of 50.3:49.6.

[138] The AIC score incorporates a penalty of two points for each parameter estimated in the model to ensure parsimony.

only provide an estimate of the upper limit of the population turning point, since they ignore technological overspill. The position of a quadratic turning point in cross-sectional data cannot therefore be used to reject the EKC hypothesis.

Conclusions

In addition to a general bias towards analytical frameworks at the lower end of the hierarchy outline in section II, many studies contain flaws in their analysis or interpretation, which further bias their results towards more pessimistic conclusions about the existence of and potential for decoupling Ecological Footprint from Income.

In summary, I suggest the following principles for analyses of this issue, in addition to those outlined in section II:

1. Per capita data should be used wherever possible, to separate the effects of income and population growth on Footprint, and if necessary to separately investigate the relationship between population and income.

2. In studies which use time series (including panel studies) marginal changes, rather than initial levels, should be used (c.f. Jorgenson & Burns 2007).[139]

3. Caution should be exercised whenever transformed variables are used (c.f. Dietz et al. 2007, and Jorgenson & Burns 2007). Ideally, samples which are skewed towards the lower range of the data (which represents “the past” when viewed longitudinally) should be avoided altogether. Log-transforming variables does not change the fact that one still has only sparse data at higher income levels, despite this being the zone of greatest interest. If logged variables are used, the functional form of any model in the untransformed data should be examined.

4. If a quadratic approach is used, it important not to rely only on the sign of quadratic term, but also to locate the turning point (as Bagliani et al. 2007 do) together with confidence intervals. However, if cross-sectional analyses are used, one should be aware that turning points may be biased upwards, and therefore the position of a turning point itself cannot be used as justification for rejecting the EKC hypothesis.

[139] Parallels can also be drawn with the growth-initial regressions criticised in Chapter 3).

5. In addition to using quadratic models, it is desirable to consider several alternative models, each representing an a priori sensible hypothesis, e.g. strictly increasing, asymptotic and humped-shaped models. If concave, the first represents increasing Footprint efficiency of an economy, but only incomplete decoupling. Asymptotic models represent full decoupling at the margin (further increases in income result in no further increases in Footprint), while humped models imply intra-marginal decoupling (an EKC) – i.e. that increase in income actually reduce Footprint. Alternative models can be compared on the basis of AIC scores.

VII. A quantitative measure of cross-sectional technology pessimism

The problem with cross-sectional analyses of environmental and economic scale is that technological innovation is ignored, leading to overly pessimistic inferences about decoupling. In this section, I demonstrate the extent of upward bias in projections of future Ecological Footprint based on cross-sectional analyses by reanalysing a recent cross-sectional study by Dietz et al. (2007).

Introduction

Using cross-sectional data, Dietz et al. (2007) investigated the factors driving the Ecological Footprint of nations, and found that population, affluence, land area and latitude were the most important. Using this model, they predicted that, given continued economic and demographic growth, humanity's Ecological Footprint would continue to expand, exceeding the biological capacity of the world by 60% in 2015.

However, as Dietz et al. acknowledged, their analysis ignored technological progress. This omission is likely to lead to overestimated projections of global Footprint. In order to determine the extent of this over-prediction, I modify Dietz et al's (2007) analysis to allow a panel approach, which estimates the rate of technological progress over time (where technological progress is broadly defined as the average reduction in the Footprint intensity of economies over time[140]). I

[140] This includes preferences, policies, institutions, and human capital in addition to 'physical' technologies.

then extrapolate this rate forward to correct Dietz et al's (2007) projection of global Footprint in 2015.

Methods

Dietz et al. used data on Ecological Footprint and population from the WWF Living Planet Report 2004 (WWF 2004), and combined this with data on land area and GDP per capita from the World Bank (World Bank 2004). They also classified countries as arctic, temperate or tropical semi-subjectively depending on the location of major centres of population (T. Dietz, pers. com.) Data were available for 135 countries in all,[141] and all data referred to 2001. Using Ordinary Least Squares (OLS) regression, they estimated the following model:

Equation 4.13

$$\log_{10}(impact) = 0.562 + 0.931 \cdot \log_{10}(pop) - 0.661 \cdot \log_{10}(Y) + \\ 0.161 \cdot [\log_{10}(Y)]^2 + 0.0552 \cdot \log_{10}(Area) + 0.085 \cdot (Temperate) + 0.181 \cdot (Arctic)$$

where *impact* is aggregate Ecological Footprint and *Y* is GDP per capita. In order to repeat Dietz et al's work as a panel analysis, I exploited the fact that WWF (2004) provides an estimate of the percentage change in Ecological Footprint per capita from 1991 to 2001. Using this, I calculated Ecological Footprint per capita in 1991, for Dietz et al's (2007) 135 countries. Since WWF (2004) does not give population estimates for this year, and World Bank (2004) was no longer available, I calculated population and economic growth rates 1991-2001 from World Bank (2007) to estimate population size and GDP per capita in 1991. Latitude and land area were, of course, unchanged. Because of poorer data availability in 1991, 130 countries remained in the sample (see Hockley et al. 2008 for the list of countries).

Using this multi-year dataset, I compared Dietz et al's (2007) model (equation 4.13), with a modified version (equation 4.14 below). This model differs in

[141] In fact, data was available for 137 countries, with only the latitude classification missing for Swaziland and Serbia & Montenegro, unambiguously tropical and temperate respectively. However, to ensure maximum comparability, these two countries were also omitted from the analysis described here.

including a term t*(year-2001) representing the annual change in log(Impact) due to technological progress from 1991 to 2001.

Equation 4.14

$$\log_{10}(impact) = 0.562 + 0.931 \cdot \log_{10}(pop) - 0.661 \cdot [\log_{10}(Y)]^2 + 0.052 \cdot \log_{10}(Area) + 0.085 \cdot (Temperate) + 0.081 \cdot (Arctic) + s + t \cdot (year_{2001})$$

Note that I constrained the original coefficients to equal those in Dietz et al's (2007) model. Therefore my analysis estimates the degree of under-prediction in 1991 which occurs as a result of using Dietz et al's (2007) model, which assumes a 2001 level of technology. This is the amount by which actual $Log_{10}(Impact)$ in 1991 exceeds that predicted by Dietz et al. This implies that their model will over-estimate Ecological Footprint in 2015 if technological progress continues.

To avoid bias in estimating *t* I include an intercept term *s* in equation 4.14. The two models were fitted by OLS regression using the lm function in R (R Core Development Team 2007) and compared on the basis of AIC scores. If technological progress (*t*) has reduced Footprint over time, the annual change term should improve the model and *t* should be negative. This work is published in Hockley et al. (2008).

Results

As expected, the model including technological progress (4.14) received most support from the data (ΔAIC=8.9 compared with equation 4.13, R^2=95%), and the coefficient *t* was negative at -6.18×10^{-3} (Table 4.4), indicating that technology did improve over the period 1991-2001.

Table 4.4. Results from the analyses with constrained coefficients.

	Estimate	**Std. error**
Intercept, s	-1.06×10^{-2}	(1.32×10^{-2})
Technological progress (year-2001), t	-6.18×10^{-3}	(1.86×10^{-3})
n	130	
ΔAIC	8.9	
R^2	95%	
Annual change in impact	1.4%	

This estimate of *t* implies an annual decrease in the Footprint of a country (holding other factors constant) of around 1.4%. Adjusting Dietz et al.'s (2007)

projection accordingly means that the global Footprint in 2015 would be around 20% lower than they predicted, but would still exceed the biocapacity of the earth by around 27% (Figure 4.4). I also repeated the analysis using unconstrained coefficients, and obtain similar results (Table 4.5).

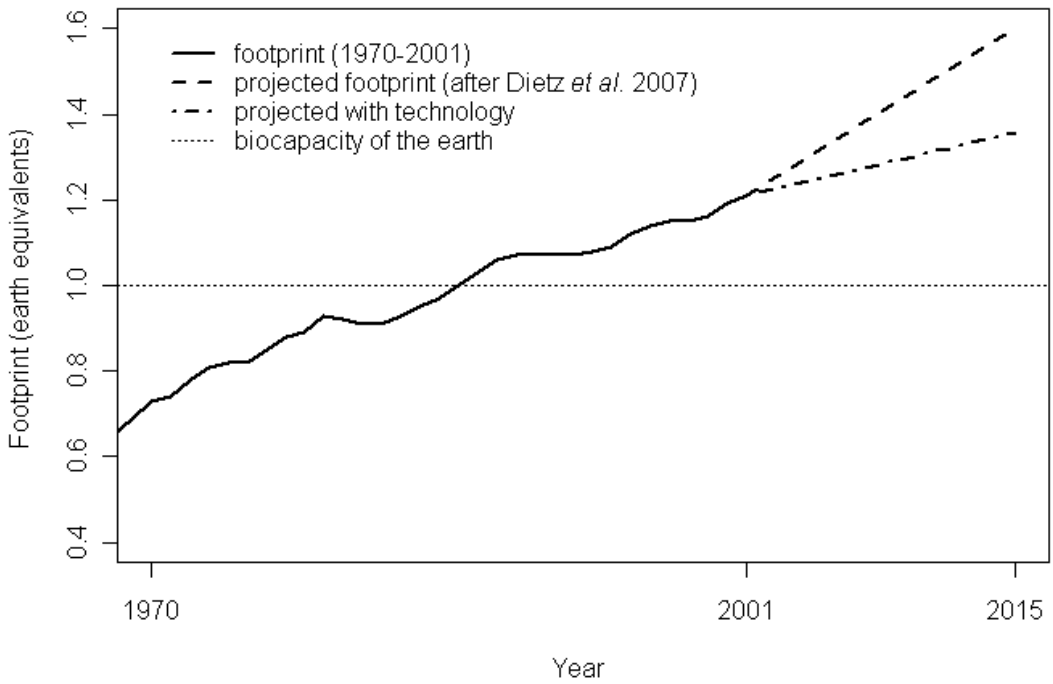

Figure 4.4. The effect of technological progress on the projection of global Ecological Footprint to 2015 (see Hockley et al. 2008).

Table 4.5. Results from the analyses with unconstrained coefficients.

Parameter	Estimate	Std. error.
(Year-2001)	-5.72×10^{-3}**	(1.80×10^{-3})
Log_{10}(Population)	0.93***	(0.02)
Log_{10}(Affluence)	-0.63*	(0.29)
$[Log_{10}(Affluence)]^2$	0.14***	(0.04)
Log_{10}(Area)	0.05**	(0.02)
Temperate	0.21***	(0.02)
Arctic	0.30***	(0.04)
Intercept	0.65	(0.53)
*p<0.05; **p<0.01; ***p<0.001;		
n	130	
ΔAIC	8.2	
R^2	95%	
Annual change in impact	1.3%	

Discussion and conclusions

Dietz et al's reliance on a cross-sectional analysis (despite data being available for a panel study) led them to over-estimate the likely global Ecological Footprint in 2015 by around 20%. While their projection suggests an acceleration of growth in global aggregate Ecological Footprint, my projection, corrected for technological progress, is consistent with it beginning to decelerate.

I modeled technological progress as a function of time for simplicity, and because I only had data from two years (1991 and 2001): the data could not support a more complex specification. However, this is an unrealistic representation of technological progress (see Section II), which is unlikely to proceed at a constant rate independent of other factors. If these other factors were included (higher incomes and human capital, increased urgency) this would likely reduce the projection still further.

VIII. Discussion and Conclusions

I have argued that technological progress could allow continued per capita economic growth, despite binding constraints on the environmental scale of the human economy. In addition, that technological progress cannot be seen as independent from economic growth: rather that a positive feedback loop exists between the two.

Using a systematic review of the literature, I have identified significant bias in published analyses of the relationship between economic and environmental scale. The primary source of this bias is the choice of analytical frameworks which ignore technological progress and its links to economic growth, though numerous other flaws also lead authors to draw conclusions about the potential for decoupling which are more pessimistic than the data allow.

The systematic nature of the bias present in the literature suggests that there has been a tendency to accept without question studies which confirm the conventional ecological economic wisdom: that decoupling economic scale from environmental scale is impossible. This bias is surprising since, as I noted in Section I, the existence of decoupling or an EKC does not imply that economic growth is unbounded by environmental constraints, nor does it imply that policy

action is not required to ensure sustainability: indeed, decoupling is likely to require policies which recognise the environmental constraints faced by human society. Although ecological economists accept this last point, they appear blind to evidence that mankind is already making progress in this direction.

5. An Environmental Kuznets Curve for Global Ecological Footprint

Abstract

I present the first test of the environmental Kuznets curve hypothesis (an inverse U-shaped relationship between environmental scale and income) using time-series data on global Ecological Footprint from 1961 to 2001. This approach captures technological effects of income growth, unlike previous cross-sectional studies. In contrast to these studies, I find evidence for the existence of an environmental Kuznets curve. I also investigate trends in the principal Footprint subcomponents, finding significant decoupling of economic growth from the CO_2 *Footprint, and a steady decline in per capita non-energy Footprint. The results suggest that, given appropriate policies, continued growth in per capita income might be compatible with declining per capita environmental impact.*

I Introduction

Given the undeniable constraints imposed by a finite environment, our quality of life can only continue to improve if the economic and environmental scales of the human economy are substantially decoupled. This decoupling is often represented as an environmental Kuznets curve (EKC), which posits an inverse U-shaped relationship between environmental scale and income (Grossman & Krueger 1995, Dasgupta et al. 2002). Investigating the existence of, or potential for, an EKC is therefore one of the most important issues in sustainability science.

Several authors have proposed a theoretical basis for the EKC, and many empirical studies have provided evidence for EKCs (see the review by Dasgupta et al. 2002). However, other reviews have taken the opposite view, concluding that the evidence for the EKC hypothesis is ambiguous or flawed (e.g. Stern 2004, Nahman & Antrobus 2005). The literature on the drivers of the Ecological Footprint, which is the most comprehensive and influential indicator of environmental impact (Wackernagel & Rees 1995), is almost universally pessimistic about the prospects for decoupling (see Chapter 4), with some studies even proposing a convex relationship between Footprint and income (e.g. Dietz et al. 2007).

In Chapter 4, I showed that a great deal of the pessimism in the Footprint-income literature results from flawed analyses, the most important flaw being the assumption that technology is unrelated to income. Similarly, much pessimism in the EKC literature comes from separating the scale and technology effects of income.[142] For example, Stern (2004) seems to regard the observation that developing countries are adopting environment regulations at lower income levels than did richer countries as evidence against the EKC hypothesis. The discussion in the previous chapter makes clear that, in fact, this is more properly viewed as evidence that the analysis is looking at the wrong scale. Given the existence of technological overspill, and the limited publicly available data, the only proper

[142] It seems rather asymmetrical that critics of the EKC are anxious to control for off-shoring of production and its environmental impacts to poor countries, but ignore the positive externalities from technological innovation in rich countries.

scale at which to evaluate an EKC in Footprint is global, using a time-series analysis, rather than cross-sectional (e.g. Dietz et al. 2007, Bagliani et al. 2008). In this chapter I use data on global per capita income and ecological Footprint from 1961 to 2001 to test for the existence of an EKC, as well as exploring trends in its major subcomponents. To the best of my knowledge, the analysis I present here is the first such test of an EKC in Ecological Footprint.

II. Methods

Ecological Footprint data came from Table 3 of the 2004 Living Planet Report (WWF 2004:32). This provides estimates of the global aggregate Ecological Footprint for each year, 1961-2001, in current gha (n=41). Cross-sectional data from successive editions of this report have been used in most previous studies of Footprint (Bagliani et al. 2008, Dietz et al. 2007, Rosa et al. 2004, York et al. 2003a,b), but this is the first study to make use of the global time-series data. The Ecological Footprint concept and data are reviewed by van den Bergh and Verbruggen (1999a) and Monfreda et al. (2004).

I converted global aggregate values to per capita values using the population estimates provided in the same table. Data on global per capita income are from the latest update of Angus Maddison's estimates (Maddison 2007, described in Maddison 2006a & b), which provide per capita GDP in 1990 International Geary-Khamis dollars. I used three different approaches to test for the existence of an EKC. First I fitted a quadratic function to the data, and determined the sign of the quadratic term, and the position of the turning point. Second, I extended this approach by considering higher order polynomials. Third, I used non-linear regression to fit alternative models to the data, each representing different hypotheses: strictly increasing, asymptotic and humped.

A. Quadratic

A common test of the EKC is to fit a quadratic function to the data, and observe the sign of the quadratic term (e.g. Rosa et al. 2004). I fitted a quadratic curve to the per capita Ecological Footprint data from 1961 to 2001:

Equation 5.1 $$F = a + b \cdot Y + c \cdot Y^2$$

where F is Ecological Footprint per capita and Y is income per capita (a, b and c are constants). I then compared this with a linear model on the basis of its Akaike Information Criterion (AIC) score:

Equation 5.2 $$F = a + b \cdot Y$$

If the quadratic function outperforms the linear function, the sign of the coefficient c on the quadratic term can be used to infer whether an EKC is present (see e.g. Bagliani et al. 2007). If the quadratic test is used, it is important to determine the position of the turning point, as well as the sign of the quadratic term. A negative quadratic may be found when the turning point lies far outside the range of the data, and merely indicates an income elasticity of impact below unity, rather than the existence of a turning point. Negative quadratics whose turning points lie near or beyond the limit of the data are functionally indistinguishable from strictly increasing or asymptotic functions. I calculated the turning point for the quadratic function from Equation 5.3

$$\text{Turning point} = \frac{-b}{2c}$$

Finally, 95% confidence intervals for the quadratic function, and its turning point, were estimated using bootstrapping. A probability distribution was produced by fitting the function to 1,000 bootstrapped samples. Confidence intervals were estimated by selecting the 25th and 975th highest prediction for each year from the probability distribution. Confidence intervals for the turning point were estimated by calculating the turning point for each bootstrap sample using Equation 5.3, and taking the 25th and 975th highest estimates. The analyses were implemented using the lm function in R (R Core Development Team 2007).

B. Higher order polynomials

In addition to quadratics, some studies have also used cubic functions (e.g. Bagliani et al. 2008). However, unlike the quadratic, which provides a simple representation of the EKC, it is not clear what hypothesis is represented by these models, or why other polynomials are not tested. To allow comparison with these studies, I extended the above analysis by fitting successively higher order polynomial functions (Equation 5.4) until the AIC score was minimised. Thus, the

AIC scores provide an objective means of model selection, preventing under- or over-fitting.

Equation 5.4 $$F = a + b_1 \cdot Y^1 + b_2 \cdot Y^2 + ... + b_{n-1} \cdot Y^{n-1} + b_n \cdot Y^n$$

C. Non-linear regressions

As noted above, care is required when using a quadratic function to represent the EKC hypothesis. This is because quadratics, being constrained to be symmetrical, can be estimated even when the data are strictly increasing. The sign of the quadratic term determines whether the function is convex or concave to the origin, but not whether the data are humped. A more thorough test, therefore, is to determine the consistency of the available data with three possible relationships between Footprint and income: strictly increasing, asymptotic or humped, again using AIC scores to determine the support received by each model from the data (Equations 5.5-5.9). Strictly increasing functions represent no or only partial decoupling. Asymptotic functions represent full decoupling at the margin. Each is consistent with the early stages of an EKC, but do not provide evidence for it. To represent the EKC hypothesis (with intra-marginal decoupling) I fitted a strictly humped biexponential function, which estimates separate parameters for the rising and falling slopes of the hump, providing a more robust test of the EKC than the quadratic function.

Strictly Increasing

Equation 5.5 Linear: $F = a + b \cdot Y$

where *b* is positive

Equation 5.6 Power: $F = a \cdot Y^b$

where *a* and *b* are positive and *b* is the impact elasticity of income. If *b*<1, the function is concave (implying partial decoupling at the margin), if *b*>1, the function is convex (implying increasing marginal impact).

Asymptotic

2 Parameter Asymptotic Exponential:

Equation 5.7 $$F = a \cdot (1 - e^{bY})$$

Where a is the value of the asymptote, and b is negative (asymptote approached from below)

3 Parameter Asymptotic Exponential

Equation 5.8 $$F = a + (b - a) \cdot e^{cY})$$

Where *a* is the asymptote, *b* is the intercept and *c* is negative.

Strictly Humped

Biexponential

Equation 5.9 $$F = a \cdot e^{bY} + c \cdot e^{dY}$$

The biexponential function may take a variety of forms: humped, U-shaped or strictly decreasing, and if humped, may have a steeper up or down slope depending on the signs and values of the parameters. If its second exponential is replaced by a constant (i.e. if *d*=0) this equation is directly equivalent to a three parameter asymptotic function, with asymptote=*c*. Thus, the last three functions are nested, with the biexponential being the global model.

Note that each model is consistent with different stages of an EKC, but if the turning point lies near to, or beyond, the upper limit of the data, only models 5.5-5.8 will be supported. Such a finding does not exclude the possibility of an EKC occurring in the future, but implies that the present data are also consistent with less optimistic inferences. Models were compared on the basis of AIC scores, and bootstrapped confidence intervals for the functions were estimated as above. All the analyses were implemented using the nls function in R (R Core Development Team 2007).

III. Results

A. Quadratic

The results of the quadratic test unambiguously favour the existence of an EKC. The quadratic model received considerably more support from the data than the linear model (ΔAIC=-66.6) supporting the inclusion of the quadratic term. In addition, the quadratic term was negative and the turning point, together with its

95% confidence intervals, lies well within the range of the data, at $5,036 or the average global income in 1988 (Table 5.1, Figure 5.1).

Table 5.1. The results of the quadratic test for an EKC in global per capita Footprint.

Parameter	Estimate	Std Error
a	-6.51×10^{-1}	1.74×10^{-1}
b	1.15×10^{-3}	7.98×10^{-5}
c	-1.14×10^{-7}	8.92×10^{-9}
Turning Point	$5,036	
95% CI	$4,858-$5,158	
Data Range	$2,833-$6,153	
ΔAIC over linear	-66.6	
Adjusted R^2	91%	

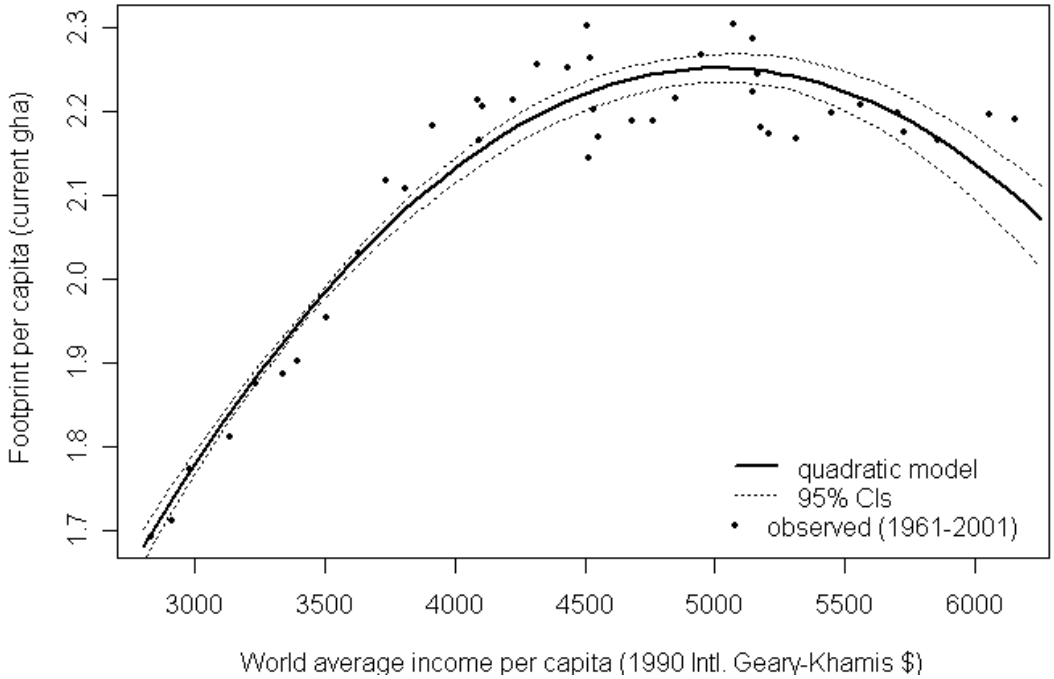

Figure 5.1. The quadratic function fitted to data on world average Footprint per capita and income per capita (1961-2001) with 95% confidence intervals.

B. Higher order polynomials

AIC score was minimised by the fifth order polynomial (Table 5.2). This supports the existence of a clear hump, with a single maximum well within the data at $4,624, or the world average income in 1984, and no minima (Table 5.3). All polynomials fitted (up to 7th order) have a clear maximum between $4,000 and $5,500, although the 4th, 6th and 7th order polynomials have minima beyond this level, due to over fitting the fluctuations in the post maximum data (Figure 5.2).

Table 5.2. AIC scores of the polynomial functions.

Polynomial	AIC	ΔAIC from best function
1st (linear)	-61.3	80.8
2nd (quadratic)	-127.9	14.2
3rd (cubic)	-137.7	4.5
4th	-141.6	0.5
5th	-142.1	0.0
6th	-141.1	1.0
7th	-140.9	1.2

Figure 5.3. Parameter estimates and turning point of the 5th order polynomial.

Parameter	Estimate	Std Error
a	31.93	18.43
b_1	-3.692×10^{-02}	2.183×10^{-02}
b_2	1.721×10^{-05}	1.018×10^{-05}
b_3	-3.837×10^{-09}	2.335×10^{-09}
b_4	4.138×10^{-13}	2.638×10^{-13}
b_5	-1.740×10^{-17}	1.175×10^{-17}
Turning point	$4,624	
Data Range	$2,833-$6,153	
Adjusted R^2	94%	

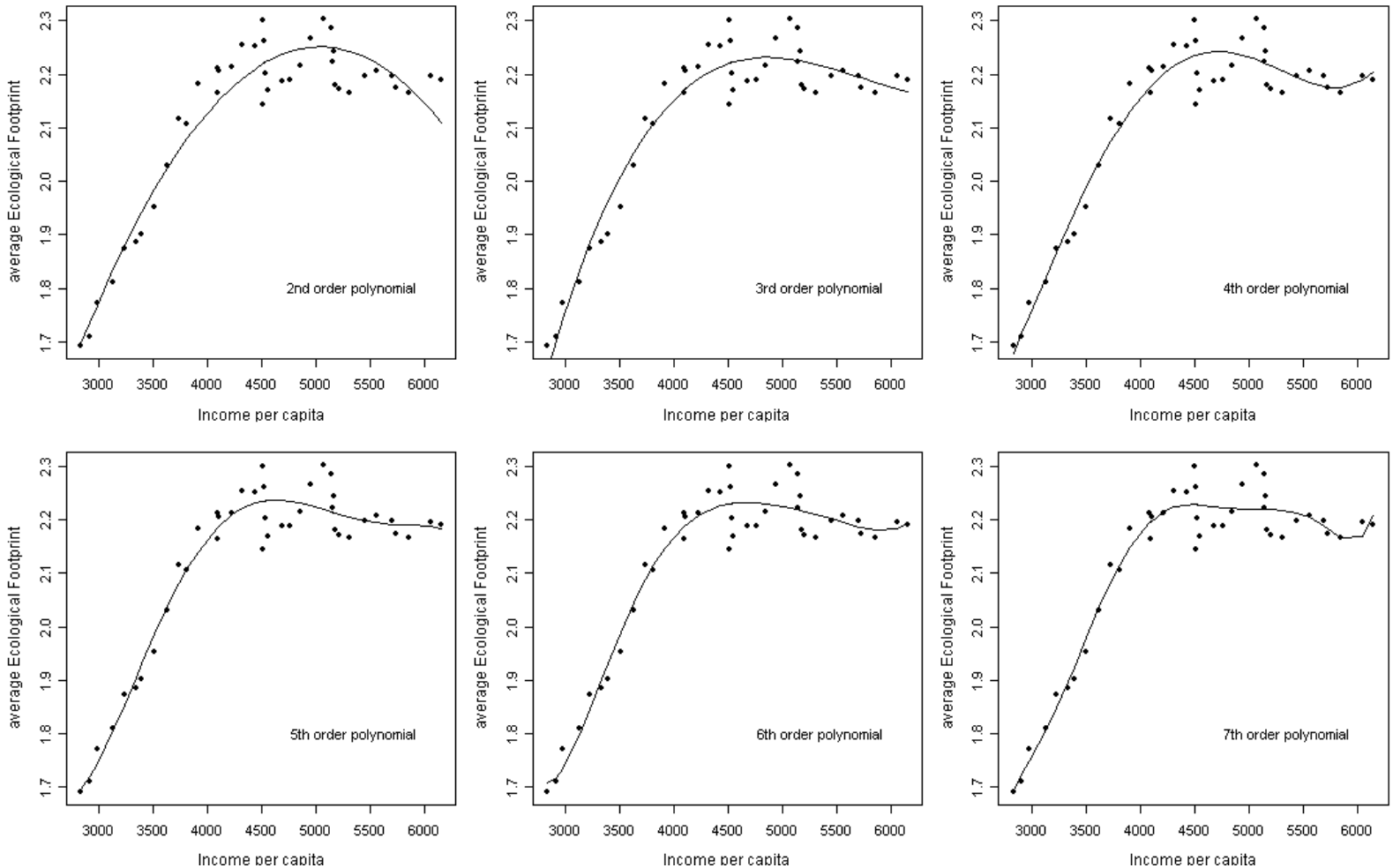

Figure 5.2 Higher order polynomial functions fitted to world average Ecological Footprint and income per capita (1961-2001).

C. Nonlinear regression

The results of the nonlinear regression are also consistent with the existence of significant decoupling of per capita global Ecological Footprint from income. First, the asymptotic functions are much better supported than the strictly increasing functions, indicating full decoupling at the margin. This implies that for the last decade or so, per capita economic growth has not lead to increases in the global Ecological Footprint per person. This conclusion is robust, with narrow 95% confidence intervals of the position of the asymptote, between 2.27 and 2.22 gha,[143] lying within the range of the data (Figure 5.1). Further, the existence of an EKC, and intra-marginal decoupling, is also supported by the non-linear regressions: the humped biexponential model receives substantially more support from the data than any other model (Table 5.4).

Table 5.4. AIC and ΔAIC scores for the five functions.

Type	Function	Parameters	AIC	ΔAIC
Strictly Humped	Biexponential	4	-135.2	0
Asymptotic	3 P. Asymptotic	3	-120.5	14.7
	2 P. Asymptotic	2	-83.6	51.5
Strictly Increasing	Power	2	-68.3	66.9
	Linear	2	-61.3	73.8
Quadratic (for comparison)		3	-127.9	7.3

Confidence intervals, however, could not be obtained for the biexponential function, as bootstrapping failed because the model failed to converge for a large proportion of samples[144]. This is because of the relatively small number of data points available after the maximum. Inspection of the parameter estimates and their standard errors (Table 5.5) indicates that the standard errors of the down slope parameters (*c* and *d*) are large relative to the estimates and t-values are

[143] Based on 500 bootstrapped samples

[144] I also attempted to produce confidence intervals using the jack-knife procedure. However, while the model converged on all leave-one-out samples (n=41), it failed to converge on all leave-two-out samples (n=820) meaning that insufficient samples were available to estimate confidence intervals.

small. Thus there are insufficient data to robustly derive the shape of the down slope and, while the available data are clearly humped in shape, it is still too early to draw any firm conclusions about the form of the curve to the right of the maximum. Remember that if *d*=0, the biexponential function collapses to the 3 parameter asymptotic function, with asymptote=c.

Although the best fitting higher order polynomials (3≤n≤7) outperform the biexponential function, they cannot easily be mechanistically justified a priori, and therefore are not well suited to drawing inferences from the data (Burnham & Anderson 2002).

Table 5.5. Parameter estimates and standard errors for the three best supported functions.

Type	Function	Parameter	Estimate	Std. error	t-value	Std. error as % of estimate
Strictly Humped	Biexponential	a	-10.32	2.255	4.6	21.9
		b	-5.475x10-4	3.510x10-4	1.6	64.1
		c	5.527	5.468	1	98.9
		d	-1.287x10-4	1.077x10-4	1.2	83.6
Asymptotic	3 Parameter Asymptotic	a	2.238	1.590x10-2	140.8	0.7
		b	-49.171	30	1.6	61
		c	1.564x10-3	1.994x10-4	7.8	12.7
Quadratic (for comparison)		a	-6.506x10-1	1.735x10-1	3.7	26.7
		b	1.153x10-3	7.977x10-5	14.5	6.9
		c	-1.144x10-7	8.921x10-9	12.8	7.8

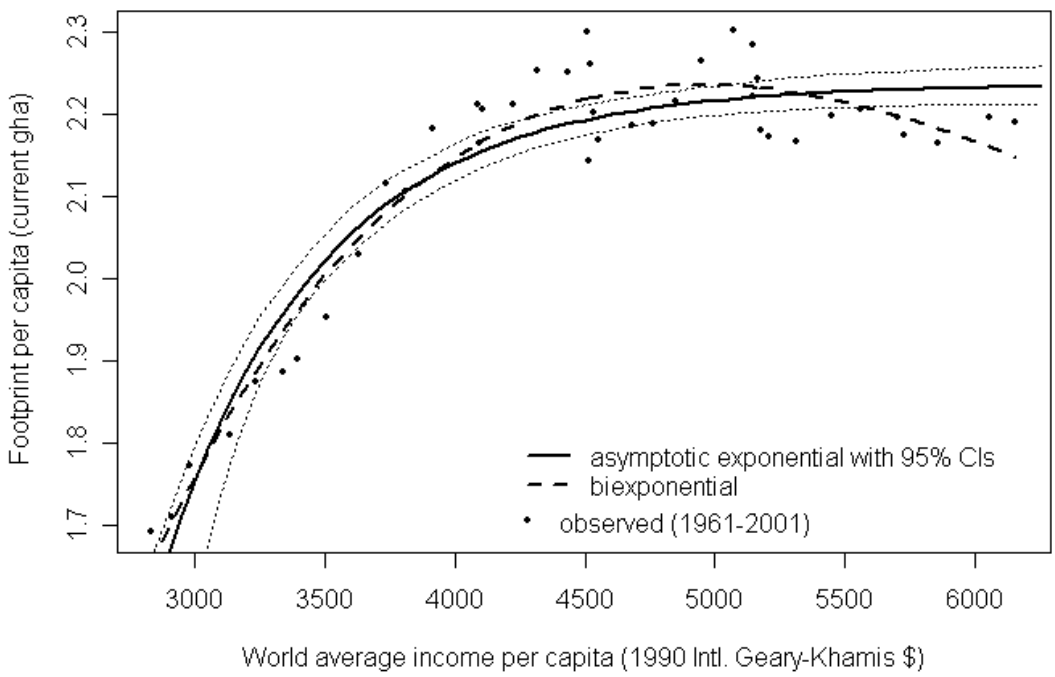

Figure 5.3. The asymptotic exponential function with 95% confidence intervals and the biexponential function (no confidence intervals) fitted to world average Ecological Footprint and income per capita (1961-2001).

IV. Footprint subcomponents

Wackernagel et al. (2002) warn that, regardless of whether there may be decoupling between environmental impact and economic scale, the world's Ecological Footprint has already exceeded the world's biocapacity. This means that the stocks of natural capital that would have to be depleted to ensure that there was no net emissions of CO_2 exceed those that remain.[145] This underlines the point that while Footprint analysis allows the two main components of Footprint, natural capital and entropy, to be compared, this does not imply that they are, in practice, fully inter-convertible or that mankind is optimising the balance between the two (van den Bergh & Verbruggen 1999a, Wackernagel 1999). It is therefore interesting to investigate trends in the subcomponents of ecological Footprint.

[145] CO_2 emissions can be reduced through reforestation (or afforestation) to sequester the CO_2, or by growing biofuels to displace fossil fuels. However, both options can either directly or indirectly displace natural capital, and in any case, the land required at current technologies exceeds that available.

WWF (2004) provides a breakdown of Ecological Footprint into three categories: "Food, Fibre and Timber"; "Built-up Land", and "Energy". The last represents increases in high entropy CO_2 and therefore represents an "entropy component", or more specifically a "CO_2 component". The first two represent the pressures on natural capital, and therefore a "Natural Capital component". The CO_2 component and the natural capital component are not only conceptually different, but have also had very different economic histories. Table 5.6 gives some illustrative dates for scarcity signals and government policies directed towards these resource types.

The nature conservation movement is thus at least a century old, implying that a value has been placed on the conservation of some natural capital for at least that long. In contrast, while there have always been incentives for increasing efficiency in the use of fossil fuels, there have not, until very recently, been any incentives for improving the CO_2 efficiency of economies, although policies aimed at other, more local, pollution problems (e.g. the Fuel Price Escalator in the UK) may also have improved the CO_2 efficiency of many economies as a by-product. Also, apart from specific price shocks in the 1970s, there have been few signs of absolute scarcity in the supply of fossil fuels, whereas the supply of land in most countries has been highly inelastic for many centuries.[146] Overall then, incentives for efficiency in the CO_2 component of the Footprint have probably lagged at least a century or more behind similar incentives in the natural capital component.

[146] The creation of land from the sea in the Netherlands, where the price of land is very high, only serves to illustrate this point.

Table 5.6. Scarcity signals, government policy and international treaties relevant to the Natural Capital and CO_2 components of the Ecological Footprint.

	Natural Capital		CO_2	
Scarcity signals	<1800s	Frontier expansion reaches W. Coast of USA[147]	1970s	Oil Price Shocks
National / regional government policies specifically targeting these externalities	1872	Yellowstone National Park created	2005	European Union Emissions Trading Scheme[148]
	1973	US Endangered Species Act		
	1979	EU Birds Directive		
	1988	EU Set-aside Scheme		
	1992	EU Habitats Directive		
International treaties signed	1971	Ramsar Convention on Wetlands	1997	Kyoto Protocol
	1973	CITES		
	1992	Convention on Biological Diversity		
International treaties enter into force	1975	Ramsar & CITES	2005	Kyoto Protocol

This suggests two hypotheses about trends in the two principal components of Ecological Footprint. First, we might expect to see efficiency improvements to occur earlier in the natural capital component than in the CO_2 component and stronger evidence for an EKC in the former. Second, since specific incentives for efficiency gains in both components are relatively recent, especially on the international level, we might expect further gains in efficiency in the future. To examine these propositions, I examine trends in each of these components of the Ecological Footprint with increasing per capita income.

Natural capital component

The natural capital component of the ecological Footprint shows a clear and consistent decline per capita throughout the period 1961-2001 (Figure 5.4), implying that the peak of the EKC, if any exists, lies to the left-hand side of the

[147] Vandenbroucke (2008)

[148] See Ellerman and Buchner (2007) Convery and Redmond (2007) and Kruger et al. (2007).

data. Because of this, the models used above are not suitable.[149] Figure 5.4 also shows world biocapacity per capita over the same period, also taken from T able 3 of WWF (2004:32). Despite rapid population growth over the period, the non-CO_2 Footprint has not yet come close to exceeding the world's biocapacity.[150] However, natural capital per capita has declined considerably.

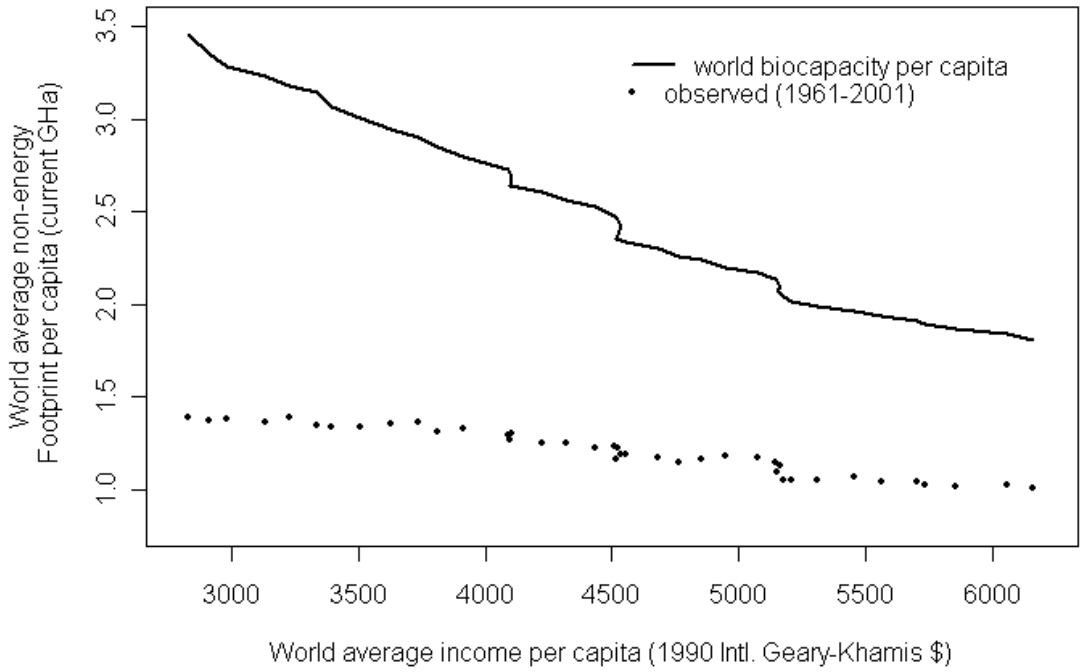

Figure 5.4. The trend in the non-energy component of global Ecological Footprint (1961-2001).

CO_2 component

The CO_2 component of Ecological Footprint shows a very different trend, increasing for most of the period (Figure 5.5). I repeated for this component the tests used above for total Ecological Footprint. The quadratic function fitted well (adjusted R^2=99.9%) and outperformed the linear (ΔAIC=100.9), and the quadratic term was negative. However, the turning point lay near the upper end of the data range at $5,728 or the world average income in 1998 (Table 5.7).

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[149] The power, two- and three- parameter asymptotic and biexponential functions did not converge, while the linear function was of course strictly decreasing rather than increasing.

[150] Note that the total space left for natural capital, which can be represented by the gap between aggregate non-CO_2 Footprint and biocapacity, cannot be seen on this graph, which shows per capita natural capital.

Table 5.7. Parameter estimates for the quadratic equation.

Parameter	Estimate	Std Error
a	-2.27	9.82x10-2
b	1.20x10-3	4.52x10-5
c	-1.05x10-7	5.05x10-9
Turning Point	$5,728	
95% CI	$5,553-$5,854	
Data Range	$2,833-$6,153	
ΔAIC over linear	-100.9	
Adjusted R^2	99.90%	

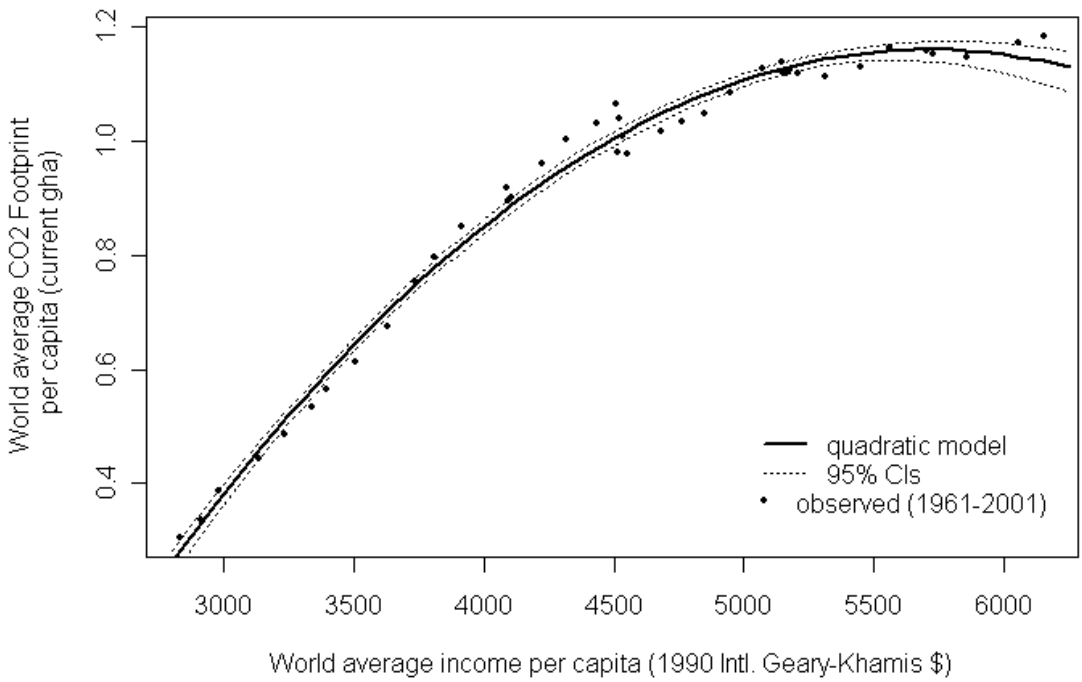

Figure 5.5. The quadratic function with 95% CIs fitted to world average CO_2 Footprint per capita.

In the non-linear regression, the quadratic function clearly outperforms the strictly increasing and asymptotic functions (Table 5.8). However, the biexponential function did not converge, since there is no evidence for any maximum in the data (Figure 5.6). Thus, in this case, the quadratic function is functionally indistinguishable from the asymptotic function (as evidenced by the position of the turning point). This case demonstrates the importance of not relying on the quadratic test of EKCs alone.

Table 5.8. AIC and ΔAIC scores for the per capita CO2 Footprint.

Type	Function[151]	Parameters	AIC	ΔAIC
Asymptotic	3 Parameter Asymptotic	3	-163.7	0
Strictly Increasing	Linear	2	-73.7	90
	Power	2	-67.8	95.9
Strictly Humped	Biexponential	4	Did not converge	
Quadratic (for comparison)		3	-174.6	-10.9

This indicates that, while the data are consistent with the existence of an EKC, the data are also consistent with an asymptotic model – it is too early to conclude that an EKC exists for CO_2.[152]

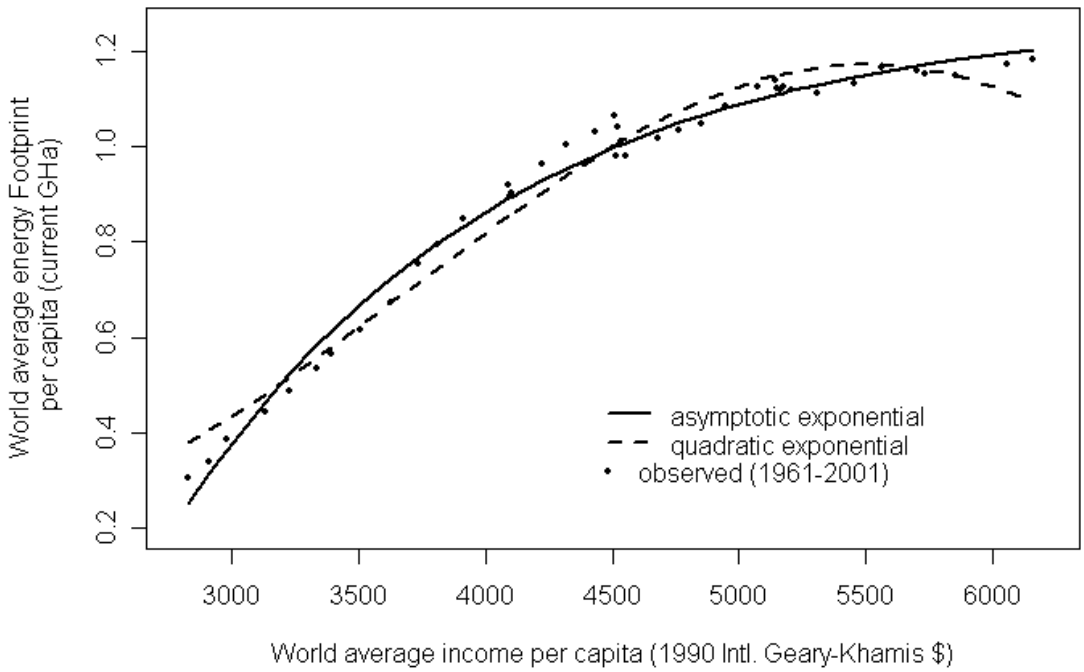

Figure 5.6. The quadratic and three parameter asymptotic functions fitted to world average CO_2 Footprint per capita (1961-2001).

[151] The two parameter asymptotic function, like the biexponential function, did not converge.

[152] Note, however, that these results are considerably more optimistic about the possibility of decoupling than the positive quadratic apparently found by Rosa et al. (2004) for national CO_2 Footprints (see previous chapter). The difference is primarily due to their use of cross-sectional data, and logged variables. As I showed in the previous chapter, a positive quadratic in the latter does not necessarily imply a convex function in the untransformed variables.

V. Discussion

Empirical assessments of the EKC hypothesis should not be viewed as binary pass/fail tests, but rather as measures of the strength of the evidence for an EKC, given the available data, compared with alternative hypotheses. Any functional form, even one which is convex, is compatible with the existence of an EKC at some point in the future. Since we lack a definitive, mechanistic model of the relationship between environmental impact and income level (and indeed, it seems plausible that, given its complexity, no model could exist) it is only possible to draw conclusions about the period covered by the data. This study has demonstrated the weakness of the quadratic approach for these purposes, and the advantages of using a strictly humped function, such as the biexponential. It has also demonstrated that the arbitrary use of cubic functions, as opposed to other higher order polynomials, without a priori justification (e.g. Bagliani et al. 2008), is of questionable utility.

The relationship between global Ecological Footprint per capita and income per capita over the period studied is best described by a humped function, although an asymptotic function cannot be ruled out. This implies that, at minimum, economic growth has been fully decoupled from growth in environmental scale, and furthermore that intra-marginal decoupling is probably occurring (an EKC). This is a striking result, and considerably more optimistic about the possibility of decoupling than those of recent studies which have used cross-sectional data, and therefore ignored technological progress (Dietz et al. 2007, Bagliani et al. 2008). It is all the more interesting given that the indicator used (Ecological Footprint) controls for off-shoring and represents a more comprehensive range of impacts than has been tested in most studies of the EKC (Stern 2004).

Although theory suggests several mechanisms by which EKCs may be produced (Dasgupta et al. 2002), an EKC remains fundamentally a descriptive rather than a deterministic model. Evidence for an EKC, such as that presented above, should be interpreted cautiously, as demonstrating that growth in income is potentially compatible with declining environmental scale, it does not ensure that this will be so. In particular, evidence for the existence of an EKC cannot be used to argue against policies aimed at improving environmental quality, since these policies are

part and parcel of the EKC hypothesis (Dasgupta et al. 2002). At most, evidence for decoupling can be used to argue against policies aimed at preventing further economic growth at the macro level (e.g. Czech 2002). In addition, Arrow et al's (1995) caveats apply: unanticipated non-linearities could result in environmental catastrophe before the EKC is completed.

As expected, the components of Ecological Footprint show markedly different trends. The natural capital component shows a steady decline per capita, but although the CO_2 component shows significant decoupling per capita, there is no hard evidence for a turning point, and certainly no decline, in CO_2. This reflects the point made in the previous chapter that technological progress (including policy development) will be related to urgency. As stated earlier, incentives for reducing the CO_2 component are relatively recent. It is seems plausible that, given suitable such policies and incentives, an EKC for the CO_2 component will emerge. In any case, the results presented here are considerably more optimistic about the potential for decoupling than those derived for CO_2 by other authors (e.g. Rosa et al. 2004) from cross-sectional data.

This chapter demonstrates that significant decoupling of Ecological Footprint from income has been achieved over the last four decades. This is in stark contrast to the rest of the literature on Ecological Footprint. Since I use the same data source, the difference seems largely to result from using a more suitable spatial scale (global rather than national) to analyse technological progress, capturing international overspill. However, without accelerated technological progress, human society will continue to overshoot the biocapacity of the earth for some time to come. Chapter 6 will conclude this part of the thesis, by combining the models developed here (representing marginal and intra-marginal decoupling) with the economic projections developed in Chapter 3, to calculate overshoot parameters for each economic projection, based on alternative technology scenarios.

6. Economic, demographic and ecological projections to 2105

Abstract

Chapter 3 discussed the use of income projections in cost-benefit analysis: the desirability of finding a global model on which all parties can agree and which is parameterised with reference to observed data, as well as subjective opinion. Some progress was made in Chapter 3 towards constructing such a model, with reference to the historical data, and in Chapters 5 and 6 environmental constraints were investigated. The purpose of this chapter is to combine these two strands of investigation, and to produce historically-referenced, environmentally-parameterised income projections that can be used in the illustrative cost-benefit analysis of nature conservation in Madagascar, which follows in the next three chapters.

I. Projecting regional income to 2105

In Chapter 4, I developed projections of per capita income for the OECD90 region and the relative income gap [153] for the ASIA and ALM regions [154] through extrapolation of historical data. I limited the analysis to data from 1952 to 1990, to allow comparison with the Inter-governmental Panel on Climate Change's projections (Nakicenovic & Swart 2000). Although at least one candidate model performed well for OECD90 and ASIA, considerable unexplained variation remained for the ALM region, the region of greatest interest for the case study. In addition, no such analysis could be carried out for the REF region, due to the lack of pre-1991 data.

To project regional per capita income for the period 2005-2105, I repeated the analysis in Chapter 4 using the latest available income data from Heston et al. (2006) covering the period 1952-2003 for OECD90, ALM and ASIA and 1990-2003 for the REF region. The results are summarised below, see Chapter 4 for full methods.

OECD90

Not surprisingly, given the excellent performance of the power function in predicting the data 1991-2003 (Figure 3.1), it once again performs best out of the three functions when fitted to the full dataset (Figure 6.1), with even larger ΔAICs compared with the linear and exponential functions (Table 6.1), such that model averaging is unnecessary (ΔAIC>16).[155] The parameter estimates are also similar to those estimated in Chapter 4 (compare Table 6.2 below, with Table 3.3).

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[153] Relative income gap is the gap between a region's income per capita and that of the OECD, expressed as a percentage of the OECD's income per capita. Thus if ASIA region has a relative income gap of 90%, its income per capita is 10% of the OECD's. See Chapter 4 for full details.

[154] OECD90 = members of Organisation for Economic Cooperation and Development in 1990, i.e. Western Europe, USA, Canada, Australia, New Zealand and Japan; ASIA = Asia excluding former Soviet Union and Japan; ALM = Africa, Latin America and Middle East; REF = countries undergoing economic reforms: Eastern Europe and the Former Soviet Union. See chapter 3 for full definitions of regions.

[155] ΔAIC of >10 implies that a model has essentially no empirical support (Burnham & Anderson 2002:70) and in the case where there are two candidate models, implies an Akaike weight of 0.7%

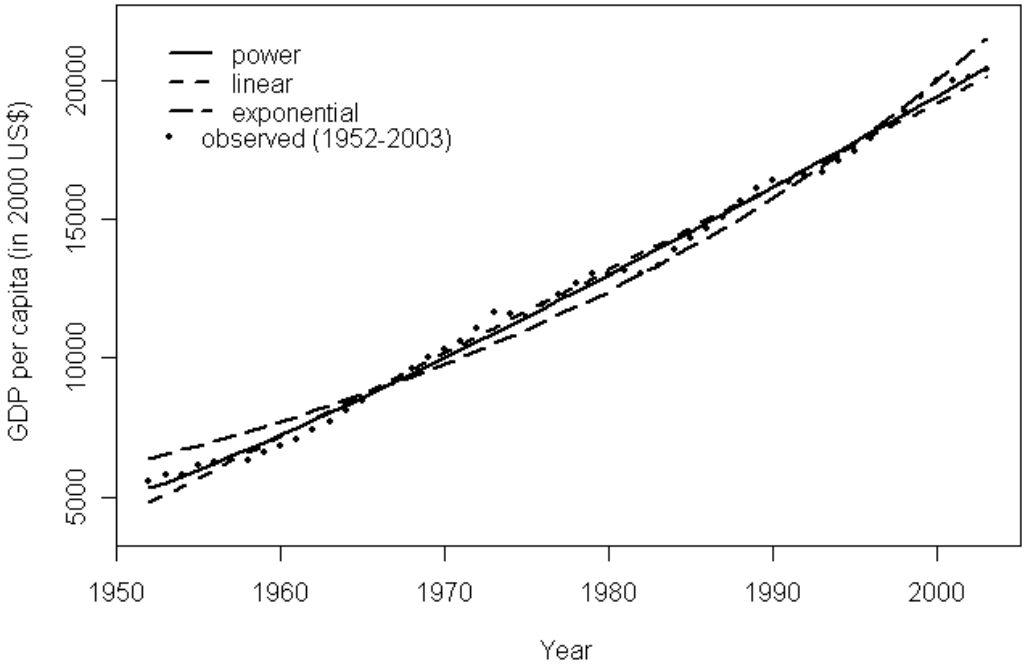


Figure 6.1. Power, exponential and linear models fitted to income per capita data for the OECD90 region (1952-2003). For region definitions, see footnote 2, main text.

ASIA

Similarly, for ASIA, the quadratic exponential function remains the best supported and provides an excellent fit to the observed data (Figure 6.2). ΔAICs are even greater than before, and there is no need to model average (ΔAIC>140). Once again, parameter b of the asymptotic exponential is negative (Table 6.2), when it must be positive for the function to have the property of asymptotic increase, and this function is therefore discarded.

(Burnham & Anderson 2002:75). In other words, the model would contribute just 0.7% of bootstrap samples when calculating model averaged projections.

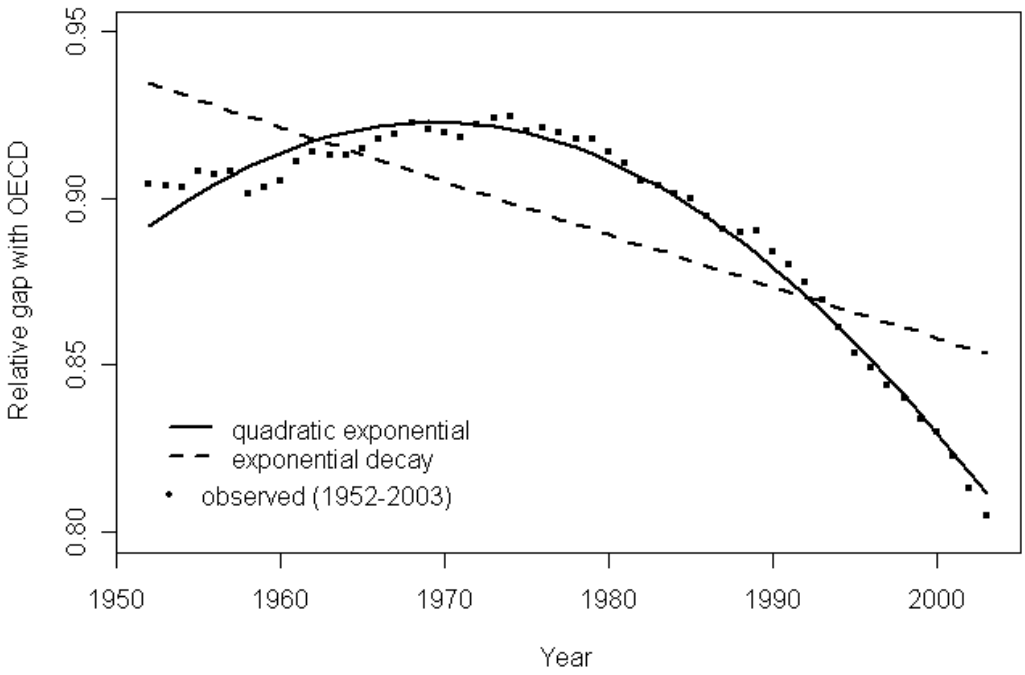


Figure 6.2. Quadratic exponential and exponential decay models fitted to data on the relative income gap between the ASIA region and OECD90 (1952-2003). For region definitions, see footnote 2, main text.

ALM

In Chapter 4 I found no evidence of convergence for this region, with parameter b of the exponential decay function and c of the quadratic exponential function both negative, and therefore only the asymptotic exponential function was retained. Extending the dataset to 2003 does not change this finding; indeed, it increases the estimate of the asymptote from 74% to 93% (Tables 3.6 and Table 6.2, Figure 6.3). This suggests that we should expect the ALM region to continue diverging from the OECD90 region.

However, as before, the fit is poor relative to those of the previous two regions. If the analysis is restricted to the last two decades of the data (1983-2003), the quadratic exponential is now concave to the origin, and is the best supported function (Table 6.1), though the asymptotic exponential function also receives some support (ΔAIC=3.1). A concave quadratic exponential function implies divergence followed by convergence – as for the ASIA region. However, the turning point lies beyond the range of the data, in 2010 (Table 6.2, Figure 6.4). Thus, this result should be treated with caution.

This highlights the sensitivity of projections to the choice of data set, when none of the candidate models explains the data well. Given this uncertainty, two alternative scenarios for ALM income are adopted for illustrative purposes. The pessimistic scenario is that ALM will continue to diverge from the OECD90 region, following the asymptotic exponential function parameterised using the data 1952-2003. The optimistic scenario is that ALM divergence will end in 2010, after which it will begin to converge, i.e. that it will follow the quadratic exponential function fitted to the data 1983-2003. These scenarios are illustrative, and relative probabilities are not assigned to them. Nevertheless, they are at least transparently derived with reference to the historical data and represent plausible, if subjective, views on the future of ALM income level.

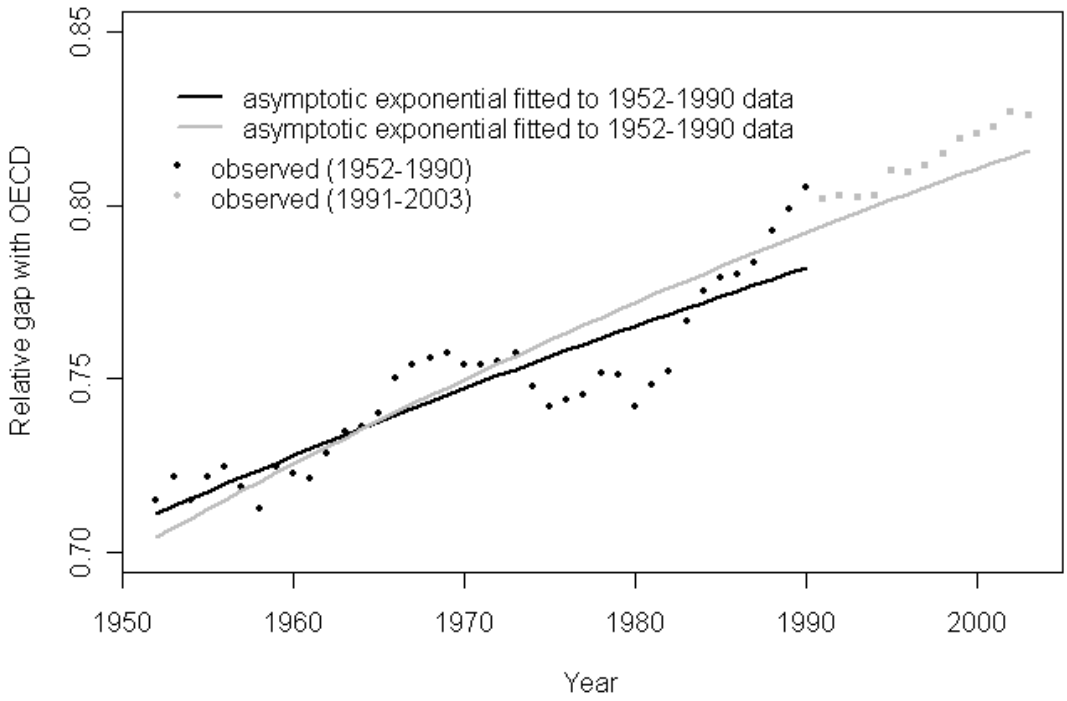


Figure 6.3. The asymptotic exponential model fitted to data on the relative income gap between the ALM region and OECD90. Data is from 1952-1990 and 1952-2003. For region definitions, see footnote 2, main text.

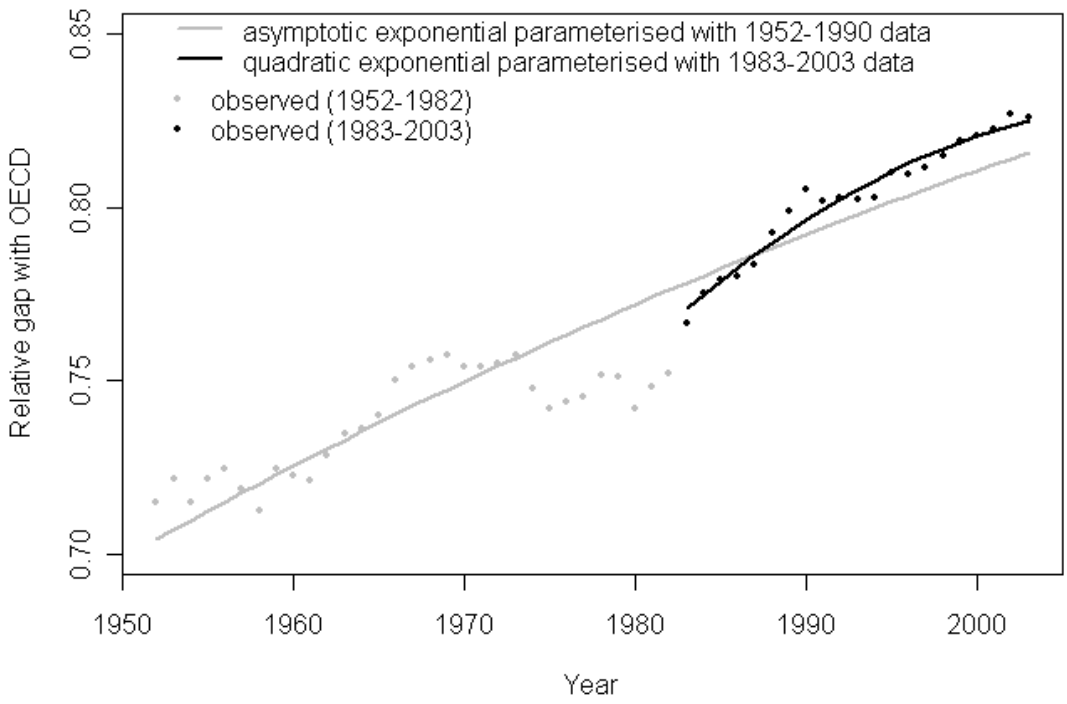


Figure 6.4. Alternative models fitted to alternative subsets of the data on the relative income gap between the ALM region and OECD90. For region definitions, see footnote 2, main text.

REF

This region was not considered in Chapter 4 because of the paucity of pre-1990 data (see Chapter 3). For the purposes of the case study, I use data from 1990-2003, analysing it in the same way as that from ASIA or ALM. Like ASIA, the region shows divergence and then convergence over the period studied, and the quadratic exponential function offers the best fit (Table 6.1, Figure 6.5). The turning point is reached in 1998 (Table 6.2). Parameters a and b of the exponential decay function were both negative, implying an increasing rather than decreasing function, and this is discarded.

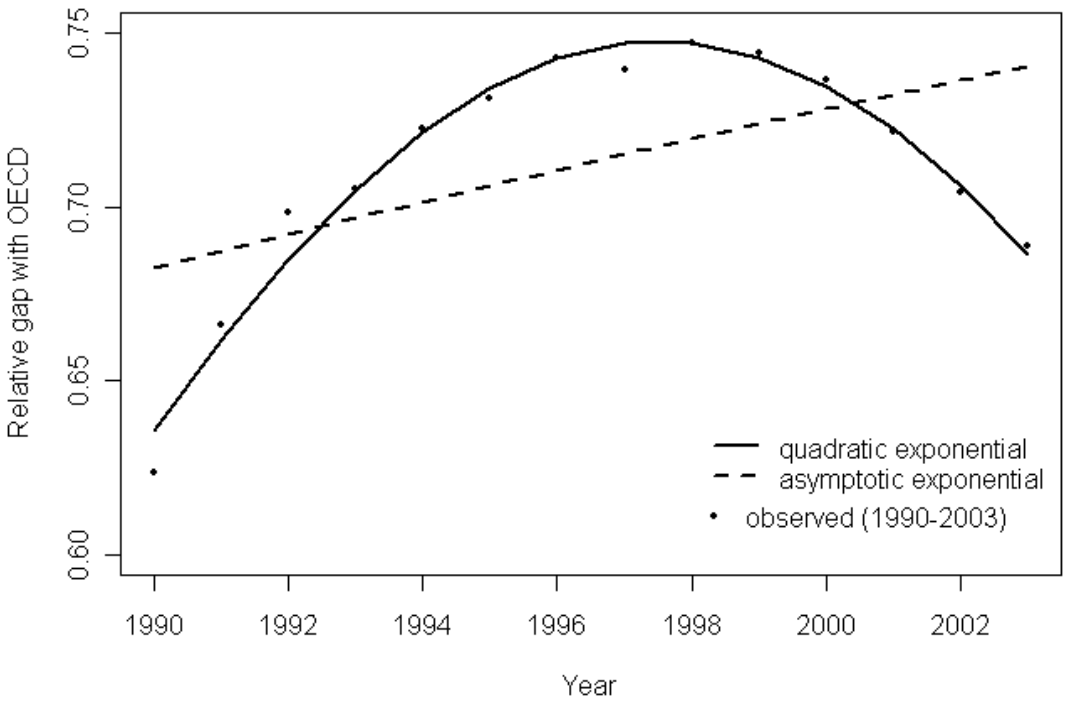


Figure 6.5. Quadratic exponential and asymptotic exponential models fitted to data on the relative income gap between the REF region and OECD90 (1990-2003). For region definitions, see footnote 2, main text.

Regional projections of per capita income

Combining the projection of OECD90 income per capita, with those of the relative income gap for ASIA, ALM and REF regions allows income per capita to be projected for each region. Figure 6.6 shows these projections from 2000 to 2105, together with the observed data and model fits. Both optimistic and pessimistic projections are shown for ALM.

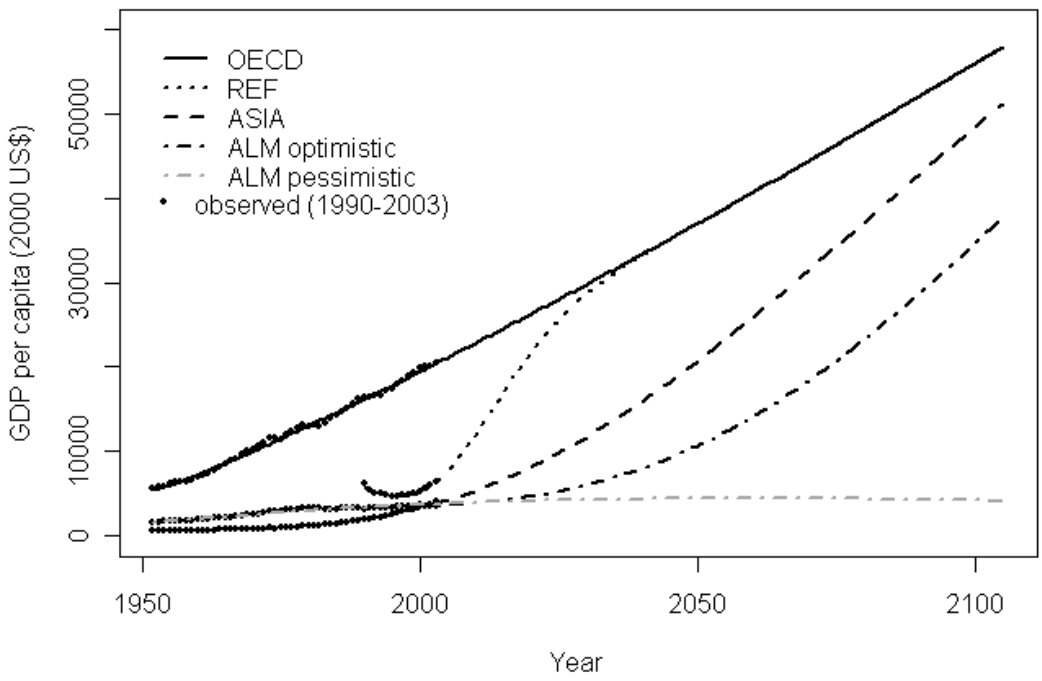


Figure 6.6. Observed GDP per capita (points), model fits and income projections to 2105.

Table 6.1. AIC comparisons of the income and relative income models, for the four regions, and the world.

	Parameters	**AIC Score**	**ΔAIC**	**Akaike Weight**
OECD90				
Power	3	749.09	0.00	1.00
Linear	2	765.56	16.48	2.64×10^{-4}
Exponential	2	819.29	70.20	5.69×10^{-16}
ASIA				
Quadratic exponential	3	-407.36	0	1.00
Asymptotic exponential	2	-266.62	140.74	N/A
Exponential decay	2	-249.46	157.90	5.16×10^{-35}
ALM				
Quadratic exponential	3	-337.04	0	N/A
Exponential decay	2	-328.05	8.99	N/A
Asymptotic exponential	2	-313.53	23.51	1.00
ALM using subset of data from 1983 to 2003				
Quadratic exponential	3	-171.73	0	0.82
Asymptotic exponential	2	-168.65	3.09	0.18
Exponential decay	2	-164.13	7.61	N/A
REF using data from 1990 to 2003				
Quadratic exponential	3	-98.06	0.00	1.00
Asymptotic exponential	2	-53.43	44.63	1.52×10^{-10}
Exponential decay	2	-52.85	45.22	N/A
Global income projection (see Section III)				
Power	4	492.38	0.00	0.99
Linear	3	501.83	9.45	8.79×10^{-3}
Exponential	3	531.06	38.68	3.95×10^{-9}

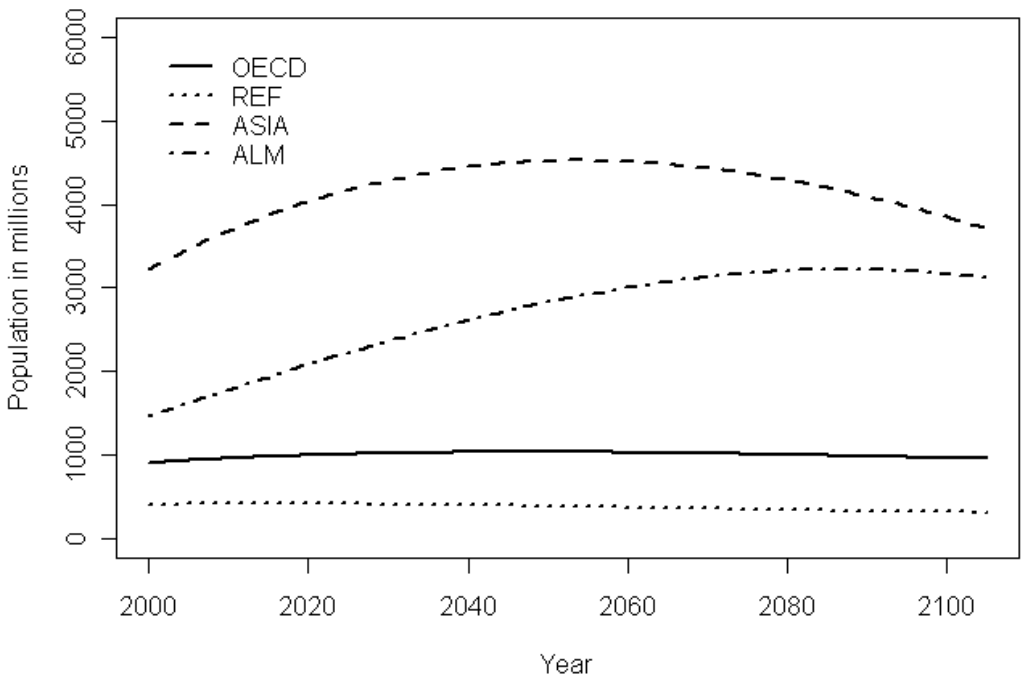
Table 6.2. Parameter estimates for the income and relative income models, for the four regions, and the world.

Model	Parameter	Estimate	Std. Error
OECD90			
Exponential	a	8.74	1.91×10^{-2}
	b	2.38×10^{-2}	4.94×10^{-4}
Linear	a	4.49×10^{3}	1.03×10^{2}
	b	3.00×10^{2}	3.39
Power	a	5.17×10^{3}	1.60×10^{2}
	b	1.68×10^{2}	2.22×10^{1}
	c	1.14	3.23×10^{-2}
ASIA			
Exponential decay	a	-6.62×10^{-2}	6.52×10^{-3}
	b	1.77×10^{-3}	2.19×10^{-4}
Asymptotic exponential	a	6.34×10^{-2}	3.83×10^{-3}
	b	-1.81×10^{-2}	1.64×10^{-3}
Quadratic exponential	a	-1.19×10^{-1}	2.21×10^{-3}
	b	4.18×10^{-3}	1.94×10^{-4}
	c	1.14×10^{-4}	3.60×10^{-6}
	Turning point	1969	
ALM			
Exponential decay	a	-3.47×10^{-1}	3.80×10^{-3}
	b	-2.94×10^{-3}	1.20×10^{-4}
Asymptotic exponential	a	2.98×10^{-1}	3.63×10^{-3}
	b	9.27×10^{-3}	4.54×10^{-4}
Quadratic exponential	a	-3.34×10^{-1}	5.34×10^{-3}
	b	1.45×10^{-3}	4.52×10^{-4}
	c	-2.76×10^{-5}	8.12×10^{-6}
	Turning point	N/A (convex function)	
ALM using data from 1983 to 2003			
Exponential decay	a	-2.57×10^{-1}	2.54×10^{-3}
	b	-3.38×10^{-3}	1.99×10^{-4}
Asymptotic exponential	a	2.29×10^{-1}	1.95×10^{-3}
	b	1.39×10^{-2}	7.29×10^{-4}
Quadratic exponential	a	-2.66×10^{-1}	3.35×10^{-3}
	b	5.54×10^{-3}	6.90×10^{-4}
	c	9.74×10^{-5}	3.02×10^{-5}

	Turning point	2010	
REF using data from 1990 to 2003			
Exponential decay	a	-3.81×10^{-1}	2.58×10^{-2}
	b	-5.52×10^{-3}	2.98×10^{-3}
Asymptotic exponential	a	3.23×10^{-1}	1.88×10^{-2}
	b	1.55×10^{-2}	7.23×10^{-3}
Quadratic exponential	a	-4.99×10^{-1}	8.67×10^{-3}
	b	4.88×10^{-2}	2.57×10^{-3}
	c	2.86×10^{-3}	1.65×10^{-4}
	Turning point	1998	
Global income projection			
Power	a	2.68×10^{3}	8.00×10^{1}
	b	1.52×10^{2}	2.85×10^{1}
	c	8.24×10^{-1}	4.62×10^{-2}
Linear	a	2.91×10^{3}	3.34×10^{1}
	b	7.53×10^{1}	1.38
Exponential	a	8.05	1.26×10^{-2}
	b	1.65×10^{-2}	4.51×10^{-4}

II. Regional and global population to 2105

I have not considered population so far in this thesis. Yet to aggregate regional income projections into a global projection (next section) demographic projections are of course required, as they are for the case study which follows. The most recent peer-reviewed long-range projection of population covering the whole world is Lutz et al. (2001),[156] which extends to 2100. I used the median population projections from this source.[157] The projections are provided in five year intervals, therefore, to provide annual estimates I fitted a third order polynomial to each projection (R^2>99%) and used this, extrapolating it forwards by five years to 2105 (Figure 6.7).



[156]More recent projections are available from the United Nations Population Division (UNPD 2006) but these extend only to 2050.

[157] Data downloaded in 5 year intervals from: http://www.iiasa.ac.at/Research/POP/proj01/IIASA_projections2001.xls 9th May 2008. Regions in Lutz et al. (2001) are aggregated into the regions considered in Chapter 4 as follows: OECD90 = North America, Western Europe, Pacific OECD; ASIA = China region, South Asia, Pacific Asia; ALM = North Africa, sub-Saharan Africa, Latin America, Middle East; REF = Central Asia, Eastern Europe, European part of the former USSR. See http://www.grida.no/climate/ipcc/emission/149.htm.

Figure 6.7. Population projections from Lutz et al. (2001), with interpolation (solid line) and extrapolation to 2105 (dotted line) using a 3rd order polynomial function (R^2 >99% in each case).

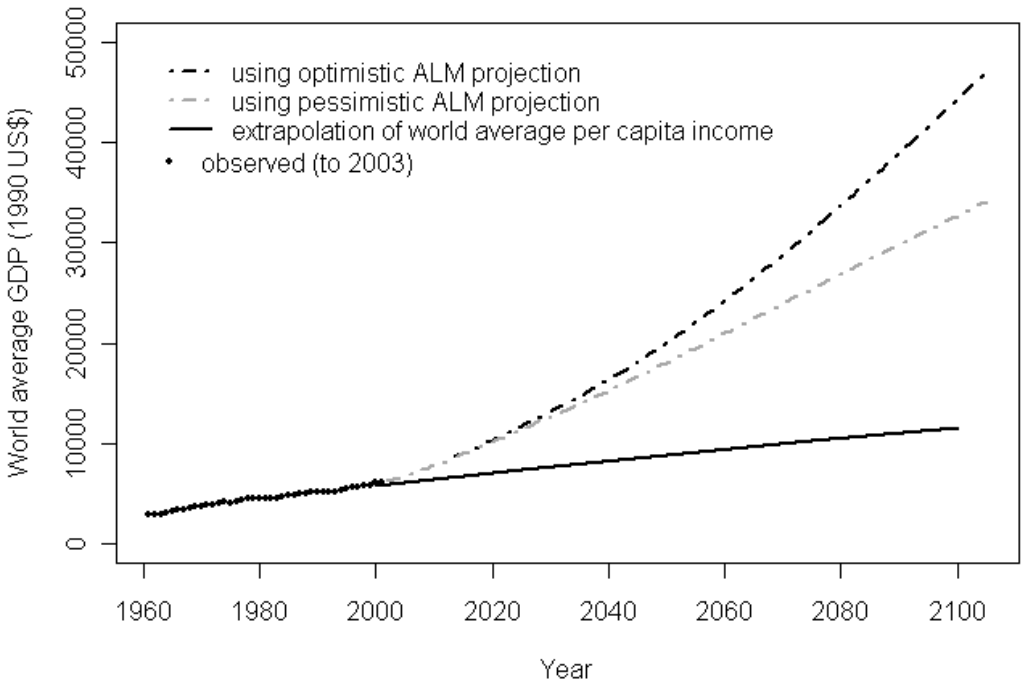
Combining unrelated demographic and economic projections in this way assumes that population and income levels are independent of one another, yet there is evidence for linkages between the two (see Barro & Sala-i-Martin 2004). However, the demographic projections used here were developed independently from any economic projection, and the developer's opinion is that "the general pattern of demographic transition ... is related to general development, but not economic growth specifically" (Wolfgang Lutz, pers. com).

III. Environmental implications of income projections: Ecological Footprint to 2105

Numerous authors have questioned the environmental feasibility of continued income growth (e.g. Georgescu-Roegen 1971[1999], Daly & Townsend 1993, Costanza 1995, see Chapter 4 for discussion) and income projections play a contentious role in environmental cost-benefit analyses. For example, Spash (2007b) questions the feasibility of the income growth assumptions made by Stern (2006), arguing that environmental limits will prevent income reaching the levels predicted. Therefore in any long-range CBA, the environmental implications of the income projections used should be investigated.

In Chapter 5 I introduced the Ecological Footprint as a way of conceptualising these limits using an accounting measure, and reviewed the existing literature on economic growth and Ecological Footprint, finding it to be biased towards pessimism. Chapter 6 continued this work, finding good evidence for there being (at minimum) decoupling of economic scale from environmental scale at the margin, and some evidence of intra-marginal decoupling – an environmental Kuznets curve. While it is impossible to predict by extrapolation something so strongly under the influence of deliberate human policy, it is instructive to project the environmental scale (e.g. Ecological Footprint) of the income projections developed above, using the best available models of the relationship between income and Ecological Footprint (taken from Chapter 5).

First, I combine the regional income and population projections, to project average world income per capita (Figure 6.8). There are two projections for the two different scenarios of ALM growth. For comparison, Figure 6.8 also shows a projection derived from directly extrapolating average global per capita income using the best supported function (a power function, see Tables 6.1, 6.2).[158] It is interesting to note that this projection differs markedly from that obtained through projecting each region individually, showing that the scale at which data is aggregated is important in comparing projections with the historical data. This has implications for the work in Chapter 4, and above, which has used data aggregated to large regions, following the Inter-governmental Panel on Climate Change (Nakicenovic & Swart 2000). The difference is due to the fact that the historical data covers the period where the ASIA and REF regions were diverging from, as well as converging on, the OECD. Averaged over this period their growth was relatively slow, therefore. However, if the convergence which is already present in the historical data continues, they will continue the current relatively rapid growth in the future, raising overall growth rates.



[158] I used data on global averaged income per capita (1961-2001) from Maddison (2006) since this is not available from Heston et al. (2006).

Figure 6.8. Projections of global average per capita GDP (1990 US$), based on: aggregating regional projections (broken lines) and on extrapolation of world average GDP per capita (solid line).

Next I project global Ecological Footprint for these income projections, using the Footprint-income models derived in Chapter 6 and the population projections from Section II, above. Global Ecological Footprint is shown Figure 6.9 and for comparison, I also show three projections developed by the WWF, in the Living Planet Report 2006: a business as usual projection (their representation of current trends) and slow and rapid transitions to more sustainable economies, as defined by WWF (2006). Ecological Footprint is shown as a factor of available global biocapacity (i.e. number of planets required), assuming that the supply of global hectares remains constant at year 2000 levels.[159] Table 6.3 provides estimates of the number of planet-years of overshoot implied by each projection, where overshoot is the amount by which global Ecological Footprint exceeds global biocapacity.

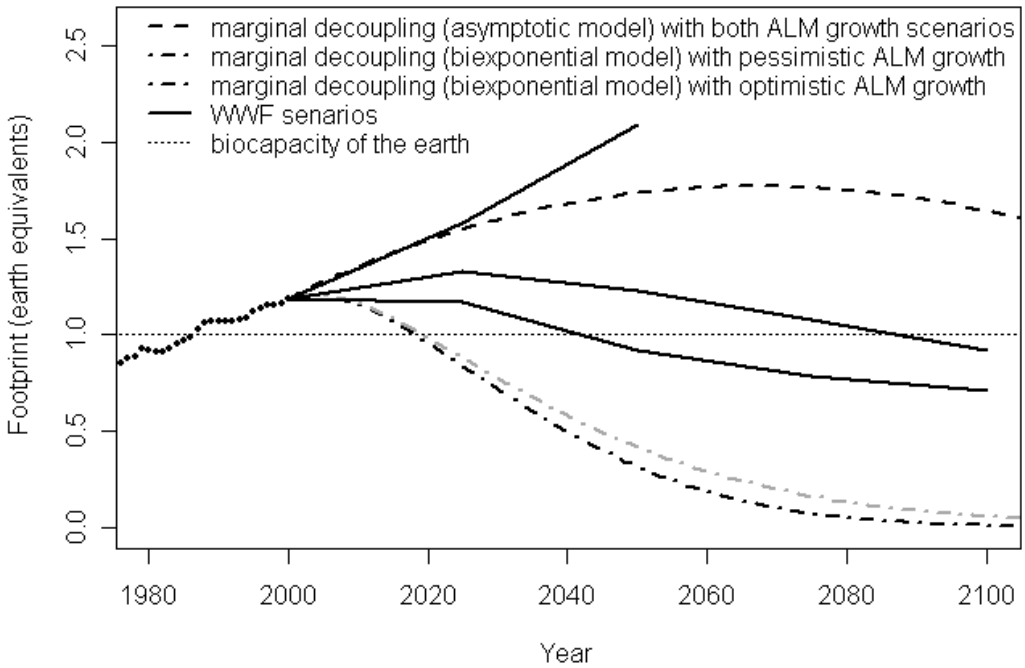


Figure 6.9. Ecological Footprint projected for each economic projection, using alternative models developed in Chapter 3.

[159] In fact the number of global hectares (GHa) on the planet has been increasing over time, due to conversion of unproductive land and forests to cropland, which has a higher biocapacity (according to the Ecological Footprint method). I assume that this process is halted in order to conserve wild nature.

Table 6.3. Ecological overshoot (in planet-years) calculated for alternative models of decoupling, income scenarios and for WWF's (2006) scenarios. A planet-year of overshoot is equivalent to the world's Ecological Footprint exceeding the available biocapacity by 100% for one year.

Decoupling model	**Economic scenario**	**Year overshoot ends**	**Overshoot (planet-years)**	
			2000-2050	**2000-2100**
Intra-marginal decoupling (biexponential)	Optimistic ALM growth	2019	2.71	2.71
	Pessimistic ALM growth	2020	2.85	2.85
Marginal decoupling (asymptotic exponential)	Any income growth scenario	>2100	26.50	63.39
WWF Scenarios				
WWF Business as usual		?	30.6	N/A
WWF Slow transition		2088	13.6	17.9
WWF Rapid transition		2042	5.9	5.9

These projections, of course, represent extrapolations well beyond the range of the observed data, and are dependent on the initial set of candidate models. Nevertheless, they illustrate several important points. First, that looking at the two lines representing optimistic and pessimistic ALM growth, faster economic growth reduces Ecological Footprint faster than slower growth does, under the biexponential Footprint-income model, in which intra-marginal decoupling occurs (Figure 6.9). However, both growth scenarios lead to rapid reductions in Ecological Footprint, more rapid than any of the WWF scenarios. Second, under the more pessimistic asymptotic income-Footprint model, in which no intra-marginal decoupling occurs, Ecological Footprint continues to rise for quite some time, and a very substantial "ecological debt" (WWF 2006) accumulates. However, this rise is driven by population growth, not economic growth (the projection of Ecological Footprint is identical for both income projections). Slower income growth makes no difference, since the asymptote of the Footprint-income relationship has already been reached. In this model, only negative income growth could reduce Footprint. To an extent, this is an artefact of the overly simplistic analysis carried out in Chapter 5, in which it was not possible to separate consumption and technological progress. It might be possible to reduce Footprint by slowing income growth, but only if technological progress was not also slowed. Therefore, rather than managing aggregate consumption, it might be

more appropriate to provide targeted incentives for greater efficiency in the use of scarce resources (e.g. carbon taxes). Finally, the projections developed in this thesis are generally more optimistic than those in WWF (2006). The reason for this cannot easily be deduced, since WWF (2006) provide no detailed information about how their projections were developed.

These projections serve to illustrate the implications of the finding that at present and based on the proxies used (GDP per capita and Ecological Footprint), the economic and environmental scales of the human economy are at worst decoupled at the margin. At best, with intra-marginal decoupling, economic growth might be compatible with reduced environmental scale. This is acknowledged by the Inter-governmental Panel on Climate Change (Nakicenovic & Swart 2000, Nakicenovic et al. 2003, Grübler et al. 2004). Thus, using these proxies, which are widely used in the sustainability literature, there does not appear to be any evidence that current trends in income growth are necessarily unsustainable. Whether or not they prove to be sustainable will depend on both the earth's capacity to tolerate overshoot, and on whether current trends in decoupling economic and environmental scale continue. The latter depends on the rate of technological progress and therefore, in part, on the policy environment and the preferences of individuals.

IV. Conclusions

This chapter has brought together work from the previous four chapters to produce income projections which can be used in the cost-benefit analysis case study which follows. The aim has been to work towards the requirements outlined in Chapter 3. First, income projections used in cost-benefit analyses should be transparently developed, clearly linked to the historical data (without being constrained by it) and probabilistic, incorporating both subjective and objective probabilities. Second, their environmental feasibility should be investigated.

7. The Ranomafana-Andringitra new protected area in Madagascar: introduction to the case study

Abstract

Increasingly, conservationists argue that conservation efforts should be prioritised towards developing countries, because of their high levels of species richness and the relatively low monetary costs of conservation. However, cost-benefit analyses of the value of conservation in the poorest countries are rare. In this chapter I introduce an empirical case study of a conservation project that aims to conserve an area of Madagascar's eastern rainforests: the Ranomafana-Andringitra corridor. Madagascar is renowned for extremely high biodiversity (it has been identified as a global priority for conservation) but also for the extreme poverty of its human population. The following chapters will use the CBA framework introduced in chapter 2 to investigate the social desirability of this project.

I. Introduction

An important challenge for the international community is to respond appropriately to the ongoing transformation of the natural environment and the consequent loss of wild nature. As noted in Chapters 1 and 2, cost-benefit analysis has the potential to help human society decide how to respond to this challenge; however, such analyses are rare. In this chapter I introduce an empirical case study of one conservation project that aims to conserve an area of Madagascar's eastern rainforests, which is threatened by conversion to agriculture, mining and timber extraction. This case study will be developed further in the next four chapters, and will serve to illustrate some of the points made in Chapter 2.

Biodiversity conservation and the objective of the case study

Nature conservation has traditionally had a diverse range of objectives and motivations. Many of the early protected areas focussed on conserving landscape and wilderness, rather than preventing the extinction of species, and as a consequence, numerous studies have noted that protected area networks may be relatively inefficient in maximising the number of species they protect (e.g. Rodrigues et al. 2004, Araujo et al. 2007, Maiorano et al. 2007). Although the authors of these studies invariably see this as a flaw, it is not immediately obvious why nature conservation should focus exclusively on maintaining the maximum possible species richness (number of species) of the country concerned (or of the whole world). The value of maintaining global or national species richness is poorly understood (Christie et al. 2006, Pearce 2007, see also references in Chapter 9), and species richness may be a poor predictor or driver of other conservation objectives, such as maintaining landscape beauty, recreation value, or ecosystem functioning (Price unpubl., Edwards-Jones et al. 1995, Schwartz et al. 2000). Thus, when Balmford et al. (2002) attempted to carry out a meta-analysis of the economic benefits of biodiversity conservation (i.e. maximising species richness), they had to abandon the task, and instead evaluate the value of conserving "wild nature" since no studies provided sufficient evidence of a linkage between species richness and the benefits they valued (Andrew Balmford,

pers. com.) Even then, they found only five suitable case studies, none of which concerned biodiversity conservation in low income countries.[160]

Nevertheless, especially in the international realm, the focus of nature conservation has become increasingly dominated by the concept of biodiversity, and work on optimising conservation planning tends to emphasise maximising global or national species richness (e.g. Wilson et al. 2006, Underwood et al. 2008).[161] Although such planning exercises have begun to include the economic costs of biodiversity conservation, the benefits are still usually measured in numbers of species conserved (Naidoo et al. 2006).

There are several problems with this approach. First, even if we accept that the proper objective of conservation is maximising species richness, the cost-effectiveness of achieving this goal can only be measured if benefits (number of species) are divided by *net* costs, i.e. costs (e.g. of land purchases) minus other benefits (e.g. of carbon sequestration). More fundamentally, using species richness as a proxy for conservation benefit is deeply problematic, for the reasons noted above: it is not clear why society should consider this to be an over-arching goal (Hockley et al. 2007).

Finally, because of the focus in international conservation on maximising the number of species saved per dollar of cost, the case is repeatedly made (e.g. Pimm et al. 2001, Mittermeier et al. 2004) that conservation efforts should be focussed in the tropics, which have relatively high levels of species richness, and relatively low costs of conservation (measured in dollar terms). However, because the relationship between species richness (wherever and whatever the species may be) and social benefit is uncertain, and because the costs of conservation tend to be

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[160] Using the World Bank's classification (World Bank 2005). One study concerned a high-income country (Canada), one upper-middle (Malaysia), and three lower middle income countries (Cameroon, Philippines and Thailand); no studies were found from low-income countries.

[161] Note, however, that when conservation seeks the support of governments, biodiversity is often defined much more broadly, to include, for example, ecosystem function (for example, the Convention on Biological Diversity defines biodiversity loss as "the long term … reduction in components of biodiversity *and their potential to provide goods and services*" Molnar et al. (2004), emphasis added.

measured using a monetary, rather than welfare numeraire, the social value of this focus on developing countries is highly uncertain.

In this case study I largely assume that there are benefits to society from avoiding species extinctions (see Chapter 9), and focus instead on the second issue, the problems associated with measuring conservation costs and benefits in monetary terms, and the implications of this for the value of conservation.

II. Study area and policy context

Madagascar

The island of Madagascar formed part of Gondwanan super-continent, splitting from mainland Africa, south America, Antarctica, Australia and India between 160 and 30 million years ago (Upchurch 2008). Its Gondwanan origins and long isolation gave Madagascar a diverse and unique biodiversity, with exceptional levels of endemism (Myers et al. 2000, Ganzhorn et al. 2001, Brooks et al. 2002). For example, 75% of mammals and 80% of flowering plants are found nowhere else and the country has more endemic families of plants and animals than any other on earth (Mittermeier et al. 2004).

People first arrived in Madagascar nearly 2000 years ago having travelled westwards across the Indian ocean via East Africa (Burney 2003). The Malagasy language is the westernmost member of the Malayo-Polynesian branch of the Austronesian language family (Wittmann 1972), and is spoken throughout the island (though strong regional dialects exist). It is unrelated to nearby African languages, although it includes many borrowings from Bantu languages, as well as Arabic, French and English. Despite the strong Austronesian influences on Malagasy culture and language, recent studies suggest that the Malagasy derive over 60% of their genetic makeup from Africa, reflecting Bantu immigration in addition to Austronesian settlers (Regueiro et al. 2008). Compared with many African countries, the island is ethnically relatively homogenous. Although the Malagasy have been classified into twenty or more ethnic divisions, or *foko*, ethnographers have argued that these divisions are relatively recent (Kottak 1971), while anthropologists note that they are better seen as relatively flexible culturally

and economically adapted groups rather than tribes (e.g. Harper 2002). Certainly, while most Malagasy would self-identify as belonging to one or more *foko*, there are considerable cultural similarities between groups, whose dialects are more or less mutually intelligible, and migration and inter-marriage between groups is quite common, although *tanin-drazana* (land of one's ancestors) remains important.

In pre-colonial times, several Malagasy rulers established control of large parts of the island, most notably the kings and queens of the Merina, who established a centralised state centred on the present-day capital Antananarivo, in the central highlands. Considerable interaction between Madagascar and the west occurred during the 18th and 19th centuries, particular with Great Britain and France. The latter invaded Madagascar in 1883 and again in 1895, annexing it in 1896. An uprising in the south east (including the study area) in 1947 was brutally repressed, and Madagascar remained under French control until 1958, when it became an autonomous state in the French community, full independence following 2 years later (Brown 2001).

Since independence, Madagascar has had five presidents and just one peaceful handover of power. Serious internal conflict has been mercifully rare compared with many African countries, but repeated political crises, together with poor governance and frequent cyclones have taken their toll on the Malagasy economy, which has shrunk by over 40% since 1960 (Heston et al. 2006). Today Madagascar is one of the poorest countries in the world, with a GDP per capita in 2003 of $758.95,[162] and 73% of the population live in rural areas (Heston et al. 2006, UNDP 2006).

The twin challenges of conservation and development in Madagascar

As in many other island regions of the world (Miller et al. 1999, Barnosky et al. 2004), the arrival of people on Madagascar was followed by a wave of extinctions among the megafauna (MacPhee & Burney 1991, Burney et al. 2003). For

[162] GDP is adjusted for Purchasing-Power Parity.

example, 17 species of large lemur disappeared in the late quaternary, probably due primarily (directly and indirectly) to man's arrival (Godfrey & Jungers 2003, Perez et al. 2005). As well as leading to extinctions among the fauna, the arrival of humans resulted in major changes in vegetation across much of the island, caused by a combination of increased fire frequency and active clearance of land for agriculture and grazing (Burney 2003).

Land conversion has continued into colonial and post-colonial times and is the result of a combination of complex drivers (Jarosz 1993, Casse et al. 2004). Swidden agriculture (known as *teviala* in the study area) is practised in many parts of the country but commercial timber extraction, mining and fuelwood collection also contribute to forest loss and degradation (Durbin et al. 2003). Hunting (Garcia & Goodman 2003, Goodman 2006, Jenkins et al. 2007, Dunham et al. 2008) and collection for the pet trade (O'Brien et al. 2003, Walker et al. 2008) also represent important threats to some species.

Madagascar's exceptional endemism, combined with high past and present rates of natural habitat conversion (Green & Sussman 1990, Consigilo et al. 2006, Harper et al. 2007), led Mittermeier et al. (2004) to recognise Madagascar and the other Indian Ocean Islands as one of the "hottest of the biodiversity hotspots" and therefore one of the world's highest priorities for biodiversity conservation.

The co-incidence of extreme poverty and biological richness presents a huge challenge to the Government of Madagascar and the international community, who are committed to both the Convention on Biological Diversity (CBD) and the UN Millennium Development Goals (MDG). The objectives of these include reducing the rate of biodiversity loss by 2010 and halving extreme poverty by 2015 respectively.[163] These aims frequently conflict, since large populations of poor people, highly dependent upon natural resources (Ferraro 2002, Jones et al. 2006), surround many areas of high conservation interest. This challenge is not unique to Madagascar, but is faced in many developing countries. To date, success in combining these two agendas has been rare elsewhere in the world (Adams et

[163] See www.biodiv.org for the CBD and www.un.org/millenniumgoals for the MDG.

al. 2004), and efforts to combine conservation and development in Madagascar have also been criticised (see Peters 1998, Ferraro 2001, Harper 2002, and below).

The history of conservation in Madagascar

Malagasy culture is rich in traditions and taboos (*fady*), many of which have been credited with offering some protection to certain species and habitat patches (Tengo et al. 2007, Jones et al. in press a). However, despite the value of these traditional environmental protection institutions (Colding & Folke 2001), it is naïve to imagine traditional people as "noble savages" living in simple harmony with nature (Buege 1996, Alvard 1998). As shown above, throughout their history the Malagasy, like all other societies, have substantially altered their natural environment.

The first protected areas in Madagascar were established by the French colonial government in the 1920s (Randriananadranana et al. 2003), and the first national parks were established in the early 1950s. Throughout the island, colonial and post-colonial governments made repeated and generally unsuccessful attempts to control *teviala* and to regulate the use of forest resources over the last century (Kull 1996, 2004). Many of these failed, either because of active community resistance, or the sheer scale of the task of monitoring and enforcing centrally imposed regulations across large areas of forest (Kull 1996, 2004).

The major internationally-funded conservation efforts of the modern era began in 1985, with the development of Madagascar's first National Environmental Action Plan (Hannah et al. 1998), which had a broad environmental focus, not limited to biodiversity conservation. However, the second environmental plan (1995-2000) was more focussed on biodiversity conservation,[164] and expanded the system of protected areas (e.g. Kremen et al. 1999, Randrianandianana et al. 2003, Figure 8.1). Many of these protected areas were based on the model of Integrated Conservation and Development Projects (ICDPs).

[164] This was the result of effective campaigning in the United States, by the 'big four' conservation organisations: WWF, Conservation International, The Nature Conservancy and The Wildlife Conservation Society (Corson 2007).

Although rigorous evaluations are surprisingly rare, protected areas in Madagascar appear to have had some success in reducing rates of forest conversion and preventing illegal mining and timber harvesting, though their impact has been highly variable (Sommerville 2005, Dollar 2006). Their success in reducing less visible threats to biodiversity, such as small-scale extractive uses (including hunting), remains unquantified and is probably less impressive: certainly considerable illegal activity continues within protected areas (Adany et al. 1994, Garcia & Goodman 2003, Jones et al. 2005, 2006). They have been criticised as being top-heavy (Peters 1998) and inefficient due to the weak linkages between biodiversity conservation and rural development (Ferarro 2001). Many authors have criticised their negative impact on local livelihoods (Marcus 2001, Harper 2002, Ormsby & Kaplin 2005), due to the opportunity costs of conservation (Shyamsundar & Kramer 1997, Ferraro 2002), and there is some evidence that they have undermined existing resource management institutions (Rabesahala Horning 2003, Jones et al. in press a).

During the third environmental plan (2001 onwards), there has been an increasing emphasis on community-based natural resource management (CBNRM). CBNRM attempts to reconcile conservation and development by exploiting the assumed synergies between them (Adams & Hulme 2001, Hockley & Andriamarovololona 2007). In Madagascar, CBNRM has been promoted through the policy of *Transfert de Gestion des Ressources Naturelles Renouvelables* or simply *Transfert de Gestion*[165] (Erdmann 2003b, McConnell & Sweeny 2005). Through *Transfert de Gestion*, the state delegates limited tenure and sustainable use rights (sometimes including commercial timber harvesting) to a legally recognised local community institution (Erdman 2003).[166] However, implementation of this policy has frequently been hurried (Josserand 2001), and both NGOs and the government have underestimated the level of support required by communities in order to fulfil their contracts, which are externally defined. This has compromised the

[165] lit. “Management transfer of renewable natural resources” or “management transfer”

[166] *Transfert de Gestion* has been implemented using two legal instruments: the GELOSE law (*Gestion Locale Sécurisée*: secured local management) and later GCF (*Gestion Contractualisée des Forêts* : Contractualised Forest Management).

long-term viability of the community institutions (Hockley & Andriamarovololona 2007).

In 2003, President Marc Ravalomanana announced at the World Parks Congress in Durban, South Africa, that Madagascar intended to triple the size of its protected areas (Norris 2006). In order to fulfil this "Durban Vision", the Government of Madagascar identified Nouvelles Aires Protégées (New Protected Areas) which would cover much of the remaining area of natural forest not already protected (Kremen et al. 2008, Figure 7.1).

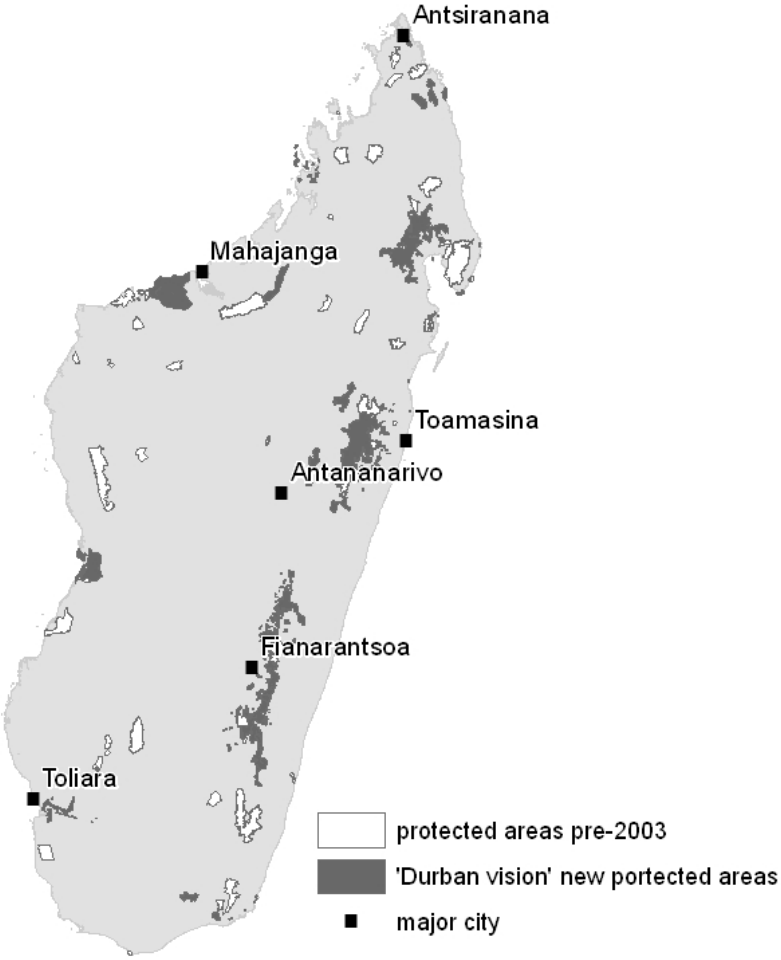

Figure 7.1. The protected areas of Madagascar prior to 2003 and the proposed expanded network (data provided by the Durban Vision group, Antananarivo, July 2007).

The Ranomafana-Andringitra New Protected Area

One area included in the Durban Vision was a corridor of natural forest in southeastern Madagascar that runs along the escarpment between Ranomafana National Park in the North, and Andringitra National Park and Pic D'Ivohibe

Special Reserve in the south (Figure 7.2). Long recognised as important for biodiversity (Goodman & Razafindrasita 2001), the area was formally identified as a candidate for a new protected area by a biological priority-setting workshop held in Fianarantsoa in January 2005.[167] The workshop extended the area traditionally considered as the Ranomafana-Andringitra corridor to include the contiguous forest as far as Fandriana in the north and Vondrozo in the south. Note that, as described below, the analysis in this and subsequent chapters was restricted to the Ranomafana-Andringitra corridor, hereafter "the corridor".

The corridor area is relatively densely populated, with most of the forest frontier communes having over 20 persons per km^2 (Minten et al. 2003: map 1.2). Most people in the area self identify as either Betsileo (on the western side), or Tanala (on the eastern side), with some Bara to the south (Minten et al. 2003: map 1.15; pers. obs.).

Biogeographically, the corridor is situated within the humid forest biome (see Goodman & Benstead 2003). It is an important refuge for biodiversity (Goodman 1996, Goodman & Razafindrasita 2001) and, together with the three protected areas it links, boasts two endemic primate species[168] and contains a wide range of rare and endemic flora and fauna. However, like most of the remaining forest in Madagascar, it is threatened by conversion for small-scale agriculture, timber extraction and mining (Freudenberger 2003). On 15 September 2006 the ministers of the Environment, and Energy and Mines signed an inter-ministerial order[169] giving temporary protection to the area, prior to it being declared a full protected area in due course.

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[167] L'Atelier Scientifique de Planification: Corridor Forestier Ranomafana – Andringitra – Pic d'Ivohibe. 17-20 January 2005. Alliance Ecorégionale (2005).

[168] The Golden Bamboo Lemur (*Hapalemur aureus*) and the Greater Bamboo Lemur (*Prolemur simus*, H. Randrianasolo pers com.)

[169] Arrête Interministériel No. 16.071 -2006/MINENVEF/MEM Portant protection temporaire de l'aire protégée en création dénomme « Corridor Forestière Fandriana-Vondrozo ».

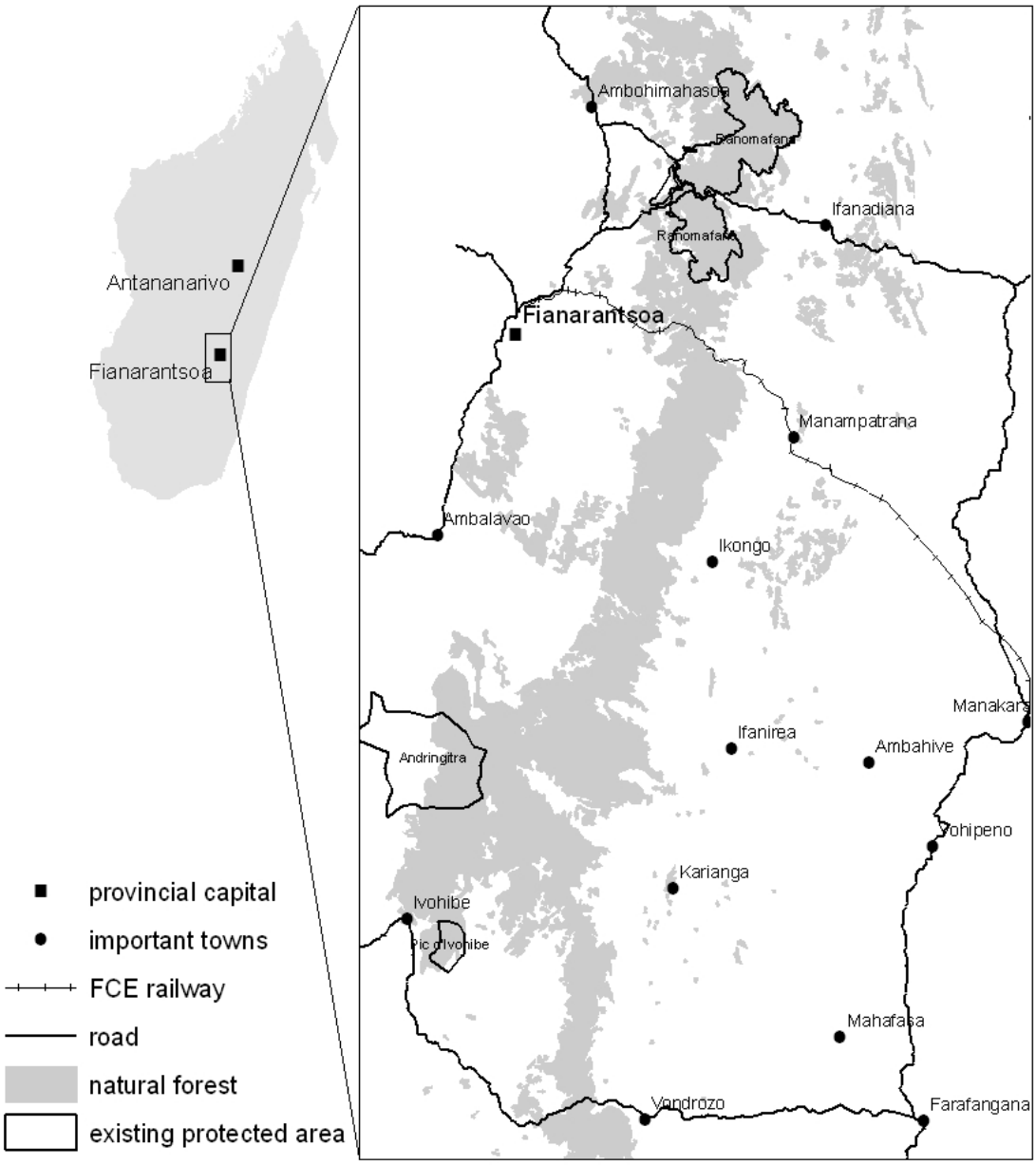

Figure 7.2. Map of the Ranomafana–Andringitra Corridor showing the existing protected areas.

III. Analytical Framework

Any CBA consists of comparing one or more projects with a non-project 'business-as-usual' case. The projects are compared on the basis of the costs and benefits they produce over a given time horizon. In the case of social cost-benefit analysis, and in the approach developed in chapter 2, the effects of the project are disaggregated by stakeholder groups. These groups may be based on geographical location, income, ethnic group, gender etc, but there are good reasons for using subdivisions which are relatively homogenous in income and in the way in which they are affected by the project. The definitions of the project scenario and the groups of beneficiaries, together with a list of potential benefits of the project form the analytical framework of the CBA.

CBA is not usually a very participatory decision-making tool: it is normally analyst driven, and most stakeholders in the project will not contribute in any direct way to the analysis.[170] Chapter 2 has already discussed in detail the problems with this approach to CBA, most of which relate to aggregation. Yet even in the earlier stages of a CBA, the analyst must make judgements that will affect the results. They will need to amass a great deal of information on a wide range of benefits, much of which will be vague, uncertain, or missing completely. The analyst is forced to decide which value is most plausible, or even to assume a particular value. Most crucially, the framework of the analysis will determine the results: if the identification of potential benefits and beneficiaries is incomplete, or if the project and non-project scenarios are not suitable, the analysis will be flawed. Therefore, although CBA aspires to objectivity, there is often considerable scope for 'observer-bias' to affect the results, even prior to aggregation.

In this section, I describe how the analytical framework was developed for the Ranomafana-Andringitra case study. The original cost-benefit analysis based on this framework (Hockley & Razafindralambo 2006) considered almost all of the costs and benefits and stakeholder groups identified. However, it used a relatively

[170] That said, some stakeholders, particularly if they commission the analysis, may have disproportionate influence over the results.

conventional approach (for example, CVs remained uncorrected, while a social discount rate was used to aggregate net benefits over time) although great emphasis was placed on the distribution of benefits. The experience of carrying out this analysis motivated much of what is described in this thesis. In order to concentrate on a manageable number of key issues, the analysis presented in the following chapters is less extensive than the original analysis, in terms of the costs and benefits and stakeholder groups considered, the reduction breadth allowing an increase in depth. In what follows I highlight the simplifications made.

Stakeholder consultation

In April 2005 I organised a workshop in the provincial capital of the study area (Fianarantsoa), funded by the US Agency for International Development, to enable a wide range of stakeholders to discuss the socio-economic implications of the proposed Protected Area.[171] Prior to organising this workshop, I had spent three years in Madagascar studying forest use by local people and was fluent in Malagasy. Around 80 delegates attended the workshop, including representatives of local communities, NGOs and government agencies, as well as independent researchers Table 7.1.

[171] 26-28th April 2005, Soafia Hotel, Fianarantsoa.

Table 7.1. The organisations and institutions represented at the stakeholder workshop held in Fianarantsoa April 2005.

Institution	Number of representatives	Explanation
l'Association Nationale pour la Gestion des Aires Protégées	4	National quasi-governmental body with authority for protected areas
Direction de l'Environnement des Eaux et du Forêts	6	Government department with authority for all forests in Madagascar
Regional government	4	Representatives of each of the four regions (large administrative unit of government) covering the study area
Local government (commune)	6	Mayors of some of the communes in the study area, or their representatives
Donor-funded projects	11	USAID, World Bank, UN Environment Programme and UN Development Programme projects
Community representatives	5	Local leaders from some of the forest-frontier communities
National NGOs	5	e.g. SAGE (Service d'Appui à la Gestion de l'Environnement) and Ny Tanintsika
International NGOs	14	Worldwide Fund for Nature, Conservation International, Wildlife Conservation Society
National researchers	10	Researchers from Malagasy universities and research institutions
International researchers	13	Researchers from universities and research institutes in France, USA and the UK.
Business	4	Representatives of ecotourism and timber extraction businesses

The central feature of the workshop was the breakout groups that each considered a different part of the socio-economic impact of the proposed protected area. In addition, presentations on a variety of subjects, from the hydrological effects of deforestation to the policy context of carbon sequestration, were followed by question and answer sessions. The results of all group discussions were presented back to the whole group, both verbally and visually, allowing everyone to comment on the results. The workshop was carried out through the medium of Malagasy, with translation into French for the small number of non-Malagasy participants who did not speak the language.

The aim of the workshop was to tap into the considerable knowledge and expertise concerning potential economic impacts of conservation in the corridor which would not otherwise be available. Key outputs from the workshop were:

1) agreed definitions of the project and status quo scenarios, i.e. descriptions of the form which the new protected area was likely to take, as well as the most likely business-as-usual scenario in the absence of the protected area;
2) identification and sub-division of the stakeholders who might be affected by the proposed protected area;
3) a comprehensive list of potential market and non-market benefits of the protected area;
4) a wealth of expert information, particularly on the hydrological aspects of forest protection, and timber harvesting in the region, which would feed into the calculations of benefit flows.

I used this list of benefits and beneficiaries to define the scope of the analysis and followed-up and cross-checked expert knowledge and data gathered during the workshop using interviews with key respondents in Madagascar between April and October 2005.

The Project Scenario

The cost-benefit analysis was designed to evaluate the desirability of conserving the forest corridor by placing it under the formal protection of a New Protected Area. It did this by comparing two alternative scenarios:

Business-as-usual scenario. Deforestation and extractive activities continue as they would in the absence of protection.

Project scenario. A new protected area is established, composed of a core of strictly protected forest, surrounded by a peripheral zone in which certain extractive uses might be permitted.[172]

[172] At the time of the workshop it was unclear whether and where commercial timber harvesting would be allowed within the new protected area, nor was it clear what other extractive activities would be allowed, and under what circumstances (Jean Razafitsotra pers. com.). At the time of writing matters are no less unclear: the arrête declaring the corridor's temporary protection does not precise the future role of commercial timber harvesting in the area, and the role of community forest management is also unclear. Hockley & Razafindralambo (2006) evaluated a number of different options.

On the advice of workshop participants, the study was limited to the Ranomafana-Andringitra portion of the corridor, for reasons of data availability. The two national parks at either end of the corridor, Ranomafana and Andringitra, together with the Special Reserve of Pic d'Ivohibe, are already protected and managed by ANGAP (l'Association Nationale pour la Gestion des Aires Protégées), the National Parks service. Although the project scenario will affect only the currently unprotected corridor the analysis considers the New Protected Area in the corridor together with the parks, and compares this with a situation where no parks, reserves or New Protected Areas were present.

Stakeholders

The workshop participants identified the following groups of stakeholders as useful subdivisions for the analysis. The aim was to identify groups which were quite homogenous in income and in the way that they will be affected by the protected area, but they were also defined partly out of convenience: matching administrative units for which demographic data is available. The groups are:

1) residents of *fokontany*[173] bordering the forest;
2) residents of communes[174] bordering the forest (excluding the previous category);
3) residents of the rest of Madagascar;
4) people of all other countries in the world.

Stakeholders considered in this thesis

The analysis presented in the following chapters will consider just two stakeholder groups. First, Group 1 above, the "local" group will be most directly affected by the project and their CVs will be largest as a percentage of income (Hockley & Razafindralambo 2006, and Chapter 10). Second, all other

[173] *Fokontany* are the lowest unit into which communities are organised by the government. Each *fokontany* contains one or a few villages and *fokontany* populations usually range from 500 to 3000 inhabitants.

[174] Communes form the next level of administration up from *fokontany*. Each commune contains up to 30 *fokontany*.

stakeholder groups are combined into one, predominantly international group. This large and heterogeneous group is justifiable only in terms of the limited range of costs and benefits considered (below).

Potential costs and benefits

The workshop participants identified many potential costs and benefits of the protected area that should be assessed as part of the analysis. These can be divided into three categories: opportunity costs, resulting from the cessation of activities; direct costs of establishing and managing the protected area; and benefits from the creation of the protected area. These are summarised in Table 7.2.

Table 7.2. Potential costs and benefits of the New Protected Area identified by participants.

Opportunity costs
All forest conversion, including teviala, will be strictly prohibited, reducing agricultural incomes.
Non-timber forest product exploitation may be prohibited or regulated in certain areas, which may reduce harvester incomes.
All logging will be strictly prohibited, reducing employment opportunities in rural areas, as well as profits for loggers.
Mining will be prohibited, reducing employment and profits. Note, however, that mining has environmental costs for communities on the edge of the forest.
Direct costs
Managing the protected area requires capital and skilled labour, taking scarce resources away from other sectors of the economy.
Benefits
The ecotourism potential of the corridor should be enhanced through protection.
Reduced forest clearance may bring hydrological benefits, e.g. reduced flooding.
Reduced deforestation may avoid greenhouse gas emissions compared to the business as usual scenario, thus reducing future anthropogenic climate change
Genetic resources will be conserved by avoiding extinctions due to deforestation.
Non-timber forest product harvesters may benefit from the establishment of no-harvest zones (which may increase yields), and from the cessation of logging and teviala.
Existence values of biodiversity will be higher in the project scenario, since deforestation will be reduced.[175]

[175] See chapter 8 for more details of deforestation rates.

Costs and benefits considered in this thesis

Table 7.2 identified ten potential costs and benefits of conservation, which a comprehensive CBA should address. However, in the CBA that follows I focus on two of these, which Hockley and Razafindralambo (2006) found to be among the most important: the international non-use values (Chapter 9) and the local opportunity costs through lost opportunity for agricultural expansion (Chapter 10).

I ignore the potential foregone revenues from timber harvesting, since it is uncertain as to whether it will be allowed in Madagascar's new protected areas and because of the great difficulty of projecting future timber prices (Price 1989). In addition, the impacts of timber harvesting on biodiversity and carbon sequestration, as well as the local benefits it might generate, are extremely dependent on the manner in which it is carried out (Ganzhorn et al. 1990, Healey et al. 2000, Hockley & Razafindralambo 2006), and therefore the effect of including timber harvesting on the results of the CBA is likely to be somewhat indeterminate. Since some form of timber harvesting is potentially compatible with both project and non-project scenarios, it is probably be best to evaluate its desirability on a case by case basis.

Similarly I do not consider mining. At present, there is no large-scale mining being carried out in the study area, although many mining permits remain extant, and there is some small-scale artisanal mining.[176] The potential benefits of mining require detailed geological data, which is not publicly available, and the local benefits are somewhat indeterminate, as with timber they depend on the manner by which the mining is carried out.

Protecting forest cover may have hydrological benefits, such as reduced flooding or improved dry season flows. However, these are rather uncertain (Bruijnzeel 1990, 2004) and remain controversial (Bradshaw 2007, Laurance 2007a, Calder et al. 2007) and their economic value has often been overstated (Chomitz & Kumari 1996, 1998).[177] The key point from this literature is that the hydrological effects of

[176] M. Freudenberger (pers. com.)

[177] This literature is reviewed with reference to the corridor by Annis & Hockley (2006).

deforestation depend heavily on what replaces the forest, with some land uses like long-fallow cropping, providing much of the same benefits as old-growth forest cover. In many cases, the worst hydrological effects could be substantially avoided by actions other than forest protection, which might have much lower opportunity costs.

Finally, forest clearance will tend to lead to net emissions of greenhouse gasses, though once again, the quantity of emissions will depend greatly on the vegetation which replaces the forest, and is extremely difficult to estimate (Fearnside 2000, Schimel 1995, Ramankutty et al. 2007). Nonetheless, net-emissions will almost certainly be higher in the non-project scenario, and including this benefit would probably increase the international benefits of conservation. The effect on the distribution of benefits from the project would depend on how greenhouse gas emissions are priced. The negative impacts of climate change are expected to be greatest in low latitude developing countries (Mendelsohn et al. 2006, Tol 2002); therefore, pricing greenhouse gas emissions based on their impacts would shift the benefits of forest conservation to poorer countries. If, on the other hand, the responsibility for curbing emissions lies with the rich countries (who have contributed by far the greatest quantity to date), as it effectively does under the Kyoto protocol (Bettelheim & D'Origny 2002), then higher emissions in developing countries implies greater cuts in the rich countries, shifting the benefits of forest conservation back to the rich countries. Finally, if Kyoto is replaced by a system which curbs the emissions of all countries (perhaps on a per capita basis), and allowed trading of unused quotas, increased green house gas emissions from clearing Madagascar’s forests will imply a direct cost to Madagascar, in quotas which can no longer be sold. This is yet another illustration that, once the arguments for abandoning conventional costs benefit analysis are accepted, the results of any CBA depends on ethical judgments about the allocation of rights (see Chapters 2 and 11).

Time horizon

Like most environmental projects, this one has impacts that are likely to persist for centuries, perhaps indefinitely. Ideally, the project should be evaluated over an infinite time horizon. However, the problem is that the longer the time horizon,

the greater the uncertainty over benefit flows, and fundamental variables such as population sizes. Although some approach must be found to deal with this issue (Price 1993, Portney & Weyant 1999a), I limit my analysis to a time horizon of 100 years (2005 to 2105).

IV. Income and population projections for Madagascar

The previous chapter developed population and income projections for four regions of the world: OECD90, ASIA, ALM and REF; into which the international community will be divided for the purposes of this case study. Here I develop income and population projections for Madagascar as a whole, and hence for the local stakeholder group.

National demographic projections

The only available country-specific population forecasts are those of the United Nations Population Division (UNPD), however these extend only to 2050 (UNPD 2007). The most recent long term projections are those of Lutz et al. (2001), which extend to 2100. However, the lowest unit of aggregation used by Lutz et al. is 'sub-Saharan Africa'. Madagascar differs from most of sub-Saharan Africa, in that it currently has a relatively low rate of HIV (around 1%) despite high rates of other sexually transmitted infections (Behets et al. 2001) – the so-called "Indian Ocean Paradox" (Dada et al. 2007). This might result in Madagascar following a higher population growth trajectory than the rest of Africa. On the other hand, population density affects growth rates (Lutz et al. 2001, 2006) and that in Madagascar is equal to the average for sub-Saharan Africa (28 km^{-2}). Figure 7.3 compares UNPD high, medium and low variant forecasts for Madagascar with a projection that assumes that Madagascar grows at the same rate in each year as predicted by Lutz et al. (2001) for sub-Saharan Africa as a whole. Lutz et al's sub-Saharan trajectory is similar to, though slightly below the UNPD's low variant. Some of the difference may be due to UNPD assumptions about fertility rates which Lutz (2006) has criticised, while the rest may be due to Madagascar-specific factors influencing the country's UNPD predictions, like those discussed above. Since the case study requires projection to 2105, I use the projection based on Lutz et al. (2001), but note that this may be an underestimate.

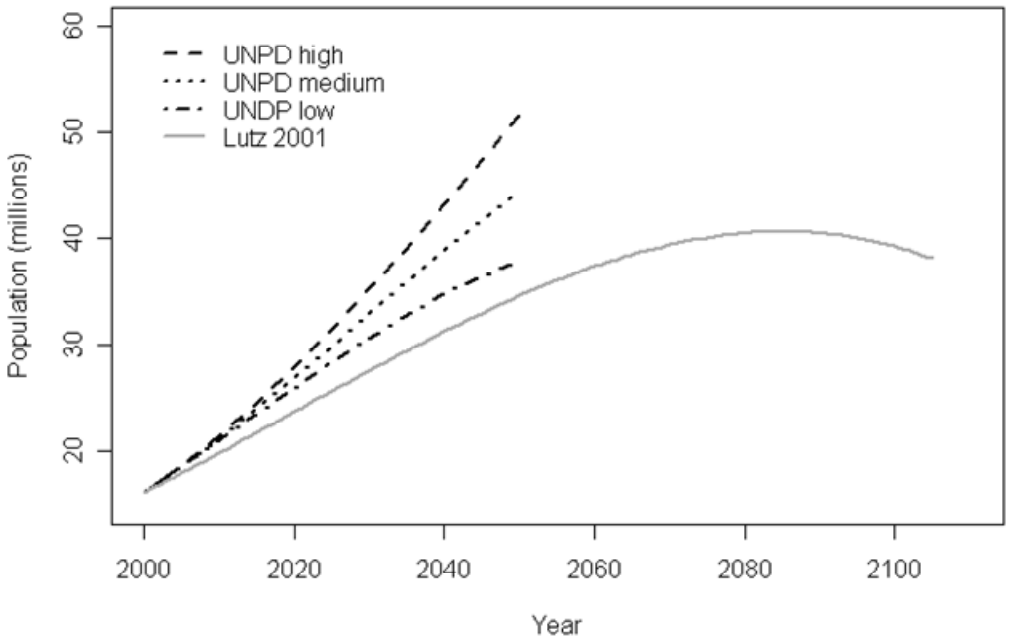

Figure 7.3. Alternative population projections for Madagascar. In grey are the high, medium and low variants from UNDP (2006) and in black, a projection derived by assuming that Madagascar follows the same trajectory forecast for sub-Saharan Africa as a whole, by Lutz et al. (2001).

National Income Projections

Madagascar's GDP per capita in 2003 was $758.95 (PPP adjusted, Heston et al. 2006), which is well below the average for the ALM region of around $3,700, and makes it one of the poorest countries in the world. The question is then, how will Madagascar perform relative to the rest of ALM? Chapter 4 reviewed the rather ambiguous literature on convergence and growth in developing countries, finding some evidence for convergence among "clubs" of similar countries, but weak or no evidence of more general convergence (Islam 2003), though this may partly be due to a failure to consider non-linear models. Africa-specific evidence on convergence is scarce. Jones (2002) finds an annual rate of convergence of 1.7% within the Economic Community of West African States, broadly in line with the approximately 2% per annum reported by numerous studies covering a wide range of different countries and groups (see Islam 2003, Abreu et al. 2005). However, McCoskey (2002) finds "little evidence ... to substantiate claims of convergence across Africa, although in some cases, smaller convergence clubs within Africa may be found".

Madagascar's economic performance has been very disappointing for most of the last four decades. After some growth during the first decade post-independence, it has declined steadily since 1971 (Figure 7.4a). Its performance relative to the ALM region has also been poor: income levels in Madagascar have diverged steadily over the period, from around two-thirds of the ALM average in 1960, to around a fifth in 2003 (Figure 7.4b).

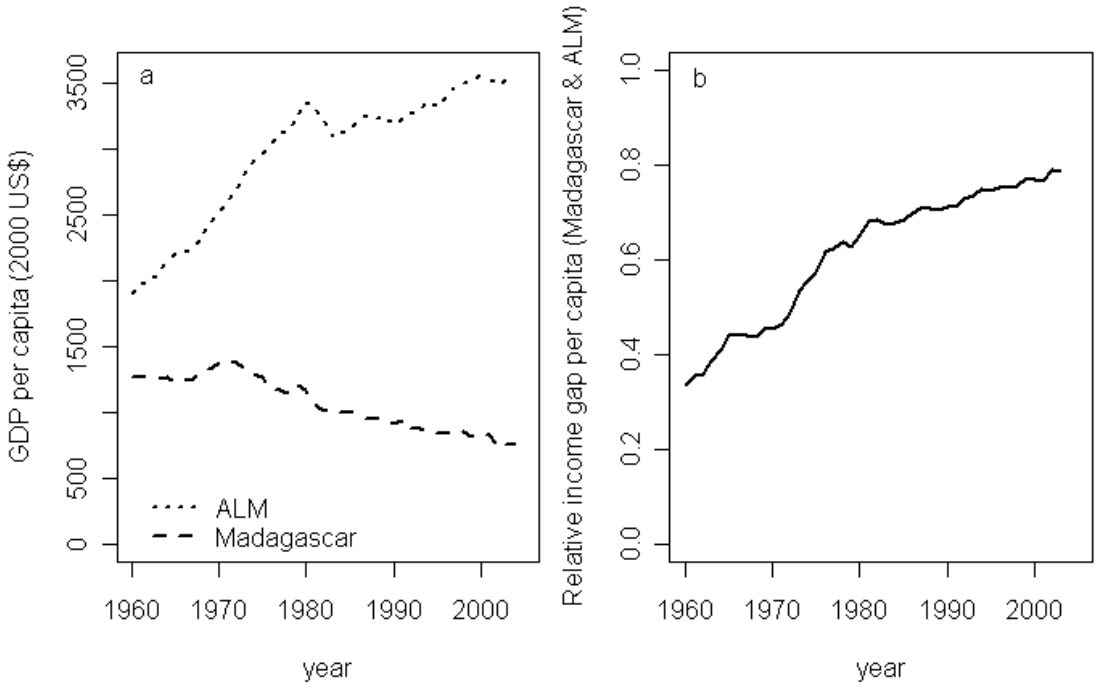

Figure 7.4 a) Time series of Gross Domestic Product (GDP) per capita for Madagascar (dashed) and ALM (Africa, Latin America & Middle East) region (dotted). b) relative gap in income level between Madagascar and ALM. Data from Heston et al. (2006).

In order to represent the uncertainty over Malagasy economic performance in the simplest manner, I assume that Madagascar converges on the optimistic ALM projection at a rate of between -0.1% year^{-1} and 2% year^{-1}. These upper and lower bounds result in Malagasy national income levels of $4,706 and $33,782 per capita respectively (these and all subsequent figures are quoted in year 2000 international dollars).

Domestic income distribution

By international standards, Madagascar has a fairly unequal income distribution with a Gini index[178] of 47.5, similar to that of Mexico, and the poorest 10% of Malagasy enjoy just 1.9% of national income (UNDP 2008). Reliable data on the sub-national distribution of income in Madagascar is not easily available. However, INSTAT (2002) and Minten et al. (2003) indicate that the Ranomafana-Andringitra corridor region is one of the poorest in Madagascar, and poverty tends to correlate with remoteness (Jacoby 2000).

I assume that the average income in the local communities is that of the lowest 10th percentile, i.e. 1.9% of national income. This implies a mean income in the local communities of $156, in the year 2000.[179] Within this group there is a great deal of variation around the mean, which I ignore.

Kuznets (1955) proposed that income inequality has an inverted-U shaped relationship with income, and that industrialisation initially increases and then decreases the share of income going to the richest percentiles, and he provided both cross-sectional and time-series data to support this (Kuznets 1963). If this were reliably the case, we might expect Madagascar's income distribution eventually to become more equitable as it grew richer, possibly after an initial increase in inequality.

Since Kuznets proposed the hypothesis, however, increasing inequality has accompanied economic growth in several developed countries (Aghion et al. 1999) and the Kuznets curve hypothesis has been challenged by many subsequent analyses (Moran 2005). It is clear that there is no simple relationship between

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[178] The Gini index measures the degree to which a country's income distribution diverges from perfect equality (Anand & Segal 2008). The higher the number, the more unequal the income distribution, with 0 corresponding to perfect equality and 100 to perfect inequality (one individual has all of the income). Gini indices range from 24.7 (Denmark) to 74.3 (Namibia), Madagascar is ranked 97th out of 123 countries (UNDP 2008). All of the highest Gini indices are found in developing countries, though the USA has a Gini index comparable to that of Madagascar (40.8).

[179] By way of comparison, Shyamsundar & Kramer (1996) estimate household incomes in Mantadia region at US$279 in 1991-1992 (non-PPP adjusted). This $253 per capita (PPP adjusted). Incomes in Madagascar have fallen on average by around 12% from 1991-2000, making this around $220 in the year 2000. However, Minten et al. (2003: map 1.12) suggest that agricultural incomes in this region are higher than in the study area. This suggests my illustrative figure is approximately accurate.

economic growth and income inequality: inequality may dampen growth, yet growth may worsen inequality (Aghion et al. 1999). Furthermore, "the extent to which the growth process actually induces rising inequality depends on the institutional characteristics of each country." (Aghion et al. 1999:1654). Madagascar's income distribution should ideally be projected explicitly in the same way as that between regions or countries. However, given the poor baseline data, and the weak and somewhat indeterminate (or at least complex) relationship between growth and inequality, I simply assume that the share of national income going to each percentile of the income distribution remains unchanged by time and economic growth.

Domestic population distribution and urbanisation

Minten et al. (2003) provide baseline population estimates for forest frontier communes. Unfortunately, population figures are not easily available below the commune level. The proportion of the corridor communes' populations living in *fokontany* bordering the forest (and hence in the "local communities" group) was estimated from those communes for which I was able to collect *fokontany* population figures first hand.[180] I then calculated a weighted average of these proportions and applied it to the total corridor population to give a corridor-wide estimate of 35% (range 14-71%), based on figures from five communes.

To project local communities' populations over time, I first assume that the project has no effect on local populations, i.e. that local population growth is exogenous to the project. In practice, the effect of a protected area on local population growth is unlikely to be neutral. ICDPs that succeed in providing benefits to people living around the protected area may lead to immigration of people seeking to tap into those benefits (e.g. Noss 1997, Schulte 2003). On the other hand, rigorously enforced protected areas, which did not compensate local

[180] I visited several of the relevant communes and in five (Maromiandra, Ambatofotsy, Ambinanitromby, Ambohimahamasina, Ambolomadinika) was able to copy down locally-held *fokontany* population figures from the commune offices. Unfortunately, these figures are not held at any higher level, and many communes did not keep records for *fokontany* level population figures. *Fokontany* were defined as bordering the forest using first hand observation of the presence of forest, combined with discussions with commune and *fokontany* officials, to determine the position of *fokontany* boundaries.

residents might conceivably lead to emigration from the area, as people move to seek opportunities elsewhere (Ewers & Rodrigues 2008). However, in a country with stagnant economic growth, and rising populations, there may be few opportunities for internal migration. Some authors have even suggested that protected areas might increase mortality and morbidity directly, by reducing food security and the incomes of local people, leaving them vulnerable to disease and less able to purchase treatment (Harper 2002). In principle, if compensation was targeted specifically at those resident in the area prior to the park's establishment, the effect on local population might be neutral, though defining those with a claim on the compensation would be difficult (a subject I discuss in more detail in chapter 11). Even if this is the case, some protected areas might seek to lower fertility rates in the local area, through improved provision of family planning services, again leading to local population growth rates that differ between project and non-project scenarios. Thus, the effect of a new protected area on population growth is indeterminate, and is highly dependent on the manner in which the protected area is established, in particular the nature of any compensation provided to local residents. For simplicity, I assume that the project scenario is neutral with respect to local population growth.

If it is assumed, furthermore, that the local community population follows the same trend as rural populations nationally, projecting local population sizes through time can be made using a projection of urbanisation in Madagascar. It is known that there is a positive relationship between the percentage of a country's population that is urban, and its income level; with urbanisation apparently resulting from economic growth, rather than the other way round (Asadoorian 2008, Bloom et al. 2008, though see Henderson 2003 who suggests that under- or over-urbanisation can reduce growth[181]).

Using data for 2004 from UNDP (2006) and Heston et al. (2006), I carried out a non-linear regression of the percentage of a country's population that is urban on

[181] Asadoorian (2008) comments thus: "Although there is evidence that urbanization depends endogenously on economic variables, long-term forecasts of the spatial distribution of population are often made exogenously and independent of economic conditions".

per capita income, for 77 countries, using the 3 parameter asymptotic function (Equation 7.1, Table 7.3, Figure 7.5).

Equation 7.1 $$\%Urban = a + (b - a) \cdot \exp(-\exp(c) \cdot y)$$

Where *a* is the asymptote, and *b* is the intercept and c is the natural log of the rate constant. This function can be used to forecast urbanisation as Madagascar grows richer.

Given the arguments advanced in Chapter 5 against the use of cross-sectional analyses for forecasting, this is somewhat unsatisfactory. It is possible that the process of urbanisation, like environmental efficiency, is subject to 'technological overspill', and therefore that urbanisation may occur even in the absence of economic growth.[182] To investigate this, I also fitted the model to the data from 66 countries for which data on population and income level was available in both 1975 and 2004 (Table 7.3). There does appear to be a time shift in the model fit at lower and higher incomes, implying that in the richest and poorest countries, there has been some urbanisation over time, independent of economic growth.[183] Certainly, Madagascar has experienced some urbanisation over the period (from 16.3% to 26.6%), despite its GDP per capita having fallen (from $1,268 to $750). Nevertheless, except at the lowest income levels, the effect is modest and, for this function at least, inconsistent over the range of incomes: for middle income countries there is a very small shift is in the opposite direction.[184] Without conducting a panel analysis using data from many different years, it is impossible to determine the nature of this shift, and I therefore use the relationship derived from 2004 as an approximation

It is likely that country-specific factors also determine the level of urbanisation, with some countries tending to be more highly urbanised than others at all income

[182] Note that economic development itself may be driven by technological overspill.

[183] Given the relatively small sample size, the shift might also be partly due to movement along the x-axis of individual countries, having high or low urbanization rates. It could also partly be an artefact of the choice of model.

[184] Note that this does not imply that individual countries have de-urbanised, but rather that the set of countries in the middle of the income range has become slightly less urbanised over time, as some countries have moved out of the middle income set, and others have moved in.

levels. Figure 7.5 shows the position of Madagascar in 2004 and 1975. In both cases it lies quite close to, but slightly below, the fitted line. In using the model to forecast Madagascar's urbanisation, I assume that its residual remains constant at the 2004 level. I assume that the rate of urbanisation does not itself affect either national or local income levels, nor does it affect national population growth.

Table 7.3. Parameter estimates for the three parameter asymptotic function of urbanisation against GDP per capita; n=77.

Parameter	Estimate	Std. Error
a	80.28	3.65
b	24.69	5.49
c	-8.92	0.26

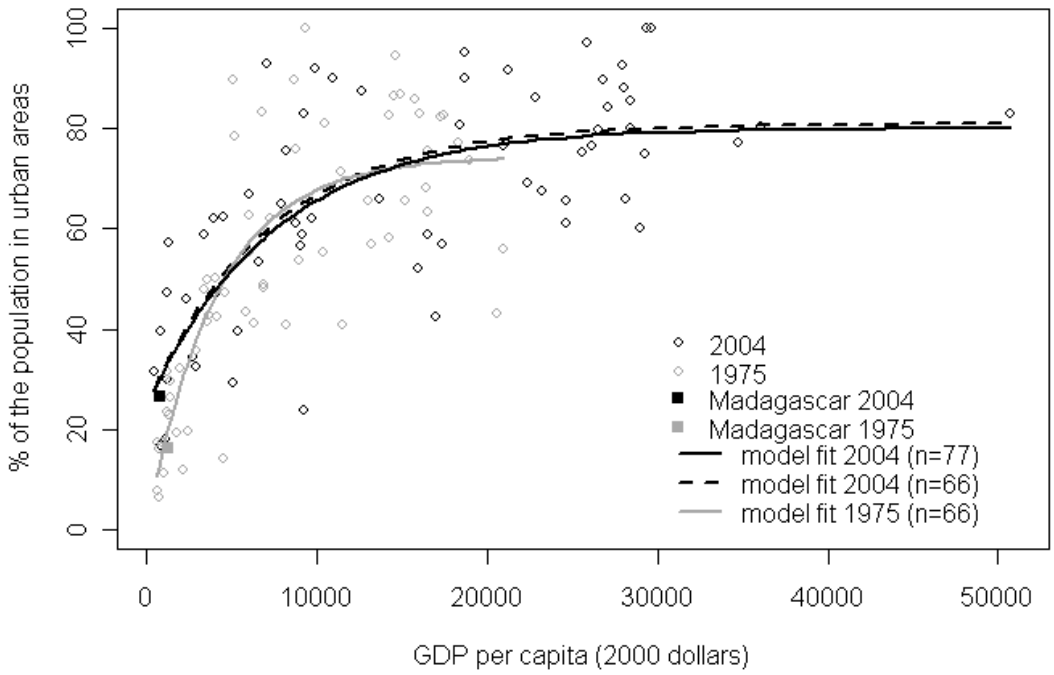

Figure 7.5. Three-parameter asymptotic functions fitted to data on the percentage of countries' populations which are urban vs GDP per capita. Data points and model fits are coloured black (2004) and grey (1975). Data points for Madagascar are shown as solid squares (2004=black, 1975=grey). Population data are from UNDP (2006) and GDP data from Heston et al. (2006). Data was available for 77 countries in 2004. Of these, data was available for 66 countries in 1975. Fits are shown for the full dataset for 2004 (solid line) as well as the reduced dataset for both 1975 (dotted line), 2004 (dashed line). The projection of Madagascar's urbanisation is based on the model fitted to the full dataset (n=77) for 2004.

V. Summary

This chapter has introduced the proposed new protected area that aims to conserve the Ranomafana-Andringitra forest corridor in south-eastern Madagascar, for which a partial cost-benefit analysis will be developed over subsequent chapters. The purpose of the case study is to contribute to the sparse literature on the net benefits of nature conservation in developing countries and to provide an empirically plausible illustration of the issues discussed in Chapter 2. This chapter has defined the framework of the case study, introduced the groups into which affected individuals will be aggregated and completed the task of developing income and population projections for each group. Chapter 8 develops projections of forest cover, for the project and non-project scenarios. Chapter 9 projects species extinctions, and the non-use values of avoiding these. Chapter 10 projects local opportunity costs, while Chapter 11 summarises and discusses the results according to the framework laid out in Chapter 2.

8. Projecting forest cover in the Ranomafana-Andringitra corridor (2005-2105)

Abstract

To estimate the costs and benefits of the proposed Ranomafana-Andringitra protected area, estimates of the likely trends in forest cover in the absence of the protected area are needed. Estimates of past deforestation do not represent deforestation rates in the absence of any conservation interventions, and extrapolation into the future is difficult because rates depend on a number of macroeconomic and social factors which may change overtime. However despite these problems, such estimates are useful for establishing the relationship between deforestation and socio-economic drivers and as a basis for projections. Unfortunately considerable uncertainty surrounds estimates of past deforestation rates. I review the available estimates of deforestation rate in the eastern rainforests of Madagascar and discuss two likely drivers of deforestation in the region. I then use the relationship between past rates of deforestation and rural human population size to project future deforestation rates in the Ranomafana-Andringitra corridor in the absence of the protected area.

I. Introduction

The main purpose of the proposed Ranomafana-Andringitra protected area is to maintain native old-growth forest cover by preventing it from being cleared for agriculture or mining, or degraded by timber harvesting (Repoblikan' i Madagasikara 2006). The differences in forest cover between the project and non-project scenarios will therefore drive many of the opportunity costs and benefits of implementing the project. One cannot assume that the simple act of designating a protected area will halt deforestation and equally, it would be wrong to simply equate a lack of formal protection with inevitable extirpation of all native old-growth forest (e.g. Dirzo & Raven 2003). Therefore, the CBA described in the next two chapters requires explicit projections of deforestation with and without the existence of the Ranomafana-Andringitra protected area, for the duration of the analysis.

Before a projection of forest cover can be developed, a brief discussion of what is meant by 'forest' is required. For the purposes of the CBA, forest would ideally be defined with respect to the functions it fulfils, and multiple definitions might therefore be used. Old-growth native forest and eucalyptus plantations might both be considered to be 'forest' for the purpose of timber extraction (though differing in important characteristics), but not from the perspective of biodiversity conservation. Conservationists therefore tend to emphasise the importance of old-growth, undisturbed, native forest to the exclusion of plantation and secondary forest, while foresters may be more inclusive (Grainger 2008, Wright & Muller-Landau 2006a, Gardner 2007). However, when estimating forest cover, one is limited by the capabilities of the methods used: satellite images, for example, may be unable to distinguish old-growth native forest from plantations or secondary forest, whether or not the end-user wished to do so (see below). In practice therefore, the measures of forest cover and loss available will be imperfect for the purpose, and their limitations must always be born in mind. Given these difficulties, it is not surprising that considerable uncertainty and controversy surrounds estimates of past and present forest cover, forest loss, and the reasons for this loss (Jarosz 1993, Bertrand & Sourdat 1998, Fairhead & Leach 1998, Casse 2004, Grainger 2008). This is well illustrated in Figure 8.1, which shows

estimates of total forest cover for Madagascar, taken from Dufils (2003, table 4.4). These uncertainties multiply when projections of future deforestation are made (see e.g. Laurance 2007b).

In this chapter, I consider estimates of past deforestation rates in the humid forests of eastern Madagascar, as well as evidence on those factors likely to affect deforestation rates over time (focussing on human population size). Based on these data, I then develop a family of projections for future deforestation rates in the study area both for the project and non-project scenarios. The estimates considered concern native forest for the most part, but generally do not distinguish old-growth from secondary forest, or relatively undisturbed forest from degraded forest. In the study area, where much of the agriculture takes the form of shifting or swidden cultivation, the treatment of secondary forest is likely to be quantitatively important. The details of how each study has treated secondary forest are given below.

Figure 8.1. Estimates of Madagascar's total forest cover (data from Dufils 2003). Figures are based on: the estimates of early authors (■); aerial photos (◆); and satellite images (▲).

II. Estimates of deforestation in the eastern rainforests

The CBA requires an estimate of the 'natural' deforestation rate in the absence of conservation activity. Unfortunately this is probably not directly observable in the

recent past (and older estimates may no longer be valid). Deforestation throughout Madagascar may be depressed by the Malagasy state's efforts to enforce prohibitions on deforestation which, while sporadic and poorly funded (Kull 2004), may at the least have raised the costs of practising *teviala*. For example, farmers report having to pay forest service officials fees for permits to clear forest.[185] There has also been considerable conservation and conservation-linked development activity in the study area since the early 1990s, which may have been at least partially successful in reducing deforestation rates (Hawkins & Horning 2001).[186] On the other hand, conservation may also increase deforestation rates in some periods. For example, Kull (2004) documents burning as a protest against perceived government heavy-handedness. Alternatively, a perceived threat to prevent deforestation can lead to opportunistic clearances during temporary absences of enforcement (e.g. during elections) or prior to the start of a conservation project – both of which have occurred in Madagascar (Kull 2004). Despite these caveats, I review below the available evidence on deforestation rates in the eastern rainforests of Madagascar.

Observed deforestation rates

Four studies have estimated deforestation in the eastern rainforests of Madagascar, though all but one (Hawkins & Horning 2001) extends beyond the study area (Table 8.1, Figure 8.2). All studies used LANDSAT images, except for the 1950s for which Green and Sussman (1990) and Harper et al. (2007) used the map prepared by Humbert and Cours Darne (1964-1965) from aerial photos.

There is a considerable variation in annual deforestation rates between areas and between time periods (range: 0.37-2.79 % yr^{-1}, Table 8.1). There are even substantial differences between the two estimates most specific to the study area, Hawkins & Horning (2001) and MIARO (2005), who estimate rates of 0.64% and 1.32% yr^{-1} respectively, despite covering broadly similar time periods and areas.

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[185] Interviews conducted in Angalampona, Miarinarivo commune, Sept 2006. The legality of these permits and fees is questionable.

[186] This conclusion should be treated with caution for several reasons. First, there is no satisfactory way to compare deforestation rates between areas (see below) and second, the estimate for the Ranomafana-Andringitra corridor includes three strictly protected areas, while the control corridor included none.

Remote-sensed estimates are subject to several sources of error and differences in methodology, which may account for some of this variation. The definition of forest cover varies between studies, as do the rules for classifying grid cells as forest or non-forest. Ideally, classifications should be ground-truthed, but this was either absent (e.g. Green & Sussman 1990, Hawkins & Horning 2001) or restricted to airborne visual surveys at only the most recent point in time (Harper et al. 2007). No methodological details were provided by MIARO (2005). Cloud cover obscured much of northern Madagascar in 1973 (Green & Sussman 1990) and much of the lowland area in the images used by Hawkins and Horning (2001). Errors will be higher for fragmented forest with a high perimeter : area ratio (Green & Sussman 1990).

Annualising forest losses estimated over relatively short time periods is also subject to error, because swidden agriculture (*teviala*) is a seasonal practice. The cutting of the forest is normally conducted during a few months before the sowing season when the weather is dry enough to allow the plot to be cleared with fire (Kistler & Spack 2003, pers. obs.) and the timing of this period may vary from place to place. Depending on the precise dates of the images used, this could result in the number of years' worth of *teviala* captured in the period being miscalculated, if the first set of images are taken directly after a *teviala* season, while the second set are taken directly before. Thus a roughly ten-year interval between images could capture between 8 and 12 years' worth of *teviala*, implying a maximum error rate of 20% (though this would be extreme).

The treatment of regeneration varies from study to study, and will affect the estimates produced, as well as their meaning. Green and Sussman (1990) could not distinguish secondary forest from old-growth forest. They found approximately 8% of forest cover in 1985 occurred outside the 1950 extent suggesting significant forest regeneration. Hawkins and Horning (2001) were also unable to distinguish the two forest types, however, they excluded cells that changed from non-forest to forest during the period studied, thus eliminating any recent regeneration. The degree to which recently regenerated forests can be treated as ecologically or economically equivalent to old-growth forest is controversial, and depends on the particular forest function in question. Quantities of different components of biodiversity, timber species populations, sequestered

carbon, and hydrology all vary with the degree of disturbance or stage of growth of forest stands (Wright & Muller-Landau 2006a,b, Brook et al. 2006, Laurance 2007b, Gardner et al. 2007, Chomitz & Kumari 1998, Bruijnzeel 2004).

All studies except MIARO (2005) provide some measure of uncertainty in their estimates: 10% (Green & Sussman 1990); "better than +/- 15% at a 95% confidence interval" (Hawkins & Horning 2001), and "89.5% accuracy in identification of forest and non-forest land" for 2000 (Harper et al. 2007). However, robust quantitative estimates of all the sources of error and bias noted above are impossible to obtain without extensive ground-truthing. Nevertheless, these errors probably do not account for all, or even most, of the variability in deforestation rates. Identifying the sources of variation between areas is important but beyond the scope of this chapter. In Section II, below, I consider some factors which may systematically affect deforestation rates over time.

Deforestation in protected areas

How effective are protected areas at preventing deforestation? Globally, several studies have demonstrated lower rates of deforestation and other degradation within protected areas compared with surrounding or control areas (e.g. Bruner et al. 2001, Nepstad et al. 2003, DeFries et al. 2005). Bruner et al. (2001) found that important determinants of a park's effectiveness are the number of guards per km^2 and the compensation of local people for opportunity costs as a result of the park's establishment. Nevertheless, even poorly funded parks appear to reduce deforestation. In Madagascar, Sommerville (2005) found that protected areas have been generally successful in reducing deforestation, while Dollar (2006) found more mixed results, with some protected areas appearing to show an increase in deforestation relative to control areas, while others succeeded in cutting deforestation. A caveat which should be attached to these results is that protected areas may displace, rather than prevent, deforestation (Armsworth et al. 2006, Ewers & Rodrigues 2008), which would lead to their effectiveness being overestimated. If this were the case, both the benefits and opportunity costs of protected areas might be also be exaggerated.

Table 8.1. Estimates of deforestation in the eastern rainforests of Madagascar (Figure 8.2 shows the location of the named forest areas).

Source	Area		Period	Rate % yr^{-1}	Rate ha yr^{-1}
Green & Sussman (1990)	Eastern rainforests	Total	1950-1985	1.96	108,571
		High[1] pop density	1950-1985	2.79	43,143
		Med pop density	1950-1985	1.85	34,286
		Low pop density	1950-1985	1.48	31,429
		High pop density	1950-1973	2.50	51,000
		High pop density	1973-1985	0.79	16,000
Hawkins & Horning (2001)[2]	Zahamena-Mantadia Corridor		1993/4–1999/2000	0.37	1817
	Anosibe an'Ala Corridor		1994-2000	1.14	2417
	Ranomafana-Andringitra Corridor		1993-1999	0.64	1,567
Dufils[3] (2003)	Evergreen forests		1953-1993	1.3	112,275
			1993-1999	2.0	119,866
Miaro (2005)	Extended Ranomafana-Andringitra Corridor[4]		1990-2000	1.32	5,468
Harper et al. (2008)	All humid forest[5]		c1953-c1973	0.6	n/a
			c1973-c1990	1.7	87,188
			c1990 – c2000	0.8	16,100

[1]Green and Sussman (1990): Population density categories: high >10 km^{-2}; med 5-10 km^{-2}, low <5 km^{-2}. The 1973 map covered only 68% of high population density areas.

[2] Hawkins and Horning (2001): rates are calculated on the basis of six years between images (actual range: 5 yr 5 months – 7 yr).

[3]Dufils (2003) category of evergreen includes some evergreen forests in western Madagascar. Based on Humbert and Cours-Darne (1964-1965) for 1953.

[4]Miaro (2005): this study includes a larger area than the Ranomafana-Andringitra corridor of Hawkins and Horning (2001) and larger than that discussed in this thesis. It includes the forest north of Ranaomafana as far as Ambositra and south of Andringitra/Ivohibe as far as Vondrozo.

[5]Green and Sussman's (1990) eastern rainforests broadly coincides with Harper et al's (2007) humid forest category.

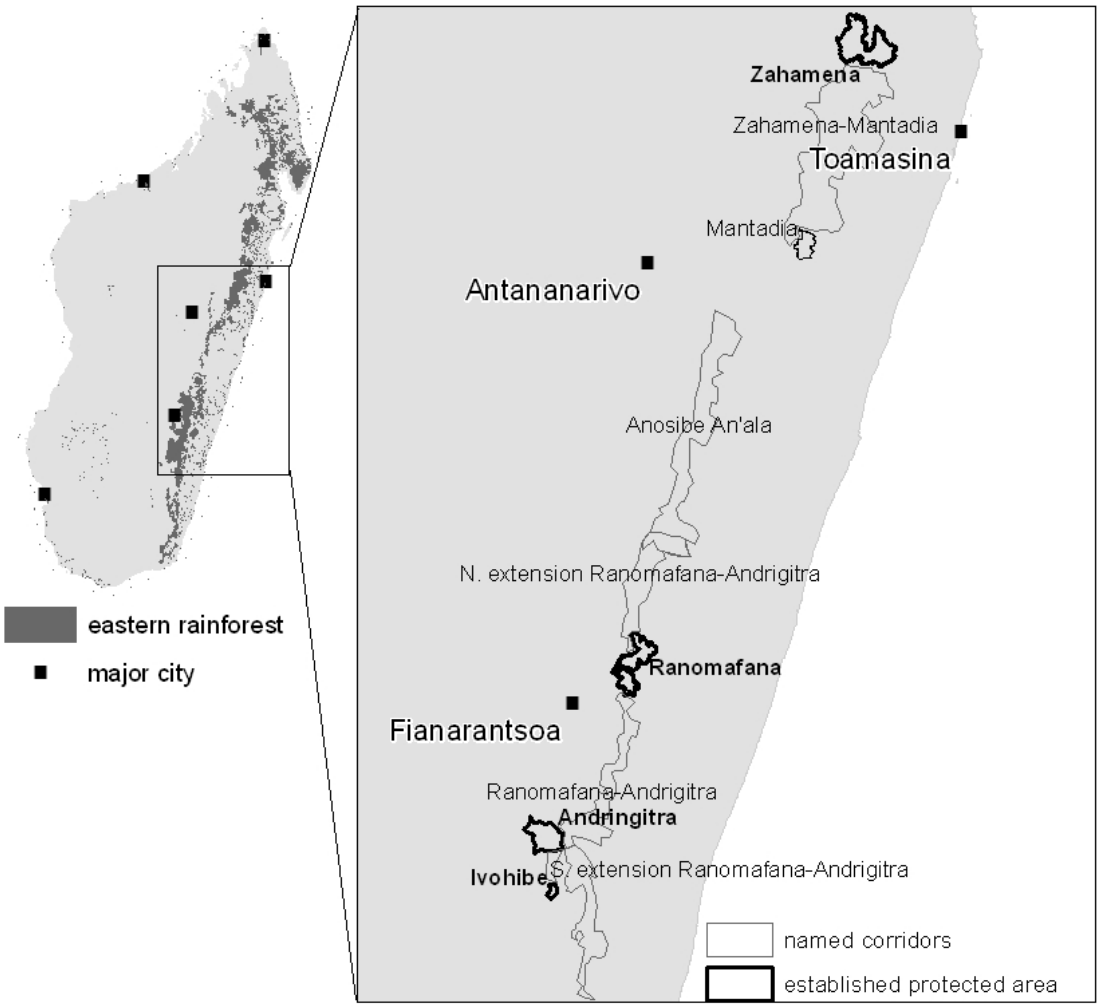

Figure 8.2. The extent of Madagascar's eastern rainforests (data from Conservation International Madagascar, December 2007) and the approximate position of the areas mentioned in the deforestation studies listed in Table 8.1.

III. Factors affecting deforestation rates over time

The difficulties in estimating past deforestation rates, though considerable, are relatively minor compared with those of projecting such rates into the future. The first problem is that very few data points through time are usually available – three at most in the studies identified above. This provides limited information about the functional form of the relationship between deforestation rates and time, or other explanatory variables.

Projection of deforestation is also hampered by the lack of a truly satisfactory unit with which to measure it, and therefore it is difficult to define a naive "no-change" projection (Makridakis et al. 1998), based on a meta-analysis of estimates. Absolute deforestation rate (ha yr^{-1}) most directly describes the conversion that is actually happening, and what, ultimately, must be projected, but this is clearly unsatisfactory for comparing and collating estimates from different areas which vary in spatial extent – estimates derived from larger areas (e.g. "eastern rainforests" as opposed to "Ranomafana-Andringitra corridor") will, ceteris paribus, show greater deforestation. Using relative, rather than absolute measures (% yr^{-1}), is one way to compare rates between different areas and periods. However, I demonstrated in chapter 4 the danger of using an exponential function to extrapolate rates of change that, for convenience's sake, have been expressed in percentages.

Below I review two principle factors which might be expected to systematically affect deforestation rates over time: accessibility and population. Previous studies have found the evidence linking economic development to deforestation to be ambiguous, and I do not consider this further here (Wunder 2001, Scrieciu 2007).

Slope angle and accessibility

Green and Sussman (1990) noted that in areas in the eastern rainforests of Madagascar defined in 1960 as densely populated, deforestation has slowed over time, whether measured in absolute or relative terms. Their explanation is that most of the remaining forest in these areas was restricted to steep slopes, the more productive land already having been cleared. Most of the Ranomafana-Andringitra corridor is classed as having a high population density by Green and

Sussman (1990), has experienced high past-levels of land conversion and much of its remaining forest is on steeply sloping land. We might therefore expect rates (however measured) to continue to decrease. The subsequent estimate from Hawkins and Horning (2001) of 0.64% yr^{-1} for 1993-1999, which is lower than the rates reported for high density areas by Green and Sussman (1990), lends some weight to this argument (Table 8.1).

Accessibility has been shown in many studies to directly influence deforestation (Mertens & Lambin 2000, Wilkie et al. 2000, Nagendra et al. 2003). The ratio of perimeter to area is likely to influence rates as it helps to determine the accessibility of the forest (Harper et al. 2007).[187] If forest cover is reduced, without becoming fragmented, this will tend to decrease the absolute length of the perimeter, while increasing its ratio to forest area. Under such circumstances, we might expect, ceteris paribus, that absolute rates would decrease, while percentage rates might increase.[188] If, on the other hand, the forest is fragmented as well as reduced in extent, the perimeter length might increase, leading to increases in both absolute and percentage rates.

Green and Sussman (1990) provide estimates of perimeter length, summarised in Table 8.2, to which I have added perimeter : area ratios and percentage change in these ratios. In high population density areas, perimeter length has decreased over the period 1950-1985, while in medium density areas it has remained constant (while forest area decreased). This may have contributed to the reduction in deforestation rates in high density areas noted above. Across all three categories, perimeter : area ratios have increased, though the rate of increase is inversely correlated with population density. Although they do not provide estimates of perimeter length, Harper et al. (2007) provide two other measures of fragmentation: the proportion of forest in blocks >500 km^2 or <100 km^2; and the proportion of forest <250 m, or >1 km from the forest edge. For the humid forests, all of these measures appear to show considerable increases in fragmentation from the 1950s to the 1970s, but almost no change in both subsequent periods (1970s-

[187] The presence of roads and other transport infrastructure are also important (Wilkie et al. 2000), and may or may not be included in any estimate of perimeter.

[188] Absolute rates might increase if deforestation increases the effective human population size (people km^{-2}), intensifying pressure on remaining forest.

1990s, 1990s-2000s), implying little fragmentation has occurred in the last 30 years.[189] Harper et al. (2007) write that "The more general pattern of deforestation in the dry and humid forests was of small-scale clearance at forest edges." Thus it seems unlikely that absolute deforestation rates will increase in the future as a result of fragmentation or endogenous increases in the accessibility of forest.

Table 8.2. Changes in forest area, forest perimeter and perimeter : area ratio of rainforests in eastern Madagascar in areas of high, medium and low human population density. Data from Green and Sussman (1990, Table 1, last two columns calculated by the author)

Year	Aerial extent (hax10^6)	Forest remaining (%)	Forest Perimeter (km x10^3)	Perimeter : area ratio (km per ha x10^3)	Change in perimeter : area ratio (%)
			High density		
Original	4.7	100	3.5	0.74	
1950	2.4	50	7.8	3.25	339
1985	0.89	19	4.5	5.06	56
			Medium Density		
Original	3.4	100	2.2	0.65	
1950	2.5	76	4.9	1.96	202
1985	1.3	38	5	3.85	96
			Low Density		
Original	3.1	100	3.4	1.10	
1950	2.7	86	5	1.85	68
1985	1.6	51	6.1	3.81	106
			Total		
Original	11.2	100	9.1	0.81	
1950	7.6	67	17.7	2.33	188
1985	3.8	34	15.6	4.11	76

Population growth and density

Another factor which has been shown to influence deforestation is human population density. Wright and Muller-Landau (2006a) provide evidence that the proportion of a country's maximum forest extent which still remains is strongly and inversely correlated with its population density. Although urban and rural

[189] The fragmentation apparently seen between the 1950s and 1970s may, perhaps, be an artefact of switching from using maps (drawn from aerial photos) for the 1950s to using LANDSAT images in all subsequent periods. It seems plausible that many small fragments of forest might have been omitted from the earlier maps.

population densities have been strongly correlated in developing countries, they find some evidence to suggest that it is the rural, rather than total population density which determines forest cover (Wright & Muller-Landau 2006a). This is crucial, since rural and overall population growth rates are expected to significantly decouple in most developing countries in the future (Chapter 7). Wright and Muller-Landau (2006a) develop projections of future forest cover by linking cross-sectional models of forest cover and population density to UN Population Division population projections. The projections differ significantly depending on whether total, or rural population density is used, though both types of projection are more optimistic than other recent estimates of future forest cover (e.g MEA 2005). In the next section, I investigate the inter-temporal relationship between forest cover and human population size in Madagascar.

IV. Human population and forest cover in Madagascar

Wright and Muller-Landau (2006a) carried out a regression of forest cover (as a percentage of potential maximum) on human population density (rural or total) for a cross section of 45 humid tropical countries. They were unable to conduct a panel or time series study, since reliable longitudinal data on forest cover is not easily available (Grainger 2008). In this section I repeat Wright and Muller-Landau's (2006a) analysis for time series data from Madagascar.

Methods

I regressed time-series data of Madagascar forest cover (Harper et al. 2007) against population (UNPD 2007, 2008). Like Wright and Muller-Landau (2006a), I used both the rural and total human population of Madagascar as the predictor.[190] Harper et al. (2007) provide forest cover data for three forest types: humid (eastern rainforests), dry (western forests) and spiny (southern forests). I use humid forest and all forest as the response variable in separate regressions. Because the analysis is restricted to Madagascar, there is no need to correct for country size; therefore I use forest cover in km^{-2}, and absolute population size

[190] UNPD population estimates are available at five-year intervals. Therefore, as in Chapter 8, I used a third order polynomial function to interpolate values for 1953 and 1973 (R^2>99%).

rather than population density.[191] Wright and Muller-Landau (2006a) fitted the model:

Equation 8.1 $$F = a + b \cdot \log_{10}(P)$$

where F is forest cover (humid or all) and P is population (rural or total). This implies negative forest cover above a certain population level, leading to complete extirpation of the forest. As an alternative, I also fitted an exponential model:

Equation 8.2 $$F = a \cdot \exp(b \cdot P)$$

which implies constant percentage decreases in forest cover, with constant absolute increases in population.

Results and discussion

For humid forest, and also for all forest, there is a strong negative correlation between human population size and forest cover, as in Wright and Muller-Landau's (2006a) cross-sectional analysis (Figure 8.3). As always, the relationship may of course be explained by a third, unknown variable that is correlated with both forest cover and human population. However, since most forest in Madagascar is cleared for small-scale agriculture, there is at least an obvious mechanistic link between the two variables.

For humid forest, rural population outperformed total population as a predictor (as in Wright & Muller-Landau 2006a) and ΔAIC was large (>5). However, the reverse was true for all forest types combined (Table 8.3, Figure 8.4). Similarly, the logistic model outperformed the exponential model for humid forest (ΔAIC>7), with the reverse being true for all forest. The differences may be due to different factors driving deforestation in different forest areas. For example, charcoal production, which predominantly serves the urban markets, is a major driver of deforestation in the southern spiny forests, but relatively unimportant in the case study area. Both functional forms give quite similar predictions for humid forest (Table 8.4, Figure 8.3, Figure 8.4).

[191] The results are of course unchanged if forest cover is expressed as a percentage of potential, and population density is used instead, since both variables are simply divided by constants (original area of forest, and area of Madagascar, respectively).

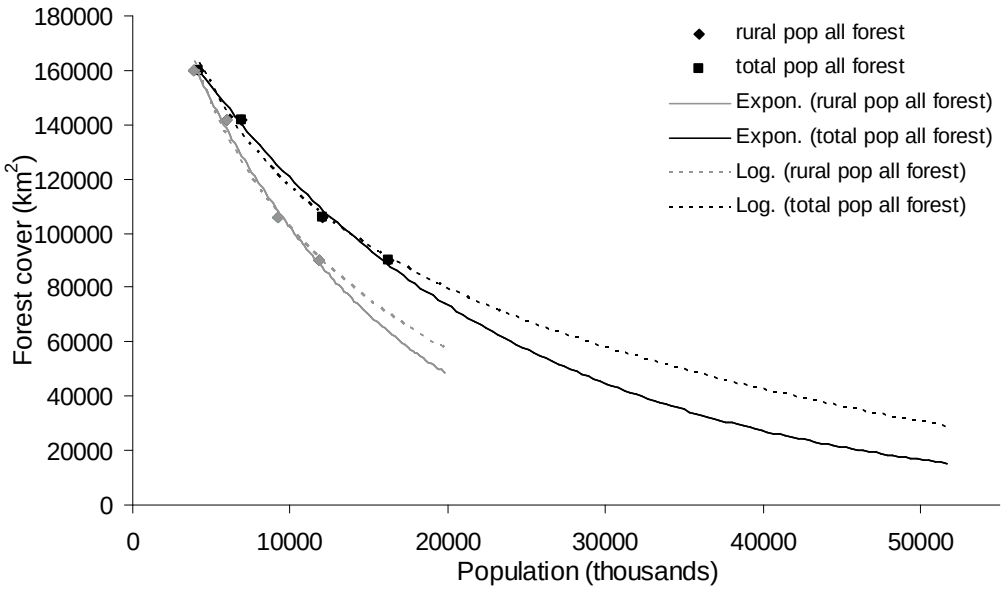

Figure 8.3. Time series of all forest cover (data from Harper et al. 2007) against population (data from UNPD 2007, 2008) for Madagascar. Exponential (solid) and logarithmic (dashed) model fits are shown and extrapolated to the limits of population projected in the previous chapter for 2005-2105.

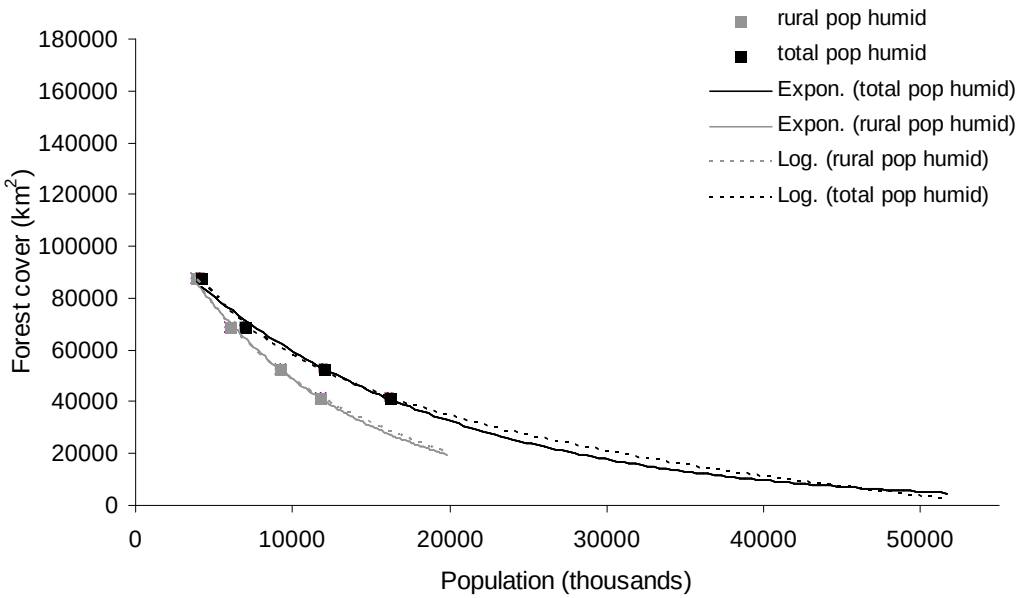

Figure 8.4. Time series of humid forest cover (data from Harper et al. 2007) against population (data from UNPD 2007, 2008) for Madagascar. Exponential (solid) and logarithmic (dashed) model fits are shown and extrapolated to the limits of population projected in the previous chapter for 2005-2105.

Table 8.3. AIC scores for exponential and logarithmic models of forest cover regressed on human population size, for all forest and humid forest and for total population and rural population.

Model	Predictor	Parameters	AIC	ΔAIC
All forest				
Exponential	Total population	2	80.10	0.00
Exponential	Rural population	2	81.72	1.62
Logarithmic	Total population	2	83.59	3.50
Logarithmic	Rural population	2	84.23	4.13
Humid Forest				
Logarithmic	Rural population	2	59.12	0.00
Logarithmic	Total population	2	64.18	5.06
Exponential	Rural population	2	71.99	12.87
Exponential	Total population	2	76.25	17.13

Note: total and rural population is for the whole of Madagascar, not only forested areas.

Table 8.4. Parameter estimates for exponential and logarithmic models, using total or rural population to predict all or humid forest cover.

Model	Predictor	Parameter	Estimate	Std. Error	t value	Pr(>|t|)	Reduction in forest cover as population increases by:	
							1 million	10%
All forest types combined								
Exponential	Total population	a	203859	6593.2	30.92	0.0010		
		b	-5.17×10^{-5}	3.55×10^{-6}	-14.55	0.0047	5.03%	
Exponential	Rural population	a	224289.7	10509.4	21.34	0.0022		
		b	-7.81×10^{-5}	6.55×10^{-6}	-11.92	0.0070	7.51%	
Logarithmic	Total population	a	644798.7	52777.6	12.22	0.0066		
		b	-131571.3	13324.8	-9.87	0.0101		5,446km^2
Logarithmic	Rural population	a	744719.4	68175.6	10.92	0.0083		
		b	-160552.9	17627.9	-9.11	0.0118		6,646km^2
Humid forest								
Exponential	Total population	a	115914.7	4537.6	25.55	0.0015		
		b	-6.53×10^{-5}	4.52×10^{-6}	-14.45	0.0048	6.33%	
Exponential	Rural population	a	131041.1	3573.3	36.67	0.0007		
		b	-9.91×10^{-5}	3.98×10^{-6}	-24.88	0.0016	9.43%	
Logarithmic	Total population	a	391025.1	4662.2	83.87	0.0001		
		b	-83032.1	1177.1	-70.54	0.0002		3,437 km^2
Logarithmic	Rural population	a	454828.9	2957.3	153.80	0.0000		
		b	-101514.8	764.6	-132.76	0.0001		4,202 km^2

V. Forest cover projections for the case study

The only past estimates of deforestation rates for the Ranomafana-Andringitra corridor (Hawkins & Hornings 2001) were from the 1990s. They cannot be used as estimates of deforestation in the absence of conservation as they include three protected areas and significant conservation activities were established in the corridor linking the protected areas by the mid 1990s (Hawkins & Horning 2001). Other deforestation estimates for the eastern rainforests / humid forests (which include the corridor) show considerable variability across space and time, and comparisons are difficult because of the lack of an appropriate unit.

As noted in Section II, deforestation rates appear to be slowing in high population density areas like the corridor (Green & Sussman 1990), and there is no evidence that the forests are becoming more fragmented over time (Green & Sussman 1990, Harper et al. 2007). My analysis above suggests that humid forest cover in Madagascar is negatively related to rural human population size by a logarithmic function (as found for a cross-section of countries by Wright & Muller-Landau 2006a).

Deforestation projections in the non-project scenario

Given the considerable uncertainty over initial deforestation rates, I propose a set of projections which are illustrative rather than predictive, and aim to capture a broad range of possible futures. For the initial deforestation rate (2000-2005) I use a range of rates from 0.5% to 2.5% per year. This captures the full range of deforestation rates that have been estimated for the eastern rainforests (Table 8.1).

In order to project forest cover into the future, I assume the existence of a logarithmic relationship between rural human population size and forest cover, as found in section IV. This is calibrated for each initial deforestation rate, by selecting values for the parameters a and b in Equation 8.1 (repeated as Equation 8.3 below), to match the actual rural population sizes in 2000 and 2005, the actual forest cover in the corridor in 2000 (from Hawkins & Horning 2001), and the forest cover that would have been present in 2005, had each initial deforestation rate pertained over the period. Parameter values are calculated with Equation 8.3,

substituting values for 2000 and 2005, and solving simultaneous equations as follows (Equation 8.4, Equation 8.5):

Equation 8.3 $$F_t = a + b \cdot [\log_{10}(P_t)]$$

where F is forest cover, P is rural population, and t is the year index. Rearranging, and inputting values for 2000 and 2005, and solving simultaneous equations gives:

Equation 8.4 $$a = F_t - b \cdot [\log_{10}(P_t)]$$

Equation 8.5 $$b = \frac{F_{00} - F_{05}}{([\log_{10}(P_{00})] - [\log_{10}(P_{05})])}$$

Thus, if a logarithmic function is assumed to link forest cover in the eastern rainforests and Madagascar's rural population size, then forest cover can be projected into the future using the projections of population size developed in Chapter 7.[192] Figure 8.5 shows projections of forest cover for the range of rural populations projected in the previous chapter.

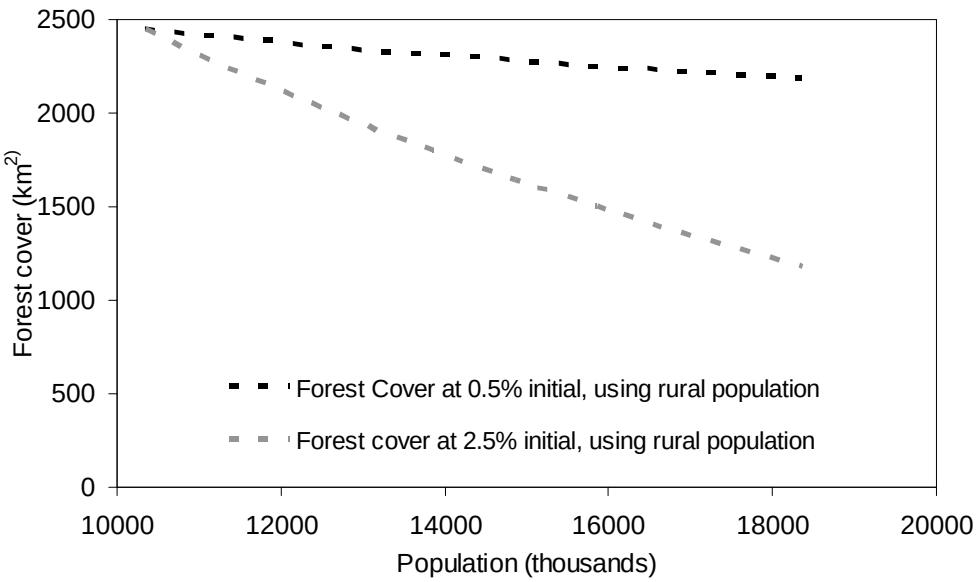

Figure 8.5. Forest cover projected using a logarithmic relationship between total population (solid lines) or rural population (dotted lines), for two illustrative initial deforestation rates (black = 0.5% yr^{-1}, grey = 2.5% yr^{-1}). Projections are shown over the full range of population sizes projected in chapter 8.

[192] Recall from the previous chapter that rural populations in the corridor are assumed to grow in proportion to the rural population of Madagascar as a whole.

Four illustrative projections of forest cover through time are shown in Figure 8.6. The most optimistic projections are those which are based on the most optimistic economic / urbanisation[193] projection for Madagascar (convergence on ALM region =2% year^{-1}). In these projections (dotted lines) forest cover declines until around 2030, when the rural population is predicted to peak, after which they begin to recover. As I discussed above, the ecological and economic importance of secondary forest is the subject of considerable debate, and varies according to the context, and the particular value under consideration (existence values, hydrology, carbon sequestration etc). The implications of this predicted recovery will be discussed in chapter 9, including the possibility that the loss of forest cover, or important functions of the forest, might be irreversible. For the time being, this re-grown forest can be thought of as potential forest. Forest cover projections based on the most pessimistic economic and urbanisation projection (convergence on ALM region =2% year^{-1}), decline until around 2085.

It is interesting to note that the higher the assumed initial deforestation rate (black lines) the faster the recovery, after the population peak. Thus for the most optimistic economic/urbanisation scenario, the forest cover projection which assumes the highest initial deforestation rate (2.5% yr^{-1}) actually exceeds that which assumes the lowest rate (0.5% yr^{-1}) by around 2070 (though a higher proportion of this forest would be recent re-growth, rather than old-growth forest). It is tempting to dismiss this as an artefact of the model, but in fact it captures an important point. If forest cover is chiefly threatened by rural population growth, then the more rapacious is the growing population (i.e. the greater the hectares required per person), the faster the pressure upon the forest is relieved when the population begins to shrink (concerns over irreversibility aside).

[193] Since I have used only one model linking urbanisation to economic growth, rate of urbanisation is driven by the choice of economic scenario.

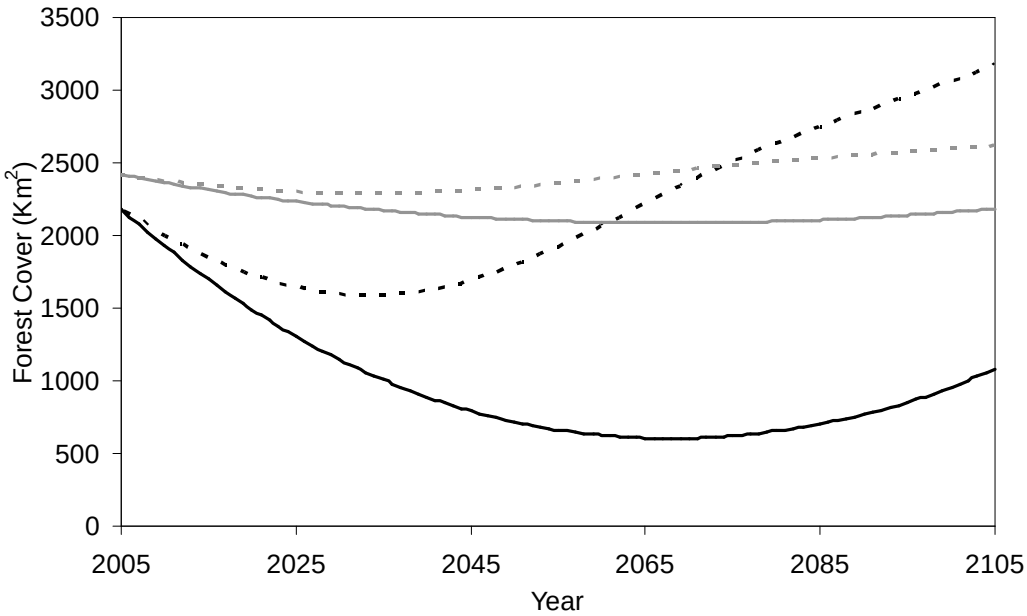

Figure 8.6. Four illustrative forest cover projections, assuming that forest cover is logarithmically predicted by rural population size. Dotted lines show the most optimistic economic and urbanisation scenario, solid lines the most pessimistic. For each economic scenario, projections are shown for two initial deforestation rates, 2.5% yr^{-1} (black) and 0.5% yr^{-1} (grey).

Deforestation in the project scenario

In this case study, I assume that the protected area will be funded and managed such that deforestation is successfully eliminated within its boundaries. This assumption is made partly for simplicity, and partly because the study area is a medium to high conservation priority within Madagascar (Kremen et al. 2008, Fig 2a), which is itself considered as one of the countries of highest conservation priority globally (Mittermeier et al. 2004). Therefore, adequate funding ought to be available.[194] I do not explicitly consider the displacement of deforestation to other areas, which is unlikely to be a serious problem in this case, since the proposed protected area would include virtually all natural forest in the area (Repoblikan' i Madagasikara 2006) and all priority areas for conservation (Kremen et al. 2008 Fig 2b).

[194]This assumption is convenient but probably naïve. At the time of writing (20 months after the area received temporary protection) no significant additional management or enforcement capacity had been put in place (Mijasoa M. Andriamarovololona pers. com. 26th May 2008). However, the purpose of the CBA is to evaluate the social desirability of properly implementing and enforcing the protected area, regardless of whether this is likely to happen. The costs of half-heartedly implementing conservation programs with inadequate funding may be large, but do not concern us here (see Hockley and Andriamarovololona 2007 for such a case).

VI. Summary

Published deforestation estimates for the eastern rainforests of Madagascar show considerable variability across space and time, and the true rate of deforestation in the absence of conservation cannot be estimated with any great certainty. This uncertainty can only increase when deforestation is projected into the future. In this chapter I developed a set of deforestation projections for the non-project scenario, which incorporate the observed variability in initial deforestation estimates, using rural human population size to predict forest cover. These projections, like those of population and income developed in previous chapters, are illustrative and open to challenge, but are transparently derived from observed historical data. The sensitivity of the CBA's results to this uncertainty will be explored in subsequent chapters.

9. The value of avoided extinctions

Abstract

Previous chapters have developed projections of economic growth and rural population size for Madagascar (Chapter 7 and, based on these, projections of deforestation in the corridor Chapter 8). In this chapter I investigate what the predicted rates of deforestation in the area might mean for the biodiversity of the corridor, and what the value might be of avoiding the predicted extinctions. First, I use the link between habitat area and species richness (the species-area curve) to predict the future species richness of the corridor through time, as a percentage of maximum species richness. Then I correct this prediction, taking into account the fact that extinctions do not immediately follow area reductions, i.e. that there is an extinction lag. Finally, I provide a tentative estimate for the annual international value of these averted extinctions based on published values of willingness to pay for species conservation.

I. Introduction

Loss of habitat is one of the major threats to wild nature worldwide and is considered by the world conservation union (IUCN) to be the most important threat to many taxa (Groom et al. 2006). It is not only the absolute rate of habitat loss which is a cause for concern but fragmentation of habitat into isolated patches (Fahrig & Merriam 1994, Turner 1996, Debinski & Holt 2000). Deforestation and fragmentation are major drivers of biodiversity loss in tropical forests (Dodson & Gentry 1991, Renjifo 1999) including the eastern rainforests of Madagascar (Brooks et al. 2002). The species area curve (MacArthur & Wilson 1967) is often used to predict the relationship between forest loss and extinction (Andren 1994).

Society clearly considers the conservation of wild nature to be a worthwhile pursuit. At least $6 billion is spent worldwide on managing protected areas annually (Balmford et al. 2003) and in 2002 at least 190 countries committed to achieving a significant reduction in the current rate of biodiversity loss by 2010 (Balmford et al. 2005). This suggests that biodiversity has a value (Gowdy 1997). However, although case studies valuing particular ecosystems or species abound (see Loomis & White 1996 for a review), we lack systematic information about the value of wild nature in general, and about which components of wild nature are valued (Nunes & van den Berg 2001, Christie et al. 2006). For example, although there is evidence that diversity itself is valued in some circumstances (Naidoo & Adamowitz 2005), in other contexts it appears to be trumped by other, non-biological factors (Edwards-Jones et al. 1995). One important value of wild nature is attributable to the provision of goods and services by natural ecosystems (e.g. Turpie et al. 2003), although the link between biodiversity per se and ecosystem function is hotly debated (Schwartz et al. 2000). The usefulness of species richness and genetic diversity for pharmaceutical development has been proposed as an important value of biodiversity, but estimating these values has proved very problematic (Simpson et al. 1996). With respect to non-consumptive and non-use values, beauty, ‘naturalness’, and ease of access clearly have a role in determining the value of a particular area (Edwards-Jones et al. 1995, Price 1978), and non-biotic elements of landscapes are also valued (Webber et al. 2006).

As described in Chapter 7 and 8, the Ranomafana-Andringitra corridor (hereafter "the corridor") is threatened by the conversion of natural forest to agriculture. In section II, I use the species area curve and projections of forest loss from Chapter 8 to predict the future species richness of the Ranomafana-Andringitra corridor through time, as a percentage of maximum species richness. In section III, I correct this prediction, taking into account a possible lag in species extinctions. In section IV, I review published estimates of willingness to pay (WTP) for species conservation and, in section V, I use these to estimate the value of avoided extinctions by protecting the Ranomafana-Andringitra corridor.

II. Deforestation and extinction

It is well established in ecology that smaller parcels of habitat contain more species per area than larger parcels. The relationship between habitat area and species number was first established using observations from islands (MacArthur & Wilson 1967) but has since been found to be applicable in the context of continental habitat islands (Brown 1971) such as remnants of forest in landscapes which are converted to other land uses (Pimm & Askins 1995). The relationship is frequently represented as a power function, known as a species area curve (e.g. Pimm et al. 1995, Brooks et al. 1999b). This relates the area of intact habitat, A, to the number of species remaining within it, *S*:

Equation 9.1 $$S = cA^z$$

where *c* and *z* are constants. Following other authors (Brooks et al. 1999b, Wright & Muller-Landau 2006a) I take the value of z to be 0.25, which is well established as a reasonable average for tropical forest ecosystems. Thus, for a given projection of forest cover in the corridor, this function can be used to calculate the number of species predicted to remain in the forest (S_p), as a proportion of the maximum number (S_{max}):

Equation 9.2 $$S_p / S_{\max} = A_p^z / A_{\max}^z$$

For simplicity, I treat the corridor as a solid block, isolated from other forest, and assume that all of the species within it are restricted to forest habitat. To a first approximation these assumptions seem reasonable: the available evidence

suggests that fragmentation is not important in the corridor (see Chapter 8); most endemic species in Madagascar are known to be restricted to natural forest (Goodman & Benstead 2003); and the corridor has minimal connectivity to other areas, relative to its size.[195] The percentage of species being lost will be a function of the deforestation rate (Figure 9.1).

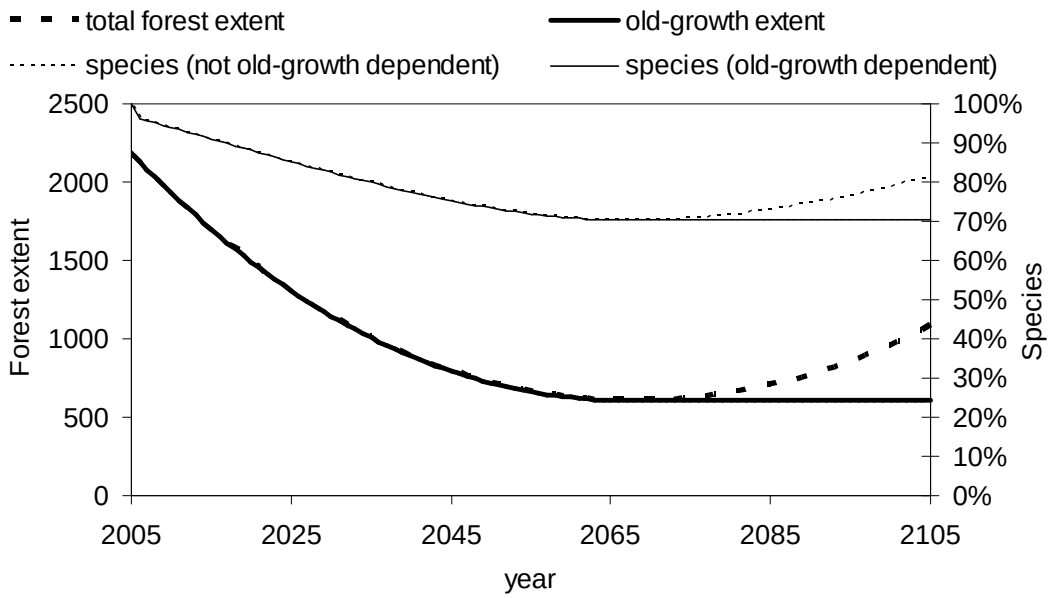

Figure 9.1. Prediction of the percentage of species surviving in the Ranomafana-Andringitra corridor from the species-area curve (equation 9.2) under the most pessimistic scenario for deforestation (stagnant economic growth and high (2.5%) initial deforestation rate; taken from Chapter 8).

[195] Although the portion of corridor considered here does connect to forest at its northern and southern ends, biodiversity in Madagascar is know to be restricted by river systems, which cut off the corridor from other forested areas (Wilme et al. 2006).

III. Extinction lags and relaxation time

Species area curves have been shown to accurately predict extinctions in temperate forests, where the deforestation took place centuries ago and records of species presence and absence are available for long periods (Pimm & Askins 1995). Few extinctions have yet been recorded in the tropics, where deforestation has continued to the present day historical data on the presence of species is much patchier, and many species are yet to be described (Brooks et al. 1999b). However several studies have found that species-area curves, when applied to recent habitat losses, predict the number of species classified as threatened with extinction (Brooks & Balmford 1996, Brooks et al. 1997), suggesting that species extinctions follow habitat loss with a time lag – a process known as relaxation (Brooks et al. 1999b).

The rate of relaxation is very important if, as the analysis in the previous chapter suggested, forest cover might be expected to recover through natural regeneration of secondary forest as rural population sizes decline, due to urbanisation and demographic transition. Wright & Muller-Landau (2006a) used the existence of extinction lags, together with their predictions of forest loss and gain, to argue that the tropical extinction crisis may have been overstated. However, the ability of forest regeneration to 'rescue' species depends on the relaxation rate, and on what proportion of species are able to survive in regenerating secondary forest. There are few estimates of the former (Brooks et al. 1999b), and little information about the latter (Gardner et al. 2007).

Although relaxation rates have been estimated for oceanic islands (Diamond 1972) and prairies (Leach & Givnish 1996), I am not aware of any for large tracts of tropical forest. However, Brooks et al. (1999b) estimated the speed at which relaxation took place, by reconstructing the history of fragmentation and loss of bird species for the five small parcels of the Kakamega rainforest in Kenya. They assumed that relaxation followed an exponential decay function, whereby the proportion of 'excess' species I (where $I=S_{max}-S_p$), decays over time as follows:

Equation 9.3 $$I = e^{(-kt)}$$

where k is a constant and t is time. Assuming this functional form, they estimated a half life of between 23 and 80 years. The half-life appeared to be positively correlated with the size of the fragment (range: 100-8600 ha in their study), but also negatively correlated with its distance from other forest patches (range: 0.5-9.4 km), and the sample size (5) of that study was too small to distinguish between these effects. Thus, while the corridor's larger size (247,700 ha in 2000) might imply a longer half-life, as far as species which are endemic to the corridor are concerned (see below) it is completely isolated (there can be no immigration of endemic species from outside of the corridor). Without further research, tt is impossible to say which effect will dominate.

Figure 9.2 repeats Figure 9.1 above, adding the estimated numbers of species remaining, taking into account the time lag to extinction (assuming an exponential decay function and a half-life of 50 years). In these calculations, because of the time lag, regrowing secondary forest is able to 'rescue' species which can utilise it before they become extinct from the corridor (dotted line), but not those which are reliant on old-growth forest (lower solid line). However, species are assumed not to return from other forest areas, into areas of regrowing secondary forest.

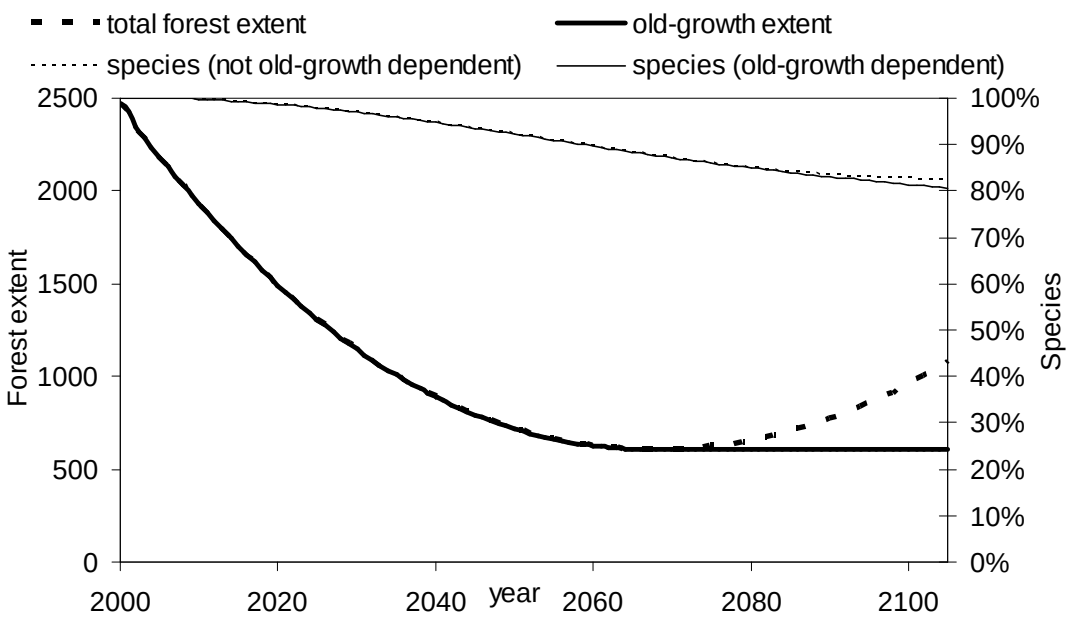

Figure 9.2. Projected species extinctions for the most pessimistic forest cover scenario (stagnant economic growth and high initial deforestation rate of 2.5%) assuming a relaxation rate with a half-life of 50 years. S_{max} in 2005 is not the number of species found in the corridor, but the number not already committed to extinction by past deforestation.

Given that estimates of relaxation rates are so rare and variable, and that they will depend on many factors (including the degree of degradation within the habitat), it is impossible to draw any firm conclusions about the correct relaxation rate to apply to the corridor. As a consequence, Figure 9.3 demonstrates the sensitivity of the predictions to variations in the assumed half-life. These are moderate, within the range considered (10-150 years). The percentage of original species lost by 2105 is increased from 9% (150 year half life) to 30% (10 year half life). The results are insensitive to alternative half-lives exceeding 150 years (6% of species lost with 250 year half-life, data not shown). However, this effect is due to the functional form (exponential decay) assumed for the relaxation process, and most studies, including Brooks et al. (1999b), do not have enough data points through time to determine the actual functional form.

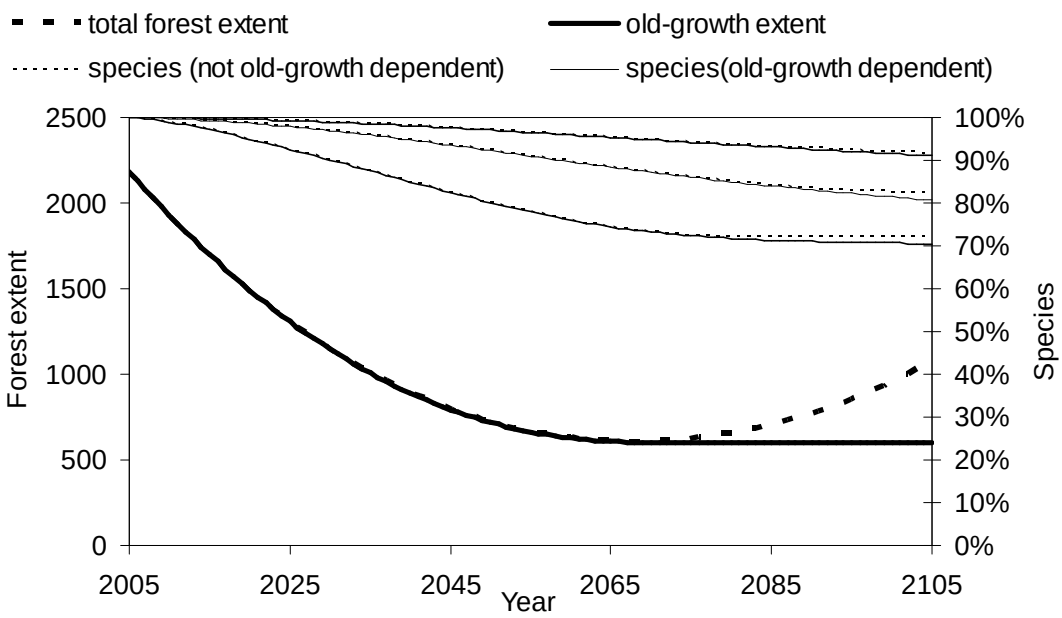

Figure 9.3. Projected species extinction rates for the most pessimistic forest cover scenario (Fig. 9.1) showing the effect on percentage of species surviving of variation in the half lives: 10 (bottom pair of thin lines); 50 (middle pair of thin lines), 150 years (top pair of thin lines).

IV. Estimating the non-use value of protecting biodiversity

Existence values of biodiversity are often estimated from contingent valuation studies (Loomis & White 1996, Bateman et al. 2002). However there are a number of difficulties associated with this approach to valuing biodiversity. The degree to which contingent valuations identify true willingness to pay (WTP) has been questioned (Pearce 2007). There may be a gap between willingness to pay estimated by contingent valuation studies and actual payments made when contributions are required (Foster et al. 1997, Kamuanga et al. 2001). Other problems may be that people refuse to make trade-offs which require the substitution of biodiversity for other goods and that their understanding of the biodiversity concept is insufficient to allow them to give meaningful values in contingent valuation studies (Spash & Hanley 1995).

Another common criticism of valuations of single sites or species is that respondents may attribute all of their WTP for conservation in general, to whichever specific site or species they are asked about in the study, an effect called "embedding" (Kahneman & Knetsch 1992). Thus, the study reveals not the WTP for the site in question, but for *all* wildlife sites (Price unpubl. provides a good summary of this and other such problems).

Despite these problems with contingent valuation, there are few other approaches available which can provide estimates for non-use values of biodiversity. For my CBA, an estimate of the international biodiversity value of the Ranomafana-Andringitra corridor is required. To reduce the problem of embedding described above, I looked for estimates of people's aggregate WTP for conservation in general from which to estimate the share of that WTP which is attributable to the corridor.[196] Unfortunately, such aggregate studies are rare (Menzel 2005, S. Menzel pers. com.). I found just two such studies that were suitable and I review them below.

[196] Note that, as I discuss below, this may still over-estimate the value attributable to the corridor, since it infers marginal values from average values, which are likely to be higher.

Candidate studies

US willingness to pay to conserve tropical rainforests (Kramer & Mercer 1997)

Kramer & Mercer (1997) used contingent valuation to estimate the willingness of US citizens to make a one-off payment to conserve an extra 5% of tropical rainforests throughout the world (over and above the 5% already in protected areas). They estimated mean WTP per household to be US$26 (in 1996 dollars), equating to $10.84 per capita (in year 2000 international $). The corridor would appear to fit this valuation object well, since it represents an extension to the protected area network in Madagascar, itself one of the highest priority conservation areas in the world (Mittermeier et al. 2004). The corridor accounts for around 0.5% of this additional 5% of the world's tropical rainforests.[197] This would imply a one-off willingness to pay to conserve the corridor of $0.054 per American.[198]

German willingness to pay to prevent extinctions (Menzel 2005)

Menzel (2005) used a dichotomous choice method in telephone interviews with 1,017 people between April and May 2001 to estimate the monthly contribution which German residents over the age of 18 were prepared to make, in the form of a "biodiversity tax", for the "protection of half of the endangered species expected to become extinct in the next 10 years" (Menzel 2005:33). The study asked respondents to reply for themselves, and not for their family or household, yielding a value per adult. The mean response was approximately €108 per year[199] or $106. Converting this to a per capita value by dividing it by the ratio of all German residents (82m, Heston et al. 2006) to adult residents (66m, Menzel 2005) gives $85 per capita per annum. However, Menzel (2005) states that this could be an overestimate, because people may, despite instructions to the contrary, have

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[197] World tropical rainforest extent = 995 ha x 10^6 (FAO 2001). Corridor extent in 2000 = 247,700ha (Hawkins & Horning 2001). 247,700/[0.05x995x10^6]=0.5%. This is an average, not marginal value, and thus (roughly) the value of protecting the corridor as part of the larger project.

[198] This and all subsequent dollar values are in year 2000 international dollars (using PPP exchange rates from Heston et al. (2006), and dollar deflators from Bureau of Labor Statistics (2008).

[199] Respondents who refused to answer, or dropped out of the survey were assumed to have a WTP of zero.

replied on behalf of their household. Therefore, I have divided the original figure by the number of people per household[200] giving $45 per annum (equating to $3.7billion nationally) which Menzel views as conservative.

This figure of $45 per annum per person represents the annual willingness to pay to protect "half of the species expected to become extinct in the next ten years" (Menzel 2005). I estimate the relative importance of the corridor in meeting this objective by estimating the proportion of this set of species which are found within the corridor. Because the data are most reliable for birds and mammal species, I restrict the calculation to these taxa. There are 362 critically endangered birds and mammals in the world (IUCN, quoted in Mittermeier et al. 2004) of which four are found in the corridor, three being found nowhere else.[201] Thus, the corridor accounts for approximately 1.66% (3/[0.5x362]) of the set of species considered by the contingent valuation study of Menzel (2005),[202] implying an annual WTP to conserve the corridor of $0.74 per capita.[203]

Comparison and interpretation of the estimates

The two estimates differ greatly in their estimate of WTP for conservation of the corridor. The estimate derived from Menzel (2005) of $0.74 per annum is far larger than that from Kramer & Mercer (1997) of a one-time only $0.054,

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[200] Calculated using the number of households in Germany quoted by Menzel (2005).

[201] The three species endemic to the corridor are: the tufted-tailed rat (*Eliurus penicillatus*), golden bamboo lemur (*Hapalemur aureus*) and the greater bamboo lemur (*Prolemur simus*). The critically endangered Madagascar serpent eagle (*Eutriorchis astur*) is also found in the corridor as well as other areas of Madagascar (H. Randrianasolo pers com). Clearly, loss of suitable habitat in the corridor would increase the probability that this bird would go extinct, making the calculation presented here conservative. Since the publication of Mittermeier (2004), the white collared brown lemur (*Eulemur albocollaris*) has been described, and classified as critically endangered. This species is found at the southern end of the corridor (including the extension to Vondrozo), as well as in the two fragments of forest in the Manombo Special Reserve and Mahabo Forest (Irwin et al. 2005, David Knox pers. com.). However, this fifth species is not included in my calculation because it is additional to the total in Mittermeier et al. (2004).

[202] Note that, strictly, the value of conserving the corridor is therefore contingent on other conservation projects undertaken worldwide, since this calculation effectively assumes that the corridor would be among the sites chosen to conserve the first 50% of critically endangered species. Given the very high priority attached to Madagascar by conservationists (Mittermeier et al. 2004), on account of its high rates of endemism, and the corridor's relatively high priority status within Madagascar (Kremen et al. 2008), this seems a reasonable assumption.

[203] Once again, this uses an average, rather than marginal value per species. the marginal value of protecting the corridor species (if the remainder of the 50% had already been protected) would probably be lower.

regardless of the discount rate and time horizon assumed by the respondents in each case.[204] Only a small proportion of the difference in WTP is due to the difference in importance of the corridor in achieving the stated goal (1.66% versus 0.5%) which results from the very high endemism, but relatively small area of the corridor (the corridor accounts for 0.8% of the world's critically endangered birds and mammals, yet only 0.025% of its tropical rainforests).

There are several other factors which could account for this difference. First, awareness of global (as opposed to local or national) conservation issues has probably increased in most western countries between Kramer & Mercer's study in 1993 and Menzel's in 2001.[205] Second, the proposed payment vehicles (one-time payment in Kramer & Mercer (1993) versus monthly payments in Menzel (2005)) could have influenced the WTP: 6 cents per month may not sound so different from a 5.4 cents one-off payment, if respondents did not carefully consider their responses. However, Loomis & White (1996) found that one-time WTP estimates were significantly higher than annual estimates, when other important characteristics were held constant. Third, there may be differences between the sampled Germans and Americans in their enthusiasm for international conservation, their belief in its efficacy, or their feelings of responsibility towards ecosystems in other countries (Menzel [2005] found that the latter two attitudes significantly affected WTP). These differences may or may not reflect differences in the benefits felt by respondents as a result of successful conservation. Fourth, the survey method (mail versus telephone) may have increased the stated WTP in Menzel's (2005) study which used the latter, since respondents may have felt under greater pressure to agree to bids when speaking to the interviewer. This may or may not be a flaw, in the case of a global and non-excludable good like biodiversity, which might be prone to free-riding, even when not actually making payments (Sen 2001).

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[204] Assuming that the first payment was collected at the same time in each case.

[205] On the other hand, Loomis and White (1996) hypothesise that WTP in the US may decrease over time, as a result of negative publicity associated with the Endangered Species Act. Although they find a negative relationship between study year and WTP, it was not significant and was difficult to separate from refinements in methods.

More substantially, the project proposed in Menzel's (2005) study (protecting 50% of endangered species) seems more ambitious than merely protecting 5% of tropical rainforests, although this is taken account of when calculating the corridor's contribution. However, contingent valuation studies do not always seem to be sensitive to the magnitude of the benefit proposed, with respondents apparently indifferent between saving 20,000 or 200,000 birds (Desvouges et al. 1993, cited in Sen 2001), although Loomis & White (1996) found that the size of the proposed increase in an endangered species' population was positively related to WTP. Although it is common practice in contingent valuation to randomise bid levels, to check for starting point bias in responses, it is less common to determine the effect of changing the size of the proposed benefit. While this is understandably difficult (though not impossible) in studies of single species or sites, it is certainly possible in studies of this type (e.g. 5%, versus 50% of species).

It is impossible to say whether these factors can account for all of the difference in WTP between the two studies, or whether the object of valuation (area of forest versus number of species) is also responsible. I would note, however, that while the global scope of these two studies offers some protection against the type of embedding effects noted above, it seems plausible that they remain vulnerable to embedding in terms of the precise nature of the object valued. For example, it seems quite plausible that many of the respondents in Menzel (2005) gave their WTP for nature conservation in general, rather than for saving half the species expected to go extinct in the next 10 years, and therefore one should be careful in drawing any firm conclusions from these studies about the relative merits of species richness versus landscape based conservation. Even if Menzel's (2005) estimate is taken as intended, there is no reason to expect that the WTP stated would, if disaggregated, apply equally to all species in all locations (Loomis & White 1996): indeed, the species found in the corridor might not even make it into people's preferred 50% of species! However, since charismatic species rely on appropriate habitats for their existence, there is a case for attributing some portion of the values of charismatic species to the other species with which they share a habitat (e.g. Sergio et al. 2006).

Another difficulty in interpreting these studies is that the counterfactual remains unknown. Intuitively the net benefits from protecting a forest should be contingent on what would have happened in the event that the forest was not protected. These expected net benefits will not simply be a function of the true counter-factual, but also of the respondent's perception of the counter factual. Thus, we do not know how many species (or what percentage of the world's total) respondents in Menzel (2001) thought would go extinct in the next 10 years, or what the respondents in Kramer and Mercer (1997) believed would happen to the rainforests if they were left unprotected. As noted in Chapter 8, considerable uncertainty exists over how much rainforest would be lost (both globally and in the corridor) if action was not taken (Dufils 2003, Grainger 2008).

As with deforestation, and for much the same reasons, the proportion of the world's species which are likely to go extinct in the future is highly uncertain (Wright & Muller-Landau 2006a,b, Gardner et al. 2007). In addition, the public's perception of deforestation or species extinctions may be biased either down (through ignorance of the problem) or up (through exaggeration by environmental organisation, Lomborg 2001). This is important, since it seems likely that the perceived threat to species and their habitats would influence the WTP for action. Indeed, Menzel (2005) found that a respondent's perception of the threat posed to global biodiversity was an important factor in determining their WTP.

Because of the difficulty of interpreting the one-off estimate provided by Kramer & Mercer (1997) and because of my focus in this case study on conservation based on species-richness, I use Menzel's (2005) estimate to derive an international annual benefit from protecting the corridor. However, I reiterate the caveats noted above, that this cannot be taken as evidence for the value of conservation focussed on species-richness per se, relative to other forms of nature conservation in developing countries. I also note that, in my view, the annual WTP estimated by Menzel (2005 ($45 per capita, equivalent to $0.74 per capita to conserve the corridor) seems high.

V. Combining economic valuations and extinction projections

Although it appears relatively straightforward to derive the share of Menzel's estimate accounted for by the corridor (as above), it is more difficult to precisely apply this willingness to pay to protect "half of the endangered species expected to become extinct in the next 10 years" to a WTP for protecting the corridor, over time. First, as noted above, many of the species which will actually go extinct in the next ten years may already be committed to extinction by past habitat loss meaning that preservation of the corridor at its 2005 extent alone will not save them. In addition, the contingent valuation was framed as an ongoing, monthly financial commitment, yet the object of valuation related only to species predicted to go extinct in the next ten years. Presumably, the benefits to respondents would increase over time, as the number of averted extinctions increases.

For simplicity, I interpret this WTP ($0.74 per annum) as being the annual benefit received by respondents, from the continued existence of the species found in the corridor, which are not already committed to extinction. I assume that this applies to all of the corridor's species, and not only those which would have gone extinct in the non-project scenario. Thus, the WTP is a maximum compensating variation (CV_{max}) representing the value to respondents of averting the complete extirpation of the corridor and its species. Therefore, the actual net benefit of the project to a respondent in any given year (CV) is determined by the CV_{max} (annual WTP estimated above) multiplied by the proportion of the corridor's species which have been saved by the project:

Equation 9.4 $$CV = CV_{\max} \cdot ([S_{project} - S_{non-project}] / S_{\max})$$

Where $S_{project}$ is assumed to equal S_{max}, and $S_{non\text{-}project}$ is determined by the deforestation projection, species area relationship and relaxation rates, as above. Recall that S_{max} is the number of species present in the corridor in 2005 that are not already committed to extinction (Equation 9.2).

This approach is not completely satisfactory, since it does not precisely map onto Menzel's (2005) contingent valuation question. However, it goes some way to addressing the problem that we do not know the respondent's perception of the counter-factual (important, given the illustrative purpose of the analysis), and

avoids the need to predict future extinctions from past deforestation. If the WTP of $0.74 is accurate, this approach may underestimate true benefits, since it values species which would not have gone extinct. Or it may over-estimate true benefits since it ignores the extinctions already destined to occur, and which the project would not prevent. It is, in any case, best thought of as an illustrative rather than definitive value, which nonetheless serves the purpose of motivating the subsequent discussion in Chapter 11.

Projecting existence values through time and space

Of course, it is not only Germans who are likely to value the existence of species – nature conservation movements are present in many countries. The value of $0.74 per intact corridor (CV_{max}), per German, per year, in 2001, must therefore be extended to other countries, and other years. The value put on a species' existence may not be constant between countries and years. It is likely to depend on several factors, including an individual's income and preferences.

Many contingent valuation studies, including Menzel (2005), find a link between the income of a respondent and their stated WTP for environmental goods (e.g. Kramer & Mercer 1997). Of course, this may partly be explained by a greater ability to pay (see Chapter 2), but whether existence values rise or fall as a proportion of income as people get richer will depend on the income elasticity of existence value. The value placed on the intact corridor, CV_{max} can be represented as:

Equation 9.5 $$CV_{\max} = a \cdot Y^{e}$$

where a is a constant, Y is income and e is the income elasticity. If e is greater than unity, existence values will rise faster than incomes. a can be parameterised such that:

Equation 9.6 $$a = 0.74 / Y_{G01}{}^{e}$$

Where Y_{G01} is the mean income of German residents in 2001[206] ($25,319, Heston et al. 2006). Unfortunately, Menzel does not provide any information about the

[206] Measured, as before, in year 2000 international dollars.

income elasticity of WTP in her study (Menzel 2005:35). Therefore, in later analyses (see Chapter 11), I include a range of elasticities, centred around unity, in projecting how existence values will change over time. Of course, the residents of different countries may also differ, on average, in their preferences for nature over other things, even holding income levels constant, and preferences for nature may change over time, independent of income changes (including due to spillover effects from rich or culturally dominant countries). For simplicity, I ignore these possibilities here.

I also ignore the possibility that Malagasy value their 'own' biodiversity more highly than do equivalent non-Malagasy. Although there is qualitative evidence that rural Malagasy do value the existence of certain biodiversity components (e.g. Jones et al. in press a) estimates of the existence values of biodiversity to very low income people is sparse (for example Turpie 2003 estimates this for South Africans, but here lowest income category is $100 per month, an order of magnitude higher than that of rural Malagasy). However, the existence values of biodiversity to local Malagasy will affect their behaviour, tending to reduce deforestation rates, and therefore the estimates of net opportunity costs considered in the following chapter.

Figure 9.4 shows the results of combining economic projections (from Chapters 6 and 7), with deforestation projections (Chapter 8) and the species-area relationships and relaxation rates (assuming a half life of 50 years), to project aggregate global non-use benefits of protecting the corridor. Non-use values rise over time as a result of global economic growth, and because the number of avoided extinctions also rises. Aggregate non-use values are very sensitive to the biological urgency of conservation to prevent species extinctions (within the range considered plausible in Chapter 8), being far higher under the most pessimistic deforestation scenario than under the most optimistic. The implications of this for the overall value of conservation are explored in the next chapter. Lower values for the income elasticity of CV_{max} (e) lead to marginally higher initial values, but considerably lower values in the future.

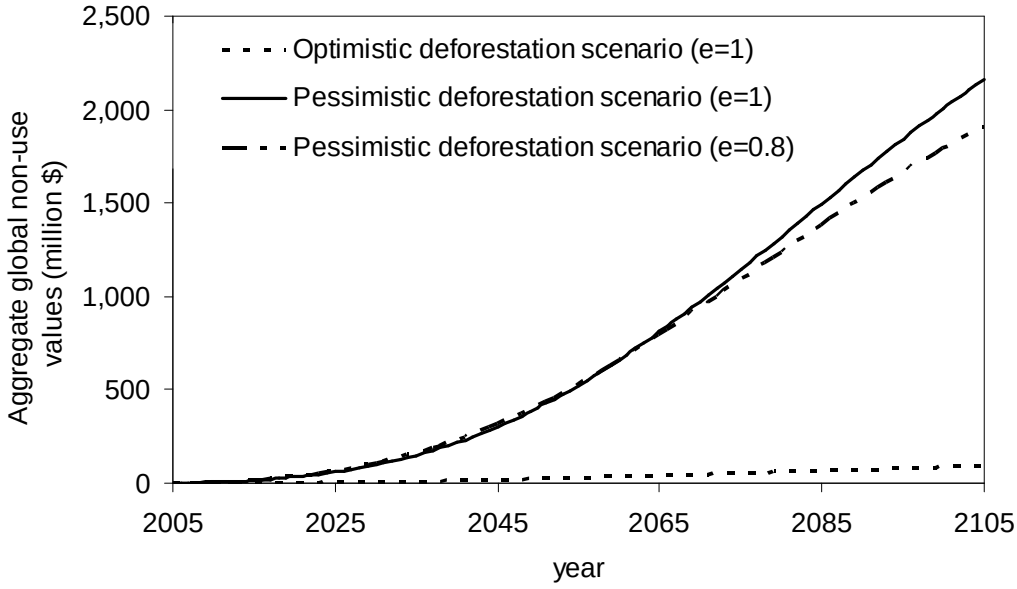

Figure 9.4. Projected aggregate global non-use values of protecting the corridor using two different deforestation scenarios: pessimistic (initial deforestation rate = 2.5%, pessimistic Malagasy economic growth) and optimistic (initial deforestation rate = 0.3%, optimistic Malagasy economic growth), as well as two different values for e (the income elasticity of CV_{max}). The half-life of the rate at which species relax to extinction is assumed to be 50 years.

VI. Conclusions

This chapter has discussed the complexity of estimating the net-benefits of protecting the corridor, in terms of global non-use values. Although considerable uncertainties persist concerning the rate at which species become extinct following habitat loss, and the degree to which regrowing (secondary) forest can and will 'rescue' species, the uncertainties concerning the economic benefits of avoiding extinctions are probably even greater. Two potentially suitable contingent valuation studies, which address global issues and therefore provide partial protection against embedding effects, are discussed and found to be difficult to compare. Nevertheless they imply very different existence values for the corridor, although they are difficult to interpret because of the difficulty of adequately specifying the counter factual, and of mapping contingent valuation questions onto real conservation projects. Nevertheless, the projections developed here are useful in illustrating the fact that the value of any conservation project will be contingent on the true biological urgency of conservation, the implications of which will be explored in subsequent chapters.

10. The local costs of conservation

Abstract

Many areas of conservation concern in developing countries are surrounded by substantial populations of poor people who depend on natural resources for their livelihoods. One of the most important components of any cost-benefit analysis of conservation projects must therefore be the assessment of local opportunity costs resulting from restricted access to natural resources.

In this chapter I outline the main difficulties associated with estimating local opportunity costs, based on my own field experience, as well as that of other authors. I then review published estimated of local opportunity costs of conservation in the eastern rainforests of Madagascar, finding them to be significant as a proportion of local incomes: estimated at between 17 and 62%. However, I note several problems in interpreting and projecting such estimates as part of a cost-benefit analysis, including the difficulty of ensuring that opportunity cost estimates are consistent with other assumptions and projections made in the analysis, such as income growth and deforestation.

I therefore propose a simple way to link opportunity costs to income and population growth and deforestation rates, and use this to project local opportunity costs over the time horizon of the project. This analysis, while crude, demonstrates the link between deforestation rates (both in the present and future), and local opportunity costs as a proportion of incomes. This highlights one extremely important point, which I believe has received insufficient attention in the conservation literature, namely that the urgency of conservation action (which was shown in Chapter 9 to drive its economic benefits) is also likely to be related to the seriousness of its ethical implications.

I. Introduction

Millions of people around the world live in close proximity to tropical forests and are dependent on those forests for their livelihoods (Grimes et al. 1994, Byron & Arnold 1999). Many of these practice traditional swidden agriculture which is a major driver of tropical forest loss or degradation (Myers 1991, Noble & Dirzo 1997). Harvesting of non timber forest products also provides goods for subsistence use or trade (Pimentel et al. 1997). Excluding local people from forests therefore entails opportunity costs for forest-dependent communities by limiting agricultural expansion and reducing access to valuable forest products (e.g. Ferraro 2002, Balmford & Whitten 2003). These costs, which are often borne by some of the world's poorest people, have been identified as a major source of conflict between conservation projects and local people, potentially reducing the effectiveness of protected areas (Wells et al. 1990, Ghimire 1991).

The livelihoods of many of the people living around the eastern rainforests of Madagascar are based heavily on small-scale swidden agriculture (Messerli 2000, Laney 2002, Styger et al. 2007) and the harvesting of forest products such as wild honey, crayfish and timber for house building (Ferraro 2002, Jones et al. 2006). One of the most important impacts of a conservation project aimed at preventing deforestation in Madagascar is therefore likely to be the opportunity costs associated with restricting these activities (Hockley & Razafindralambo 2006). In this chapter I consider some of the difficulties associated with estimating these local opportunity costs before reviewing available published estimates. I discuss how such estimates might be interpreted, and some of the difficulties associated with projecting and using such micro-level, present-day estimates in CBAs. I then outline a possible response to these issues, using a macro-level perspective to project local opportunity costs into the future.

II. Practical difficulties in estimating local opportunity costs

Estimating opportunity costs in developing countries where markets are weak or non-existent is very challenging. The principal input is the individual's own time, although the labour of others may be hired or borrowed (against reciprocal labour, or a share of the production), and wage rates are difficult to estimate. Land or extraction rights are rarely if ever sold, instead they are inherited, and often managed through communal institutions rather than as private property. Capital inputs are low; borrowing is rare. Formal capital markets are difficult for local people to access, and local markets may be shallow, often informal and governed by institutions that may not be easily understood by the outsider. Much production is for subsistence use and cash exchanges represent only a small proportion of transactions. This is true of the lives of a great proportion of humanity, but is not proportionately reflected in the activities of economists (Dasgupta 2007b).

Attempts to estimate the opportunity costs of a conservation area tend to take the form either of (i) micro-level investigations of cash-flow or production function analysis of households (e.g. Ferraro 2002, Straede & Treue 2006), or (ii) contingent valuation studies aimed to investigate the compensation level at which household's would be willing to accept restricted forest access (e.g. Richards 1994, Shyamsundar & Kramer 1996).

Below, I outline the main practical challenges to this work, based on my own field experience in eastern Madagascar. Between September 2001 and September 2006, I spent 36 months living and working in the study area, as a research assistant, independent researcher, PhD student and consultant, during which time I became fluent in spoken Malagasy. One of the aims of my research was to quantify local people's use of the forest, for agriculture and forest products. This work is summarised in Hockley et al. (2002, 2003, 2005b, 2006) and Andriahajaina et al. (2005) and some has been published as Hockley et al. (2005a), Hockley &

Razafindralambo (2006), Hockley & Andriamarovololona (2007), Jones et al. (2005, 2006, 2007a & b, in press a and b).[207]

Quantifying forest product use

Several reviews have proposed guidelines for the assessment of forest product use by local people (Godoy & Lubowski 1992, Gram 2001, Sheil & Wunder 2002). Rather than provide a comprehensive review, I focus here on the issues I found to be most important in my own field work.

Methods for estimating the use of forest products can be thought of as lying upon a spectrum of research intensity per subject. At one end is direct recording of forest product harvesting; by following harvesters (Zeleznik & Bennett 1991, Muchaal & Ngandjui 1999), recording products as they enter the village (Stearman 1990, Wilkie & Curran 1991, Hockley et al. 2005a) or monitoring forest entry/exit points and recording the flow of harvesters and goods (Appasamy 1993, Wickramasinge et al. 1996). Less direct monitoring techniques include focus group discussions or semi-structured interviews with key informants (Hegde et al. 1996, Paoli et al. 2001, Larsen 2002), harvester diaries (Gram 2001) and rapid rural appraisal (Hellier et al. 1999, Hockley et al. 2002, Sambou et al. 2002, Marshall & Newton 2003), and finally administered questionnaires (Shyamsundar & Kramer 1996, Ferraro 2002).

Rapid methods can get information much more quickly than methods involving direct observation but at a cost to detail and possibly accuracy (Belshaw 1981, Godoy et al. 1993). There has been considerable debate about the use of 'quick and dirty' methods for collecting social science data relative to more detailed methods (e.g. Stocking 1980) but few studies are available which validate the rapid techniques (Adams et al. 1997). For example, prior to the work by colleagues and I (Jones et al. in press b), only one study (Gavin & Anderson 2005) had compared the results of one-off interviews and regular reporting as methods of estimating levels of natural resource use.

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[207] I also supervised the work of others, on wild honey (Andriamarovololona 2003), bamboos (Andriamarovololona 2005), pandans (Tayer 2005), and the commercialisation of forest products (Rakoto 2004).

Having trialled the use of rapid interviews to estimate the use by local people of the forests (Hockley et al. 2002), colleagues and I established a system of daily interviews with forest product harvesters (described in Hockley et al. 2005a and Jones et al. 2005). After this study had been running for over two years, Jones et al. (in press b) carried out one-off interviews with harvesters, looking back over the previous year, and compared the results of the daily and one-off interviews. Because the two types of interview covered the same period, this provided a more satisfactory test of recall reliability than Gavin and Anderson's (2005) study. However, there are several reasons why our study provides only an upper bound on the reliability of one-off interviews. First, it is quite possible that the experience of regularly reporting and quantifying their harvests during the preceding year improved the recall reliability of respondents. Second, by the time of the one-off interviews, the interviewers were well known to the harvesters, having worked in the village for over three years. This meant that there was little incentive to deliberately mislead the interviewer about harvesting activities, since these had been directly observed over the previous year, and since the interviewers were very familiar with local practices. Third, the one-off interviews were conducted in an unhurried fashion, and made full use of Rapid Rural Appraisal techniques to elicit quantitative information; these are likely to be more reliable (though slower) than administered questionnaires.[208] The same interviewers conducted every interview, reducing the potential for alternative interpretations of survey questions. Fourth, the study focussed on just two of the most frequently harvested products (crayfish and firewood), rather than attempting to cover the full range of forest products.

Bearing these caveats in mind, the results of the study suggest that the one-off interviews provided relatively accurate information about the quantities of forest products collected per individual, although there was a tendency to over-estimate collection activity among low volume harvesters (Jones et al. in press b). If this is a general trend, it could lead to significant over-reporting, since the subjects of this study were relatively heavy harvesters by the standards of this village, which

[208] Experience shows that by using RRA it is difficult to conduct more than two interviews per day. Most questionnaire-based studies (e.g. Ferraro 1994) aim to complete far more than this.

is itself one of the most important forest-product-harvesting villages in the region (pers. obs.) Gavin & Anderson (2005) also reported an apparent tendency to over-report harvesting volumes.

Thus, while the results of this study are encouraging for the use of one-off interviews in what were close to ideal conditions, the performance of rapid one-off interviews with more ambitious sample sizes remains unquantified, particularly with regard to low-volume, low-frequency, subsistence harvesters, which are likely to form the majority of respondents in the study area (and in the study of Ferraro 1994, 2002), considered below). My opinion is that studies aiming for large sample sizes using administered questionnaires during rapid visits to an area are likely to be subject to large biases, the direction of which may be unknown and difficult to predict.[209]

Another challenge is to estimate the spatial use of the forest for forest product harvesting. If most exploitation only occurs within the outer fringes of the potential protected area, closest to the villages, then having a buffer where forest product extraction is allowed will reduce local opportunity costs (Hockley & Razafindralambo 2006). It may be possible to relocate harvesting activities carried out further inside the forest to a forest buffer zone or secondary forest, though at some cost to product quality / size or harvesting efficiency.

In the long-term study of forest product use by villagers from Vohiparara in the Ranomafana-Andringitra corridor (hereafter “the corridor”, reported in Hockley et al. 2005a), I found that most harvesting occurred relatively close to the village (Figure 10.1). If a buffer zone of at least 2.5 km in from the current edge of the forest was established, opportunity costs from lost access to forest products would fall to close to zero (Hockley & Razafindralambo 2006). Unfortunately, collecting such spatial information is costly and time consuming: this study involved daily interviews over the period of a year, including mapping of local place names using GPS (Hockley et al. 2005a).

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[209] With colleagues I trailed this approach (though using RRA techniques rather than questionnaires) in a village which we had not previously visited, with the aim of quantifying resource use. This experience contributed heavily to my negative opinion of such methods, and subsequent in-depth research in the same village (Andriamarovololona 2003) confirmed the unsatisfactory nature of much of the data collected during the initial visit, which we discarded.

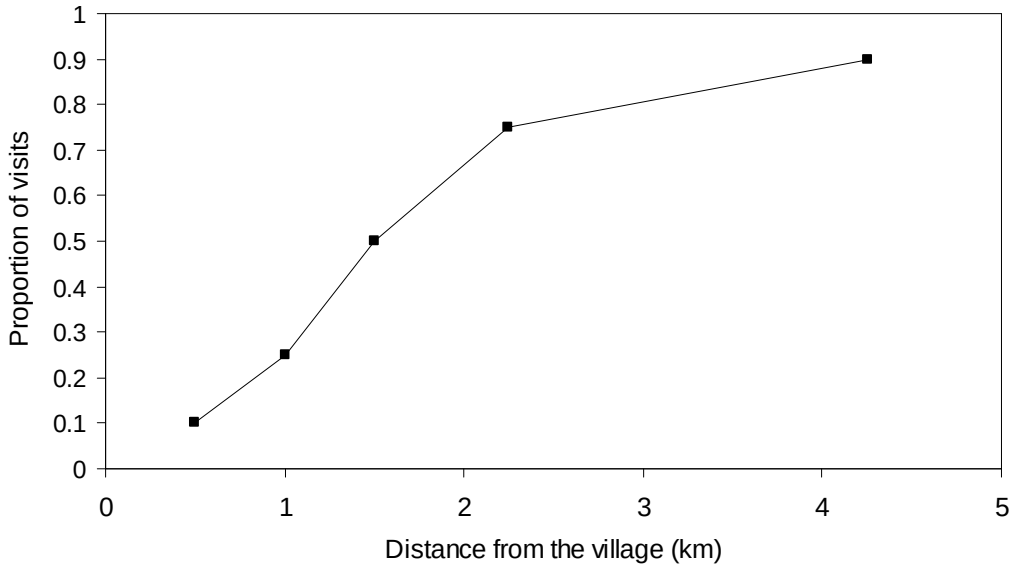

Figure 10.1. The cumulative proportion of forest product collecting trips carried out with increasing distance from a harvesting village, Vohiparara, in the Ranomafana-Andringitra corridor. Data are taken from a year-long study of forest product use described in Hockley et al. (2005a) and the graph appears in Hockley and Razafindralambo (2006).

A further challenge to identifying the opportunity costs of restrictions on forest product harvesting opportunities is to identify the degree to which the domestication of key species or the use of substitutes is possible. This will, of course, vary from species to species and domestication of forest products is not always possible (Jones et al. 2007a). However if substitutes are available or domestication can occur, the true opportunity costs of lost access will be much less than the value of the harvested products. Note, however, that domesticated alternatives may not be accessible to poorer households who lack the capital necessary (Hockley et al. 2003).

Finally, one of the most fundamental difficulties in estimating opportunity costs from micro-level data on household forest use is that it is often extremely difficult to specify the counter-factual – i.e. what households would do their access to the forest is restricted by a protected area, or when forest is lost to agriculture (Laney 2002). Most studies equate the opportunity costs of lost access to the forest with the benefits obtained by using it, which are rendered net by subtracting a shadow wage rate from the gross returns from forest use (e.g, Ferraro 2002). However, such shadow wage rates may be extremely difficult to estimate in areas like the corridor, where labour is seldom sold. In addition, any shadow wage rate that is

estimated will only be valid for marginal changes in the allocation of labour. Yet, as the studies reviewed below demonstrate, the effects of forest conservation are likely to be intra-marginal, rendering such shadow wage rates invalid.

Opportunity costs due to *teviala* prohibition

Estimating the opportunity costs of prohibitions of *teviala* and other swidden systems is extremely difficult. *Teviala* is conducted for a variety of reasons, not always directly connected to present day needs, including securing access to land for future generations (Hockley & Andriamarovololona 2007). Direct approaches to opportunity cost estimation rely on constructing a production function for *teviala*, to determine the relative returns to labour, land and capital. This approach is fraught with difficulties even in relatively conventional farming systems (Ellis 1993). It is even more difficult in the case of *teviala* in Madagascar. *Teviala* plots are seldom sold, and then only in extremis (Harper 2002), making a meaningful price impossible to ascertain. Labour inputs are almost impossible to estimate, since even the work of clearance and planting is organised semi-informally, making great use of the *haona* system, whereby farmers work for each other, for rewards which are often difficult to define, including payoffs in social capital terms (Ferraro 1994). Measuring labour inputs is further complicated by the unstructured nature of much of the work (which is not easily rendered into person-days) and the substantial time input of children and those looking after them, particularly for pest control (pers. obs.)

There is considerable inter-plot variability in biophysical characteristics (e.g. slope angle, aspect, soil nutrients) but also in the way *teviala* plots are used: the crops planted, and the fallow period between cropping (pers. obs.). This makes productivity of both land and labour difficult to determine, even for a single cropping period. However, contrary to the assumptions of some authors (e.g. Kremen et al. 2000) *teviala* plots, particularly on the eastern side of the eastern rainforests of Madagascar , can remain productive over cropping-fallow cycles for generations (Erdmann 2003a, Styger et al. 2007), and land clearance is as much an investment for the future as it is a short-term production decision. This means that the returns to both labour and land must be assessed over the productive lifetime of the plot. One consequence of this is that inter-temporal issues become

important, and the rate of time preference used by researchers may be very different from that of local people (see next section).

A second, more practical, consequence is that assessing the lifetime productivity of a plot is heavily reliant on farmer recall over several decades, which may be impossible since land is passed down and across generations and requires interviewing several different farmers for each plot, some of whom are likely to be very old or dead. From my experience, each plot must be visited in person by both the interviewer and interviewee (if they are willing), since even land-poor households will often have several, scattered plots which are difficult to define (to the interviewer). Even defining a plot is difficult or impossible, since plots are often expanded gradually, meaning that any plot more than a few years old is likely to be a mosaic of areas with different forest conversion and management histories. Since land is rarely sold, most respondents have only a hazy grasp of formal measurement units and I have found that area estimates reported in interviews can be very unreliable. Directly obtaining even rough measurements of a plot's area (necessary to link opportunity cost estimates to hectares of forest saved) can be extremely time consuming, since plots are frequently large, with many areas in a state of overgrown fallow (which may be functionally indistinguishable from the land category of "secondary forest"), steep, and rocky. Finally, ownership of plots within an extended family is difficult to determine, and often changes depending on the circumstances of the constituent households. This makes it identifying the population who will be affected by prohibitions on *teviala* difficult. In the case of a ban on agricultural expansion at the forest frontier, existing *teviala* land may be reallocated within a family, protecting the livelihoods of frontier farmers to some extent but leading to ripple effects spreading back from the forest frontier. On the other hand, the livelihoods of certain households, who lack a strong extended family, may be very badly affected by any restrictions on forest conversion. As with forest products, the counter-factual is very difficult to define, due to the unreliability of estimated shadow wage rates and the difficulty of predicting how labour will be re-allocated in the event that the forest frontier is closed and, once again, labour re-allocation may be intra-marginal.

Further complicating matters, *teviala* plots are often converted to irrigated (paddy) rice agriculture. In some cases, conversion to paddy may be carried out almost straight after forest clearance, in others, conversion takes place when the soil has begun to lose fertility (Hockley & Andriamarovololona 2007). Clearance of vegetation and cultivation in *Teviala* plots on the surrounding slopes may be undertaken to prevent shading of the paddy) or even to erode nutrients from the hillside into the paddy (Kull 2004, pers. obs.)

My field work generated extremely useful qualitative information about the conduct and importance of *teviala* (which I use below in interpreting and projecting the estimates of others). However, I concluded that achieving opportunity cost estimates with acceptable levels of precision would require an almost anthropological level of detail to the work, implying a very large research effort for a reasonable sample size.

Contingent valuation: a possible alternative for estimating local opportunity costs?

The contingent valuation method offers an alternative to direct, analyst-constructed estimates of opportunity costs. Contingent valuation studies are relatively common-place in developed countries with largely market-based economic systems (Hanemann 1994). However, they have been relatively rare in developing countries (Shyamsundar & Kramer 1996), but are rapidly increasing,[210] probably in part because of the difficulty of constructing revealed preference estimates of CVs in societies which lack an all-pervasive market-based economy. Yet, contingent valuation relies on a market analogy, and the absence of markets may weaken this analogy for the participants. For example, Shyamsundar & Kramer (1996, reviewed below) state that "[our] method solicits simple ''Yes'' and ''No'' responses to an offered bid, and therefore mimics market type everyday behavior." Yet this does not mimic everyday market behaviour in a country where haggling and non-market institutions (e.g. familial ties and power

[210] For example a search for TOPIC=("contingent valuation*" AND "developing countr*") in the Web of Science database (9th June 2008) showed a faster-than-linear increase from just 0-1 articles per annum 1990-1992 to 13 articles in 2007. The same search, this time omitting the developing country term, showed a linear increase from 15-30 per annum in 1990-1992 to 206 in 2007.

relations) dominate many exchanges. Onwujekwe et al. (2008) find evidence that contingent valuation studies based on “structured haggling” improve the validity of responses.

Linguistic and cultural barriers (including within countries) may also pose problems, and researchers using contingent valuation approaches may have increased difficulty in providing credible scenarios and payment vehicles to respondents, who are often used to the unreliability of governmental and non-governmental institutions (Whittington 2002, Whittington 2004, Chaudhry et al. 2007). These difficulties are unlikely to be solved simply through the use of non-monetary payment units, such as rice (as implied for instance by Shyamsundar & Kramer 1996).

A final problem with contingent valuation studies, wherever they are carried out, is that while it is common to look at the consistency of responses with economic theory (e.g. whether they are predicted by observed socio-economic variables), it is very rare that the results of contingent valuation are compared with actual behaviour. This makes it impossible to determine whether contingent valuations are consistently biased, either up or down. I know of only one example where the results of a developing country contingent valuation study have been compared with actual behaviour. Kamuanga et al. (2001) estimated the willingness of rural people in Burkina Faso to contribute (money and labour) to a project to eliminate tsetse fly. In this case, the project was of potentially great importance to the respondents, and was credible and quite well understood by them, having been implemented in other villages in the area. Nevertheless, the authors found that the contingent valuation study consistently and significantly over-estimated people’s actual willingness to contribute to the project. Such biases are likely to be at least as large (but of unknown sign) in cases where the project is more difficult for respondents to evaluate (such as the permanent and unprecedented loss of access to their forests).

Conclusions

Estimating local opportunity costs of conservation in developing countries is extremely difficult and whichever method is chosen it is likely to require very considerable research effort relative to the population size. Any estimates may

well be affected by significant biases of unknown sign and magnitude. In the next chapter I discuss the implications of these large decision costs and considerable uncertainties for CBA as an enterprise. In my own case, these difficulties led me to focus my own field research on aspects of natural resource use other than quantitative estimates of opportunity costs (e.g. the viability of community forest management), and to focus in this part of the thesis on the use of data on costs and benefits within CBA (next chapter). Below I review estimates of the local opportunity costs of conservation in the eastern rainforests of Madagascar produced by other authors, and discuss their interpretation and projection, subject to the caveats noted above.

III. Published estimates of local opportunity costs

Four estimates of the local opportunity costs of conservation in the eastern rainforests of Madagascar have been published: Ferraro (1994, 2002), Shyamsundar & Kramer (1996), Kremen et al. (2000) and Minten (2003). However, the assumptions made by Kremen et al. (2000), based on Masoala forest (in the north-east of Madagascar), seem implausible for the study area of the corridor. For example, they assume that the only benefit derived by local people from clearing forest is a single crop of rice, whereas the observations of myself and others (Ferraro 1994, Messerli 2002, Styger et al. 2007) suggest that cleared land is used for many years. For this reason I discount the Kremen et al. (2000) study.

Local opportunity costs of the Ranomafana National Park (Ferraro 1994, 2002)

Ferraro (1994, 2002) carried out a cash-flow analysis of local forest use in the periphery of the soon-to-be-established Ranomafana National Park, at the northern end of the Ranomafana-Andringitra corridor, during 1990-1991. The analysis was based on a household questionnaire administered by a team of non-governmental nurses to 490 households in 17 villages, together with the results of semi-structured interviews and other information, such as market prices, gathered at the same time. Many of the concerns raised in section II above apply to this study, including the reliability of one-off estimates of forest use, and the difficulty

of converting gross benefits to net benefits, and therefore opportunity costs. For example, Ferraro was not able to estimate the benefits which would be obtained by local people from *outside* the park, in the event that they were denied access to resources *inside* the park, and therefore his estimates rely heavily on combining estimated wage rates with estimated labour inputs, to determine opportunity costs. These difficulties are acknowledged by Ferraro, who deliberately erred on the conservative side, and considered his estimates to represent the lower bound of opportunity costs.[211]

Local opportunity costs of Mantadia National Park (Shyamsundar & Kramer 1996):

In July 1991 Shyamsundar & Kramer (1996) conducted a contingent valuation study to establish the willingness to accept compensation (WTA) of farmers located in the periphery of Mantadia National Park in the north east of Madagascar. They used a dichotomous choice method, using rice as a "currency"[212] to elicit the annual quantity of rice which respondents felt would compensate them adequately for lost access to the forest within the National Park "every year from now on" (Shyamsundar & Kramer 1996).

Local opportunity costs in Maroantsetra (Minten 2003):

Minten (2003), estimated the willingness of farmers in the Maroantsetra area of north-eastern Madagascar to accept compensation for halting forest clearance. WTA was again measured in rice,[213] this time using a stochastic payment card method (following Wang and Whittington 2000, 2005). As with the study of Shyamsundar & Kramer (1996), the CVM question asks for "the quantity ... of

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[211] One of the most important reasons for believing that the estimates may under-estimate opportunity costs is that local forest use in the area was already depressed prior to the park's establishment (c.f. Chapter 8), due to the area's status as a relatively well enforced classified forest (*Forêt Classée*), and Ferraro (1994, 2002) noted a tendency to under-report forest activities during the household survey, because of respondents' fears of sanctions by the Forestry Department (Ferraro 1994, 2002). Local people are unlikely to have been convinced of the non-governmental nature of the health team, since, prior to the mid-nineties, virtually all salaried workers in rural Madagascar (other than priests) were government functionaries (F. E. Rakoto, pers. com.).

[212] The unit was a *vata* of rice, which the authors state equalled 30 kg.

[213] The units were *sobika*, a basket containing approximately 12 kg of rice. Minten (2003) converted these quantities to Francs Malgache (FMG)

rice every year that you would need" to be equally satisfied as in the case where access had not been restricted (Minten 2003:6, emphasis added).

Comparison and interpretation

The three estimates of opportunity costs from these studies are shown in Table 10.1, converted to a common base year and PPP-adjusted to allow comparison with estimated local income levels (as defined in Chapter 7). Ferraro (1994) presented his opportunity cost estimates as net present values (NPVs) discounted at 5% per annum over a 60 year time horizon, but he also provided an annualised figure, which allows tentative comparisons to be made with the other studies, which estimated (constant) annual CVs. The estimates show a considerable spread (range= 97% of mean), which illustrates the great variability and uncertainty associated with estimates of opportunity costs. However, the estimates are all important when compared with local incomes.

Table 10.1. Three estimates of the local opportunity costs of forest conservation and the percentage of this accounted for by lost opportunities for *teviala*. Figures are rounded. WTA, willingness to accept.

Study	**Method**	**Annual opportunity costs capita^{-1} (2000 US$[1])**	**Percentage of costs due to teviala prohibition**	**Per capita opportunity costs as % of local incomes in 2000**	
				Total	**Teviala**
Ferraro (1994, 2002) [2]	Cash-flow analysis	27	52	17	9
Shyamsundar & Kramer (1996)	WTA, dichotomous choice	46	-	29	-
Minten (2003)	WTA, payment card (median)	96	48	62	30
	Mean	71	50	45	22

[1] Ferraro (1994, 2002) used 1991 US dollars, Shyamsundar & Kramer (1996) used 1991 US dollars, Minten (2003) used 2001 US dollars. Dollar deflators are taken from US Bureau of Labor Statistics (2008).

[2] Annualised value provided by Ferraro (1994 Table IV), based on the NPV (which assumed a 5% discount rate and 60 year time horizon).

Ferraro's (1994, 2002) estimate is lower than the other two, and there are several reasons which might explain this, aside from regional variability and the conservative assumptions noted by Ferraro. First, Ferraro's (1994, 2002) estimates

are discounted at 5% over a limited time horizon (60 years), while the other two estimates represent, at least theoretically, the respondents' own annuity estimates, derived from their own private time preference rates and time horizons. *Teviala* is often practised with long-term objectives in mind, including the provision of land to one's descendents (as discussed above), so the treatment of time will clearly be critical. Although it is commonly assumed that residents of low income countries have high time preference rates (e.g. Kremen et al. 2000[214]), this is difficult to reconcile with the low or even negative income growth rates observed in many countries (Chapter 7, Price 1993). Although those who are acutely poor may show very high rates of time preference during times of crisis (e.g. famines), there is no reason why the chronically poor, who have little reason to expect their incomes to improve dramatically, should do so, and there is evidence that residents of low income countries demonstrate only modest rates of time preference (Moseley et al. 2001).[215] Even if time preference rates are positive true welfare costs may not be well represented if annual costs are discounted (see Chapter 2). In fact, in generating his projections of opportunity costs, Ferraro assumes stagnant technology and a decline in levels of soil fertility which almost certainly imply falling real incomes. This is difficult to reconcile with positive discount rates (Chapter 2, Price 1993).

The second important factor which influences Ferraro's (1994, 2002) opportunity cost estimates is that he uses an estimate of per capita deforestation (0.003% capita^{-1} yr^{-1}) to extrapolate field-level estimates of *teviala* productivity to the household and population levels. This estimate corresponds to an equivalent annual deforestation rate for the corridor of around 0.3% per year which is lower than the range of estimates taken from the literature and discussed in Chapter 8. It is quite possible that Ferraro's deforestation rate estimate, derived as it is from ground-level observation, is more accurate than the remote sensed estimates,

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[214] Kremen et al. (2000) quote high interest rates charged on loans in Madagascar as support for assuming a high positive discount rate with respect to local benefits. However, these rates are likely to reflect risk on default. Real (and sometimes even nominal) interest rates on savings in Madagascar are often negative, once inflation has been taken into account (pers. obs.).

[215] Although poor people generally face higher mortality rates, the difference in the uncorrected population mortality rate between rich and poor countries is actually fairly small, because the population is these countries tends to be younger (WHO 2008).

which probably double-count some clearing of land in the fallow phase of the *teviala* cycle as deforestation ? of previously unconverted forest. When projecting opportunity costs into the future they must be contingent on the assumed rate of deforestation: deforestation rate may be slower than that estimated from remote sensing studies, or, if not, opportunity costs are likely to be higher than estimated by Ferraro.

This brings us to the issue of projecting opportunity costs through time, as part of a CBA of the form described in Chapter 2. First, estimates of costs should be consistent with other assumptions made elsewhere in the analysis; for example, rates of income growth or deforestation, and this is true of costs estimated either using cash-flow or production function analyses, as well as contingent valuations. Indeed, as will become clear in the next chapter, the level of opportunity costs relative to incomes over time is likely to be at least as important as the dollar value of costs themselves (which are the only important consideration in a conventional CBA). If incomes are predicted to rise over time (possibly justifying "discounting" on the basis of diminishing marginal utility of income) it is important to know whether opportunity costs themselves are likely to be a function of income over this time period. This seems very likely in the case of local costs. The dollar values of the products of field and forest will not be independent of macro-economic variables like the national income level, since this will affect them through determining demand for the products, transport infrastructure (which will greatly affect farm-gate prices of both products and inputs like fertiliser) and local technological progress (which may increase yields). As described in the previous chapter, demographic variables are likely to affect deforestation rates, and therefore both aggregate and per capita opportunity costs.

Summing up estimation of local opportunity costs

It is clear from the discussion above that the estimation of near-term local opportunity costs through direct means (whether cash-flow analysis or CVM studies) presents considerable difficulties to the analyst. When it is necessary to project these costs into the future, further problems arise, particularly in ensuring that the estimates are compatible with assumptions and projections made elsewhere in the analysis. As a result, in the next section I outline a macro-level approach to the estimation and projection of opportunity costs. While incomplete

and simplistic, this provides a way to derive projections of opportunity costs which are at least consistent with other components of the CBA.[216]

IV. A macro-level approach to estimating and projecting local opportunity costs

The previous sections have demonstrated the great difficulty of estimating local opportunity costs of conservation - the prodigious quantities of data required to do so and the difficulty of projecting the resulting estimates in a way which is compatible with assumptions made elsewhere in the CBA, and with intra-marginal re-allocation of labour. The aim of this section, then, is to explore the value of taking a macro-perspective of opportunity costs as a function of macro-economic variables.

For simplicity I focus only on costs associated with agricultural production, discounting those associated with forest product harvesting. This is both because forest product collection in the study area, although widely carried out, is relatively low in value (e.g. Jones et al. 2006) and because enforcement of prohibitions on forest product harvesting is unlikely to be fully effective in the project scenario. I base this assumption on the observation that exploitation of forest products continues within long established national parks (Jones et al. 2005, Jones et al. 2006, pers. obs.) which are smaller and better funded than the proposed new protected areas like the corridor (Hockley & Andriamarovololona 2007).

The model

The preceding chapters have developed projections, with reference to empirical data and published studies, of the following variables.

National Population Size. Chapter 7 developed projections for population size. These are assumed to be determined by non-economic factors, and rely on the projections of Lutz et al. 2001.

[216] As I explain below, these opportunity costs are scaled to the same proportion of incomes as found by the three published studies (Table 10.1).

National Income Level. This is determined with reference to vanguard technological progress in the OECD (extrapolated from historical data, see Chapter 7) and a convergence parameter, which is allowed to vary to represent varying degrees of optimism about Madagascar's progress relative to other developing countries (Chapter 7).

Rural Population Size. This is assumed to be a function of national population size and income level, the function's form having been estimated through empirical analysis (Chapter 7).

Rural income levels. These will be determined by national income levels, and determined by technological development (broadly defined to include institutions within Madagascar and infrastructure). Given the uncertainty over the relationship between inequality and economic development (reviewed in Chapter 7) I assume that these are a fixed proportion of national income levels.

Forest cover. Chapter 8 reviewed the evidence linking forest cover to the size of the rural population. Although there remains some uncertainty over whether deforestation is driven by the national or the rural population, and whether it might also be affected by macro-economic variables, I assume that it is driven solely by changes in the rural population size, the function being parameterised according to the assumed initial deforestation rate(between 0.3 and 2.5% per annum) over the period 2000 to 2005, relative to the observed population change over that period, as described in Chapter 8. I discuss this important assumption further below.

Given the functional forms and parameter values estimated for these projections, what can we infer about local opportunity costs of forest conservation? Note that, if the rural income per capita, Y_r, is known or estimated, and the rural population, P_r, is known to derive a significant proportion, f, of its income from farming, and the current area of land available, A, is known,[217] the gross income per capita per hectare which rural people receive from their land can be estimated thus:

[217] This could either be estimated from estimates of actual landholdings, or simply from the known land area of the population of interest. I use the estimates of land-holding size from Minten (2003).

Equation 10.1 $$fY_r(\$capita^{-1}ha^{-1}) = \frac{fY_r(\$capita^{-1}) \cdot P_r}{A}$$

Of course, it is possible that the importance of agriculture in rural incomes (*f*) will fall with economic development, representing 'urbanisation of the countryside'. However, this will only decouple income levels from agricultural productivity if the off-farm income is not dependent on the agricultural sector. If non-farmers are earning a living selling goods and services to farmers, it seems unlikely that such incomes could rise while farm incomes fell. If off-farm income was independent of agriculture, and rose while agricultural incomes fell, it seems likely that deforestation would slow. Therefore, for simplicity, I assume that *f* is constant at 100%, and therefore that all of the shift away from agriculture is accounted for in the predictions of urbanisation, thus equating the 'rural population' of the forest frontier fokontany[218] with the 'farming and forest clearing population'. In this and the following chapter I refer to this as the 'local' community.

Although Ferraro (1994, 2002) assumed declining productivity levels on agricultural land with time since conversion from forest, there is no macro-level evidence to suggest that rural incomes or population densities decline substantially with distance from the forest frontier and therefore with assumed time elapsed since deforestation (Minten et al. 2003).[219] This is not unreasonable, since with increasing distance away from the forest frontier the decline in soil fertility of upland land may be offset by greater investment in lowland land (conversion to paddy rice), and by easier access to markets and fertiliser. Thus, for simplicity, I assume that there are no systematic differences in the economic returns from land-holdings near or far from the forest frontier. Then, under the non-project scenario (with *f* set at 1), as agriculture expands beyond the boundary

[218] Fokontany are the lowest administrative unit in rural Madagascar, and I have assumed that the people living within fokontany bordering the forest are those bearing the local opportunity costs of conservation (see Chapter 7 for more details).

[219] Note, this does not imply that returns to labour per hectare are identical across all lands at any given time. Extended families manage land for the long term, investing in clearing some land, while planting older land and intensifying agriculture on even older land. Overall, at the macro level, incomes and population densities may not be greatly different at or behind the agricultural frontier. However, to reflect uncertainty over this, I scale my opportunity cost estimates to be broadly comparable with those reviewed above and summarised in Table 10.1, as a percentage of local incomes.

of the would-be protected area, Y_{PA}, the aggregate agricultural income coming from land within what would be the protected area (A_{PA}), can be estimated as:

Equation 10.2 $$Y_{PA} = (\frac{Y_r \cdot P_r}{A}) \cdot A_{PA}$$

where A_{PA} is determined by the area deforested, and hence by the rural population, as described in Chapter 8, and above. The main point that I wish to make here is that it is unreasonable to assume that rural incomes per capita are rising, while the productivity of land continues to fall, if agriculture makes up a large proportion of rural incomes.

Equation 10.2 estimates the aggregate income derived from agricultural activity within the would-be protected area's boundaries. To determine the true opportunity costs to local residents, (CV_{local}), this net income must be multiplied by a parameter, O, reflecting what the productivity of this labour might be

Equation 10.3 $$CV = Y_{PA} \cdot O$$

If labour productivity outside the protected area was high relative to that inside, O would have a low value (~0) and opportunity costs would also be low (~0). Now, the rate at which the marginal product of labour declines as the quantity of labour increases on land outside the protected area is likely to be positively correlated with the initial deforestation rate. If, for a given increase in population, agricultural land area expands greatly (high initial deforestation rate), this suggests that the marginal productivity of labour on existing agricultural land outside the forest is declining sharply, relative to the costs of clearing new land. If on the other hand, a given increase in population results in little deforestation, this suggests that the marginal product of labour on existing agricultural land is relatively high and constant.

In order to link opportunity costs as a proportion of gross incomes directly to deforestation rates via this mechanism of productivity of labour on existing agricultural land, I therefore scale this opportunity cost parameter to co-vary linearly with the initial deforestation rate, $D_{t=0}$, such that average opportunity costs as a proportion of income lie within the range estimated by the studies reviewed above (Table 10.1):

Equation 10.4 $$O = a + b \cdot D_{t=0}$$

where *a* and *b* are constants.

Clearly, the above analysis is simplistic, and, with the information available, does not solve the problem that the redeployment of labour when access to land in the protected area is restricted remains unknown, as it is in the already published opportunity cost estimates, but it does at least ensure that the parameter *O*, is linked to other variables in an appropriate way. To estimate *O* any other way would require more macro-level evidence than is currently available on changes in population density and incomes, moving away from forest frontiers where the forest remains open, has been closed, or has been exhausted, to determine how the productivity of labour is affected by population increase in an area with a fixed supply of land. Evidence from detailed anthropological studies of agriculture (reviewed in Boserup 1965) suggests that technological innovation, driven by necessity, maintains labour productivity even while the population increases, though leisure time, and therefore overall welfare, may decline. The parameter *O* reflects the degree to which agricultural productivity responds to additional labour, and therefore represents a point on the Malthusian-Boserupian spectrum, with Malthus (1798[1999]) expecting a poor response (and therefore starvation) while Boserup argued that productivity responds better than Malthus expected, due to the adoption of new technologies (Boserup 1965).

The model also links opportunity costs directly to income levels in Equation 10.1 (which will help determine the social weight placed upon these opportunity costs in the next chapter), and also to deforestation rates (in Equation 10.4), which determine the benefits of conservation action (Chapter 9). This allows the range of opportunity cost estimates reviewed above to be projected in a way which is internally consistent, something which is vital in CBAs of the type attempted here. It also illustrates an important point which has received insufficient attention in the conservation literature, and which I discuss below.

Illustrative results and discussion

The importance of the initial deforestation rate in determining aggregate opportunity costs in the model is clearly demonstrated in Figure 10.2. The effect of income growth on aggregate opportunity costs is buffered, because while income growth raises opportunity costs per hectare it also reducing rural

population growth through urbanisation. Under high income growth, opportunity costs per capita peak earlier than under low income growth, but then fall due to rural depopulation (Figure 10.2). In a conventional CBA, naive application of a discount rate which was independent of income growth, might lead to the conclusion that opportunity costs were more important under high income growth (see Chapter 11).

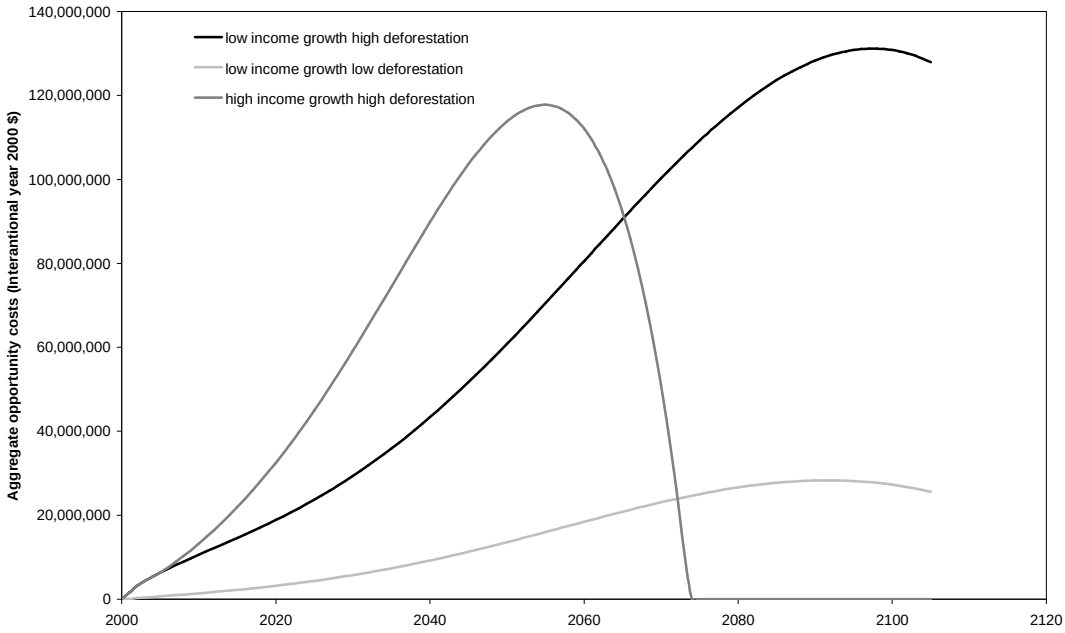

Figure 10.2. The effects of initial deforestation rate and income growth on aggregate annual local opportunity costs of preventing all future deforestation in the proposed protected area, the Ranomafana-Andringitra corridor (in year 2000 international dollars).

After five years opportunity costs, however, are a greater proportion of income under the low income growth scenario than under the high income growth scenario (Figure 10.3). Lower initial deforestation rates imply lower opportunity costs both in dollar terms and as a percentage of income.

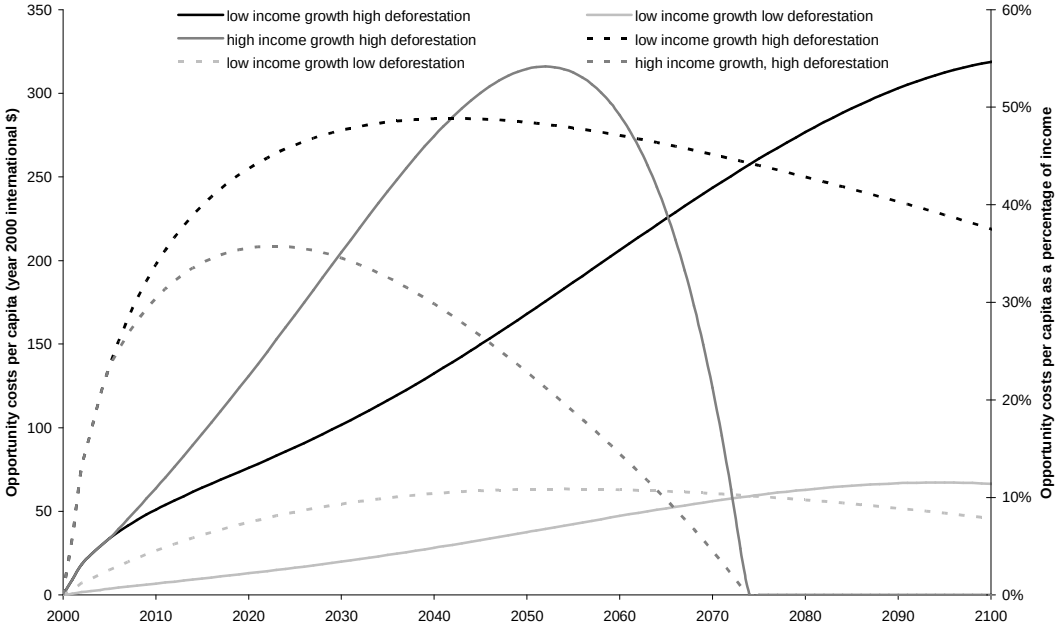

Figure 10.3. Local opportunity costs per capita, in international $ (solid lines) and as a percentage of income (dotted lines).

The urgency of conservation and its ethical implications

The model indicates that if initial deforestation rates are high (given observed population growth rate), there are high opportunity costs as a proportion of incomes. The link between the biological urgency of conservation (and therefore the international non-use benefits of conservation action) and opportunity costs as a proportion of income is in fact two-fold.

First, as shown above, the higher the estimates of initial deforestation rates, for a given observed rate of population increase, the higher are the subsequent opportunity costs as a percentage of lost gross income, since high initial deforestation rates indicate rapidly declining marginal productivity of labour on land outside the protected area.

Second, the higher the subsequent rural population growth rate, the higher the subsequent deforestation rate (if the forest is not protected) and the greater the proportion of agricultural land that will lie in what would be the protected area. Gross incomes lost due to prevention of deforestation will therefore be higher as a percentage of total incomes.

Together, this means that the more urgent and important conservation appears (high initial and projected deforestation rates), the higher will be the opportunity

costs as a proportion of incomes (though they may still be important if initial deforestation rates are low). This point is crucial, and will be explored in greater depth in the next chapter. Much of the international conservation agenda is driven by a sense of urgency, and an assurance that we must act now to avert species extinctions (e.g. Pimm et al. 2001). Conventional economic analyses of conservation, such as those reviewed by Balmford et al. (2002) reinforce this impression, by focussing on the net, aggregated benefits of conservation, which appear to be large. However, the more urgent conservation appears, the more serious are the ethical concerns with respect to the livelihoods of local people, and therefore the more important becomes the mechanisms by which conservation is achieved. This issue will be explored in much greater depth in the next chapter; for now it suffices to note that the relationship between the urgency of conservation (viewed from a biological perspective) and the net benefit of hasty action may well be indeterminate.

11. The value of biodiversity conservation

Abstract

Several studies have attempted to demonstrate the value of biodiversity conservation using conventional cost-benefit analysis techniques. In this chapter I use a partial cost-benefit analysis of the Ranomafana-Andringitra new protected area to demonstrate that, under conservative assumptions about the marginal utility of income, the net value of conservation is likely to be negative unless complete and efficient compensation is assured. I suggest that the effects of conservation on social welfare are likely to be worst where conservation appears most urgent. These findings suggest that current approaches to conservation, which seek to minimise dollar costs by directing conservation efforts to the developing world, may be counter-productive, and detrimental to social welfare.

I. The conventional economic case for biodiversity conservation

In previous chapters I developed illustrative projections of two of the most important costs and benefits of the proposed Ranomafana-Andringitra protected area. Local opportunity costs are high relative to local incomes (Figure 10.3), which are some of the lowest in the world, but much smaller in aggregate terms than the international non-use values (Figure 11.1). This result is qualitatively similar to that found in a more comprehensive CBA of the same case study (Hockley & Razafindralambo 2006), as well as in several other CBAs of conservation in developing countries (e.g. Yaron 1999, 2001, Kremen 2000) and this pattern is considered by some to be general (Balmford & Whitten 2003). In every year, the net benefits of the project (sum-of-CVs) are positive, evaluated according to the Kaldor-Hicks criterion (Figure 11.1).[220] Again, this result may be quite general (Balmford et al. 2002) and the observation that conservation in developing countries 'makes economic sense' has provided an impetus to strident calls to protect biodiversity in developing countries (Pimm et al. 2001, Balmford et al. 2002).[221]

[220] In the early sections of this chapter I will deliberately avoid questions of inter-temporal aggregation. In any case, the figures are such that, over the lifetime of the project, whichever form of inter-temporal aggregation was applied, the results would be unchanged.

[221] However, some apparently believe that such evidence is unnecessary to prove the case for conservation (e.g. Collar 2003)

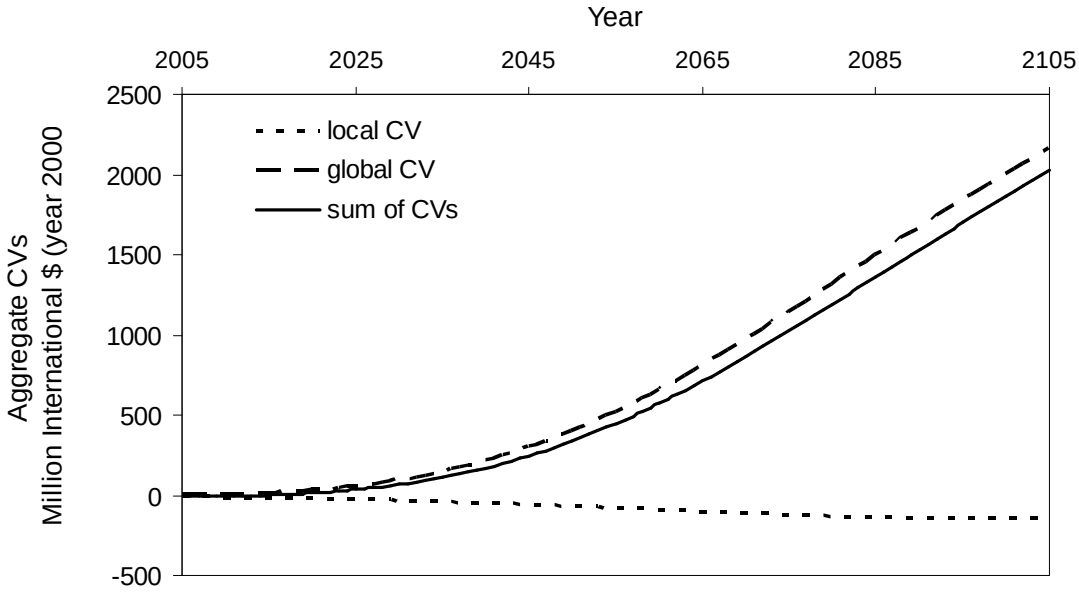

Figure 11.1. Aggregate compensating variations for the project (from Chapters 9 and 10). The sum of local communities' CVs (local, dotted line) is lower than the sum of CVs of all other stakeholders (global, dashed line). Thus, the sum of CVs in each year (according to Kaldor-Hicks criterion) is also positive (solid line). Results are shown assuming pessimistic economic growth in Madagascar (-0.1% y^{-1} convergence), 2.5% initial deforestation rate, income elasticity of existence values=1; but are qualitatively unchanged by varying parameter values across a reasonable range (0.3-2.5% initial deforestation rate, -0.01% to 2% annual convergence rate, 0.8-1.2 income elasticity of existence values).

II. Correcting CVs and measuring social welfare

However, I argued in chapter 2 that the Kaldor-Hicks criterion could not legitimately be applied except where certain conditions were met, either: i) there are no large disparities in income levels; and costs and benefits are small as a percentage of incomes; or alternatively: ii) winners and losers form part of the same society, where income can be redistributed at no cost, in order to achieve a social optimum. Neither of these conditions holds here, and Kaldor-Hicks must be abandoned.[222]

[222] An analogy that suggests itself here is that of a parametric statistical test that can only be used if certain assumptions (such as normality of the underlying data) are met. Thus, we have to resort to the economic equivalent of non-parametric statistics.

CV Correction

First, as I described in Chapter 2, CVs must be corrected for the diminishing marginal utility of income, such that they are no longer measured in dollars, but rather in terms of utility. A conventional approach to this is to first assume that utility U is related to income Y as follows:

Equation 11.1 $$U = \begin{cases} \dfrac{Y^{1-\eta}}{1-\eta} & \eta \neq 1 \\ \log_{10}(Y) & \eta = 1 \end{cases}$$

where η is the elasticity of the marginal utility of income (Layard et al. 2008). It follows that a CV measured in monetary units can be converted to a utility-based numeraire, correcting for the diminishing marginal utility of income thus:

Equation 11.2 $$corrected\ CV = U(Y + CV) - U(Y)$$

where U(...) is calculated as per equation 11.1. However, the units are arbitrary. To convert them to a more meaningful unit, we can divide through by a standard unit of utility that is specific to the project. For example, $U_{t=2005}^{OECD}(\$1)$, the utility value of one international dollar to someone with the average OECD income in 2005 ($\overline{Y}_{t=2005}^{OECD}$) is:

Equation 11.3 $$U_{t=2005}^{OECD}(\$1) \approx U(\overline{Y}_{t=2005}^{OECD} + 1) - U(\overline{Y}_{t=2005}^{OECD})$$

This represents one dollar's worth of utility to someone with that annual income (hereafter $\$_U$).

Estimates of η differ, with HM Treasury (2003) advocating unity, while Dasgupta (2007c) argues that it should be between 2 and 4. In addition, some authors (e.g. Price 1989) suggest that the marginal utility of income should approach infinity not at Y=0, but rather at some minimum level of income necessary to maintain survival, Y_s, such that Y-Y_s is substituted for Y in equation 11.2 above.[223] This has

[223] Dasgupta (2007:6) also suggests this, when he writes: "In fact, to suppose that [η] is 1 is also to suppose that starvation isn't all that painful!

the effect of greatly increasing the utility value of costs and benefits to those with incomes close to Y_s, and little effect on those with higher incomes. I take η=1, and make no adjustment for survival incomes (i.e. marginal utility approaches infinity at Y=0), and thus make only a very conservative correction for the diminishing marginal utility of income. Nevertheless, this value of unity is quite sufficient to illustrate the points I wish to make, as shown by Figure 11.2 which plots corrected-CVs for the Ranomafana-Andringitra corridor new protected area project (hereafter 'the project'), and the sum-of-corrected-CVs. The effect of valuing project effects using corrected- rather than uncorrected-CVs is enormous: welfare losses exceed gains by several orders of magnitude, over the time horizon considered, even using a conservative value for ρ.

Thus, it can clearly be seen that, under conservative assumptions about the elasticity of marginal utility of income, and a utilitarian social welfare function (of which more below), conservation is shown to be economically highly undesirable (over the time horizon considered). It is difficult to say how general this result may be, however the example of Yaron (1999, 2001) considered below, suggests that it is not unique to this case study. It is important to stress that this analysis relies on nothing that is not standard economic theory, even if it is not always standard CBA practice.

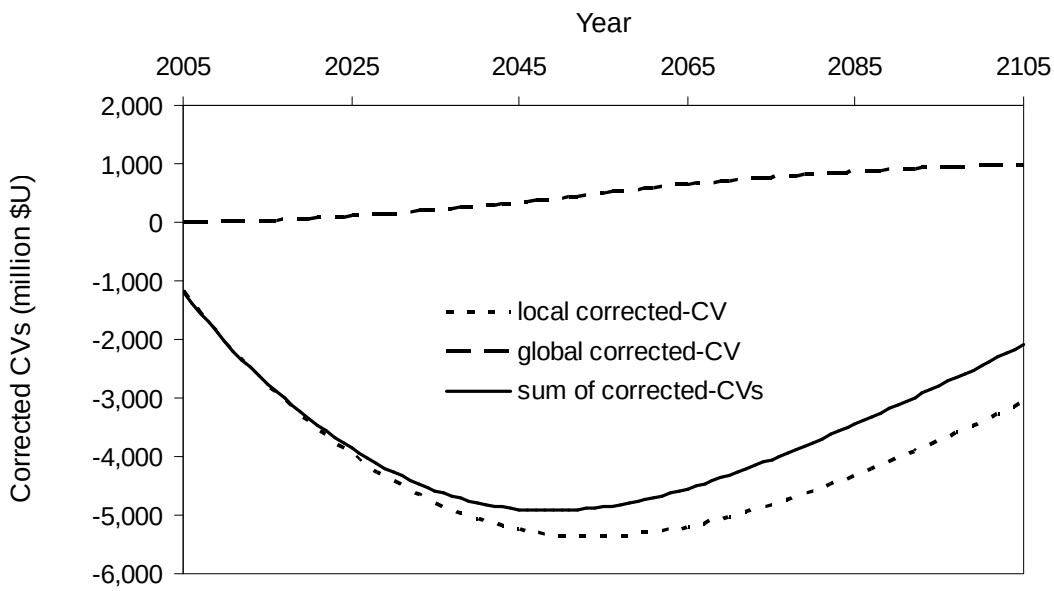

Figure 11.2. Aggregate corrected compensating variations for the project for: local communities' (local, dotted line)and all other stakeholders (global, dashed line). Sum of

corrected-CVs is also shown (solid line), and are negative throughout the period considered. CVs are calculated as per Figure 11.1, correcting CVs as per equations 11.1, 11.2 and 11.3 for the diminishing marginal utility of income (η=1). Values are corrected to constant utility units $\$_U$, worth \$1 of utility to someone with the mean OECD income in 2005, $U_{t=2005}^{OECD}(\$1)$. Results are shown assuming pessimistic economic growth in Madagascar (-0.1% y^{-1} convergence), 2.5% initial deforestation rate, income elasticity of existence values=1; but are qualitatively unchanged by varying the values of these parameters across a reasonable range (0.3-2.5% initial deforestation rate, -0.01% to 2% annual convergence rate, 0.8-1.2 income elasticity of existence values).

Intra-temporal aggregation and social welfare functions

The sum-of-corrected-CVs presented in Figure 11.2 implies that utilities are additive, and therefore that society wishes to maximise a utilitarian social welfare function. This means that society is indifferent as to which of its members gains or loses a given utility increment. However, several alternative social welfare functions have been proposed, including Rawlsian (Equation 2.2), which aims simply to maximise the utility of the lowest utility person, and prioritarian (Equation 2.3), in which utility gains to individuals are weighted according to their utility levels, though not infinitely (Johansson 1993). As I noted in Chapter 2, the process of summing individuals' utilities in a social welfare function is often seen as part and parcel of correcting for the diminishing marginal utility of income to the individual, i.e. correcting in one step for the changing *social* marginal utility of income. One reason for seeing it this way is that the two things: marginal utility to the individual and to society, may be difficult to separate, at least on the basis of evidence from individual consumer's behaviour. Also, the effect of applying a prioritarian social welfare function can be achieved by increasing the value of η, above that derived from individual 'self-regarding' behaviour (Chapter 2). However, such a view is incompatible with, say, a Rawlsian social welfare function: society may recognise that a moderately well-off person would be happier if they were to receive some increase in their income, while at the same time choosing to accord exclusive priority to the interests of the least-well off.[224]

[224] This raises, of course, the questions of who determines society's preferences, and in what sense an individual is happier if they were to receive an income increment which their society would rather had gone to someone else. Are their wishes in conflict with that of their society? Or are self-serving and disinterested preferences meaningfully separable, such that they would be *happier* with the increment than without, but would *prefer* it to go elsewhere?

This discussion also ignores the existence of any other rights that an individual may have to utility increments (or to the property and actions that provide them) aside from those which result from their relative utility level. Yet most societies are plainly not organised in such a fashion, and other rights, such as those to property and free action, are also recognised. In Figure 11.2, and all subsequent analyses, I conservatively assume that utilities are additive, i.e. I assume a utilitarian social welfare function. However, as I proposed in Chapter 2, I will refer to the importance of rights (other than those deriving from an individual's relative utility) in several places below, since I find them to be a helpful way of reconciling moral intuition and observed behaviour. For now, it suffices to note that the above analysis is neutral with respect to the rights, of local residents and others, to the utility gains and losses resulting from implementing or not implementing the project.

Inter-temporal aggregation and net present value

Since the CVs are now corrected to represent their presumed social value (under conservative assumptions), it is as legitimate to sum them over time, as it is within years, if a zero rate of pure time preference is assumed. In Chapter 2 I argued that the only proper basis for a pure rate of time preference was the probability of exogenous catastrophic change, either the risk of extinction of humankind, or unforeseen technological change which renders project effects obsolete. Both of these are hard to quantify, and while the former component must be strictly positive, though hopefully very low, the latter could take either sign: events may come to pass which render the project's effects (avoided non-human species' extinctions) *more* (rather than less) valuable than foreseen (Price 1993). For simplicity, I therefore take the pure rate of time preference to be zero. Calculated on this basis, the net present value (NPV) of the project presented in Figure 11.2 is -377,903 $x10^6$ $\$_U$. Of course, the opportunity cost of capital cannot be ignored. However, as argued in chapter 2, it is best dealt with when considering compensation, which I do below.

III. The effect of CV correction: a further example

The problem of relying on conventional CBAs based on the sum of CVs is vividly illustrated by the following example.

Reviewing over 300 case studies of nature conservation, Balmford et al. (2002) found just five studies which met their criteria of completeness. Drawing on these studies, Balmford et al. compared the net present values of conserving substantially unaltered wild nature with those of conversion or significant degradation. They found a large variation in benefit cost ratios of conservation, from 1.16 to 3.79 (mean=2.43 s.e.=0.56), with the two terrestrial examples noticeably lower (mean = 1.19, Table 11.1).

Table 11.1. Benefit cost ratios of conserving wild nature, from Balmford et al. (2002).

Ecosystem (Country) and Study	Conservation NPV	Conversion NPV	Benefit:Cost Ratio
Coral Reefs (Philippines) White et al. (2000)	$3300	$870	3.79
Mangroves (Thailand) Sathirathai (1998)	$60,400	$16,700	3.62
Wetlands (Canada) van Vuuren and Roy (1993)	$8800	$3700	2.38
Tropical Forest (Cameroon) Yaron (2001)	$2570	$2110	1.21
Tropical Forest (Malaysia) Kumari (1994)	$13,000	$11,200	1.16
Mean +/- Std error			2.43 +/- 0.56

All five studies calculated their net present values using the conventional sum-of-uncorrected-CVs. Unfortunately, none of the case studies provide sufficient information about the distribution of costs and benefits to determine how correcting CVs for unequal marginal utilities of income, or assuming non-utilitarian social welfare functions, would affect the results. However, Yaron (1999), provides more detail about the Cameroonian example cited by Balmford et al. (Yaron 2001). Using the information given in this report, it is possible to carry out a very rough-and-ready adjustment for the diminishing marginal utility of income.

The case considered by Yaron (1999, 2001) is the conversion of natural forest to small scale agriculture versus its management for sustainable forestry. The benefits considered include private benefits of production under the two systems,

as well as social benefits. The later included those accruing at the local or national level (e.g. flood prevention and non-timber forest products) and the global level (carbon sequestration and option, bequest and existence values of biodiversity).

Methods

Yaron (1999) provides a breakdown according to two stakeholder groups (Cameroon and international) of the net benefits (CVs) from conservation and conversion of natural forest in Cameroon (Table 11.2). Mean incomes of these two groups are taken from UNDP (2004). Conservatively, taking the elasticity of marginal utility of income to be unity (as per HM Treasury 2003), I calculated corrected-CVs for each group, measured in dollar's-worth of utility to the richest group (international).[225]

Table 11.2. Net benefits of conservation (through sustainable forestry) and conversion (to small scale agriculture) from Yaron (1999), and mean per capita incomes from UNDP (2004).

Stakeholder group	**Net benefits (present value) ($US)**		**Mean income per capita**
	Conversion	**Conservation**	
Cameroon	677	91	2,000
International	252	913	7,804
Total Net Present Value	929	1,004	
Total benefit cost ratio	1	1.21	

Notes: data taken from Table 5 of Yaron (1999:35), and UNDP (2004). Sterling values converted to dollars using £1=$0.6. Numbers may not sum due to rounding.

Results

The results are dramatic: because the costs of conservation are borne principally by the poor in Cameroon, while the benefits accrue globally, adjusting for the diminishing marginal utility of income changes the benefit cost ratio from 1.21:1 in favour of conservation, to 2.28:1 in favour of conversion (Table 11.3, Figure 11.3).

[225] Because the breakdown of costs and benefits provided by Yaron (1999) did not give per capita costs, I weighted costs and benefits using the relative *marginal* utility of income at the average income level (as per Pearce et al. 2006), rather than weighting intra-marginal costs using intra-marginal weights, as in equation 11.2 above.

Table 11.3. Net benefits of conversion and conservation, adjusted for the diminishing marginal utility of income.

Stakeholder group	Net benefits (present value) (US$'s-worth of utility to richest group)		Correction factor for marginal utility of income
	Conversion	Conservation	
Cameroon	2,640	353	3.9
International	252	913	1
Total Net Present Value	2,893	1,267	
Benefit cost ratio	2.28	1	

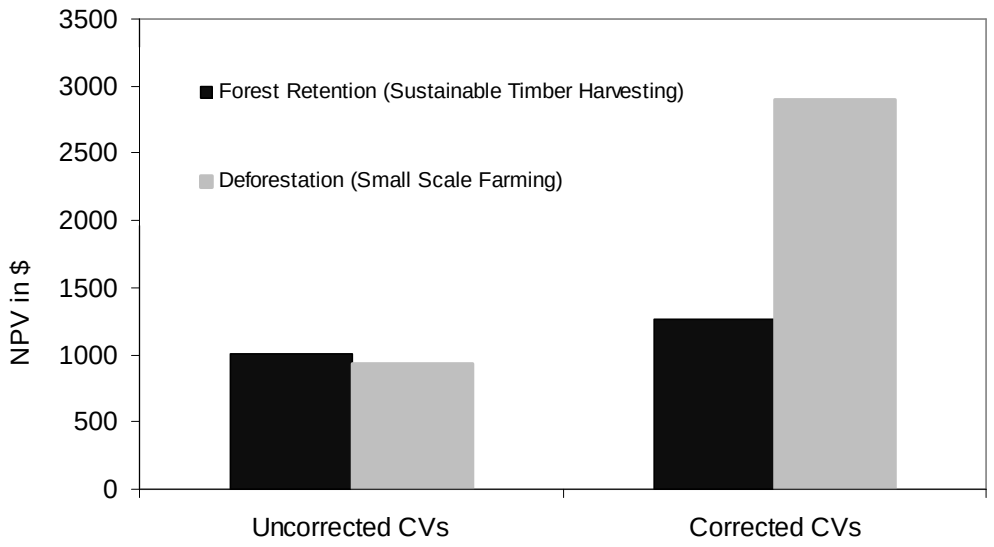

Figure 11.3. The effect of using corrected-CVs on a CBA of nature conservation, based on data from Yaron (1999) and UNDP (2004) using a conservative estimate of the elasticity of the marginal utility of income (unity).

Discussion

Of course, the analysis carried out above is very crude, and other considerations, or appropriate compensation mechanisms, could easily make the net present value of conservation positive again. Nonetheless, it serves to illustrate an important point: the effect of moving away from conventional CBAs is large compared with the benefit cost ratios found by Balmford et al. (2002), and could easily reverse their conclusions. Benefit-cost ratios are not objective measures, like metres or tonnes, but rather they are social constructs, dependent on value judgments for

their meaning. They cannot therefore be used in the way that Balmford et al. (2002) have used them, to argue decisively for the desirability of conservation.[226]

IV. The biological urgency and social desirability of conservation

In chapter 10, I noted the link between the urgency of conservation (initial and projected deforestation rates) and the magnitude of local opportunity costs as a proportion of local incomes. I concluded that as conservation action appeared more urgent, so its ethical implications became more serious. This is well illustrated in Figure 11.4, which plots the annual sum-of-corrected CVs (calculated as for figure 11.2) for two initial deforestation rates (0.3% and 2.5% year^{-1}), holding constant all other factors (economic growth, η, etc). The sum-of-corrected CVs are considerably more negative if initial deforestation rates are high (2.5% year^{-1}) than if they are low (0.3% year^{-1}). The true net economic costs of conservation are far higher under the high deforestation rate scenario, than under the low deforestation rate scenario. Thus, without effective compensation (see section V, below) the true value of conservation is likely to be inversely proportional to its biological urgency. Given the difficulty of ensuring full compensation (see Section V), this result calls into question the strategy of prioritising conservation efforts in those areas deemed to be most at risk (e.g. Myers et al. 2000), a point which has been made by some conservationists, who seek to reduce conflict between the creation of protected areas and humans (e.g. Balmford et al. 2000). However, a more novel implication of this analysis is that, since the welfare cost implied by a dollar of opportunity cost will be larger, once corrected for the marginal utility of income, if born by a low-income person than by a high income person (Equation 11.1), and since dollar opportunity costs are likely to be higher when incomes are higher (Chapter 10), the strategy in conservation planning of minimising the dollar value of opportunity costs (e.g. Naidoo et al. 2006) by concentrating efforts in the developing world (Balmford et

[226] Balmford et al. (2002) do make reference to compensation in their discussion, but nowhere do they discuss whether the value of conservation, and therefore their results, might depend on it.

al. 2000) will be counter-productive, *unless* effective compensation is ensured. I turn to this possibility below.

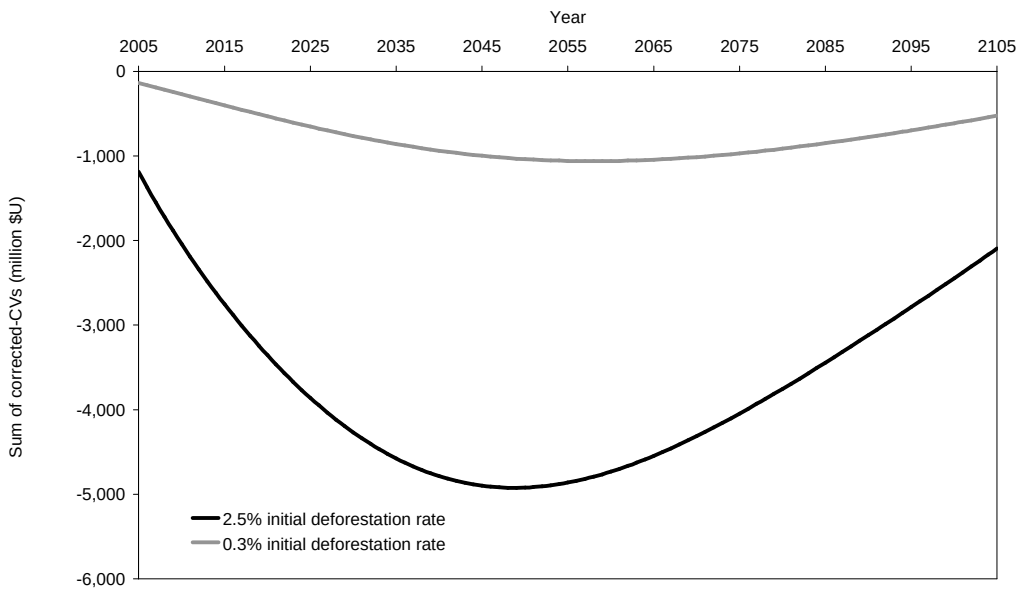

Figure 11.4. Sum-of-corrected-Compensating Variations (CVs) for the project for alternative deforestation scenarios: high initial deforestation rates (2.5% year-1, black) and low (0.3% year-1, grey). Corrected-CVs are calculated as per Figure 11.2. Values are corrected to constant utility units $\$_U$, worth $1 of utility to someone with the mean OECD income in 2005, $U_{t=2005}^{OECD}(\$1)$, using elasticity of marginal utility of income=1. Results are shown assuming pessimistic economic growth in Madagascar (-0.1% y^{-1} convergence), 2.5% initial deforestation rate, income elasticity of existence values=1; but are qualitatively unchanged by varying the values of these parameters across a reasonable range (0.3-2.5% initial deforestation rate, -0.01% to 2% annual convergence rate, 0.8-1.2 income elasticity of existence values).

V. Compensation

Compensating project losers at the expense of project winners has the *potential* to turn the value of conservation strongly positive. However, it is worth emphasising here that this is only true if compensation is actually implemented, and is sufficiently effective and efficient. Although some conservationists acknowledge the desirability of compensation (e.g. Balmford and Whitten 2003), others argue that it is unnecessary (Collar 2003), and the issue is entirely ignored in much of the literature on conservation strategy and economics (e.g. Pimm et al. 2001, Balmford et al. 2002, Naidoo et al. 2006), where the Kaldor-Hicks potential compensation paradigm reigns unchallenged. The analysis so far has demonstrated the very substantial importance of not aggregating CVs according to

the Kaldor-Hicks criteria. This section demonstrates the importance of paying proper attention to the empirical realities of compensation.

The neglect of compensation in economic analyses of conservation is surprising, given that the conservation literature is replete with examples of the failure of conservation projects to adequately and efficiently compensate for local opportunity costs (Peters 1998, Ferraro 2001, Wells et al. 1990). Where compensation is assumed in economic analyses, thereby reducing local opportunity costs, the authors rely on simplistic assumptions about the true cost of effective compensation. For example, despite noting that development projects in Africa have a 50% failure rate (World Bank 1993), Kremen et al. (2000) assume a 100% success rate for the compensation mechanism considered by their CBA, which would leave local people better off than before restrictions were imposed on their forest use. As I show below, if such assumptions turn out to be false, the true welfare value of the project will be changed enormously.

If we consider compensation as a transfer of money (which may purchase goods and services, and therefore enhance utility) from those who benefit from the project to those who lose from it, two important parameters will be the efficiency of the transfer and the completeness of the compensation. The first concerns the costs of transferring money (or anything else which increases welfare) from winners to losers: the 'leakiness' of Okun's leaky bucket (Okun 1975). The second concerns how complete the compensation of losses is. Projects that succeed in compensating 100% of local costs, with 100% efficiency, will simply increase local CVs to zero, and reduce international CVs by the same dollar costs.

If a compensation program is implemented to transfer money from those who bear net costs as a result of the project (in my study this is the communities local to the forest frontier [227], 'local'), and those who benefit ('global'), then post-compensation CVs for the local communities are given by:

Equation 11.4 $$CV_{local}^{withcompensation} = (1-C)\cdot CV_{local}^{withoutcompensation}$$

[227] *Fokontanys* bordering the forest: see Chpater 7.

where C is the completeness of compensation, as a proportion of opportunity costs. The full costs of compensation, which are born by the global beneficiaries are given by:

Equation 11.4 $$\cos ts\ of\ \ compensation = (P_{local} \cdot (C \cdot CV_{local}^{withoutcompensation})) / E$$

where P is size of the population negatively affected by the project and E is the efficiency of compensation, such that for every dollar deducted from a beneficiary of the project, E dollars actually reach those who have lost as a result of the project. In the analysis below, I assume that the costs of compensation are shared amongst the global beneficiaries (OECD, ASIA, rest of Madagascar, etc) in proportion to their aggregate benefits from the project, i.e. that beneficiary CVs are reduced by a constant percentage, equal to the total costs of compensation divided by the total global benefits from the conservation achieved by the project. For the present, compensation is assumed to take place within each year, such that there is no delay between raising compensation funds from beneficiaries and distributing them to those bearing costs, though I return to this below.

For simplicity, I also assume that the costs of transferring income from beneficiaries to losers are deadweight costs, such as hiring project staff in a competitive labour market. I also ignore the possibility that compensation mechanisms may be misdirected at either end. For example, the costs may be raised from the wrong people, e.g. rich country residents who did not benefit from the project (which is likely if funds were raised through general taxation, as in the Global Environment Facility, Menzel 2005). In addition, compensation might be inaccurately disbursed, and paid to those who did not actually suffer any costs from the conservation project (e.g. Sanchez-Azofeifa et al. 2007). Both of these cases are quite likely, in which case this portion of the compensation program would represent a simple transfer of income from rich to poor countries (not necessarily richer to poorer people, for example if it is appropriated by rich people in the poor country), with associated deadweight costs. The effect of such inaccuracies on the project's value will depend on the rights-weightings applied to these transfers in the CBA. If we assume that people have a greater claim to their own income than to benefits resulting from the project, and further that such claims outweigh the effect of the changing marginal utility of income as it is

transferred from rich to poor, then such inaccuracies in raising and distributing compensation will tend to reduce the value of the conservation project, in addition to any deadweight costs of the transfer.

Complete compensation

Because of the relative magnitudes of dollar costs and benefits, if compensation is complete (i.e. C=100%, $CV_{local}^{withcompensation} = 0$), net benefits may be positive, even with quite low compensation efficiencies. For example, the NPV of the project is positive with compensation efficiencies of around 13% or greater (Figure 11.5, assuming parameter values as per Figure 11.2). I know of no estimates of the efficiency of compensation achieved by conservation projects, although Peters (1998) reported that, of the funds allocated in the US to support the Ranomafana National Park Project, just 2% was allocated to projects aimed at increasing local incomes as compensation for the project. This is not an estimate of compensation efficiency (the project had other objectives, and income-raising projects may have raised incomes by more (or less) than the money spent on them), but Peters' account provides a good description of the overhead costs of many internationally funded conservation and development projects.

Incomplete compensation

The effect of reduced completeness of compensation (C<100%), however, is dramatic and non-linear. If the completeness of compensation is reduced to 85%, compensation efficiency must be 100% for the project to break even (NPV=0, see Figure 11.5). With C=85% or lower, the NPV of conservation is negative, even with complete efficiency. If completeness falls below 85%, it becomes impossible for the project to break-even. At C=80%, even compensation efficiencies of >1000% (i.e. for every dollar deduction from a beneficiary of the project ten dollars reach those who have benefited from the project) are not sufficient to ensure that the project breaks even.

This reflects the very high marginal utility of income in the local communities. Note that in calculating the whole-project completeness of compensation figure of 85%, I assume that compensation coverage is even, i.e. that all local residents are compensated for exactly 85% of their costs. If compensation is spread less evenly, the project value would decline further, since the value of a dollar of

uncompensated losses rises as losses increase as a proportion of income. Again, I reiterate that the correction applied to CVs and the social welfare function used to aggregate CVs are both conservative.

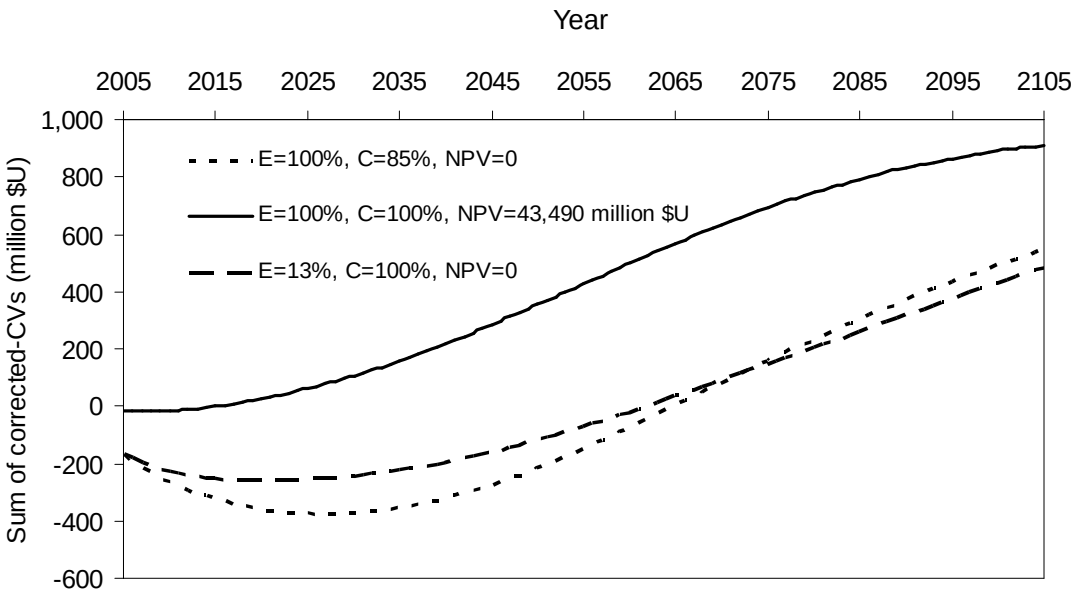

Figure 11.5. The effect of compensation efficiency (E) and completeness (C) on the value of the conservation project. Sum-of-corrected-Compensating Variations (CVs) are plotted shown for three different combinations of C and E. Net present values (NPVs) are also calculated as described in section II, above. Corrected-CVs are calculated as per Figure 11.2. Values are corrected to constant utility units $\$_U$, worth $1 of utility to someone with the mean OECD income in 2005, $U^{OECD}_{t=2005}(\$1)$, using elasticity of marginal utility of income=1. Results are shown assuming pessimistic economic growth in Madagascar (-0.1% y^{-1} convergence), 2.5% initial deforestation rate, income elasticity of existence values=1; but are qualitatively unchanged by varying the values of these parameters across a reasonable range (0.3-2.5% initial deforestation rate, -0.01% to 2% annual convergence rate, 0.8-1.2 income elasticity of existence values).

This analysis demonstrates the effect of assuming that compensation is completely and costlessly achieved by the project when, in fact, that may not be the case. Of course, the completeness of any compensation project will be extremely hard to measure either ex ante or ex post, for the simple reason that opportunity costs are difficult to measure, as described in Chapter 10. This raises questions about how we can ensure that conservation is carried out such that it does make “economic sense” (i.e. is socially desirable).

Irreversible welfare losses due to delayed action

Proponents of action on environmental issues, such as climate change and biodiversity loss, often refer to the possibility that irreversible changes will occur

if action is not taken (e.g. Arrow 1995). For example, the conservation benefits of protecting the Ranomafana-Andringitra corridor (hereafter "the corridor") arise from preventing species extinctions, which (with current technologies) are irreversible, and habitat loss, the reversal of which is difficult (Hardwick et al. 2004), if not impossible. Such non-reversible changes pose a challenge to conventional economics, in which changes are traditionally assumed to be marginal and reversible (Dasgupta 2008). Non-reversibility in natural systems leads some to invoke the precautionary principle, arguing for immediate and decisive action, even before the likely implications of inaction are clear (e.g. Pimm et al. 2001).

However, it is perhaps not so well recognised that irreversible welfare losses can occur in the near-term, as well as in the distant future, and as a result of environmental action, rather than inaction.[228] One example, which is relevant to the case study, is when protected areas are created before compensatory projects have succeeded in raising local incomes. This scenario may be quite common since, given adequate funding, it is relatively quick to deploy guards to protect the forest against encroachment, while compensatory projects may take longer to achieve real increases in local incomes. For example, Hockley & Razafindralambo (2006) found that while sharing ecotourism revenues might eventually be sufficient to compensate for local opportunity costs in the corridor, even if optimistic projections of tourist numbers are used, it would take several decades for full compensation to be achieved. Durbin & Ratrimoarisaona (1996) document how past projections of ecotourism numbers in Madagascar have been over-optimistic. Alternatively, protected areas may use rural development projects to compensate local people but, as acknowledged by Kremen et al. (2000), such projects have a high failure rate (World Bank 1993).

Now, once a compensation project is up and running, it might be possible to 'back-date' compensation, ensuring that lifetime opportunity costs as a result of the project are reduced to zero. However, dead people cannot be compensated, and anyone dying between the start of the conservation restrictions and the start of the compensation project, will have suffered uncompensatable (i.e. irreversible)

[228] I owe this insight to Landsburg (1995).

welfare losses. In figure 11.6, I estimate the irreversible welfare losses due to a ten-year delay in commencing compensatory projects after conservation restrictions have been implemented in the corridor. I take the annual mortality rate in the local communities to be 1.3% per annum, which is the figure for Madagascar as a whole (WHO 2008), and assume that, once the compensatory project has started, compensation is backdated for all surviving residents. I also assume that opportunity costs and income levels are not related to age and mortality risk, which probably underestimates irreversible losses (see e.g. Hardenbergh 1993, Harper 2002). By way of comparison, I also plot global aggregate welfare losses due to species extinctions which would occur if protection was delayed by 10 years (2015, instead of 2005). Panel A of the figure presents the results taking η (the elasticity of marginal utility of income) to be 1, and Panel B uses η =2, the lower end of values suggested by Dasgupta (2007c). In the former, conservative, case irreversible local welfare losses are small relative to global losses. However in the second case, as η rises to 2, they become very high. Once again, I have used a purely utilitarian social welfare function to aggregate and compare losses, which is probably conservative.[229] I also assume that welfare losses from past species extinctions are not reduced with the passing of time, by adaptation or technological progress.[230]

Thus, responding to a perceived conservation emergency by hastily implementing protected areas, without paying proper attention to compensatory mechanisms, is quite likely to result in very significant and irreversible near-term welfare losses. This is likely to be a common issue when evaluating the merits of precipitous action at the expense of those who are relatively poor in the present generation. This is not to say that acting in a precautionary manner can never be justified, but

[229] I have ignored bequest values in this analysis, and therefore the possibility that an ancestor's welfare losses can be compensated by raising the incomes of their descendents. However, I see no reason why bequest values can only operate forwards in time, and, whereas the descendents may be aware of and saddened by the treatment of their ancestors, their ancestors may be unaware of the treatment accorded to their descendents.

[230] In fact, there is good evidence that the effects on welfare of negative and positive shocks (e.g. limb amputations and lottery wins) is reduced substantially over time through adaptation (see Adler & Posner 2008 on injuries, Gardner & Oswald (2007) and Kuhn et al. (unpublished) on lotteries). Whether these assessments can be extrapolated to species extinctions is unclear.

merely to highlight the fact that irreversibility is not a purely environmental phenomenon.

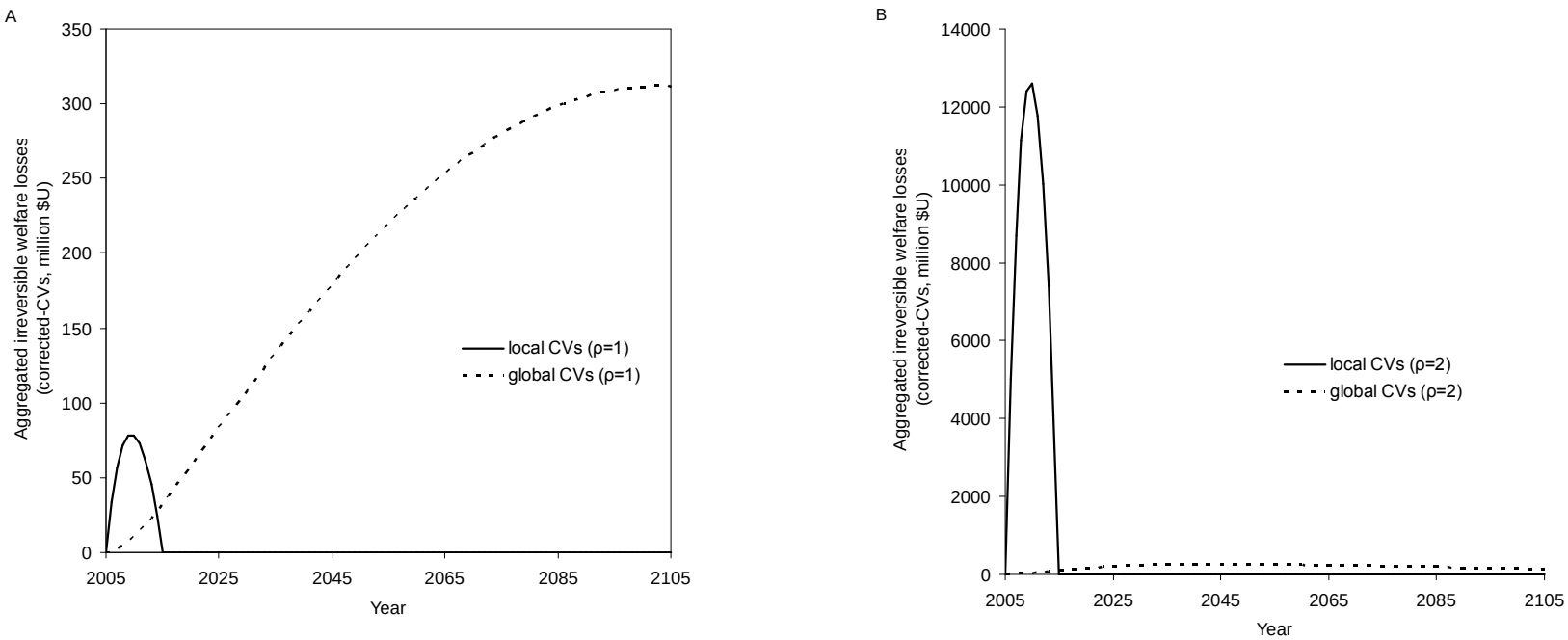

Figure 11.6. Irreversible welfare losses due to a ten-year delay in the implementation of conservation project (dotted line) and a ten-year delay in starting complete compensation (solid line). Two scenarios are presented: a conservative value (unity) for the elasticity of marginal utility of income, η, (Panel A); a high value for η (2) (Panel B). In both panels, initial deforestation rate = 2.5%; Malagasy economic convergence = -0.1%; and survival income =0; and a utilitarian social welfare function is assumed.

VI. The price of rights

One of Sen's three "foundational principles" of CBA was additive accounting: "benefits and costs are defined, ultimately, in the same space" (Sen 2001:102). This is true whether CBAs measure benefits and costs in monetary- or utility-based numeraires. However, I have argued in Chapter 2 and again above, for the incorporation of rights into CBA. How can rights be measured in the same space as utility? I propose that rights can be valued, in terms of the social welfare opportunity cost.

For example, the analyses above have assumed that each individual has an equal right to the utility lost or gained as a result of the project, i.e. local people have just as much right to convert the forest as OECD residents have to expect species to be conserved. In that case, all costs and benefits are weighted at unity, and decisions are based solely on maximising the (utilitarian) sum of utility gains. However, suppose that we contrast this situation with one in which local people, regardless of their low income status, have no moral right to clear the forest, which is, after all, the legal situation. In this case, the opportunity costs to local people from conservation would be zero-weighted in the analysis (while benefits from non-use values resulting from successful conservation are weighted at unity).

In the former case, where society insists on the forest-use rights of local people, social welfare is maximised by paying (at minimum) complete compensation to the local people. However, even with 100% efficiency, this reduces social welfare compared with the case where local costs are zero-weighted by 5,998 x 10^6 $\$_U$ (NPV= 43,490 x 10^6 $\$_U$ versus 49,488 x 10^6 $\$_U$), which can be thought of as representing society's willingness to pay for recognising local rights. If society continues to insist on these rights, as compensation efficiency drops to, say, 50%, social welfare gain over the period considered (2005-2105) decreases to 37,492 x 10^6 $\$_U$, representing a willingness to pay of 11,996 x 10^6 $\$_U$ to protect these rights. Note that abrogating rights does not actually increase the sum of human happiness, it merely redistributes it from those performing morally indefensible activities (e.g. clearing forest), and whose happiness is not considered, to those who are not. Such a 'moral calculus' is implicit when governments, such as

Madagascar's, outlaw the clearing of forest without compensation (often encouraged by international conservation NGOs).

VII. Conclusions

It is highly likely that with realistic values for the elasticity of marginal utility of income, any conservation project in developing countries which does not succeed in completely and efficiently compensating local opportunity costs will have a net negative effect on social welfare. Social welfare effects are likely to be worst where conservation appears to be most urgent, from a biological perspective. The effect of the project on social welfare is highly dependent on the efficiency and completeness of compensation, which may be very difficult to ensure and impossible to measure. Thus, unless very considerable attention is paid to compensation mechanisms, ensuring that they are fully functioning prior to instituting restrictions on local resource use, it is very difficult to make any meaningful case for conservation.

12. Discussion

I. Key findings

Part I introduced the importance of global environmental issues which present a formidable challenge to the decision-making capabilities of human society. I then review cost-benefit analysis (CBA) as a tool with which to analyse these issues, and reach decisions about the desirability of action. The most important points made in Part I were (i) that CBA cannot be both ethically neutral *and* decisive and (ii) that conventional CBA is so simplified (particularly with respect to inter-personal aggregation) that when applied to complex issues its results can be misleading or meaningless. Therefore, in CBAs of global environmental issues analysts must roll back the simplifying assumptions which underlie conventional CBA. This observation, which is not novel (Price 1993, Spash 2007b) but is perhaps under-appreciated, has motivated the work in the subsequent chapters.

One of the most important simplifying assumptions of conventional CBA is that the income levels of all populations everywhere always increase over time in an exponential manner, and with a common rate. Or, alternatively, that the market interest rate available everywhere (and at which all consumption gains may be invested) is constant through time and space (Price 1993). This is the departure point for Part II, which asks what we can know about future income growth.

In Chapter 3, I looked at what the historical data, if extrapolated forwards, might tell us about future income trends. In doing so, I explored the controversy surrounding the most important long-range income projections, those of the IPCC's Special Report on Emissions Scenarios (SRES, Nakicenovic & Swart 2000). The results of this analysis (which I deliberately limited in scope to the terms of the original debate[231]) are mixed. I found the SRES to be broadly consistent with historical growth in the OECD region, while being somewhat more pessimistic about growth in Asia, and rather more optimistic about growth

[231] Castles & Henderson (2003a,b), Nakicenovic et al. (2003) and Grübler et al. (2004).

in Africa, Latin America and the Middle East, than the historical data suggest.[232] Overall, the evidence suggested that the evolution of income levels over time is poorly represented by exponential functions, rather variable over time and space, and quite likely to be flat or negative in some places and periods. I therefore concluded by arguing for the use in CBA of income growth projections that are explicitly related to, but not constrained by, the historical evidence, and which incorporate both subjective and statistical probabilities, rather than taking the form of discrete scenarios to which no relative probabilities are attached. That this will be more onerous than current practices, (which discount future costs and benefits assuming uniform, exponential growth) is not in doubt but with modern computational capabilities can be achieved.

Chapters 4 and 5 addressed a question often posed by ecological economists in respect of CBAs (e.g. Spash 2007b), and economic policy in general (Czech 2002): can income growth can continue in the future as it has in the past, without hitting the buffers of environmental constraints? In other words, can the economic scale of human society continue to increase, without its environmental scale exceeding bio-physical limits? Having reviewed the literature on this question, I focussed on the evidence provided by a commonly used pair of proxies for the economic and environmental scale of human society: GDP and Ecological Footprint (Wackernagel & Rees 1995). I demonstrated in Chapter 4 that previous analyses of this issue (e.g. Dietz et al. 2007) contain several flaws, the most notable of which is that they focus on the consumption effects of economic prosperity (which are easily attributable to the country of origin), ignoring the technological progress which also accompanies it, and which is harder to attribute to its country of origin. In section VI of chapter 4, I demonstrated the quantitative effect of this limitation in previous analyses (e.g. Dietz et al. 2007).

Having argued that the true sample size of any study which did not attribute technological effects to their country of origin was effectively one (the world), I proposed that in the absence of detailed data on technological overspills[233], one might as well analyse a single time-series for the world as a whole. This I did in

[232] The former communist world was not considered for lack of pre-1990 data.

[233] For example by tracking “marker” innovations, or patent citations.

Chapter 5, finding evidence consistent with complete decoupling of per capita environmental scale from economic scale *at the margin* (i.e. economic growth does not increase environmental scale) and some evidence consistent with *intra-marginal* decoupling (economic growth reduces environmental scale). This finding suggests, in contrast to previous studies (e.g. Rosa et al. 2004), the possibility of an environmental Kuznets curve.

Finally, Chapter 6 concluded Part II by bringing together the income growth projections developed in Chapter 3, with the models linking economic and environmental growth from Chapter 5, and published population projections (Lutz et al. 2001), to project global Ecological Footprint, over the coming century. The results indicate that if intra-marginal decoupling were to be achieved, the extent to which the environmental scale of human society exceeds the biological capacity of the earth ("overshoot") could decline swiftly, turning negative by around 2020. If, on the other hand, only marginal decoupling was achieved in the future, overshoot would continue to rise, and would remain positive throughout the century, driven not by growth in income but in population. Thus, an important conclusion of Part II is that, while we cannot be certain whether future increases in income levels will take place without environmental catastrophe (Arrow 1995), there is no evidence from this analysis that they must necessarily be the *cause* of that catastrophe, and some evidence that they may help to avert it by enabling investments in technological progress to be made. However, I emphasise again that these results provide no grounds for complacency, and no support to those who argue that political action on environmental issues is unnecessary since technological progress (narrowly defined as the products of R&D labs) will save the world. My reason is that technology is defined in my analyses (and in most economic analyses) to include policies and individual preferences, as well as patentable inventions. On the basis of these analyses, I concluded that the economic projections developed in chapter 3 (and refined in chapter 6) are environmentally *plausible*, and retained them for Part III of the thesis (with caveats about the need for improved environmental policies firmly in mind).

Part III focussed on a specific case study – the conservation of biodiversity through protection of the Ranomafana-Andringitra forest corridor in south-eastern Madagascar. The purpose of this case-study was to provide a real-life illustration

of the problems of conventional CBA, noted in chapter 2. Although only partial (several costs and benefits were excluded from the analysis), it nevertheless illustrated important aspects of the economics of biodiversity conservation.

Biodiversity conservation and the case study of the Ranomafana-Andringitra forest corridor, were introduced in chapter 7. In chapter 8, I developed explicit projections of the counterfactual, i.e. what would happen to forest cover in the corridor were it not protected. One of the most important conclusions of the chapter is that great uncertainty exists about what would happen if action was not taken (a feature of many environmental issues - Grübler & Nakicenovic 2001), and that this uncertainty is partly linked to variables, such as income growth in Madagascar, which play important roles elsewhere in the analysis. This underlined the importance of ensuring that CBAs are internally consistent (Price 1993), a point which recurred in Chapters 9 and 10. I found evidence for a positive relationship between income level and urbanisation, and between rural population size and forest cover. Although the relationship is descriptive, and not necessarily causal, I assumed for subsequent chapters that forest cover in the absence of conservation would be driven by rural population size, and hence by income level.

Chapter 9 continued the work of specifying the counter-factual, reviewing and applying evidence linking forest cover with species extinctions. Here again, uncertainties persist, for example over the ecological suitability of regrowing forest for forest-dependent species, and the time lag between habitat loss and extinctions. However, in my opinion, these uncertainties are small relative to the great uncertainty over the true economic value of avoiding extinctions, which I also reviewed in this chapter. Nevertheless, I linked values obtained through contingent valuation to predicted extinctions, in order to estimate the non-use value of protecting the Ranomafana-Andringitra forest corridor. This valuation can only be taken as illustrative but I am not aware of any similar studies which explicitly analyse all the links in this chain: from economic growth, through urbanisation to forest cover, and hence species extinctions and non-use values. This analysis illustrates the point that the benefits of conservation action are highly uncertain, and linked to the severity of habitat loss and species extinctions

in the counter-factual, which are in turn linked to income and population. The implication of this became clear in the following chapter.

Chapter 10 examined local opportunity costs of conservation, reviewing the difficulties associated with estimating them, and the uncertainties surrounding them. Against this background, the main conclusion of the chapter was that, whatever the uncertainties, there are good reasons to expect opportunity costs (as a proportion of incomes) to be directly and positively linked to the biological urgency, and hence the global non-use benefits of conservation, which drive much of conservation policy. This means that, the greater the apparent urgency of conservation, the more ethically serious an undertaking it is.

Chapter 11 concluded part III, by comparing the local costs and global benefits of conservation. Although the net-benefits of conservation are large and positive when evaluated using the Kaldor-Hicks potential compensation criteria, I showed them to be very negative once costs and benefits have been corrected for the diminishing marginal utility of income. As a consequence, my analysis demonstrated that the economic case for conservation made by others (e.g. Balmford et al. 2002) rests on an assumption of complete and efficient compensation, the plausibility of which has received inadequate empirical attention in conservation. If compensation is absent, delayed, or incomplete, it is very likely that conservation will be detrimental to human welfare. Since achieving full compensation in the developing country context may be difficult, because of very imperfect information about opportunity costs, the present strategy of prioritising conservation efforts towards the developing world may be misguided.

II. Limitations

The question explored in this thesis: “How should we decide what to do about global environment issues?” is both deep and broad. The complexity of the issue means that it cannot be completely resolved, and in exploring one aspect of the question, six other, equally interesting and apparently essential, lines of enquiry rear up to divert one’s attention. As such, it is perhaps not surprising that my

ambitions for the preceding chapters have not been completed. I list below a selection of this unfinished business.

Chapter 2 outlined a generalised CBA framework, which was partially enacted in Chapter 11. However, much remains to be explored, and it would be interesting to investigate further the value of different approaches to aggregation, in terms of their ability to communicate information to decision-makers.

Chapter 3 demonstrated the statistical and subjective uncertainty associated with projecting variables such as income into the future. The same uncertainty is associated with projections of urbanisation (Chapter 7), deforestation (Chapter 8), extinctions (Chapter 9) and local opportunity costs (Chapter 10). Ideally, a CBA should incorporate this uncertainty (which, given the many interactions, is as likely to compound itself as to cancel out) into the final results. It is striking that rigorous estimates of confidence in the final results of CBAs are rare (Tol 2005), and I would have liked to have taken a more rigorous approach to this here. Similarly, I was struck by the rarity of ex post assessments of CBAs.[234] Although it is too early to assess the accuracy of any projections made in this CBA, it would be interesting to revisit earlier CBAs of proposed nature conservation projects.

Chapters 4 and 5 illustrated the importance of incorporating technological progress into analyses of the environmental impacts of economic growth, but did nothing to investigate the source of this progress. This issue has been explored in analyses of other environmental impacts, e.g. pollution (Stern 2004) but not, to my knowledge, for such a broad-based measure as Ecological Footprint. The lack of such an analysis greatly limits the conclusions which can be drawn from the global time series analysis presented in Chapter 5.

The value of biodiversity, and avoided extinctions remains, in my view, almost completely unknown (see also Price unpubl.), and Chapter 9 made numerous, rather unsubstantiated assumptions in an effort to produce an illustrative and plausible projection of values. Similarly estimating local opportunity costs of

[234] A search for Topic=(("cost-benefit analys*" or "benefit-cost analys*") AND (ex post)) on Web of Science (June 2008) yielded just 24 records. Scrutiny of abstracts and papers showed just three involved the comparison of ex post with ex ante CBAs (Boardman et al. 1994, Aubert 1999, Lunney 2001).

conservation in the poorest countries (where land markets are almost non-existent) remains an enormous challenge. Chapter 10 identified many of the problems, but was less successful in solving them. The biggest problems are probably those of accounting for intra-marginal changes, and in projecting costs and benefits through time.

Chapter 11 highlighted the importance of explicitly accounting for the costs and limitations of compensation mechanisms when assessing conservation projects in developing countries. The design of such projects presents enormous challenges in developing countries where de facto and traditional property rights often remain unofficial, and this is an issue which I would have liked to explore more (Hockley & Andriamarovololona 2007 provides further discussion of this).

There is, no doubt, much that I have omitted. Oscar Wilde reportedly said “I’m not young enough to know everything”, and writing this thesis has certainly aged me.

III. Cost-benefit analysis for global environmental issues

This thesis has demonstrated some of the complexity of CBA when applied to global environmental issues. Yet this complexity is inherent in the issues, not the methods used to analyse them. If a single decision must be reached on behalf of society a great deal of information must be weighed and aggregated or else ignored. This presents decision-makers with a weighty burden, which cost-benefit analysis may be able to lighten.

Unfortunately, conventional approaches to CBA produce results which are difficult to interpret and are justifiably controversial. There is a danger that they may lead decision makers to over- or under-weight factors excluded from the analysis, and so for CBA to do more harm than good. Despite the caveats that may dutifully be attached to it, it is hard to avoid the conclusion that CBA pretends to a decisiveness it cannot deliver. It seems to me that Kaldor’s (1939) goal, of a fully objective economics delivering important advice to decision-makers, is unattainable in the complex context of global environmental issues. If stripped down to its objective core, in which inter-personal utility comparisons are impossible, the scope of economics would occupy only a fraction of the territory it

presently claims. However, I do not believe that economists should retreat from the policy arena – they have much to offer. Instead, they must subordinate themselves to society. Their job should be to provide a framework for decision making, in which all ethical viewpoints can be represented. The objective of this framework should be to isolate and clarify controversies, whether empirical or normative, not to assume them away. It is not possible for an economist to conduct a CBA in isolation, except as a purely exploratory exercise.

It seems to me that only in such a subordinate role can economists achieve Keynes' (1931[1991]) ambition, "to get themselves thought of as humble, competent people on a level with dentists". Economics is not inherently unethical or misanthropic: the problem is not with economics *per se*, but with its oversimplification. The devil is in the lack of detail and this thesis has been an exploration of this detail.

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Appendices

A: A review of the SRES controversy

B: Are the SRES projections of PPP-based GDP valid? A review and analysis of the Penn effect

C: Sampling the Penn World Table data

D: Published papers

Listed below are all of the papers I have published during the period covered by my PhD. Only two of these (marked *) come directly from work included in this thesis. I include all of these papers except those marked with an X.

Hockley, N.J., Jones, J.P.G., Andriahajaina, F., Manica, A. & Randriamboahary, J.A. (2005) When should communities and conservationists monitor exploited resources? *Biodiversity and Conservation,* **14**, 2795-2806.

***Hockley, N.J.,** Edwards-Jones, G. & Healey, J.R. (2007) Maximizing the efficiency of conservation. *Trends in Ecology and Evolution,* **22**, 286-287.

*__Hockley, N.J__., Jones, J.P.G. & Gibbons, J. (2008) Technological progress must accelerate to avoid an increase in the overshoot of global Ecological Footprint *Frontiers in Ecology and Environment,* **6**, 122.

Jones, J.P.G., Andriahajaina, F.B., **Hockley, N.J.**, Balmford, A. & Ravoahangimalala, O.R. (2005) A multidisciplinary approach to assessing the sustainability of freshwater crayfish harvesting in Madagascar. *Conservation Biology,* **19**, 1863-1871.

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XJones, J.P.G., Andriahajaina, F.B., **Hockley, N.J.,** Crandall, K.A. & Ravoahangimalala., O.R. (2007) The ecology and conservation status of Madagascar's endemic freshwater crayfish (*Parastacidae; Astacoides*) *Freshwater Biology,* **52**, 1820-1833.

XJones, J.P.G., **Hockley, N.J.** & Andriahajaina, F.B. (2007) The potential of native species aquaculture to achieve conservation objectives: freshwater crayfish in Madagascar. *International Journal of Biodiversity Science and Management,* **3**, 217-222.

Jones, J.P.G., Andriamarovololona, M.A. & **Hockley, N.J.** (in press) The role of taboos and social norms in conservation in Madagascar. *Conservation Biology*.

Jones, J.P.G., Andriamarovolona, M.M., **Hockley, N.J.**, Gibbons, J.M. & Milner-Gulland, E.J. (in press) Testing the use of interviews as a tool for monitoring trends in the harvesting of wild species. *Journal of Applied Ecology*.

A. A review of the SRES controversy

I. Castles and Henderson's critique

The most important of Castles and Henderson's criticisms can be summarised as follows.[1]

1) The IPCC scenarios projected GDP_{MER} for the most part. This is the wrong metric to use: GDP_{PPP} should be used instead.

2) By using GDP_{MER}, they have overstated the 1990 gap in per capita income between rich and poor countries. The SRES also assumes an over-optimistic rate of convergence of poor countries on rich countries. Over-estimating the gap, and then overestimating the speed at which the gap is closed, produces growth rates for developing countries which exceed those historically seen, or which can plausibly be expected. As evidence for this, they made selective comparisons of growth rates in per capita GDP_{MER} from the SRES projections with per capita GDP_{PPP} from the historical past.

3) Those GDP_{PPP} projections that the SRES does include alongside GDP_{MER} (from the MESSAGE group of models), are not true PPP projections.

4) Throughout their critique Castles and Henderson argue that the SRES has not encompassed the lower bound of emissions, since it has not encompassed the lower bound of growth. To this end, they focus particularly on the scenario with the lowest emissions (B1), when making comparisons with the historical data.

II. The SRES team's reply

The SRES team responded to Castles and Henderson's critique thus.

1) First, the SRES reflected the wider scenario literature, and data availability, in predominantly using GDP_{MER}, and was innovative in including any GDP_{PPP}

[1] Other criticisms, while perhaps valid, concern the process and personnel of the IPCC, and do not directly concern this thesis except to note that Castles and Henderson allege that the IPCC has drawn, however unintentionally, on the expertise of too narrow a professional milieu, with little input from economists, economic historians and national statisticians.

projections at all. In any case, they argued, GDP_{MER} is the preferred measure for emissions scenarios (since commodities like oil are traded internationally), and for measuring *growth* in income, though GDP_{PPP} is the best measure of *static* welfare.

2) Castles and Henderson's comparisons of projected GDP_{MER} growth rates with historically observed GDP_{PPP} growth rates does not compare like with like. Assuming a steady relationship between GDP_{MER} and GDP_{PPP}, for poor countries which are converging on the reference economy (the USA), growth rates will be higher for GDP_{MER} than for $GDP_{PPP.}$ [2] When Castles and Henderson's comparison is repeated using growth rates from the GDP_{PPP} projections, or with GDP_{MER} growth rates from the past, they show that the projected developing country growth rates are broadly in line with historical experiences of countries like the US and Japan.

3) The allegations that the GDP_{PPP} projections are false is unfounded, since the projections display exactly the properties which would be expected of such projections: namely that the difference between the GDP_{PPP} and GDP_{MER} series is largest for the poorest regions, and declines as these countries converge on the OECD region.

4) Castles and Henderson are guilty of selection bias, in selecting the B1 scenario for most of their comparisons of developing country growth rates. This scenario shows the 2nd highest developing country growth rates of all scenarios. Castles and Henderson confuse high-*emissions* with high-*growth*, and assume that the low-emission scenario would show even lower emissions if its growth rates were reduced. In fact, they demonstrate that re-running the B1 scenario with lower developing region growth rates actually results in higher emissions. This is due to the lower capital turnover and slower adoption of new technologies, which results from lower growth. The B1 scenario owes its low-emissions to optimistic assumptions about technological progress, rather than low-growth.

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[2] This is because the MER growth rate captures increases in the income level due to both genuine growth, and to a reduction in the degree to which the income level is underestimated by MER measures. As countries approach the income level of the reference economy, MER and PPP series will tend to converge (for the reference economy they are identical).

As regards 4), the fact that Castles and Henderson confused low-emissions with low-growth is surprising, given their strident critique of the SRES team for, they alleged, doing just that – assuming that more growth means more emissions. Whether or not the IPCC has stated that belief elsewhere, it was clearly not assumed in the SRES scenarios. The SRES team were correct to highlight Castles and Henderson's selectivity, and the historical plausibility of the SRES economic projections is dealt with in the main body of the chapter. I deal comprehensively with 3) in Appendix B. Below I examine the first two points.

III. The suitability of PPP or market exchange rates

What should be projected, GDP_{MER}, GDP_{PPP} or both? The SRES team noted that they were constrained by their terms of reference to review the literature rather than carry out new analyses. If the literature on the relationship between emissions and GDP has predominantly used GDP_{MER}, there was, they argued, little they could do except follow suit. However the SRES team also offered two reasons for preferring GDP_{MER}, over GDP_{PPP} in general, regardless of the limitations imposed by the terms of reference of the SRES. Neither is well-founded.

A. Growth vs static comparisons

Nakicenovic et al. argue that while GDP_{PPP} is the best measure for "(static) comparisons of economic welfare (income and consumption) across different world regions and countries", GDP_{MER} is "the preferred measure for GDP growth". This is spurious: what then is the preferred measure for growth in welfare?

B. Elaborate statistical constructs

The SRES team argue for the use of MER, on the grounds that it is "a directly observable economic variable, as opposed to PPP, which is an elaborate statistical construct".

This is false on two counts. First, all GDP series measured in constant prices (regardless of the currency) are elaborate statistical constructs, and the conversion of GDP in current prices to GDP in constant prices (i.e. taking account of inflation), for a given country, is no simpler, and indeed is directly analogous to, the conversion of GDP in national currencies to GDP in a single currency across nations using PPP exchange

rates. Thus, at best, MER is a *less* elaborate construct than PPP, but elaborate nonetheless. Second, Ryten (2004) argues that true market exchange rates are not directly observable, requiring elaborate statistical constructs to be employed to derive a true MER figure: i.e. one which measured the income of citizens relative to the prices of imported, rather than all goods.[3] Ryten also makes the important point that:

> "it is better to use an elaborate construct that attempts to deal with the right question than a simpler one that answers accurately the wrong concerns"

Similarly, the considerable problems associated with calculating PPPs are no excuse. As Ryten (2004) says: "The only viable alternative to the use of inadequate PPP-based estimates is better PPP-based estimates."

C. Projecting Emissions

However, when considering whether GDP_{MER} or GDP_{PPP} should be projected, another question must first be answered – what are we projecting income for? If the answer is that we project income in order to estimate the welfare of citizens in the future, there is no doubt that GDP_{PPP} should be used. This measures the purchasing power of their income, in terms of the prices they actually face.

However, the aim of the SRES scenarios was not solely (or even predominantly) to project welfare, but rather to project emissions: income projections were combined with a model of the relationship between income and emissions, to give an emissions projection. Just because GDP_{PPP} is the best proxy for welfare, it does not follow that it will be the best predictor of emissions, a point that the SRES team failed to make strongly enough, and Castles and Henderson and Ryten (2004) failed to grasp. Had this distinction been more clearly drawn by either side, the debate may have been more constructive and illuminating.

Since GDP_{PPP} is the best volume measure of economic activity in a country, it may be hypothesised that it will be the best predictor of emissions. However, the SRES team

[3] Interestingly, however, Lequiller and Blades (2007) say: "Although PPPs are more suitable than exchange rates ... they are a statistical construct rather than a precise measure". This comment relates only to the method of conversion into national currencies, so does not affect the first point made here.

argue that since most emissions producing fuels are traded goods, GDP_{MER} will give a better indication of the capacity of a nation to purchase and burn these fuels.

In a sense, then, the choice of which metric to use in order to project emissions is surely an empirical question, and the opposing views could be tested using appropriate data. Economic activity has a statistical relationship with emissions: not all units of GDP produce the same quantity of emissions. Thus, our understanding of the relationship between the two boils down, in essence, to a panel regression of observed emissions on observed GDP (with other independent variables that would presumably be the same for both GDP_{MER} and GDP_{PPP} regressions). The question then becomes: which of the two GDP measures best predicts emissions? In other words, which measure is the best proxy for the (unknowable) variable of interest: the level of emission-producing economic activity? Of course, theory should allow those familiar with emissions to predict which will be best, but based on the debate between Castles and Henderson and the IPCC, there appear to be arguments in support of both metrics. It is intriguing that neither side of the debate cited any empirical evidence when discussing this question. This suggests that no study has yet determined which of the two metrics best predict emissions in the historical data. van Vuuren and Alfsen (2004) present data which suggests that that both GDP_{PPP} and GDP_{MER} appear to have a fairly tight relationship with emissions, although their analysis is restricted to a cross sectional analysis from a single year, and they do not report any measures of goodness of fit. This is an issue that merits further investigation.

The irony in this is that if, as Castles and Henderson appear to believe, MER and PPP are linked by a strong and stable Penn effect (see Appendix B), they should be perfectly co-correlated, and therefore equally good predictors of emissions (as we will see, they are not).

Combining PPP and MER

Ryten (2004) is extremely sceptical of the value of predicting both GDP_{PPP} and GDP_{MER}:

> "For reasons which I do not fully understand, the authors believe that if PPPs have deficiencies and rely on questionable assumptions, the information they provide would be improved if used in conjunction with

> MERs. The mechanism that would make it possible for the irrelevant to assist the infirm is not specified"

However, it is clear that Ryten is still thinking in terms of GDP as a measure of economic activity, or income, rather than as a predictor of emissions. It is possible that MER could carry some information not captured by PPP, which would improve the prediction of emissions. Of course this would not be true if the two were perfectly co-correlated.

IV. The effect of using GDP_{MER}

A. Will MER bias growth rates upwards?

Castles and Henderson allege that the use of MER, combined with the assumption that poor countries will converge on rich countries, leads to exaggerated growth rates (and therefore exaggerated emissions). Clearly, *ceteris paribus*, over-optimistic convergence rates will lead to over-optimistic growth rates. However, the supposed role of GDP_{MER} in the over-estimation of growth rates is a perfect red herring.

Assume first that MER and PPP are related through a strong and stable Penn effect. In a situation where poor countries are converging on the reference economy (the USA), their rate of growth measured by GDP_{MER} will be higher than when measured by GDP_{PPP}. It makes no sense to speak of 'growth rates' without first précising whether one means growth in GDP_{MER} or GDP_{PPP}, and it makes no sense to compare projected MER growth rates with historical PPP rates as Castles and Henderson do.[4] There is no reason whatsoever why the use of GDP_{MER} should impart an upward bias on the rate of growth in GDP_{MER}!

B. Will MER bias emissions upwards?

Leaving aside for now the question of whether the GDP_{MER} growth and convergence rates were high compared to the appropriate historical data. Would the use of growth

[4] Nakicenovic et al. compare GDP_{MER} and GDP_{PPP} with Celsius and Fahrenheit. This is not a good analogy, since the latter pair are linked by a known and constant mathematical relationship, whereas the former are, at best, linked through an imperfectly understood, and noisy, statistical relationship. They are closer to the truth when they refer to the comparison of "apples and oranges".

rates measured in GDP_{MER}, which are higher (for poor regions) than those measured in GDP_{PPP}, lead to an over-estimation of emissions?

There are two ways to answer this question. The first, adopted by Grübler et al. 2004 in their response, was to re-run the scenario in question (B1) with lower growth rates, and compare the results. They found (as Nakicenovic et al. 2003 had predicted) that in fact decreasing growth rates would increase emissions. This is due to the lower capital turnover and slower adoption of new technologies. The B1 scenario owes its low-emissions to optimistic assumptions about technological progress, rather than low-growth[5].

The second response is to note that this again is a red herring. As described above, emissions projections result from combining projections of income, with a model linking income to emissions. That model is parameterised based on statistical analyses of past emissions and income. Clearly, the same form of GDP must be used in deriving the statistical relationship as is projected. If the changes are made consistently throughout the model, switching from MER to PPP will produce little or no effect, as demonstrated by Holtsmark and Alfsen (2004) and Tol (2006), who comments:

> "The sensitivity to the exchange rate is purely due to imperfect data, imperfect statistical analysis of data, a crude spatial resolution, and imperfect models."

It is not clear how McKibbin et al. 2004 obtained their result that using GDP_{MER} would overestimate emissions by 40%, but it is possible that they simply adjusted growth rates without fully re-parameterising their model.

[5] The assumption by Castles and Henderson that higher income necessarily leads to higher emissions is inexplicable given their fierce condemnation of the SRES report for apparently assuming just that. As they themselves say, since technology both determines the relationship between income and emissions, and is itself dependent on income, the relationship between income and emissions *over time* is indeterminate. However, it does appear that there are inconsistencies in the IPCCs message – while the SRES scenarios as a whole treat the relationship between income growth and emissions as indeterminate, other sections of the IPCC have called for restraint of growth in order to curb emissions.

B. Are the SRES projections of PPP-based GDP valid? A review and analysis of the Penn effect

One of the main criticisms Castles and Henderson (2003a) made of the SRES was that it did not project GDP at purchasing power parities (GDP_{PPP}) but rather at market exchange rates (GDP_{MER}). When Nakicenovic et al. (2003) pointed out the PPP-based projections produced by the MESSAGE group, Castles and Henderson (2003b) responded stating that these were not sound, since they did not display the properties they expected. This claim was denied by Grübler et al. (2004) and the SRES team robustly defended the projections. The purpose of this Appendix is to examine these conflicting claims and then to carry out an empirical investigation of the relationship between the projections of GDP_{PPP} and GDP_{MER} in the SRES projections: the Penn effect (Asea & Corden 1994).

I begin by presenting a brief introduction to purchasing power parities, their relationship to market exchange rates and the Penn effect in Section I. I then review the criticisms made by Castles and Henderson (2003b) of the PPP-based projections in the SRES, and the response of the SRES team (Grübler et al. 2004) in Section II. In Section III I introduce the empirical analysis used to investigate the Penn effect in the historical data and in the SRES projections. Section IV presents and discusses the results of this analysis, and Section V summarises and concludes.

I. Purchasing power parities and the Penn effect

> "The Penn effect is an important phenomenon of actual history but not an inevitable fact of life. It can quantitatively vary and, in different times and places, trace to quite different process, as we shall see." (Samuelson 1994, p206).

In order to compare income levels across countries, per capita GDP measured in each national currency must be converted to a common currency (usually the US dollar). There are two principal exchange rates used, market exchange rates (MER) and

Purchasing Power Parity rates (PPP). PPP exchange rates account for the fact that price levels differ between countries: in order to produce a true volume measure of GDP, and to represent the spending power of income in a country, this should ideally be accounted for.[6] Thus, the difference between GDP_{MER} and GDP_{PPP} for a given country is due to the country's price level relative to the reference economy, usually the USA. Countries with low prices will have a higher GDP when using PPP than MER.

Since the work of Bela Balassa (1964) and Paul Samuelson (1964) it has been a widely acknowledged 'stylised fact' of economics that poor countries generally have lower price levels than rich countries (Bergin et al. 2006), a phenomenon that has become known as the Penn effect.[7] Thus, converting GDP from national currencies to dollars using market exchange rates tends to underestimate income and production in poor countries, since prices are lower in those countries. The choice of exchange rates (PPP or MER) can make a substantial difference to poor countries' GDP: PPP-based estimates for the poorest countries can be 3-4 times larger than those based on MER.

The model proposed independently by Balassa and Samuelson in 1964 showed that differences in productivity between traded and non-traded sectors might explain the Penn effect. High income countries are more technologically advanced, and therefore have higher productivity levels, than low-income countries. However, they argued that the technological advantage was greater in the traded sector than the non-traded sector. Through the law of one price, all countries would benefit from price reductions in the traded sector, but the effects of lower productivity in the non-traded sector would be felt only within the high-income country. This, combined with wage differentials between countries would mean that overall price levels would differ between countries – the Penn effect (see Asea & Corden 1994 for an overview). However, this is only one possible explanation, as the quote from Samuelson above shows.

In the next Section, I examine the claims made about the SRES GDP_{PPP} projections by the critics, and for the most part I discuss them in the context of a constant and strong

[6] The process is directly analogous to accounting for different price levels through time (inflation) to produce GDP in constant prices, and faces all of the same problems and complexities relating to the inconsistency of consumption baskets through time and space.

[7] It has also been called the "Penn effect" after the Penn World Tables produced by the Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania. These tables have been used in most analyses of the issue.

Penn effect. In subsequent Sections, I empirically examine the reality of a dynamic Penn effect.

II. Castles and Henderson's critique and the SRES response

> "It is true that the MESSAGE scenarios, prepared by the International Institute of Advanced Systems Analysis (IIASA), all show what are described as PPP-based figures for GDP, alongside those that are identified as MER based. However, it is not explained how these PPP-based series are derived, nor is it clear what economic meaning can be given to them. What is clear is that they do not, as is claimed for them, represent changes in GDP for any of the four regions that are distinguished or for the world as a whole." Castles and Henderson (2003b: 422).

Castles and Henderson claim that the GDP_{PPP} series reported by the MESSAGE group of projections cannot be considered sound, because they do not display the properties they expect. They make three statements about the properties that they believe a true GDP_{PPP} series would display, when compared to the corresponding GDP_{MER} series, which I evaluate below.

First, they argue that:

> "...it is when poor and rich countries are brought together in a single grouping, and in particular when growth is estimated (or projected) for the world as a whole, that the divergence between MER-based and PPP-based measures of output growth will be greatest. For groups of countries with broadly similar levels of GDP per head, whether rich or poor, the two measures will diverge much less: they may well be close, though they will virtually never be identical"

This is not necessarily the case. Castles and Henderson appear to be ignoring the effect of convergence of poor countries on the rich, ironic given that this is one of their principal bugbears. In a world where poor countries are converging on rich countries, and if we assume the Penn effect holds, then the greatest divergence between PPP and MER will be expected for the region whose countries differ most in income level from the reference economy and not from each other. In the case of the SRES projections, this is the ASIA and ALM regions, followed by the REF region, while the OECD

region, which includes the USA and countries of similar income levels, would be expected to show the smallest difference between GDP_{PPP} and GDP_{MER} growth rates, and this is in fact the case.

Castles and Henderson go on to say:

> "The proportionate changes shown in this series for the 'OECD 90' group of countries are identical with those for the MER-based series, which they would not be if they were genuine measures of GDP; for the developing regions, the divergences between the two series are impossibly great; and for the world as a whole the divergences are intermediate"

And

> "Looking at specific figures makes it clear that, whatever the MESSAGE PPP series may reflect, it is not an alternative measure of the growth of GDP. Over the period 2000-30, for example, the GDP of the 'ASIA' region is shown in the B1 MESSAGE scenario as increasing in MER terms by a factor of 5.6, whereas for the PPP series the figure is 3. Such a gap could not possibly have arisen from differences in weighting which are the only true source of divergence between the two measures of GDP growth. The MESSAGE GDP series expressed in PPP terms is not such a measure - it is mislabelled."

This criticism is difficult to evaluate except empirically (which I do below). For the OECD90 as a whole the PPP series would be expected to be very similar to the MER series, since price levels across the OECD region are similar to those of the USA, whereas the ALM and ASIA regions will show the greatest divergence[8].

One would expect the ratio of GDP_{2030}:GDP_{2000} to be larger for MER series than for PPP series if the developing country in question is converging on the US, as they are in these scenarios. The two series will converge as the income level (and therefore price level) of the region converges on that of the reference economy. Thus, GDP_{MER} will grow faster than GDP_{PPP}, for a developing economy which is converging on the US.

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[8] Castles and Henderson's claim is surprising and appears to be inconsistent with the rest of their critique. Elsewhere they tell us that MER will exaggerate developing country growth rates, compared to PPP, if convergence is assumed.

The question is therefore whether the differences in growth rates are too large – and Castles and Henderson provide no analysis showing this, they merely assert it to be true. It also depends on assuming a strong and stable Penn effect. If the Penn effect were to disappear, the two series would quickly converge.

The contentions of Castles and Henderson are unhelpfully vague, given their strident claims that the GDP_{MER} series will exaggerate developing region growth rates for a given level of convergence. As I argued in Appendix 1, it won't exaggerate the 'growth rate' as such, since growth rates are specific to the metric chosen (GDP_{PPP} or GDP_{MER}) but GDP_{MER} will grow faster[9]. The question is whether it is growing excessively fast, compared to the PPP scenarios.

The SRES team seem to have understood the Penn effect, and cite empirical research demonstrating its existence:

> The disparities between PPP and MER measures are very high especially for the developing regions of the world. Figure 1 shows that these disparities decrease with development so that they are significantly lower by mid century and slowly converge toward the end of the century. This is consistent both with the economic theory (Voeller, 1981) and with inter-country comparisons (de la Esconsura, 2000; Kravis et al., 1978). The insert in the figure highlights these differences between the two measures during the next three decades across the scenarios. Nakicenovic et al. (2003, p191).

This is, broadly speaking, correct, though I investigate below whether the PPP series do in fact behave as expected. Note, however, that they make no reference to any change in the relationship between GDP_{PPP}, GDP_{MER} and the price level, instead justifying convergence in the two series by reference to "economic development", which is rather vague.

Ryten (2004) disputes this claim of expected convergence between PPP and MER. However, he refers to whether we should expect a single country's GDP_{PPP} to be closer

[9] This is because the GDP_{MER} growth rate captures genuine increases in the income level as well as a reduction in the degree to which the income level is underestimated by MER measures. As countries approach the income level of the reference economy, MER and PPP series will tend to converge (for the reference economy they are identical).

to its GDP_{MER}, and states that because of increasing currency instability, this will not be the case. Yet the SRES team, in proposing a convergence in GDP_{PPP} and GDP_{MER} are speaking *on the average*, i.e. that the consistent differences between GDP_{MER} and GDP_{PPP} will disappear over time, not that all differences will disappear. In regionally aggregated data such as that projected by the SRES, one would expend currency fluctuations to cancel out, and therefore that a region with the same average income as the reference economy would not show any difference between GDP_{MER} and GDP_{PPP}.

Castles and Henderson, in making their criticisms, appear to have assumed a strong and stable Penn effect into the future. In the next two sections, I examine this empirically.

III. Empirical investigation of the Penn effect through time

A. Introduction

The Penn effect is defined by Bergin et al. (2006, p2044) as the existence of "a positive and statistically significant slope estimate β" when regressing the price level of country i (relative to the USA) on its relative income (converted using PPP):

Equation B.1 $$\ln(\frac{P_i}{P_{USA}}) = \alpha + \beta \cdot \ln(\frac{Y_i}{Y_{USA}})$$

Where P is price level and Y is income per capita.

Thus, if the Penn effect exists in the data, countries with low income levels relative to the USA will have low relative prices levels, as explained above.

Bergin et al. (2006) estimate the BS (after Balassa-Samuelson) coefficient β, in each year, from 1950-1998 (Figure B.1), using data from the Penn World Table, and show that the strength of the Penn effect has increased consistently over the period studied, the BS coefficient becoming significant at the 95% level in the late 1950s.

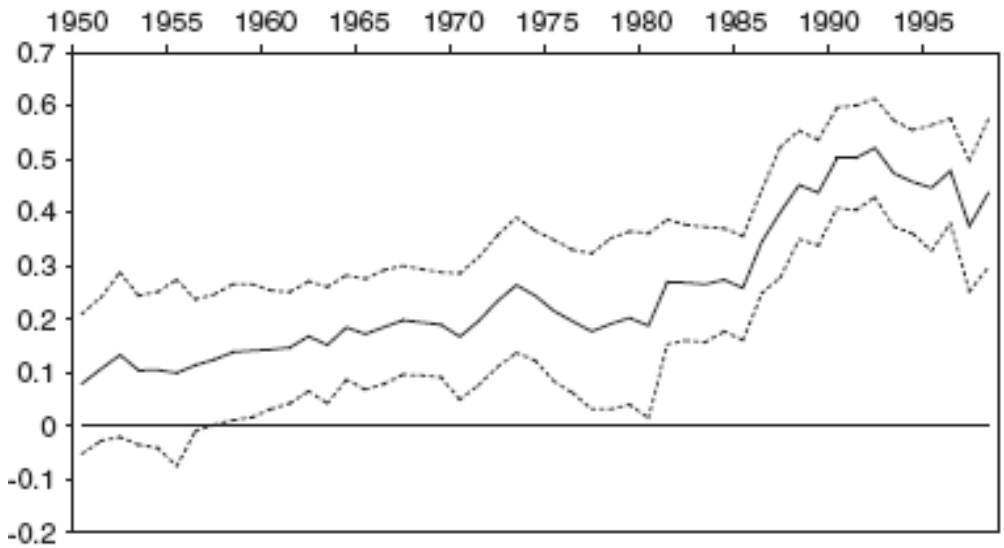

Figure B.1. The BS coefficient (solid line) and 95% confidence intervals (dotted lines) 1950-1998. Using a constant sample of 53 countries starting in 1950, from Penn World Table version 6. Reproduced from Bergin et al. (2006, Figure 2b, p2046).

B. Data and Methods

The analysis presented in this Appendix follows Bergin et al.'s (2006) method, with one exception. Because the SRES projections are presented aggregated into four regions, I calculated *P* and *Y* relative to the OECD, rather than the USA[10]:

Equation B.2 $$\ln(\frac{P_i}{P_{OECD}}) = \alpha + \beta \cdot \ln(\frac{Y_i}{Y_{OECD}})$$

As I demonstrate below, this does not substantially affect the results, but does mean that the absolute values for β may not be directly comparable to those estimated by Bergin et al. (2006).

I proceed as follows. I begin by repeating Bergin et al.'s analysis on data from the latest Penn World Table (Version 6.2, hereafter PWT, Heston et al. 2006) using Equation B.2, to demonstrate that the results are qualitatively unchanged by using price levels and incomes relative to the OECD average. I also present additional results illustrating the explanatory power of the Penn effect during the period, the effect of broadening the sample of countries, and extend the analysis to 2003 to explore the recent trend. For this and subsequent analyses I use three samples of the data, each containing the maximum set of countries for which information is available, starting in 1952, 1970 and 1993 respectively (see Appendix C). Both Chain and Laspeyres GDP series were used, but gave almost identical results, those for the Chain series are presented. Although data were not aggregated to regions, analyses were carried out with and without countries from the REF region, to allow direct comparison with the second analysis, below.

Second, I repeat the analysis using data aggregated into the four regions used in the SRES. I demonstrate that the results are robust to this aggregation.

Finally, I carry out the same analysis for the SRES projections. Note that some scenarios did not differ in their economic projections, therefore only the following scenarios were analysed (the duplicates are given in brackets: A1, A1T (=A1G=A1C), A2, B1 (=B1T), B1High, B2.

[10] However, the base-country used by the IPCC for GDP calculations was the USA, and therefore Y is measured US dollars, and P is relative to the USA (USA=100).

Regressions were run as linear models using Ordinary Least Squares (OLS) regression in R (R Development Core Team 2007) using the *lm* function.

To determine the support that the inclusion of the $\ln(\frac{Y_i}{Y_{OECD}})$ term receives from the data, I calculated the Akaike Information Criteria (AIC) score of the model shown in Equation B.3 as well as a model containing only the intercept term (Equation B.4):

Equation B.3 $$\ln(\frac{P_i}{P_{OECD}}) = \alpha + \beta \cdot \ln(\frac{Y_i}{Y_{OECD}})$$

Equation B.4 $$\ln(\frac{P_i}{P_{OECD}}) = \alpha$$

The AIC score is equal to twice the log-likelihood of the model, plus a 'penalty' equal the number of parameters multiplied by two. Lower AICs scores therefore indicate stronger support from the data and relative AIC scores can be used to select parsimonious models from an *a priori* defined set of economically appropriate models (Burnham & Anderson 2002). Adjusted R-squared were calculated to estimate the % of the variance explained by the model.

Note that all of these analyses use data from countries with varying data quality. It would be useful to repeat them taking account of data quality, i.e. by eliminating poor quality data, using the scores in PWT table A column 11. See also Hill (2007) for a discussion of the quality of PPP exchange rates. Note also that, following Bergin et al, I have not weighted countries (e.g. by population) nor have I distinguished between countries with free floating interest rates, and those who, for example, peg their currency to the dollar, e.g. China.

IV. Results and Discussion

A. The stability of the Penn effect in the historical country-level data

Figure B.2 shows estimates of the BS coefficient for each year from 1952 – 2003. As in Figure B.1 above, the data show a relatively weak Penn Effect until the mid-eighties, when it strengthened considerably, before levelling off in the late nineties. The size of the BS coefficient is also similar, ranging between about one and five. Thus, the change to using income and price levels relative to the OECD has not affected the results[11].

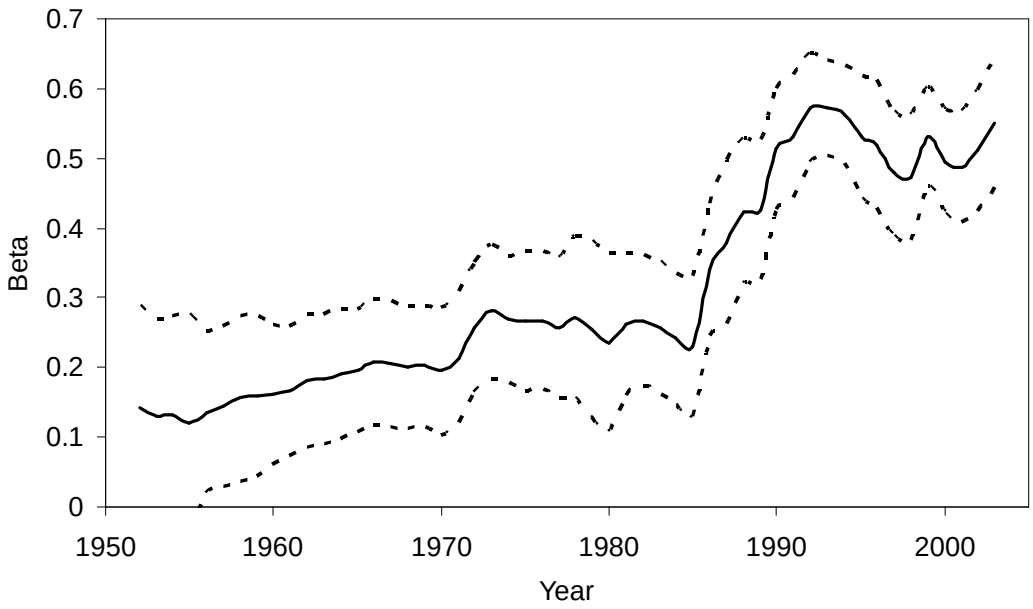

Figure B.2. Evolution of the estimated BS coefficient, beta (1952-2003).

Figure B.3 shows ΔAIC for the slope and intercept model compared to the intercept only model. By convention, models which differ by 10 or more from the best supported model (ΔAIC >2) are considered to lack any support from the data (Burnham & Anderson 2002), those 2 or less may be considered to have substantial support. This demonstrates that, for the period prior to the 1960s, estimates of price level given income should be made using both models (averaged using Akaike weights), further reducing the size of the Penn effect. In addition, Figure B.3 shows the evolution of the

[11] The small differences that exist between the two sets of results probably result from the slightly different sample and dataset. However, since Bergin et al. do not list the countries in their sample, and the PWT version 6 is no longer available, this cannot be verified.

adjusted R^2. This demonstrates that only since the mid-eighties has the Penn effect explained more than 50% of the variance in price level, for this relatively narrow sample of countries.

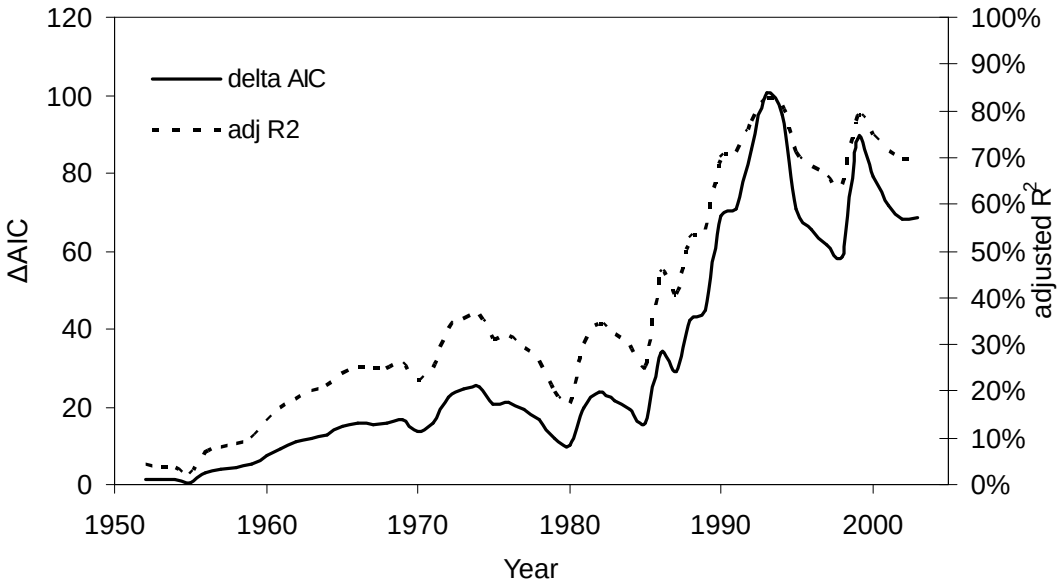

Figure B.3. Evolution of ΔAIC and adjusted R^2 of the slope and intercept model compared to the intercept only model (1952-2003).

Bergin et al. do not present evidence on the effect of broadening the sample of countries in any given year, and their analysis, like this one, used only a relatively small sample of countries, largely excluding Asia and Eastern Europe / former USSR.

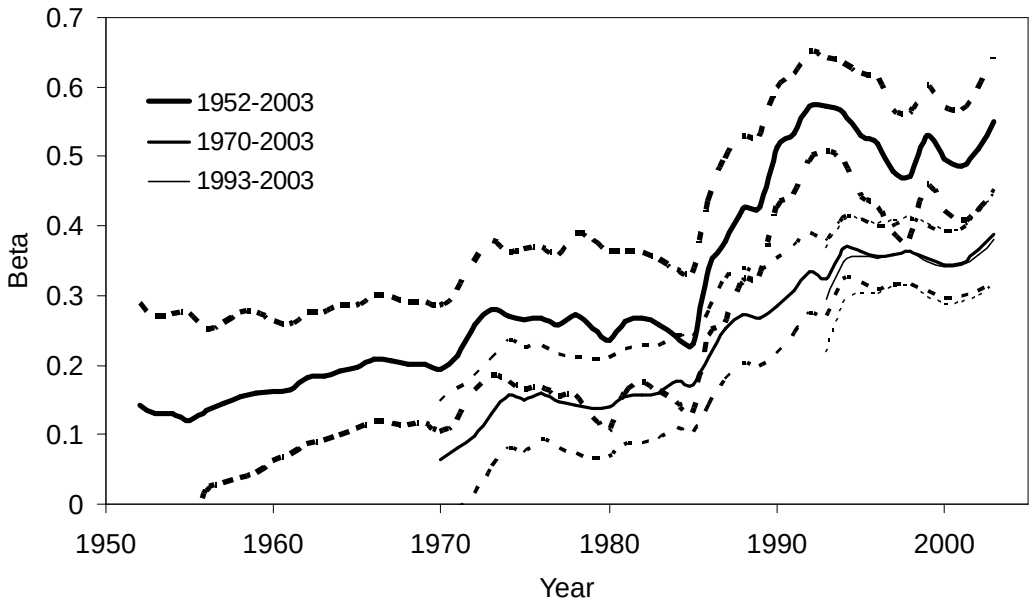

Figure B.4 shows that if the sample of countries is broadened to 150 countries (1970-2003 series), the Penn effect weakens considerably. Throughout the 1990s this broader sample of countries shows BS coefficients which are only 50-70% of that shown by the narrower sample, with the Penn effect often explaining 30-40 percentage points less of the variation in price level. In 1990, when most of the modelling teams for the SRES had begun, the Penn effect explained just 30% of the variation. Broadening the sample still further, to include most of the countries in the REF region (1993-2003) makes little difference to the estimate of the beta, but reduces the adjusted R^2 by around another ten percentage points (Figure B.).

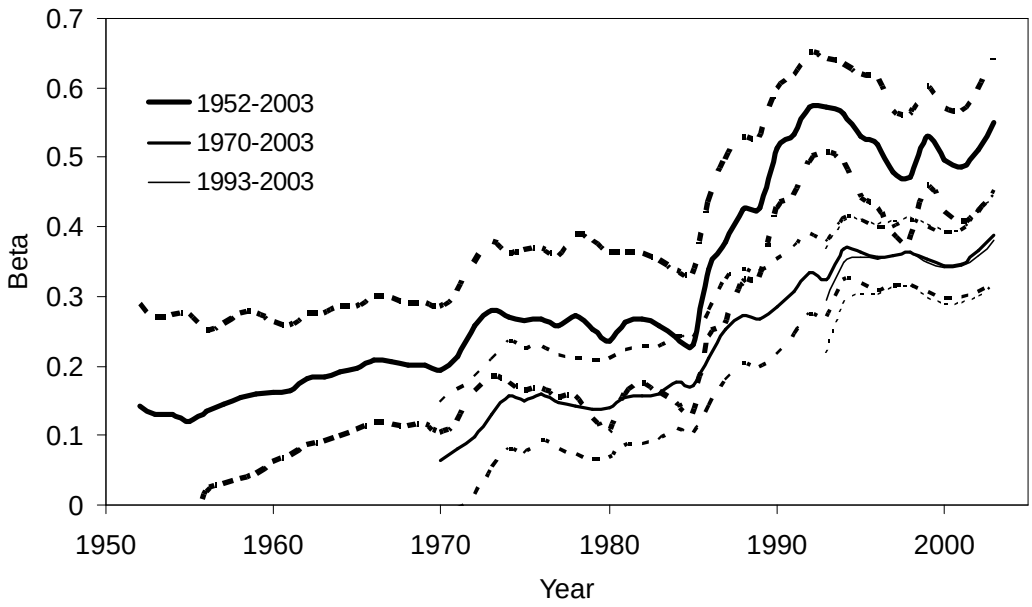

Figure B.5. Effect on the Penn Effect of broadening the sample of countries (and therefore reducing the number of years for which data is available).

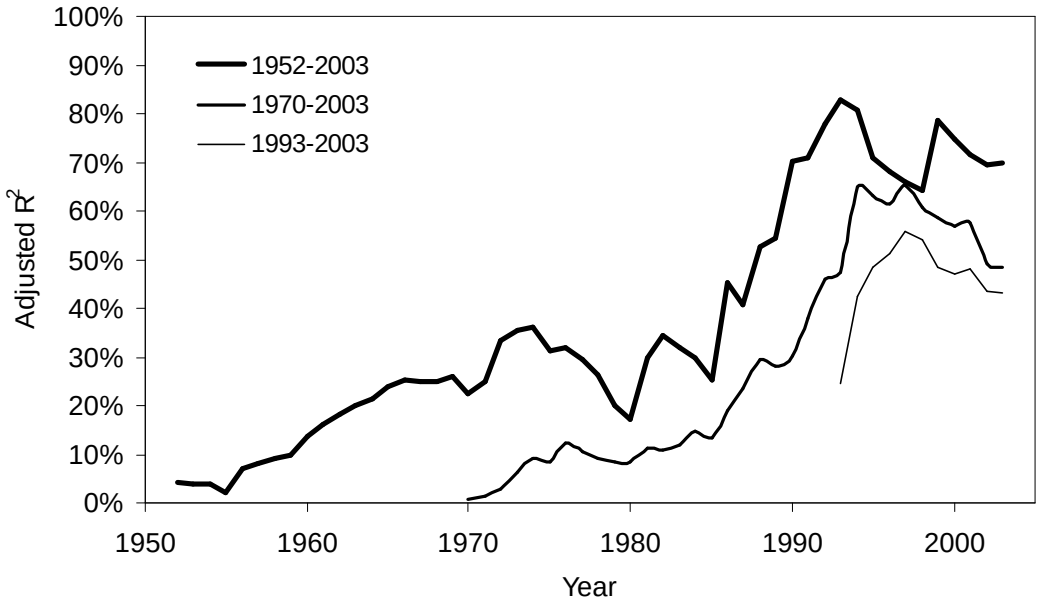

Figure B.6. Effect on the adjusted R^2 of broadening the sample of countries (and therefore reducing the number of years for which data is available).

Because of a lack of data, the 'stylised fact' of the Penn effect has until recently been derived from short time series and or relatively narrow, unrepresentative, samples of

countries[12]. Recent improvements in the data available allow a more expansive view and demonstrate that the Penn effect is far from stable over time[13]. Nor is it an iron rule of accounting that can be blindly applied: the model assuming the Penn effect currently explains just 40% of variance in price level on a global scale, and appears to be weakening in its explanatory power. The results suggest that the Penn effect may be relatively strong within groups of countries that are more or less similar in certain characteristics, thus, we might speak of a conditional Penn effect. Further investigation of the characteristics determining the strength of the effect over time or between groups of countries is warranted but outside the scope of this section[14].

B. Estimating the BS coefficient from the aggregated data

The year-by-year analysis above was repeated for the historical data after it had been aggregated into the SRES regions. Figure B.7 compares estimates of beta 1952-2003 from the aggregated data with those from the country level data. The aggregated measure shows a similar trend to that derived from the country level data. Thus, it appears that regionally aggregated data provides a reasonably unbiased estimator of beta, over the historical period, when compared with country level data.

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[12] The inclusion or otherwise of a country in the PWT is not random: in particular, the PWT has poor coverage of communist / post communist countries prior to 1993.

[13] Note, however, that this was acknowledged by Samuelson in 1994.

[14] It would be interesting to analyse trends in the Penn effect accounting first for fixed exchange rates, and then looking at trade liberalisation / trade volumes, in particular exports and imports as a % of GDP. The PWT includes a variable OPENK – which is Imports + Exports as % of GDP. *Ceteris paribus*, an increase in OPENK should weaken the Penn effect. Cross-sectionally, residuals from the Penn model should be correlated with OPENK: i.e., countries with above (below) average relative income level and above (below) average price level (i.e. those which tend to increase beta), should have below average OPENK. i.e the sign of the correlation between OPENK and the residual should change either side of the mean of relative income.

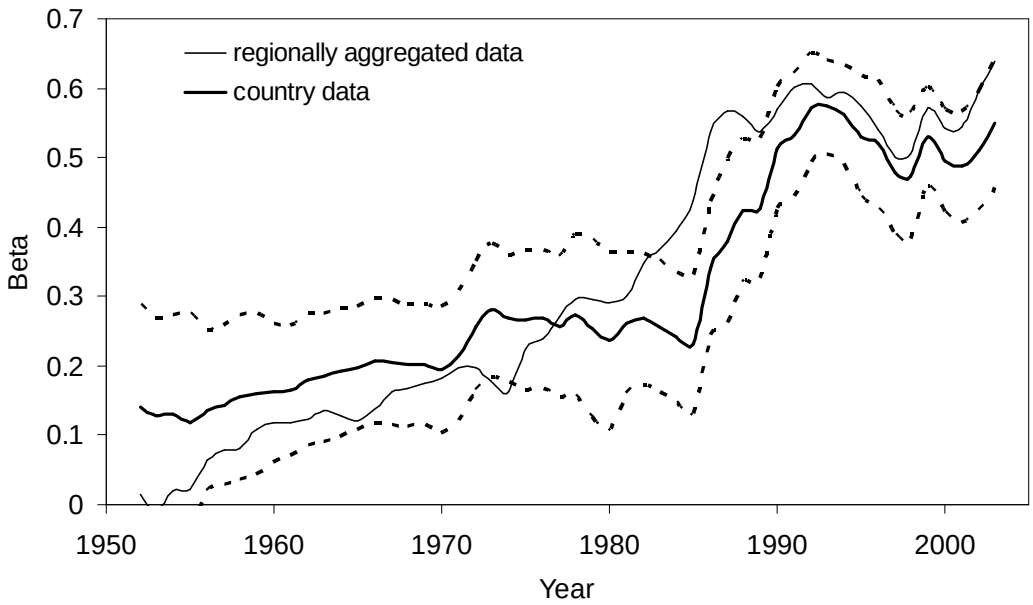

Figure B.7. BS coefficient estimated from regionally aggregated data and country level data (with 95% confidence intervals). For the regionally aggregated data, each data point was weighted by the number of countries in the region.

C. SRES Projections

Figure B.8 and Figure B.9 plot estimated values of the BS coefficient for the SRES MESSAGE scenarios (with and without REF countries respectively), together with estimates from the country-level historical data (1952-2003). In all scenarios other than A2, beta declines steadily from 1990, though always remaining broadly within the historically observed range, implying a gradual weakening of the Penn Effect. The A2 scenario maintains beta at roughly 1990 levels, until almost the end of the century, before declining sharply. No scenarios foresee a significant increase in the strength of the Penn effect.

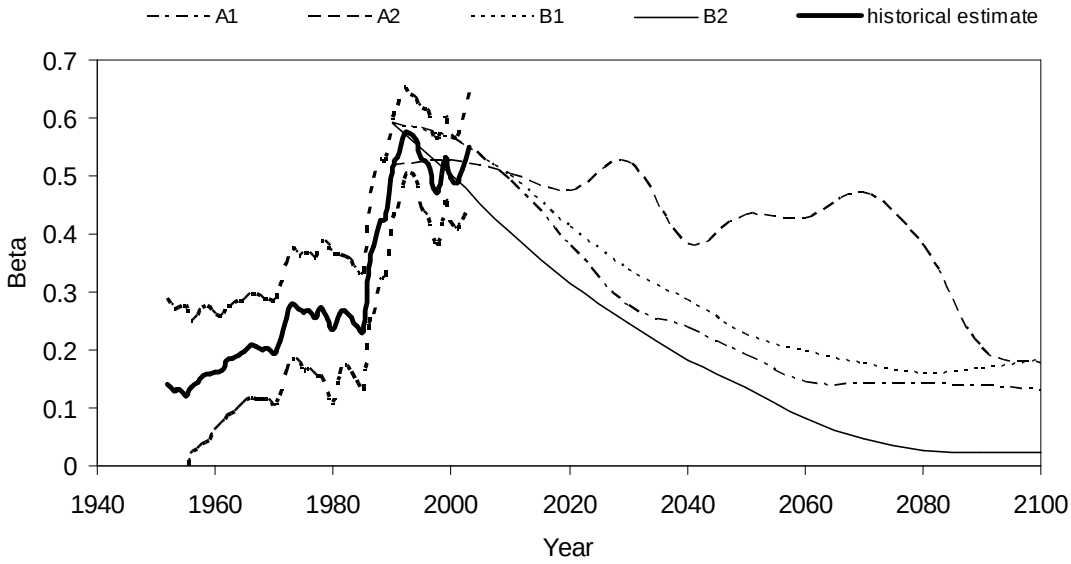

Figure B.8. Estimates of beta from the SRES MESSAGE scenarios (including REF countries), compared to those from the historical data (with 95% confidence intervals).

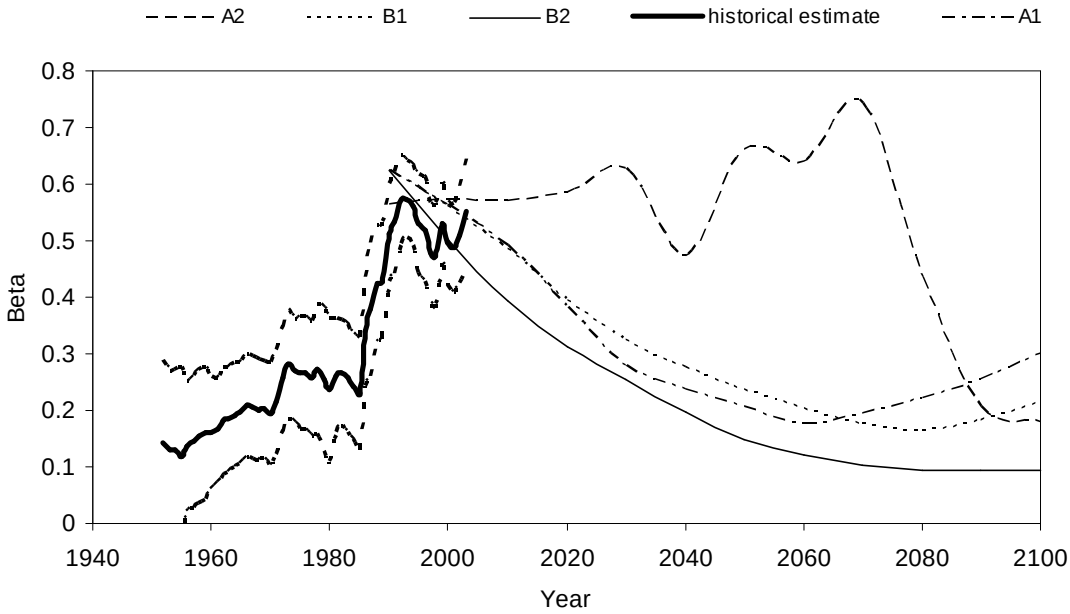

Figure B.9. Estimates of beta from the SRES MESSAGE scenarios (excluding REF countries), compared to those from the historical data (with 95% confidence intervals).

Discussion

Section II highlighted the vague nature of the Castles and Henderson's criticisms of the SRES GDP_{PPP} projections, and their assumption of a strong and stable Penn effect. It is harder to determine whether the SRES team assume a strong and stable effect in their response, since it is difficult to differentiate between convergence in price levels due to

convergence in income levels (which would be consistent with the Penn effect) and convergence for some other reason, such as globalisation (see below), and their use of the term "economic development" is sufficiently vague to encompass both possibilities.

As Samuelson (1994) stated, and Bergin et al. (2006) empirically demonstrated, the Penn effect has not been stable over time. Therefore, the zero-order comparisons implied by the Castles and Henderson critique are overly simplistic: there is no reason why projections should assume a strong and stable Penn effect lasting long into the future, with BS coefficients fixed at 1990 levels. Furthermore, the BS coefficients derived from the SRES projections lie within the range seen historically, at least as far as it is possible to tell from the aggregated data provided by the SRES. The Castles and Henderson critique is therefore shown to be overly simplistic.

However, finding fault with the critique does not automatically vindicate the SRES. I have shown that while the level of BS coefficients displayed by the SRES projections are historically reasonable, the projections do assume a very particular evolution of the Penn effect over time, and it is to these trends which I now turn.

A. The theory and future evolution of the Penn effect

The causes of the Penn effect are poorly understood (Samuelson 1994, Bergin et al. 2006) and therefore the likely future trends are especially difficult to determine. The textbook Penn model did not in fact 'explain' the Penn effect, but rather propose a means by which it might come about: a difference in productivity between traded and non-traded sectors (Bergin et al. 2006). The Penn model did not explain *how* such differences might arise, nor make any predictions about their stability.

Bergin et al. (2006) propose a mechanism by which such differences may arise and be sustained, by hypothesising that sectors or companies which show large productivity increases may be disproportionately likely to become traded:

> "In particular, while standard [Balassa-Samuelson] theory must assume that productivity gains are concentrated by coincidence in the existing traded goods sector, our model accounts for how productivity gains in the production of particular goods can in turn lead to those goods becoming traded endogenously." Bergin et al. (2006, p2043)

Productivity increases in the services sector, previously assumed to be untraded, have led some to conclude that the Penn effect will weaken (Bergin et al. 2006, p2060). However, Bergin et al. note that services are increasingly being traded, in a 'flatter' world (see Friedman 2005 for discussion of this process)[15]. Now, *ceteris paribus*, a tendency for high productivity sectors or agents to become traded would act to strengthen the Penn effect, and increase beta. However, if parallel improvements in technology and free trade lead to a general increase in the proportion of the consumption basket which is traded, this would act to reduce the weight given to non-traded goods, weakening the Penn effect.

Bergin et al's model cannot therefore lead to an ever increasing, or even stable Penn effect unless there is a similar tendency on the part of low-productivity sectors to cease to be traded at an equal rate, and this seems unlikely given the pace of technological and institutional developments tending to increase cross-border trade in nearly all sectors.

Interestingly, Bergin and Glick (2007) show that international price dispersion (a necessary but not sufficient condition for the Penn effect) fell from 1990-1997, before rising. The trend was found to be correlated with oil prices – increasing oil prices reduce trade, increasing price dispersion. The trend in price dispersion mirrors the trend in the Penn effect, for the 1952-2003 sample, though not the wider samples. This would imply that the strength of the Penn effect in this group of countries at least, was driven by trade volume, rather than by productivity differentials. If ever-increasing globalisation is assumed, the Penn effect is likely to weaken regardless of what happens to productivity differentials, since all sectors will become heavily traded.

Therefore, there are two main forces that can act to produce the Penn effect, each is necessary but not sufficient without the other. First, the pattern of global trade must be such that a significant proportion of the consumption basket remains untraded. Second, productivity gains must be concentrated in the traded sector. Bergin et al. (2006) propose one mechanism by which this latter process could occur, but do not deal explicitly with the effect of an apparently general increase in the proportion of the consumption basket which is traded.

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[15] Bergin et al. do acknowledge that the increased trade in services probably results from improved telecommunications as well as productivity increases.

The above discussion serves to show that the Penn effect is not stable over time, and is poorly understood. It is therefore highly uncertain how it will change in the future. It also demonstrates that the Penn effect cannot be considered independently of the scenarios themselves. One of the factors which distinguish the scenarios is the degree of globalisation that is assumed (Nakicenovic & Swart 2000). If this is a primary determinant of the Penn effect, one would not expect all scenarios to assume the same BS coefficients, and the BS coefficient must be determined endogenously[16]. Indeed, scenario A2 is characterised by lower trade flows than A1 (Nakicenovic & Swart 2000), which would imply a *potentially* stronger Penn effect[17], which appears to be the case. However, the B2 scenario is also supposed to represent a less globalised future, yet shows the lowest BS coefficients. This could of course be due to an assumption that productivity differences would be spread evenly across sectors, in this scenario.

Thus, the trends in BS coefficient displayed by the SRES projections appear to be reasonable, though representing strong assumptions about certain economic processes like trade and productivity growth across sectors. Given the lack of clarity surrounding the SRES scenarios, it is impossible to know whether these trends represent deliberate and carefully calculated projections, or are merely artefacts. They do not explicitly mention trade flows increasing or productivity improvement differentials declining in their response to Castles and Henderson.

If we look more closely at the GDP_{MER} and GDP_{PPP} series for individual regions, for example for the B1 scenario, they show some interesting properties (Figure B.10). For the three developing regions, the Penn effect apparently disappears as each one converges on the 1990 OECD income level, not the contemporary OECD income level. Furthermore, for the OECD region, the GDP_{PPP} and GDP_{MER} series appear to diverge, with the GDP_{PPP} series below that of the GDP_{MER}. These properties are consistent with the SRES team having determined the relationship between GDP_{PPP} and GDP_{MER} based on the income level of a region at any given time relative to the US income level in 1990, rather than the contemporaneous US income level. If this is the case, they are

[16] If it is in fact necessary to project both MER and PPP, as the SRES team contend (Nakicenovic et al. 2003) and Castles and Henderson (2003a, b) and Ryten (2004) deny.

[17] This still requires the assumption of productivity gains concentrated in the traded sector.

flawed. However, there may be other reasonable explanations as to why the Penn effect is assumed to disappear from developing countries at a certain level of economic development, yet still persist and in fact strengthen in the rich countries.

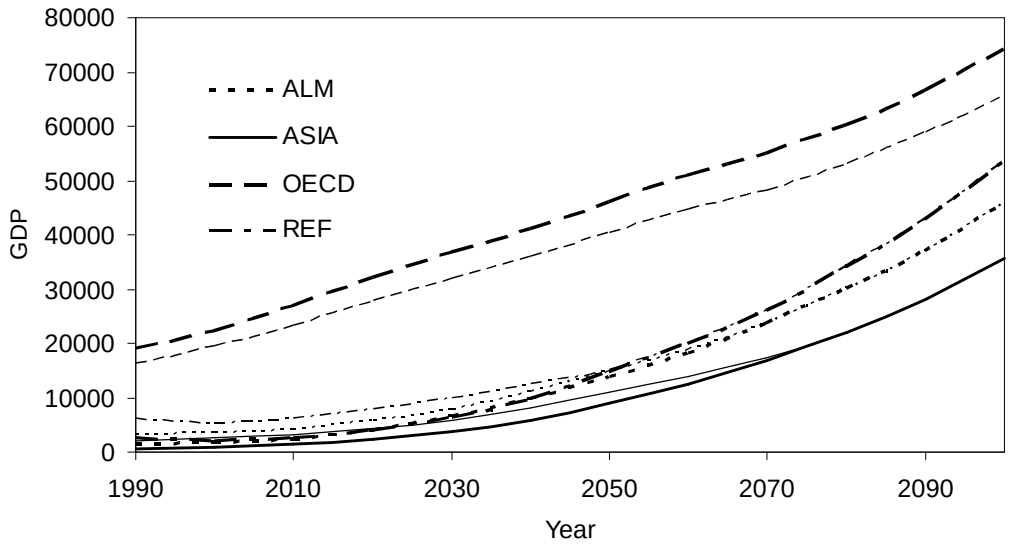

Figure B.10. GDP_{MER} (bold lines) and GDP_{PPP} (pale lines) projections compared for the B1 scenario.

V. Conclusions and implications

This appendix has shown the Castles and Henderson critique to be simplistic and historically naïve. It also shows that the SRES projections display reasonable (though particular) trends in the BS coefficient, which may be consistent with the individual characteristics of the scenarios. The SRES team themselves have robustly defended the GDP_{PPP} projections as valid (Grübler et al. 2004) and it therefore seems reasonable to evaluate these projections in this chapter. However, it is not entirely clear how the GDP_{PPP} projections have been derived and this raises some suspicions as to whether the GDP_{PPP} projections genuinely represent the assumptions of the SRES team, i.e. whether the trends in the BS coefficient are reasonable, but derive from inappropriate and unintentional assumptions[18]. This highlights the need for greater clarity in the

[18] It might therefore be appropriate to repeat the analyses in the main body of the chapter for GDP_{MER} projections, and possibly for a new GDP_{PPP} projections based on the GDP_{MER} projections and using the BS coefficients estimated from the between region analyses of the SRES scenarios. It is unlikely that this would qualitatively change the results, however. For example, for the B1 scenario above, it would have the effect of slightly increasing the GDP_{PPP} growth rate of the OECD region, in order for the GDP_{PPP} and GDP_{MER} series to converge rather than diverge.

presentation of future income projections that project both GDP_{MER} and GDP_{PPP}, and for greater historical awareness from those who would criticise them.

C. Sampling the Pen World Table data

The Penn World Table (PWT) runs from 1950 to 2004. However, coverage (of all variables except population[19]) is incomplete for some countries (Figure C.1). The first point to note is that data is unavailable prior to 1990 for most of the countries making up the REF region (<20% by population), and this region is therefore excluded in all of the analyses. Data is also missing for very many countries outside the OECD90 region for 2004, and thus all 2004 data is discarded. Note that there is a substantial jump in coverage of Asia in 1952 and smaller jumps in 1960 and 1972, and small jumps for ALM in 1952 1955, 1960 and 1970. Coverage of the OECD90 region is good in all years (>85%), with small improvements in 1952 and 1970.

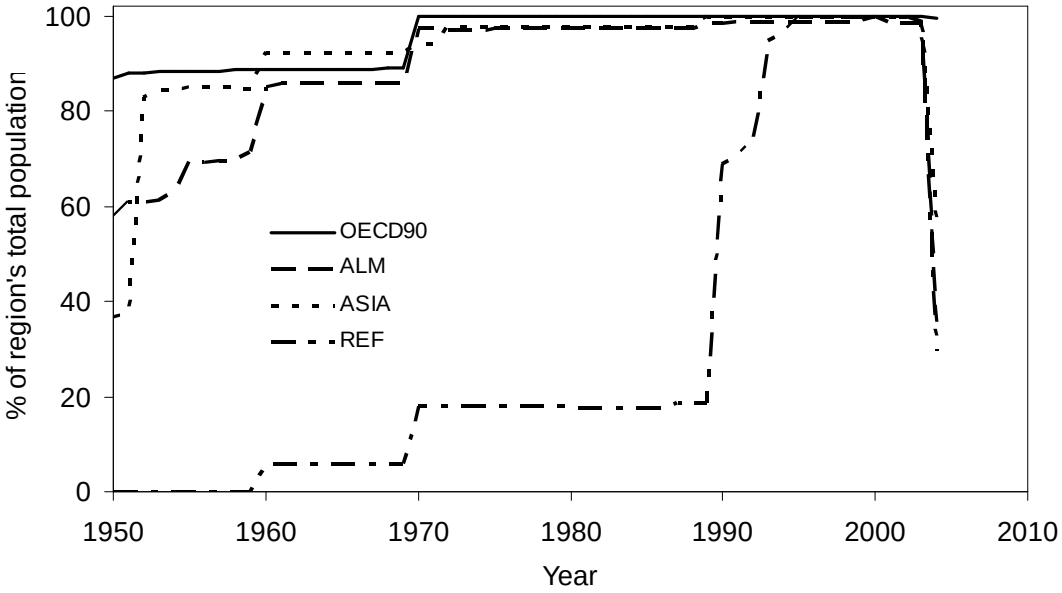

Figure C.1. Regional Coverage of the Penn World Table 6.2 The percentage of each SRES region's total population for which income and price level data is available from the Penn World Table 6.2 NB Total population is based on figures from the PWT, and therefore doesn't include any countries that are entirely missing from the Penn Table.

[19] The population estimates come from the US Census Bureau. Because these represent the best guess of the true population figure, they may differ from official population statistics published by country governments and collated by the UN.

I took samples starting in 1952, 1960 and 1970. Each contains data aggregated from a consistent set of countries but series starting later include more countries. There is an obvious advantage to using a longer sample period, as long as this is representative of the period as a whole. For the OECD, Figure C.2 below shows clearly that the longest sample (1952-1990) shows identical trends to those of later samples where they overlap. For the developing regions, coverage is poorer in 1952, and the time series differ more (Figure C.3 and Figure C.4). Dividing through by a factor to equalise to 1990 income levels, however, shows that for ALM at least, the longest time series is representative of later series, showing the same trend and differing only in relative income level (Figure C.5). For ASIA, the same is broadly true although there appears to be some advantage to using the 1960-1990 sample which shows more similar rates of convergence with the more complete series starting in 1970 than does the 1952-1990 sample (Figure C.6). For all subsequent analysis I use the full 1952-1990 sample.

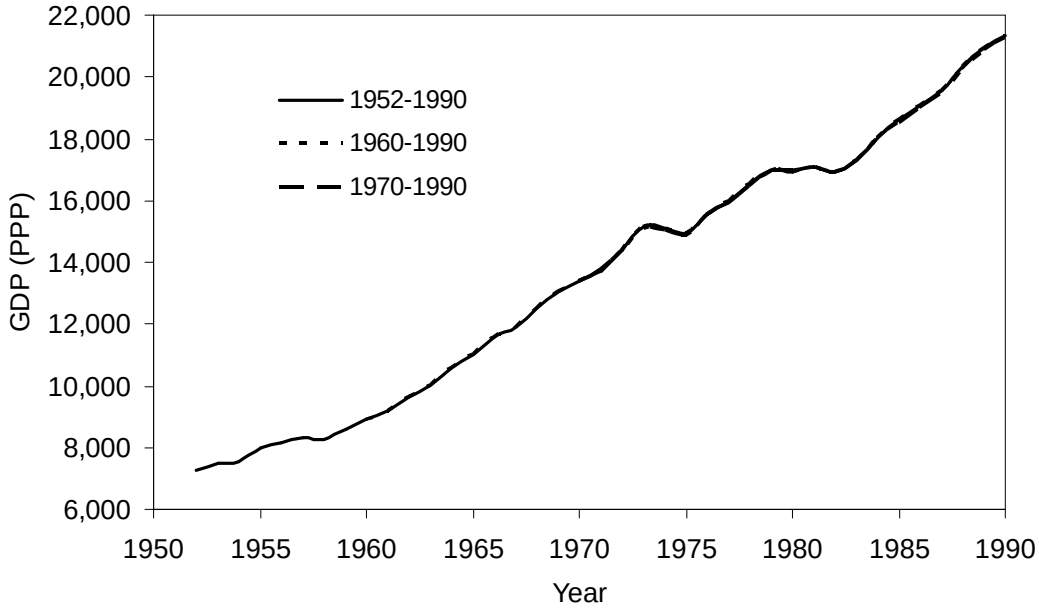

Figure C.2. Three samples of OECD income data.

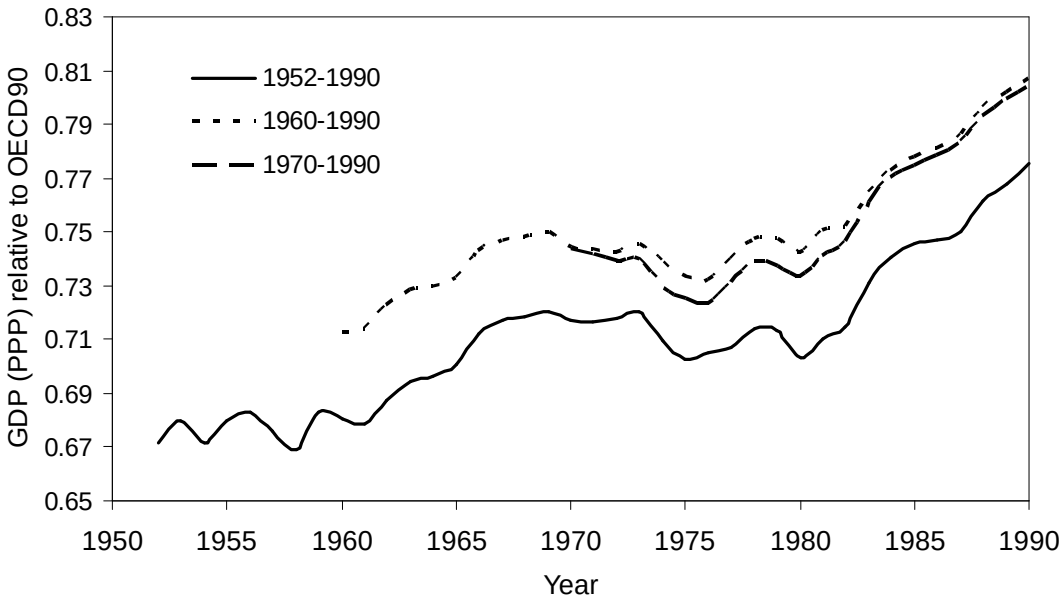

Figure C.3. Three samples of data for income (relative to OECD) for the ALM region.

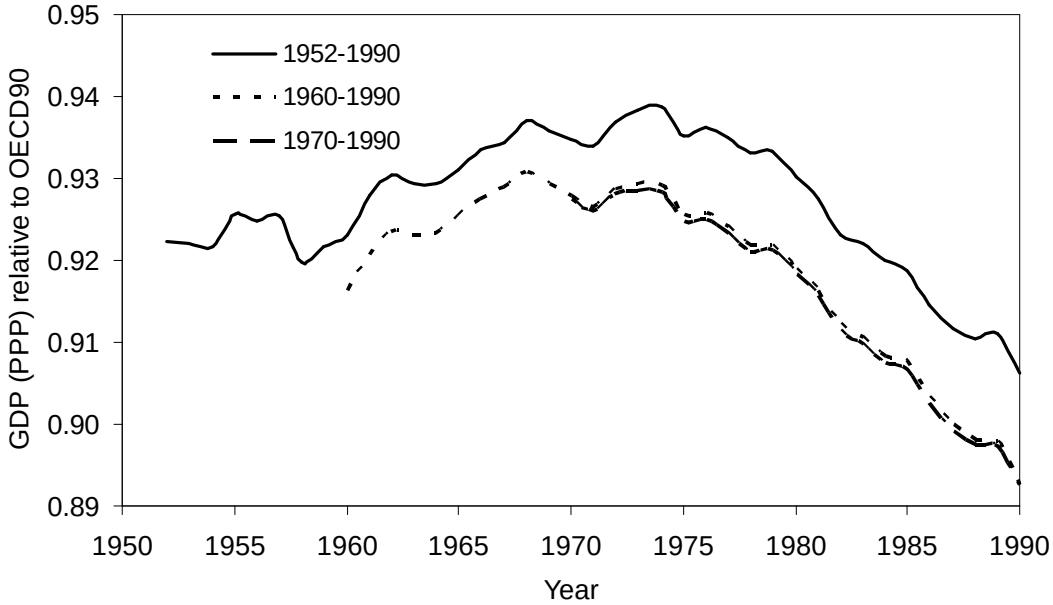

Figure C.4. Three samples of data for income (relative to OECD) for the ASIA region.

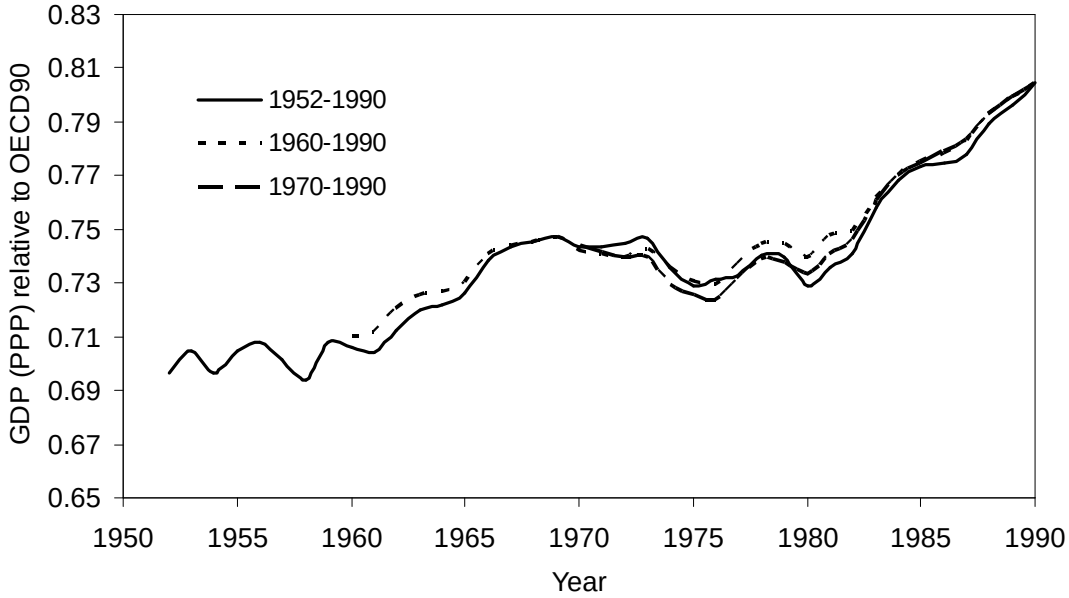

Figure C.5. Three samples of data for income (relative to OECD) for the ALM region with the 1952-1990 and 1960-1990 data divided by a factor to equalise 1990 level with 1970-1990 sample.

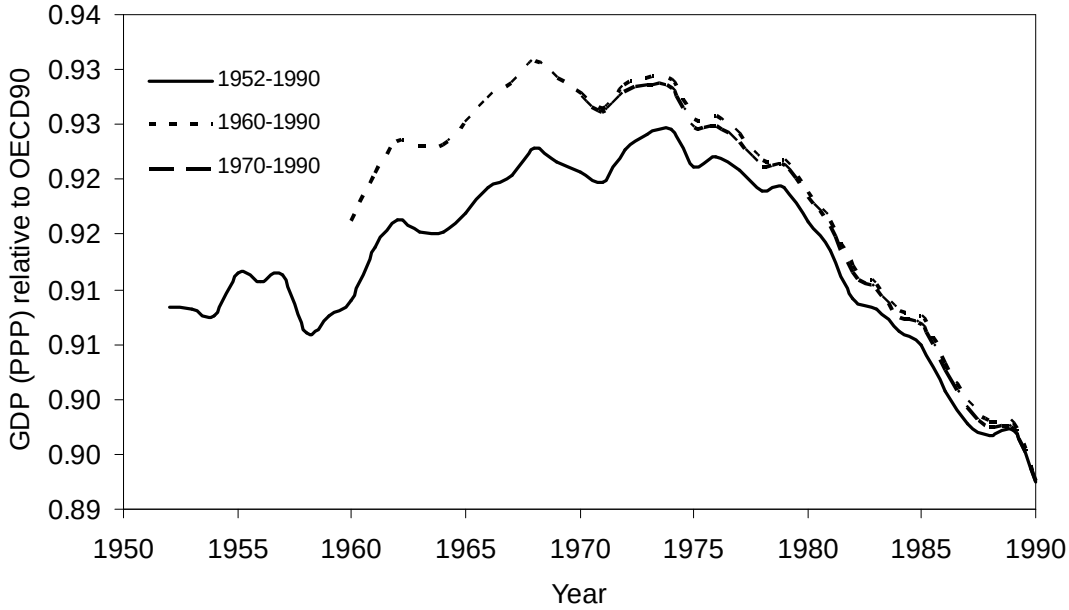

Figure C.6. Three samples of data for income (relative to OECD) for the ASIA region with the 1952-1990 and 1960-1990 data divided by a factor to equalise 1990 level with 1970-1990 sample.