LETTERS

Pondering Greenhouse Policy

In his article "An optimal transition path for controlling greenhouse gases" (20 Nov., p. 1315), William D. Nordhaus takes a giant methodological step forward. His dynamic integrated climate-economy (DICE) model couples my and Starley Thompson's globally averaged climate model (*1*) with economic dynamics in order to evaluate the economic efficiency of different carbon tax scenarios. Nordhaus is one of the few economists who appropriately tries to balance the potential costs of CO_2 emission controls with the external costs of unmitigated climate change—those that would occur in the absence of such controls.

In his article, Nordhaus wisely notes some of the inadequacies of his approach: (i) developing countries and natural ecosystems are underrepresented; (ii) other market failures are omitted, as are high leverage opportunities (*2*); and (iii) learning may mitigate costs. To these caveats, I would add the absence of surprise scenarios of change in climate (for example, ocean current flip-flops or super hurricanes), or ecology [for example, the synergism of habitat fragmentation with climatic change and its implication for species extinctions (*3*)], or economy (for example, cost breakthroughs in non–fossil energy supplies), or security [for example, the creation of environmental refugees and political instability (*4*)].

Most important, in a longer version of DICE (*5*), Nordhaus plots the absolute world per capita consumption from 1965 to 2105 using three DICE scenarios: an "unconstrained" case (that is, no carbon taxes), an "optimal" case (that is, modest carbon taxes sufficient to reduce emissions enough to have economic output decreases balanced by Nordhaus' assumed surprise-free climate change externality of less than 1% gross world output), and a supposed Draconian "20% cut" case that Nordhaus recommends against from the policy perspective as enormously expensive. But is this latter option really so Draconian in a total view? Figure 9 in that longer version shows that the unconstrained or optimal paths from 1965 to 2105 share a virtually indistinguishable difference and also exhibit a growth of about 470% in per capita world consumption (the measure of economic well-being used by Nordhaus). But the "20% cut" case, which requires major carbon taxes of hundreds of dollars per ton in order to reduce global warming by about 40% to 2105, is hardly different from the other cases when it is plotted as absolute welfare change over time, rather than as a welfare difference as shown on figure 5 of Nordhaus' *Science* article. Thus, by Nordhaus' own reckoning the "20% cut" option represents "only" a 450% increase in personal economic well-being as opposed to a 470% increase in the unconstrained case! Such "climate insurance" (that is, a 20% emissions cut) against plausible climatic, ecological, economic, or political surprises only causes the world to wait about one decade more to achieve the same level of economic status (470% growth since 1965) as in the unconstrained case. Are we so risk-prone or greedy as a society that a 10-year delay in achieving what is already a dramatic increase in per capita consumption is too expensive a premium to pay for planetary environmental insurance? The answer is, of course, a value judgment, not an automatic result of any quantitative method, no matter how advanced.

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4. C. Tickell, *Climatic Change and World Affairs* (Univ. Press of America, Lanham, MD, 1986).
5. W. D. Nordhaus, "Rolling the 'dice': An optimal transition path for controlling greenhouse gases," paper presented at the Annual Meeting of the American Association for the Advancement of Science, Chicago, IL, 8 February 1992.

Although praiseworthy in many respects, the article by Nordhaus has two problems. The first is that he considers the impact of uncertainty to be minor, an assumption that has been shown to be doubtful (*1*). The second, and perhaps more subtle, problem arises from framing the analysis in terms of the prospects of a mythical "average global citizen." The rich, technologically advanced nations are likely to have little trouble coping with a wide range of problems that result from climate change; unfortunately, poor countries are unlikely to be so lucky (*2*). A much greater proportion of their economy originates in climate-sensitive activities, and these countries have fewer financial and technological resources to adapt. Nordhaus acknowledges this problem but does not develop its implications. A world with the rich nations growing richer and the poor nations declining or even starving because of climate change is likely to be a world with wars and terrorism.

By averaging across nations, Nordhaus seems to assume that the rich nations will be so rich that they can afford to pay for losses experienced by the poor nations. The citizens of rich nations may not choose to be so generous in, say, 2050. Also, transferring income is difficult and expensive. The best-intentioned efforts to help the poor in other nations seem destined to contribute to the enrichment of the few, unless a large organization is put into place to target the aid. Such organizations are expensive, judging by U.N. operations or by the full social cost of voluntary groups. Even aside from costs, the best efforts of the United States and other nations have been less than wholly successful in helping poor nations (*3*). One possible conclusion is that significant global climate change could do irreparable damage to poor nations and that the only way to prevent the damage is to prevent the climate change.

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Nordhaus' climate-economy optimization model bears little relevance to policy decisions. For example, Nordhaus uses a function to describe emissions mitigation costs that does not account for free or cost-saving measures such as certain improvements in residential and commercial energy efficiency. This assumption conflicts with the findings of his reference 17, a recent report from the National Academy of Sciences (*1*).

Furthermore, the model's description of control costs does not reflect any effect of mitigation policies on the rate of technological change. It is unrealistic to assume that policies that could affect global output by about 1% would not induce the development of new technologies (for example, cheaper, more efficient photovoltaic cells and energy storage systems) that would reduce costs.

Nordhaus notes that market failures, like air pollution and damages to unmanaged ecosystems, are not accounted for by the model's damage function, $d(t)$. The United States currently spends more than 0.5% of its gross national product to mitigate air pollution alone, most of which originates with the use of fossil fuels. Large reductions in the use of these fuels to slow global warming would also reduce the need for existing and additional air pollution controls, which would result in savings comparable to the $d(t)$ values calculated by Nordhaus. Also, the arbitrary nonlinear form of $d(t)$ minimizes damages from climate change occurring over most of the next century.

All proposed geoengineering options have potentially costly side effects that are not accounted for in this study. For example, to counteract a warming equivalent to a CO_2 doubling would require the artificial reduction of the global average insolation by 2%, with additional reductions required as greenhouse forcing increases further. Reduced insolation would affect agricultural production and natural ecosystems.

Questions about the structure of the model go beyond the usual criticisms of intergenerational discounting. A policy-maker must consider not only the potential damages from a "most-likely" warming scenario but also those from extreme scenarios. The unequal distribution of impacts is also relevant. A probabilistic framework that accounts for uncertainties in climate sensitivity, the damage function, and mitigation costs might produce more enlightening results. In addition, surprise outcomes, like the ozone hole, defy quantification. To paraphrase (and alter) Nordhaus' conclusion, they raise the possibility of a major waste of resources if greenhouse gas policies don't go far enough.

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1. National Academy of Sciences, Committee on Science, Engineering, and Public Policy, *Policy Implications of Greenhouse Warming: Mitigation, Adaptation, and the Science Base* (National Academy Press, Washington, DC, 1992).

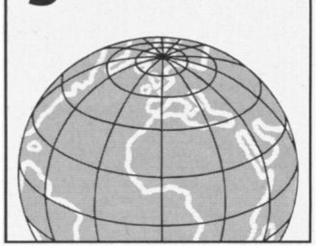
Response: The dynamic integrated climate-economy (DICE) model is an attempt to integrate economic and geophysical considerations so that we can determine policy options (emissions restraints and carbon taxes) that balance the costs of action against the damages that result from inaction. These letters represent thoughtful but critical assessments of my methodology and conclude that my results cannot be used for policy purposes. There are two main lines of argument: The first is a series of specific methodological and scientific reservations about individual assumptions; the second is a qualitative conclusion that quantitative approaches like the DICE model cannot capture the full range of social, economic, political, and environmental complexities.

Most of the specific criticisms in these letters are addressed in my original article and have been subjected to rigorous sensitivity and risk analyses elsewhere (*1*). Among the issues that I have analyzed and found to be of minor importance to outcomes are alternative climate models, alternative specifications of the carbon cycle, alternative cost functions of the kind Oppenheimer discusses, a multi-region specification of the kind that Dowlatabadi and Lave might endorse, alternative specifications of the damage function in the spirit of Oppenheimer's critique, and some specifications of induced innovation.

All uncertainties are not created equal, however. Sensitivity analyses (*1*) have identified two issues of major importance: the issue of revenue recycling and the rate of time preference. Oppenheimer is correct to point to issues of risk and uncertainty; preliminary work (*1*) suggests that ignorance about the costs, benefits, and science base might lead to more stringent controls and to a carbon tax that is about double that calculated in the "best-guess" case.

The advantage of analytical approaches like the DICE model is that the significance of alternative assumptions can be determined relatively easily. One argument against economic models, raised by Oppenheimer, is that they do not account for the possibility of "free or cost-saving measures" to reduce emissions. The National Academy of Sciences' study (*2*) concluded that between 10 and 40% of emissions in the United States could be reduced or mitigated at low or no cost. As a participant in that study, I believe that the low end of the range is more realistic; others are even more skeptical and hold that free mitigation is completely implausible. Assuming optimistically that emissions could be costlessly reduced by 30%, a DICE-model run shows that the efficient carbon tax rate and rate of costly emissions reduction would be slightly *lower* than in the base case.

Because models can never be literal re-



productions of complex economic or natural systems, the question arises as to how we should use the results of the DICE model for policy purposes. Schneider presents an intuitive argument that more ambitious policies might be warranted. He refers to a policy of stabilizing emissions at 80% of 1990 levels, which would have a present-value cost, relative to the efficient path, of $11 trillion (3). Annuitizing this at a discount rate of 4% would yield an income reduction of slightly over 2% of global income. Schneider suggests that this might be justified as a "premium to pay for planetary environmental insurance." I demur. Before laying out $11 trillion, shouldn't we inquire into the risks and the payoffs? Stabilizing emissions only slows the rate of climate change by about one-third. Moreover, while it would cost "only" 2% of our income, this outlay should be weighed against other claims on our insurance and investment dollars—declining educational attainment, crumbling science laboratories, hazardous Russian nuclear reactors, and dangerous American streets—at a time when net investment in the United States is only 4% of national income. In light of our tremendous needs and our meager means, the modest but real restraints on emissions suggested by the DICE analysis are a reasonable goal for the next few years.

The three previous letters caution against taking the numerical results literally—a caution that I would endorse. Quantitative economic models are useful in developing a menu of choices. These should not replace reasoned value judgments that weigh economic growth, distributional considerations, and environmental concerns. But Oppenheimer is too nihilistic in arguing that quantitative approaches like the DICE model "bear little relevance to policy decisions." Ironically, he would have us accept the numerical findings of the climate models while rejecting the calculations of the economic models. The point of empirical economic analyses is to include as fully as possible all costs and benefits so that competing objectives, like living standards and the value of environmental services, can be sensibly balanced.

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3. W. D. Nordhaus, *Resour. Energy*, in press.

Priority Envy

Au contraire, Professors Cohen and Bickart (Letters, 6 Nov., p. 876)! I retain some class preparation notes from October 1969 that positively and irrefutably establish my priority in the use of the term "physics envy" as it is applied to biological scientists and to the supposed inferiority of that science to physics. I used the term in a general biology class at the University of Utah and was so proud of my perception and originality that I must have used it several times later that day in my coffee group at the Student Union. I suppose that other claimants listened from the next table or subsequently heard the term through the grapevine.

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Corrections and Clarifications

In the Perspective "Glowing avalanches: New research on volcanic density currents," by Greg A. Valentine and Richard V. Fisher (19 Feb., p. 1130), the photograph credit line should have read, "[Courtesy Richard P. Hoblitt, U.S. Geological Survey]."

In the article "An optimal transition path for controlling greenhouse gases" by William D. Nordhaus (20 Nov., p. 1315), equation 5 was misprinted. It should have read $c(t) = C(t)/P(t)$.