

**FOREWORD**  
**BY LAWRENCE SUMMERS**

ARCHITECTURES FOR AGREEMENT:  
ADDRESSING GLOBAL CLIMATE CHANGE IN THE POST-KYOTO WORLD  
Edited by Joseph Aldy and Robert Stavins  
Cambridge University Press

This is an important book on an important subject. Joseph Aldy and Robert Stavins are to be commended for bringing together, under the auspices of the Environmental Economics Program at Harvard University, such a distinguished group to think through critical aspects of one of the most important policy problems the world faces.

Many public policy problems are at their root political. In these cases, it is reasonably clear what should be done; the challenge is to get the parties to agree on a plan of action, given the complex political constraints they face. Opinions will differ, but I would place the Israeli-Palestinian conflict, U.S. social security reform, and the completion of the Doha Trade Round in this category. Other problems, like the repair of the American health care system or how to address radical Islamic terrorism are more profound in the sense of needing conceptual work on what the right approach is before it is realistic to aspire to political consensus. Global climate change falls within this latter set of problems, so I think it is particularly appropriate that an institution like Harvard devote its formidable intellectual resources to creative thinking about possible solutions.

As an economist who has served in government, I am particularly pleased to see this volume's emphasis on what might be called policy engineering—the development and implementation of new frameworks to address pressing problems. As traditional engineering rests on but must extend the work of the physical sciences, so also policy engineering rests on but must extend the work of the social sciences. There is a tendency in modern intellectual life to undervalue engineering relative to basic science, and this is no less true with respect to policy

engineering and social science. Therefore, volumes like this one make a contribution that goes beyond their particular content. For a number of reasons, I believe that global warming poses uniquely difficult challenges in policy engineering.

First, the essential issues involve a longer time horizon than with respect to any other major public policy problem. Probably the closest alternative is issues associated with entitlements and aging societies where the horizon is a quarter century or so and where success in taking prompt action has not been conspicuous in any democracy. While some costs are already being felt, the greatest costs associated with global warming are unlikely to be felt for half a century or more.

To start, this raises important analytical questions. Traditional approaches to policy analysis rely on what economists refer to as intertemporal discounting. Benefits and costs of a given policy that take place in the future are given less weight to reflect some combination of the human tendency to value benefits today more than tomorrow, the ethical observation that people in the future are likely to be richer than people today, and the observation that through investments it is possible for society to obtain one dollar's worth of goods in the future by spending less than one dollar today.

The Federal government as matter of practice in making investment decisions or doing cost benefit analyses typically uses a 7% discount rate to reflect these considerations.<sup>1</sup> At this rate a \$1000 benefit or cost avoided (after correcting for inflation) fifty years out is worth less than \$35 today. If one looks one hundred years out, \$1,000 of benefit is worth about a dollar. Even at a 3% rate, a \$1000 benefit 100 years out is worth only about \$50 today. Essentially it is on this basis that the Copenhagen consensus group of economists placed global warming at the bottom of a list of development priorities.<sup>2</sup>

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<sup>1</sup> OMB Circular A-94, Revised, October 29, 1992. The September 2003 revision of OMB Circular A-4 on Regulatory Analysis recommends the use of 3% and 7% real discount rates in government cost-benefit analysis, and consideration of lower discount rates for policies with intergenerational benefits or costs.

<sup>2</sup> Lomborg, B., ed. 2004. *Global Crises, Global Solutions*. Cambridge: Cambridge University Press.

While this approach has weight—and ethical force in light of the likelihood that our descendants will be far wealthier than we are--it fails to give adequate weight to the moral intuition that most of us have of our obligation to posterity. The greatest acts of statesmanship—starting with the American Revolution and establishment of our Constitution have been motivated by a concern for posterity not by cost-benefit analyses. How best to recognize this obligation in carrying out policy analysis while at the same time maintaining some rigor in recognizing that resources are scarce and become increasingly so over time, is a question that deserves much more analysis than it has received to date.

Equally as important as the analytical questions involved with the long horizons associated with global warming are the issues of structuring decision making processes. How can democracies of finite-lived citizens electing leaders every several years find their way to taking steps with real costs whose primary benefits will be enjoyed only by subsequent generations? The question is not just one of assuring that commitments are made but also, as the experience in a number of countries with the Kyoto treaty illustrates, that they are honored.

The second respect in which global warming stands out as a public policy problem is our inability to predict with confidence its impacts or even to enumerate with conviction all the possible impacts. We are in the realm of unknown unknowns. My lay person's reading of scientific evidence of the kind marshaled by Daniel Schrag in Chapter • suggests that there is now little room for debate about the reality of large-scale, anthropogenic climate change. Those who question the reality of human impact on climate look increasingly like those who are still arguing about whether tobacco has harmful health effects. But to say that a human signature on global climate change has been conclusively established is not to say just how large this signature is or what its impact will be. There is a possibility that climate change will prove relatively gradual in its effects and that some impacts will be benign, such as agricultural improvements due to increased carbon dioxide concentrations. On the other hand, there are clearly malign effects such as coastal flooding due to rising sea levels, and a whole range of

negative possibilities from increasing storm activity to large scale melting of polar ice sheets. There are also likely to be important micro-climatic effects that bear on particular regions.

Of particular concern is the possibility of non-linear effects involving positive feedback effects. If initial climate impacts are self magnifying, as for example the melting of polar ice raises global temperatures, then relatively small impacts can have rapid and profound consequences. But it is very difficult to judge the likelihood or quantify the consequences of such scenarios. It cannot be responsible public policy to ignore risks until it is conclusively established that they will play out. On the other hand, I think economists are right to have great difficulty with the so-called “precautionary principle” favored by many environmentalists, which essentially calls for always assuming the worst. What is needed is an approach that recognizes our inherent ignorance and seeks to preserve as much flexibility for future generations—by neither allowing greenhouse gases past a point of unreasonable risk nor preventing the energy use that is essential if standards of living globally are going to rise. In this regard, it would be useful to imagine how some of the approaches described in this volume could evolve with different possible scenarios for the impact of global climate change so as to evaluate their robustness.

The third respect in which global warming stands out as a public policy problem is that for any successful solution it requires international cooperation at a scale to which we are not accustomed. Although developed countries still account for the majority of carbon emissions, the balance will soon tilt towards developing countries. Today, most of the growth in emissions of greenhouse gases is coming from developing countries, and within a decade, if current projections prove accurate, developing countries will account for more emissions than all OECD member countries combined.<sup>3</sup> Moreover, the areas of the world that are likely to experience the greatest impacts from climate change such as Bangladesh are not especially important sources.

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<sup>3</sup> Energy Information Administration. 2006. *International Energy Outlook 2006*. Washington, DC: Department of Energy.

Conversely some important source countries such as Russia are likely to be much less burdened and may even benefit from global climate change.

There is not much precedent for international cooperation involving the combination of the breadth of countries and the magnitude of the policy commitments that will be necessary to address global warming. Tom Schelling in his chapter in this volume rightly holds out NATO as perhaps the most significant historical example of countries making major commitments to one another. I cannot help but wonder whether NATO would have been formed in the late 1940s if the Soviet threat was fifty years off. Nor I suspect could it have been formed without the singular role of the United States whose security commitment to Europe made participation an easy choice for European countries. In a world where there is no dominant actor in global climate change, reaching agreement will be more difficult.

An alternative and perhaps more encouraging example of international cooperation is the European Union in which countries have made very significant economic commitments to one another, sacrificed substantial sovereignty, and indeed engaged in significant resource transfers. Yet the Common Market and now the European Union arose out of particular historical circumstances that seem difficult to replicate quickly on a global scale.

The difficulty of reaching and enforcing international agreements with the necessary scope is reinforced by particular aspects of the global climate challenge. The countries where the opportunities to reduce the emission of greenhouse gases most efficiently and with the greatest leverage, such as India and China, currently have very low emissions per capita and have contributed less cumulative emissions to the atmosphere than some developed countries, and so assert a strong moral claim to being left alone to catch up with the industrial world. Moreover, unlike with CFC's or lead in gasoline, there is no qualitative principle like zero emissions that can serve as a focal point for agreement. Any targets or commitments that result from negotiation will inevitably appear somewhat arbitrary.

New approaches to international cooperation will be required if strong steps are to be taken with respect to global climate change. It has been said that in democracies fear does the

work of reason. Perhaps as concern increases about the consequences of global warming, the willingness of nations to enter into truly binding agreements will increase. But I suspect considerable imagination will be required as to how agreements can be made attractive to the major developing countries or made to be effective without their participation.

### **Policy Observations**

During the 1990s, I was an active participant in the Clinton-Gore administration's policy process leading up to Kyoto. It gave me a clear sense of the difficulties involved and broadened my perspective beyond the economists' narrower view I had previously taken. While I continue to believe that careful, rigorous, and thorough economic analyses of alternative proposals is a *sine qua non* for effective policy-making in this area, in the remainder of this forward, I want to focus on some of the non-economic considerations and perspectives I believe, based on my experience in government and at Harvard, need careful attention in any informed debate on global climate change policy.

First, there is the constraint imposed by politics that governments are unlikely to write substantial checks to each other pursuant to international treaties in the not too distant future. It is revealing in this regard that the Maastricht Treaty, which called for relatively limited financial penalties within the European Union for the violation of fiscal rules to which everyone was deeply committed, has not succeeded in constraining behavior or led to the successful imposition of fines.

Estimates differ as to the size of the potential transfers connected with global climate change, but it seems likely that achieving full international efficiency could require transfers in the range of tens of billions of dollars. As one who has sought, with mixed success, to induce the U.S. Congress to support transfers in low hundreds of millions of dollars to international financial organizations at a time when the U.S. economy was imperiled by international financial instability, I am skeptical that U.S. policy would ever contemplate transfers in the billions of dollars. I fear this kind of political constraint may be every bit as real as the various natural constraints imposed by the laws of chemistry and physics.

This is not a counsel of despair. It is rather a call for incorporating these constraints into the design of optimal frameworks. One potentially fruitful approach involves assigning emission rights not to nations but to corporate entities, and allowing these rights to be traded even across international borders. Clearly we need some national allowances. We could construct national aggregates from total transfers, just as there is a focus on current account balances today. Other important questions raised by this approach concern the treatment of new enterprises and whether they are differentially handled in developed and developing countries. These issues are taken up in chapters 3 and 4, where approaches characterized by “targets and timetables” are examined.

The second approach--the focus of chapters 5 through 8, involves seeking to coordinate domestic policies alternatively categorized as “harmonized domestic policies” and “coordinated and unilateral policies.” The idea here parallels what is done in international trade agreements where nations accept commitments made by other nations. While subject to the difficulties noted above, this approach has I believe considerable potential. It is reinforced by international emulation effects of a kind not factored into standard “realist” analyses in political economy. Because of emulation effects, the scope for coordination may actually be greater than it seems. I am struck by the example of the Basel Banking Accords, where countries have adjusted their domestic banking systems even when they are not of great systemic importance, simply because they want to be part of the international club. It would be a serious mistake for the United States not to recognize the considerable moral impact on the international community of any meaningful actions it takes to reduce greenhouse gas emissions.

A second broad observation goes to the need to be catholic in pursuing multiple areas of policy research. I do not think it is jaundiced to recognize that there is a possibility that climate change will prove to have been much more seriously consequential than today’s scientific consensus suggests and at the same time that we will have done too little to prevent the problem. I very much hope on both counts that such a situation never materializes. It does, however, raise the question of geo-engineering, a topic which is an anathema to environmentalists. Geo-

engineering involves the kind of intrusion into a highly complex ecosystem that we should all fear most. The prospect of trying to balance one man-made global effect with another is certainly disconcerting, and it may well turn out that there are no viable geo-engineering solutions. Nonetheless, there is a prospect and a possibility that viable solutions could be identified. Such solutions could be crucial in a time of what otherwise could be moments of environmental catastrophe. They could also contribute to reducing the adverse effects of climate change at a fraction of the cost of more direct approaches.

Without necessarily being as enthusiastic as Tom Schelling is in Chapter • or Scott Barrett in Chapter •, I think it would be a shame if this was not a more active area of research in the future than in the past. Much as it is prudent to invest in carbon dioxide emissions abatement because of the potential for enormous unforeseen costs, it is also prudent to invest in geo-engineering research for the prospect of its enormous and as-of-yet unforeseen benefits. If successful strategies were to be identified, carefully tested and implemented and the difference between benefits and costs proves to be sufficiently large, then even a relatively small probability of success justifies carrying on an active research program.

In the same way that many who were rightly concerned about nuclear war abhorred any consideration of a post-nuclear environment for fear that it would lower the nuclear threshold, many environmentalists fear that legitimating discussions of geo-engineering will undermine efforts at more direct approaches to the climate change policy challenge. I sympathize with this concern and this is why I have treated the geo-engineering issue gingerly in these observations. I suspect, however, that there is at least a small chance we would live to regret treating the whole subject as taboo.

The third broad observation concerns a policy aspect that is probably very important in practice, but which is very uncomfortable for economists. It is the area that environmentalists refer to as “win-win” situations. These are situations where reducing energy use or carbon dioxide emissions makes a company more profitable or lets a non-profit organization free up valuable resources. Economists tend to be skeptical of such opportunities, because it is an article

of faith for them that profit-maximizing firms do not systematically leave hundred dollar bills on the table. Others seem to see such opportunities as pervasive. It is not uncommon to hear claims that increasing energy efficiency would yield double-dividends. On the one hand, it would decrease emission of greenhouse gases; on the other hand, such improvements would pay for themselves.

My experiences as a kind of CEO at Harvard have frankly made me more sympathetic to these claims than my previous experience as an academic economist. I have come to see that what economists would call agency problems often lead to inefficiency. At Harvard, for example, building architects were judged on what their designs cost per square foot to build with no attention paid to subsequent operating costs. Similarly, those with construction responsibility were judged on whether they brought a building in under or over projected cost, not on what happened to energy outcomes. It is little wonder campus buildings are often less energy efficient than optimal, and that opportunities to make marginal investments in energy efficiency with an internal rate of return far greater than any sensible hurdle rate – of 15 or even 20 or 30 percent – are foregone. Establishing the so-called “green campus fund” during my tenure had the effect of changing budgeting procedures to recognize some of these impacts. The gains were substantial.

I doubt that Harvard is unique in not providing all the right internal incentives for energy efficiency. The appropriate policy to address this reality is less clear. I worry about trying to overcome these internal problems of corporations with regulation that by its nature is likely to be imperfectly targeted. Universal government standards cannot be the answer. Voluntary efforts associated with ideas like the “triple bottom line” seem more fruitful to me, but it is hard for them to be the basis for binding national commitments. We now have a rich theory of why and how firms exhibit what Harvey Leibenstein called “x-inefficiency,” based on the problems of harmonizing internal incentives.<sup>4</sup> These kinds of problems are probably even more important in the case of public enterprises and firms in complex environments where maximizing profits may

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<sup>4</sup> Leibenstein, H. 1966. “Allocative Efficiency Versus “X-Efficiency.” *American Economic Review* 56(3): 392-415.

not be the most important objective. Applying some of these ideas to the environmental area would seem to be very fruitful.

There is an additional aspect related to efforts to force technological improvements. A great deal of experience with public policies ranging from new weapon systems to infrastructure projects like Boston's "Big Dig", to programs like Medicare prescription drugs suggests that the costs of new public initiatives that represent a step into the unknown typically exceed initial estimates by a wide margin. On the other hand, in the environmental area, the tendency has been the opposite, most conspicuously in the case of sulfur dioxide control programs introduced to control acid rain. Once commitments were made, the costs were much less than initially predicted.

One plausible hypothesis as to why the experience is so different in the environmental area comes from considering the sources of forecasts. Weapons systems, infrastructure projects and entitlement programs all have cost estimates produced by or at least relying on information from those who strongly support them. The major source of information on the costs of environmental policies is likely to be businesses who would rather avoid new regulation and who may underestimate the new technologies that will be created if sufficient incentive is provided. It may be right to extrapolate somewhat from previous environmental initiatives and conclude that the cost of policy commitments may actually prove over time to be less than now appears.

### **Conclusion**

In the physical and life sciences, it is taken for granted that basic research pays off remarkably over time. No one doubts that research on the inner working of cells unconnected to any particular disease will pay off over the long run, or fails to recognize that fundamental research on quantum theory ultimately pays off in new materials or even that sophisticated research in pure mathematics pays dividends in the encryption algorithms that protect our privacy. It often seems less intuitive that basic research pays off in the social scientific realm. Yet over time the impacts can be very large. I remember back in the 1970s, when the idea of market-based solutions to environmental problems was first put forward and was being

dismissed as “licensing pollution”. Auction theory was seen as a mathematical economist’s plaything for many years, but has come to shape public policy in areas as disparate as the leasing of government lands, the realizing of spectrum rights, and indeed the creation of markets for sulfur dioxide. Congestion tolls were seen as an unworkable and impracticable economic idea when first put forward, and yet have come to be highly successful in managing traffic and pollution in central London. There are many more such examples.

If the world is to successfully grapple with a challenge like global climate change, basic social scientific research in economics, international relations, organizational theory, and many other branches of the social sciences can make an important contribution. I commend the editors and the authors who have contributed to this volume, and I look forward to the widespread discussion it will undoubtedly generate.