USSEE Policy Briefs

The United States Society for Ecological Economics (USSEE) commissions and posts "Policy Briefs" from leading scholars on specific topics in ecological economics. These documents provide concise overviews concerning the problem, solutions, and policy implications of important issues in environmental policy.

The ISEE Newsletter has been including these briefs in its recent issues, most recently Stephen Schneider's contribution on 'Global Climate Change' in the June 2006 issue. Stephen's policy brief from 2003, prompted the following comment and response from Clive Spash.

Uncertainty & the Enhanced Greenhouse Effect: A Response to Schneider's "Policy Brief"

Clive Spash

In a recent article in the ISEE Newsletter Stephen Schneider made several remarks and a central suggestion which I believe need some clarification and amplification, if not correction. This is especially so because the piece was prefaced as a starting guide to the topic of global climate change.

Before getting past the first sentence I was taken aback by some historical inaccuracy. The issue did not jump up as being serious in 1995 due to the IPCC Second Assessment Report (SAR), but had been on and off the international agenda for some time (see Chapter 1 of Spash, 2002). There had been building concern, mainly amongst scientists, since the first World Climate Conference in 1979. Indeed the IPCC was set-up due to that concern reaching a peak in the late 1980s. The issue was already on the political agenda even in the USA (with Congressional hearings). In December 1990 the UN established a negotiating committee for the Framework Convention on Climate Change (UNFCCC), following on from the 2nd World Climate Conference in November.

By 1990 several nations were proposing unilateral reductions in CO_2 emissions and all European Community (EC) countries, except the UK, agreed on stabilisation of aggregate EC emissions. This informal EC agreement would have allowed Spain, Greece and Portugal to increase emissions. The US under the Bush administration remained opposed to emissions controls, while the Soviet Union was opposed to controls 'at the current time'. Divisions were clear between those proposing substantial reductions, up to 50 per cent, those proposing stabilisation of emissions (not atmospheric concentrations) at various levels, and those opposing any action. We have gone a long way backwards since 1990!

My next concern is the focus on average temperatures without any mention of deviations or distributions. Indeed the implicit assumption in referring to the IPCC figures is that only the mean global temperature is important. This is a highly misleading aspect of past global climate change modelling with its static equilibrium framing and single average shift under a double CO_2 equivalent scenario. Just as and often more important than averages are distributions across space and time. Lots of rain in Brazil does not help Australia, one day of frost in spring can kill an entire regions crops, reduced spring and summer rainfall and higher temperatures leads to more fires and so on. Species also live within thresholds and these can be crossed by changing distributions without changing averages e.g. colder winters and hotter summers, more precipitation in winter and less in summer.

The discussion of ecosystem change is conducted in terms of moving to escape, as if ecosystems were seeking a safe haven: "they are moving poleward, up mountain slopes, or both to "escape" climate change". There are however, no empty rooms on the planet left vacant by recently departed ecosystems so the description is rather misleading. Thus, ecosystems are not moving as such but changing in their composition in unpredictable ways. Forests are not lifting up their roots and moving north; species may die be replaced by other species and so the existing structures change. The old ecosystems will be unable to re-establish in the same location or perhaps anywhere else. Change is also far more uncertain than is made explicit.

This then gets to my biggest concern with the piece by Schneider. Having recognised "deep uncertainty ... and even catastrophic consequences" he tells us that we need "risk management" where "Scientists are responsible for defining the elements of risk". In two paragraphs we have moved from "the multitude of unknowns" to "potential consequences of climate change have been well-outlined". The standard probability of an event times the known outcome is the suggested approach all controlled by expert scientists.

I have spent some time elsewhere explaining the problems of using standard risk assessment approaches for addressing the enhanced greenhouse effect (see chapter 4 Spash, 2002). More relevant is the concept of strong uncertainty which admits we lack the ability to place probabilities on events and that we lack the ability to define the future (see chapter 5 Spash, 2002). The concept of post normal science has been one of the more innovative aspects of ecological

economics (Funtowicz and Ravetz, 1990; Funtowicz and Ravetz, 1992; Funtowicz and Ravetz, 1994), unfortunately Schneider pays no attention to this.

Most worrying of all, the explicit use of scenarios by IPCC experts to address strong uncertainty, which seems eminently sensible, is described as a weakness leaving the policy makers unable to make a decision? That there are multiple futures which are equally likely due to our irreducible ignorance is just not good enough. It sees that we must strive to pretend strong uncertainty does not exist.

Actually Schneider is incorrect in criticising the IPCC for failing to follow his chosen path. Despite stating that probabilities could not be placed on events, and the scenarios report explicitly avoiding this, the TAR notes its use of a classification system using 'subjective' probabilities with precisely defined quantitative confidence levels (see IPCC Working Group I, 2001: 2). Judging from his website, lobbying by Schneider may have had something to do with this. For more on the inconsistencies and contradictions in the IPCC reports see Spash (2002).

A search for evidence about the implications of GHG emissions has led to modelling exercises to reduce strong uncertainty. Reliance for prediction has concentrated on simulation models (e.g., GCMs) as opposed to historical analogue because global temperatures are expected to occur at unprecedented rates. This need for artificial scenarios is a recognition of indeterminacy and partial ignorance.

In order to avoid the whole concept of strong uncertainty Schneider is following a common line of asking for it to be transformed into weak uncertainty by pretending that scientists and experts (selected on what basis we might ask) can make guesses about the future to get "subjective probabilities". However, we are told in the conclusions that policy makers need to act without this knowledge in any case. This seems a little contradictory to the earlier request to place probabilities on the IPCC scenarios due to the inability of the decision-makers to decide. In any case, if Schneider can make a decision on the need for greenhouse gas control, as have millions of others on the planet, then what is wrong with the mythical decision-makers that they cannot?

Decisions on complex, long-term problems involving unique and catastrophic potential events, such as not controlling the enhanced Greenhouse Effect, cannot be reduced to fit within decision theoretic methods designed for repeatable events with nicely defined probability distributions and knowable consequences. Or perhaps we should let the mythical scientific experts play their guessing games and wait to see if the West Antarctic Ice Sheet really does melt, or the northern conveyer belt does get disrupted, or sea level does rises 1 or 6 meters by 2100?

The idea that uncertainty can be reduced, and even eradicated (normally by pleas for more research rather than guessing games) seems common amongst both natural scientists and economists. However, several points can be raised against such a prognosis: the persistence of weak uncertainty due to measurement errors; the persistence of strong uncertainty due to differences in the interpretation of given 'facts'; the methodological problem that evidence can only disprove but never prove a theory; the existence of irreducible ignorance; the lack of any single metric for damage assessment; and the persistence of unknown cause–effect relationships. The type of work being produced by economists exemplifies how implicit value loaded boundaries are drawn in terms of designating which knowledge is employed. While the social aspect of economic knowledge may be deemed to make it implicitly subjective, a similar methodological problem also faces natural scientists. That is, how environmental problems are characterised is seen to be determined by assumptions which restrict the focus of any given research. The IPCC for example has been at pains to avoid talking of catastrophic events on the basis of political acceptability, NOT science.

The alternative to Schneider's plea is to accept that global systems are inherently unpredictable so that many different outcomes are equally likely. Strong uncertainty must then be regarded as a property of the system rather than a failure of scientific method which can be removed by increased research budgets, or by coaxing numbers from "experts". Yet the main approach to uncertainty being put forward by both scientists and economists limits itself to weak uncertainty and fails to discuss the meaning or content of strong uncertainty. A role for some real ecological economics open here.

Oh, yes and while I remember did anyone hear about the ethical issues... well may be another time.

References:

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2006

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