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EDITORIAL

The global warming potential—the need for an interdisciplinary retrial

An editorial comment

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It was about 20 years ago today when Global Warming Potentials (GWP) became established as a method for comparing the climate effects of emissions of different greenhouse gases. Since then they have been going in and out of style. The GWP was originally presented as a climate analogue to the ozone depletion potential, to help assess the climate impacts of switching from chlorofluorocarbons to hydrofluorocarbons (and related molecules) (Rogers and Stephens 1988; Fisher et al. 1990). Interest in its wider utility, and in particular its use to compare the climate impact of emissions of CO_2 with non-CO_2 greenhouse gases, soon followed (e.g. Lashof and Ahuja 1990; IPCC 1990).

The First Assessment Report (FAR) of the Intergovernmental Panel on Climate Change (IPCC 1990) tentatively embraced the concept—as the Convening Lead Author of the relevant chapter in that assessment, I was interested to re-read what we had written way-back-when; in particular, I wanted to see whether, with hindsight, I might have changed anything. I believe that we had many of the necessary caveats in place but I was particularly struck by one statement (where the square brackets are my additions for clarity):

"It must be stressed that there is no universally accepted methodology for combining all the relevant factors into a single [metric] … A simple approach [i.e. the GWP] has been adopted here to illustrate the difficulties inherent in the concept."

But it seems that the die was cast. The IPCC retained the GWP as a metric of choice. As the Kyoto Protocol is a multi-gas treaty, it requires a method to allow parties to the protocol to place emissions of different gases on a CO_2-equivalent scale. The GWP (with a 100 year time horizon) was adopted for this purpose. Indeed, it can be argued that it was the existence of the GWP (or its endorsement by the IPCC) which led to the Kyoto Protocol being a multi-gas treaty—see e.g. Skodvin (1999) for a discussion.

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Did something go wrong here? How did "a simple approach" which was "adopted ... to illustrate ... difficulties" become established in a major piece of environmental legislation, where it had the potential to influence big investment and policy decisions? There are several hypotheses. Were the FAR authors unusually perceptive and got it right first time? Could nobody come up with anything better? Or has the IPCC been lax in assessing alternatives and driving the agenda forward? Indeed, has there been what might be termed an "inadvertent consensus", so that the IPCC and policymakers have each perceived that the other was content with the concept and didn't apply pressure to fully assess alternatives?

Assuming we can discount the first hypothesis as wishful thinking, the likely answer seems to be a mixture of the second two. Certainly there has been no shortage of assessment and criticism of the GWP concept (for example, see earlier Editorial Comments in this journal (O'Neill 2000, 2003; Smith 2003), Shackley and Wynne (1997) and Fuglestvedt et al. (2003, 2009) and references therein). The GWP (at least under the IPCC definition[1]) is the time-integrated radiative forcing due to a pulse emission of a given gas, relative to a pulse emission of an equal mass of CO_2; much of the criticism has centred on the meaning, in terms of climate impact, of CO_2-equivalence when calculated using such a method. But the attractions (such as transparency, ease of application etc) of the GWP seem to have prevailed and the lack of specificity in what aspect of climate change the GWP actually represents may have been useful in a policy context.

Criticism of the IPCC might be met with indignance by Working Group 1 (i.e. physical science) Lead Authors, but the Editorial Comment by Godal (2003) seems to hit the nail on the head. It is not Working Group 1 that I am criticising. The problem with IPCC's handling of emission indices is that it has been handled almost entirely as a physical science issue; it has failed to involve, in an integrated manner, the impacts community and, most tellingly, the economics community, within Working Groups 2 and 3 respectively.

The perspectives and intellectual frameworks that the economics community can provide in the development of emission metrics seem crucial to me, and some of the most withering criticisms of GWPs come from this community. Manne and Richels (2001) (and see the associated News and Views by Bradford 2001) stands very tall in this regard

Manne and Richels (2001) proposed, instead, "price ratios" which were based on a so-called aggregate general equilibrium model, which included, for example, subcomponents representing the economy and energy sector, in addition to representations of the climate system. One important example that they illustrated was for the case of a climate policy which aims to keep temperature change below some pre-

[1] Although the IPCC definition is in most widespread use, it is not the only one. Fisher et al. (1990) defined the GWP as the ratio of the surface temperature change due to a sustained emission of a gas, relative to the temperature change due to a sustained emission of CFC-11. As pointed out by Fisher et al. (1990) this is equivalent to the IPCC definition provided the climate sensitivity of the gas and CFC-11 are the same and an infinite time horizon is adopted; Shine et al. (2005) showed that even for a time horizon of 100 years, the equivalence is quite close. Rotmans and den Elzen (1992) adopted a quite different definition of the GWP which, in modern parlance, appears closer to a time-integrated version of the pulse global temperature change potential—its relationship to the IPCC definition does not seem to have been explored.

specified target some time in the far future; in this case, emissions of short-lived species such as methane would initially have a relatively low value, as emissions of these species now has little impact on temperatures far in the future; but as the target temperature is approached, the emissions of the short-lived species grow in importance, and there should be more incentive to reduce them. A similar result has also been shown by, for example, O'Neill (2003) and Johannson et al. (2006, 2008). As shown by van Vuuren et al. (2006), this perspective might lead to quite different decisions on which gases to mitigate, and when, than would be found using the conventional usage of the GWP (i.e. adopting the same time horizon regardless of when the emission reductions occur).

I have heard it stated that the Manne and Richels type of approach is *the* way emission indices should be constructed. However, while such approaches clearly gain in intellectual rigour, there is a lack of transparency (at least, when viewed by this physical scientist); they include assumptions on, for example, marginal abatement costs, which are highly uncertain and potentially controversial, especially when applied many decades in the future. Straightforward interpretations of the resulting outputs do not seem to yet be available—does the behaviour of the resulting price ratios originate from the physical aspects of the climate system or from the economics, or some combination of the two? The GWP, at least, is more transparent, has fewer embedded assumptions and it is easy to understand the behaviour of numerical values.

But this comes back to the heart of the issue about the IPCC process. The Editorial Comment by O'Neill (2000) was entitled "The jury is still out on global warming potentials"; it is an excellent overview of issues, but perhaps what he did not do, as a barrister might have done, was to ask "who is on the jury?". In terms of IPCC assessments, it has been Working Group 1 scientists, myself included, who have largely stood as judge and jury in this debate and this seems to have bred a conservative physical science approach and fed the inadvertent consensus.

Fortunately, there are signs that the inadvertent consensus is being challenged. Together with the other papers cited herein, our attempt to mimic some aspects of the Manne and Richels (2001) results, using a purely physical science approach, the Global Temperature Change Potential (GTP) (Shine et al. 2005, 2007), appears to have had the agreeable effect of contributing to pressure for a renewed debate.

The integral nature of the GWP means that the memory of the emission of a short-lived but radiatively-strong gas is retained, long after the pulse itself has decayed to zero. By contrast the pulse form of the GTP calculates the surface temperature change at some given time after the emission. It has one aspect of "memory" that the GWP does not (i.e. the thermal inertia of the climate system); however, it is an "end-point" metric, and so for long-time horizons, the GTP for short-lived gases is much lower than the GWP. For example, methane has a 100-year GWP of 25, whilst its 100-year GTP is 4, using the figures from Fuglestvedt et al. (2009).

If you happen to be a country with significant methane emissions, the effect of using either the GWP or the GTP on overall CO_2-equivalent emissions is stark. As examples, using the most recent data on the website of the United Nations Framework Convention on Climate Change (unfccc.int) (and excluding land use change and forestry), Brazil's methane emissions for 1994 would be 110% of their CO_2 emissions, when CO_2 equivalence is calculated using the GWP; for New Zealand (for 2006) the corresponding figure would be 90%. By contrast, if the GTP were used (and I make no claims that the choice of a 100 year time horizon is defensible—I merely do it for consistency with the Kyoto approach for the GWP), the figures would be just 15% and 17% respectively.

Hence, it is unsurprising that some countries are raising fresh concerns about the methodology used to calculate CO_2-equivalent emissions, and this has led to new activity within IPCC (IPCC 2009). This promises to open up the kind of cross-disciplinary debate and collaboration that I feel has been needed for several years now.

Many of the issues are discussed in more detail in the assessment by Fuglestvedt et al. (2009). There is wider realisation that there is no such thing as a "best" metric, irrespective of its intended use. The design of the metric depends on the policy it is intended to serve; different choices may be made for policies with specific aims (such as keeping below pre-specified levels of concentrations, radiative forcing or temperature) than those like the Kyoto Protocol, which specifies emission targets for each party but without a clearly-specified aim. There is an understanding that there needs to be a clear choice of impact parameter (which could be radiative forcing, temperature change, sea-level rise, economic impact etc), and its characteristic (integrated over time, value at a particular time, rate of change, etc). There is more focus on parameter choice within metrics—the stark differences between the GWP and GTP for the CO_2-equivalent emissions cited above could also be obtained using the same metric but different time horizons (e.g. Skodvin and Fuglestvedt 1997). It seems to be widely believed that the Kyoto Protocol chose a 100 year time horizon, because it was the middle one of the three (20, 100 and 500 years) that happened to be presented in IPCC reports. There is certainly no conclusive scientific argument that can defend 100 years compared to other choices, and in the end the choice is a value-laden one. And no matter how uncomfortable the concept of discounting can be to physical scientists (see e.g. Sherwood 2007), the choice of *any* time horizon short of infinity is, de facto, a decision to impose some kind of discounting (albeit one that depends on the lifetime of the gas in question—see Fuglestvedt et al. 2003).

It is certainly premature to anticipate the demise of the GWP.[2] Imperfect though it might be, it still achieves an important role in allowing the implementation of the Kyoto Protocol and there would undoubtedly be a cost to changing the method of calculating CO_2-equivalence; importantly, if there was to be a change, it would need to be done with good cause, it would require widespread consensus, it would need to be suited to the climate policy that it is meant to serve and it would have to have some degree of permanence. Further, as shown by Johansson et al. (2006) (see also O'Neill 2003), integrated climate-economic models indicate a large economic benefit in adopting a multi-gas approach rather than a CO_2-only approach in meeting some specified temperature stabilization approach; however, they also show that there is relatively little economic penalty in adopting the GWP to calculate CO_2-equivalence, rather than using their optimised approach (at least on a global level—this may not be true for individual nations).

The paper by Tanaka et al. (2009) presents a fresh test of the GWP and proposes a new alternative, which they call the Temperature Proxy Index (TEMP), which goes some way to having some quantitative assessment of a "best" time-horizon. Their

[2]Previous reports of the death of the GWP have been greatly exaggerated. Shackley and Wynne (1997) cite an anonymous scientist referring to "the end of the GWP saga" back in 1996!

test is to examine how well past temperature changes (1890–2000) can be simulated when methane and nitrous oxide emissions are replaced with their CO_2-equivalent emissions, when GWPs are used to calculate this equivalence. While this is setting a test that the GWP may never have been designed to pass, the results are valuable. Using the 20-year and 500-year GWPs leads to notable departures from a good fit. While the "Kyoto" 100-year GWP is better, it is not optimal, and systemically underestimates the observed temperature change. In the case of methane a time horizon of 44 years produces the best fit; for nitrous oxide, 70 years produces the best fit, albeit not a very good one.

Tanaka et al. (2009) then introduce a multiplier, TEMP, that is defined as the multiplier of methane and nitrous oxide emissions which produces the best fit between observed and modelled temperature change, but without any constraint that it should correspond to any particular time horizon of the GWP. Hence this new metric is defined by its ability to pass a pre-specified test, rather than being driven by some pre-specified view on which aspect of an emission best represents its consequent climate impact. For methane, the value of TEMP which achieves the best fit for the 1890–2000 period is the same as the 44-year GWP; in the case of nitrous oxide, the value is higher than can be obtained with any time horizon of the GWP. Because the basis of TEMP is a best fit over a given period it can be regarded as an integrative measure, so in this regard it is closer to the GWP than the GTP. Whether a time-integrated version of the GTP could also be applied remains an open question. The numerical value of TEMP depends on the time period that it is required to simulate—in general, it is higher for short-lived species, for shorter periods. Over long periods, the persistence of CO_2 in the atmosphere means that short-lived gases are less influential in controlling the temperature.

As Tanaka et al. (2009) acknowledge, a difficulty in applying TEMP in a forward looking case, as it would need to be for application in climate policy, is that the value will depend on future scenarios of climate change; however in this respect, it does not differ from the application of the pulse GTP proposed in Shine et al. (2007).

Given the renewed interest in emission indices, TEMP arrives at an opportune time. While it is less easy to derive values for TEMP than the GWP (because of its requirement for a more complex and hence less transparent modelling framework), its relationship to a clearly-stated behaviour of the climate system might prove to be an attractive attribute. At the very least, the presence of such an alternative approach should ensure a more informed debate.

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