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12/21 - Sandalow

1. BA Plan of Action -- Winter '99 Submissions

Jan/Feb

A. February 1 - Article 3.4
B. March 1 - Flex Mechs, Compliance
C. April 1 - Flex Mechs
D. Constituency Process

"additional items" for next meeting

- Early Jan - DOS

process for coming up w/ new items.

* Jan 4 - Margo/State meeting on sinks

March 1 - more important meeting/submission

2. POTUS/VP Meetings and Travel

A. Menem State Visit Jan. 11 Mid Jan. - proposal for science/research cooperation.
B. POTUS Mexico/Central America Feb.
C. VP/S. Africa
D. VP/Russia

- financing workshop - USAID
- "geek level" meetings - 3 of them
- "forget forms" - 1st or 2nd wk. Jan (sandalow)

B. Flex Mechs: few weeks - agenda

- Feb. 28 - submissions 4 wkshop (agenda topics)
Late March - workshops (practicable no decisions)
Late April ~~follow up meetings~~ for submissions w/ text/proposals for decisions.

compliance, participation criteria

CDM - (WRI Paper) ?

- baselines & benchmarks - early Jan. meeting - CEA/EPA/DOE/Treas/OSTP
- types of projects - eligibility
- upper staff level meeting

Gardner

EPA is in charge for DOE-CDM pack in

Compliance - Sue (State) & EPA - process memo - important March 1. submission

(2) B. - "climate friendly development" - $ to

→ Big Dec. is what kind of "meaningful part" can we live with
⊖ Aid for C.A. can be used to get C.A. buy-in.

"Bold Proposal" - USAID, State, Sam

- framework memo - Jan. 1st/2nd wk.

cc: JAF
SY
IA
SP
QF
Cong-

TO: Addressees

FROM: State/L/OES - Sue Biniaz

Rm 476

RE: Compliance

- For purposes of the meeting on February 22nd on compliance, below is a short summary of where things stand and key issues for policymakers.

- While the Protocol contains many U.S.-inspired provisions related to compliance, it calls for elaboration of the rest of the compliance regime, including the procedure(s) for determining non-compliance and most consequences for non-compliance.

- Consistent with the U.S. view, COP 4 established a group to elaborate the compliance regime on the same timetable as other issues, such as flexibility mechanisms. (We circulated a statement on compliance at COP 4, for those who are interested.)

Get it

- Compliance issues are fairly complicated; in addition to raising major policy questions (such as consequences for exceeding the target), they involve lots of technical points and interlinkages among provisions. It has been clear from the international discussions that we have thought about the issues far more than most countries.

- The umbrella group had its first substantive discussion about compliance in January. We agreed to make individual submissions, drawing on common points. (A summary of that compliance discussion is available, for those who are interested.)

- Attached is the draft U.S. submission, which incorporates the views of the mini-interagency group that has been thinking about compliance (as well as numerous points of outside groups) ; it is being circulated for full interagency comment.

  -- The submission lays out U.S. views on many aspects of the compliance regime; it also provides analysis on some of the more complicated points.

  -- Regarding the issue of the consequence for exceeding the target, it says that there should be one (a point disputed by some countries in the umbrella group) and that "it should be designed so as to promote the environmental effectiveness of the Protocol (for example, by restoring excess tonnes to the system)". It does not take a position on exactly what the consequence should be.

- Key policy issues that will need to be addressed during the elaboration of the compliance regime include:

– How much needs to be finished by COP 6. We have said, and the COP 4 decision seems to endorse the view, that the regime, including consequences, needs to be completed along with everything else. Our reasoning has been that we need to be able to present the Senate with the full picture. At the umbrella group meeting, some countries (including Canada) seemed to be taking the view that we do not really need to decide on all aspects of the regime, including consequences for exceeding the target; these could be decided later. Once views of more governments become clear, there may be a need to weigh in on this issue.

Make it diff. w/a penalty.

– What the consequence for exceeding the target should be. Our position at Kyoto (after our "borrowing" proposal was unsuccessful) was that the excess tons should be subtracted from the subsequent commitment period, with a penalty. That position did not make it into Article 18, partly because of the lateness and partly because of substantive objections. Various other proposals are coming in from environmental groups.

[illegible]

-- To what extent we should be seeking "parallelism" in terms of non-compliance consequences among Parties that bubble, trade, or do not use any mechanism. An example of the type of issue that arises is whether it is worse, from a non-compliance point of view, for a Party to exceed its target because it over-sold (through trading) than because it over-emitted. If we support parallelism, we need to ensure that we do not, through the rules for mechanisms, place ourselves at a disadvantage vis-à-vis, for example, the EU.

Compliance-Related Issues under the Protocol: Submission of the United States

Introduction

- The United States made an initial statement regarding compliance at COP 4 in Buenos Aires. That statement, which is attached:

  -- stressed the importance of the compliance regime for effective implementation of the Protocol;
  -- noted existing compliance-related aspects of the Protocol;
  -- identified compliance-related areas still to be elaborated; and
  -- raised various issues associated with the elaboration of such areas.

- This submission seeks to build on the previous U.S. submission. At the outset, it includes some general comments on the Protocol's compliance regime. It then includes a more specific discussion focusing on compliance aspects of Annex I targets.

General

- The Protocol's compliance regime needs to be developed with the following objectives in mind:

  -- transparency (which is likely to foster compliance, as well as Party and public confidence in the system);

  Paren needed

  -- credibility (while we should primarily be striving to create a "compliance system" rather than a "non-compliance" system, Parties and the public need to know that, in the final analysis, there will be appropriate consequences for non-compliance; further, credibility will be bolstered if it is clear that the system is depoliticized and cannot be "gamed)" and

  -- reasonable certainty (there needs to be a certain level of automaticity to the system, so that Parties know which actions/inactions will lead to which results and so that parallel infractions will be treated in a parallel manner; on the other hand, the system somehow needs to take account of particular circumstances).

- It is important to look at the various kinds of obligations under the Protocol (e.g., quantitative/qualitative, individual/collective, annual/continuous/end of commitment period) in considering how compliance by Parties should be reviewed.

- In several aspects, the substantive provisions of the Protocol need to be elaborated, e.g., mechanisms, national estimation of emissions, reporting. It will be important to think about the future compliance regime when elaborating such provisions. (For example, it does not appear that the current guidelines for Annex I communications would form a solid basis for assessing compliance.) It would be desirable, from a compliance point of view, if negotiators charged with elaborating substantive provisions would:

  -- make clear what is a rule (and therefore a requirement) and what is guidance (and therefore not a requirement for compliance purposes); and

  -- structure any rules in such a way that compliance therewith can be ascertained with reasonable ease.

- Whether through one procedure or more than one procedure, the compliance regime should incorporate not only enforcement features but also facilitative/help desk features (recognizing that compliance may in some cases be affected by the capacity of Parties, e.g., technical expertise, to meet their obligations).

- In developing the Protocol's compliance regime, it is important to bear in mind that we are not starting from scratch. For example, Article 8 already sets forth numerous aspects of a review process applicable to Annex I Parties. In addition, the Framework Convention has establishe~~d the~~ principles of transparency and public access in its procedures for reporting and review. Extension of these principles to the procedures and mechanisms under the Protocol (e.g., reports from the Parties, reports from the experts groups to the Parties, etc.) could serve as a valuable compliance tool.

- Non-compliance consequences will likely vary depending upon the nature of the obligation that has been violated, as well as the degree, frequency, etc., of the non-compliance.

- Achieving compliance with the Protocol's obligations will largely depend upon domestic enforcement by various Parties of their laws implementing the Protocol. The international non-compliance regime should create incentives for Parties to have strong domestic enforcement, but that regime will not be directly involved in domestic enforcement. This being so, it seems desirable for Annex I Parties (whether under Article 5 or 7 or elsewhere) to report on the domestic enforcement regime(s) applicable to domestic laws implementing Protocol obligations, particularly Article 3.

<u>Compliance with Quantified Emission Limitation and Reduction Commitments ("Targets")</u>

- Particular attention is warranted regarding Article 3 of the Protocol, along with Articles 5 and 7, which are the means by which an Annex I Party demonstrates its compliance with Article 3.

- In order to develop a system capable of assessing compliance with quantified emission limitation and reduction commitments ("targets"), it is instructive to identify the various pieces of the Protocol that are relevant to such an assessment.

- When the Protocol is "reverse engineered," it appears that calculating compliance with targets is akin to a mathematical formula, with emissions during the commitment period on one side of the equation and assigned amount on the other.

- The attached chart demonstrates the various components of the formula that would need to be calculated and reviewed in order to assess whether an Annex I Party had complied with its target.

- On the left side of the equation is "emissions," which refers to emissions during the commitment period. It should be noted that the Protocol is not based on actual emissions, but rather on estimation of emissions based on agreed methodologies. The way a Party demonstrates its emissions is by using the estimation methodologies under Article 5 and then reporting its results under Article 7.

- On the right side of the equation is "assigned amount." Emissions on the left side must be less than or equal to assigned amount.

- Assigned amount is calculated by beginning with original assigned amount, i.e., 5 years times ~~the~~ percentage for that Party inscribed in Annex B times its baseline emissions (Article 3.7). The baseline can vary in several ways, depending upon whether a Party is a country with an economy in transition (Article 3.5), has chosen 1995 for three gases (Article 3.8), and/or qualifies for including net land-use change (Article 3.7).

- Original assigned amount can then be modified in several ways:

  -- It might increase or decrease depending upon sink-related changes during the commitment period (Articles 3.3 and 3.4).

  -- It might increase or decrease depending upon the acquisition or transfer of joint implementation reduction units (Articles 6, 3.10, and 3.11).

  -- It might increase or decrease depending upon the acquisition or transfer of units of assigned amount under emissions trading (Articles 17, 3.10, and 3.11).

  -- It might increase depending upon the acquisition of CDM reductions (Articles 12 and 3.12).

  -- It might increase or decrease depending upon banking (increasing if tonnes have been carried over from a previous commitment period, decreasing if tonnes are being carried over into the next commitment period) (Article 3.13).

- Breaking the target formula into its component parts illustrates a number of points and raises several issues concerning the compliance regime, including with respect to substantive rules, procedure, and consequences for non-compliance.

- In terms of the substantive rules that make up the formula, many are already contained in the Protocol. At the same time, many elements of the formula are in the process of being elaborated, including, e.g., Articles 5 and 7, and the Kyoto mechanisms. It will be important, as noted above generally, to elaborate these provisions bearing in mind their relationship to the target formula and the need for compliance therewith to be ascertainable (for example, clarity as to what is mandatory versus advisory, ability of a third party to assess whether a Party has followed a particular rule).

  -- To take the example of Articles 5 and 7, ongoing confidence in the quality and accuracy of a Party's emissions estimation and reporting will be essential for assessing compliance with Article 3 obligations at the end of a commitment period. Therefore, in elaborating requirements under these Articles, Parties must consider not only the minimum technical requirements necessary to ensure a high standard of inventory quality, but also what should constitute non-compliance with the requirements and how potential non-compliance could be identified (e.g., what indicators might be found through the review process). Finally, Parties should consider at what level failure to comply with measurement and reporting requirements begins to undermine confidence in emission estimates and, therefore, hinders the verification of targets.

- In terms of designing the procedure to assess compliance with the various elements of the target formula, several points are worth noting:

  -- As noted above, the Protocol (Article 8) already includes a role for expert review teams in reviewing implementation of commitments by Annex I Parties. Article 8 authorizes expert review teams to conduct a thorough review of implementation and to flag implementation questions. Based on our experience under the Convention, key questions are how to improve the process generally and, more specifically, how to tailor it so that it can effectively apply to review of legally binding targets. In this regard, the process should provide more focus on development and reporting of greenhouse gas inventories. The process would also be improved by establishing an automatic link to a follow-up procedure in cases where a review team has identified a question of implementation.

  -- Given that the expert review process does not extend to determining or responding to non-compliance, there will be a need for an additional procedure(s). Numerous issues arise in connection with devising such a procedure:

There is the question of whether the procedure should automatically review each Party's compliance with its target obligation after each commitment period or, alternatively, whether compliance should only be reviewed when an issue has been affirmatively raised. It appears that the non-compliance procedure does not need to automatically review each Party's compliance with its target obligation. If there is confidence in a Party's measurement and reporting (which should be known from annual review of inventories and assigned amounts, as well as from period in-depth reviews), then the only step that is required at the end of a commitment period is a final evaluation to ascertain whether total reported emissions are less than or equal to adjusted assigned amount. This evaluation should occur automatically as part of the annual review and accounting of assigned amounts by expert review teams. Referral to a further procedure would only appear to be necessary if there is no confidence in a Party's measurement and reporting or if (taking into account the need for a true-up period) the annual review indicates that emissions exceed assigned amount.

There is also the question of the composition/expertise of any review body. As illustrated above through the target formula, compliance issues might arise under any one of the formula elements. On the left side of the equation, for example, there might be an issue concerning whether a Party has followed required methodologies; on the right side, for example, there might be issues concerning a Party's baseline emissions or whether the rules for a particular mechanism have been followed. The procedure needs to be capable of reviewing all such issues.

The nature of the procedure also needs to be related to the consequences that might flow from a determination of non-compliance. The more "binding" and serious the potential consequences, the more the procedure must afford "due process."

- In terms of consequences for non-compliance, again several points are worth considering:

  -- The target formula appears to contain three different kinds of elements: (1) elements that are obligations in and of themselves (such as reporting under Article 7); (2) elements that are pure accounting elements and not actually written as obligations themselves (such as sinks under Article 3.3); and (3) elements that are accounting elements but derive from mechanisms governed by obligations (such as CDM, trading, etc.). Non-compliance with an element that is an obligation might carry its own consequence (such as related to joint implementation under Article 6.1.c), apart from any consequence that such non-compliance may play regarding compliance with the target obligation. Concerning pure accounting elements, there cannot, strictly speaking, be "non-compliance" with such an element; rather, the value of that element in the formula would simply be adjusted to make it accurate (unless, perhaps, inaccuracies were chronic). Regarding elements that derive from mechanisms governed by obligations, to the extent a problem with such an element were due to non-compliance with the rules governing the related mechanism, additional consequences, beyond appropriate adjustment of the element's value, might be appropriate.

-- Different kinds of inconsistencies could merit different treatment:

Some kinds of inconsistencies (such as being a day late in reporting or a minor technical error of no substantive import) should be considered de minimus, not rising to the level of a Protocol violation.

Some kinds of inconsistencies, while not strictly speaking de minimus (such as missing a category of emissions in a communication), might be easily cured if the Party is given the opportunity.

At the other end of the spectrum, some kinds of inconsistencies (such as an inconsistency with Article 5 or 7 not in either of the above categories) might constitute a violation of that Article and/or non-fulfilment of a participation requirement for a Kyoto mechanism (see, e.g., Article 6.1.c on JI); it might also contribute to a violation of the target obligation.

-- Meeting targets will be an inexact science, in the sense that no Party's emissions will exactly equal its assigned amount. In this regard, we should build in a short grace period after the end of the commitment period (the analogue of a domestic "true-up" period), during which Parties can seek to remove overages.

-- Consequences for exceeding the target after an assumed true-up period should be designed so as to promote the environmental effectiveness of the Protocol (for example, by restoring excess tonnes to the system

CALCULATING COMPLIANCE WITH ASSIGNED AMOUNTS (TARGETS)*

ACTUAL EMISSIONS must be < or = to **ASSIGNED AMOUNT**

ACTUAL EMISSIONS: demonstrated through measurement of emissions (Art.5) and reporting (Art.7)

ASSIGNED AMOUNT: [5 x % in Annex B x baseline emissions] ± sinks during commitment period (Art.3.3 + 3.4) ± JI units (Art.6) ± trading units (Art.17) + CDM reductions (Art.12) ± banking (Art.3.13)

baseline emissions: based on 1990 (Art.3.7) or other year for EITs (Art.3.5/3.6) or 1995 for 3 gases (Art.3.8) + 1990 net LUC if qualify under Art 3.7

banking: + (if from previous period); − (if carrying over into next period)

* The formula would apply to Parties using an Article 4 bubble as a group unless the group exceeded its aggregate assigned amount.

TO: Jeff Frankel/
Lisa Blanc

United States Department of State

Bureau of Oceans and International Environmental and Scientific Affairs

Washington, D.C. 20520

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FROM: State/L/OES – Sue Biniaz

RE: Compliance Papers for 2/22 Meeting

To: Jeff Frankel
From: Raymond Guiteras
Subject: Emissions Targets

4 January 1999

The evidence supports the contention that the emissions targets agreed to by Annex B countries are progressive, since a negative relationship is observed between the sacrifices embodied in their CO_2 targets and income.

A country's emissions target was calculated from emissions in the base year (1990 for most countries) times the cap agreed to under Annex B of the Kyoto Protocol. The target was then expressed relative to the country's 2010 business-as-usual projection in the following manner:

target-BAU = log (target / 2010 BAU projection)

A larger number, then, implies a more lenient target. This is the variable to be explained.

A linear relationship was estimated between this target and four explanatory variables: 1995 GDP per capita (also in log form), the projected growth in CO_2 emissions from 1990 to 2010, and coal's share of energy (a high share of energy from coal could imply that a country has opportunities for low-cost abatement). A dummy variable was included to allow for the possibility of different treatment of economies in transition (EITs). The following results were found:

target-BAU	Coef.	Std. Err.	t	P>|t|
EIT dummy	-.242164	.0939599	-2.577	0.016
GDP/cap	-.1710944	.0326079	-5.247	0.000
projected e growth	-.6389962	.1301372	-4.910	0.000
coal share	-.0010662	.0009207	-1.158	0.258
constant	1.549491	.3203128	4.837	0.000

Adjusted R-squared = 0.8555

According to this estimation, emissions targets are indeed progressive, since a negative relationship between GDP and the target (relative to 2010 BAU) is observed. Holding constant for the other variables, each 1 percent increase in per capita income implies an estimated .17 percent greater sacrifice Furthermore, countries are asked to "give back" almost half (1-.63 = 37 per cent) of any projected growth in CO_2 emissions between now and the budget period. Interestingly. Economies in Transition are given more stringent targets, holding other variables constant; this presumably reflects an agreement to limit the amount of "hot air" that they would otherwise be allowed.

Note on data: Data on emissions and BAU projections were gathered from various sources. EIA data were used for EU nations, the US, Canada, and Japan; national data were used for Bulgaria, Hungary, Poland and Romania; and EPA data were used for other nations. Data on GDP per capita and coal's share of energy were taken from the EIA and national communications.

ESTIMATING THE NON-ENVIRONMENTAL CONSEQUENCES OF GREENHOUSE GAS REDUCTIONS IS HARDER THAN YOU THINK

by

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September 6, 1998

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ABSTRACT

Top-down and bottom-up models of the non-environmental consequences of policies to reduce greenhouse gas emissions embody different implicit theories of economic organizations. Yet neither approach is explicit in showing the detailed computations that must be traced if the activities of firms are to be described realistically. Specification of firms' computational processes leads to inevitably to a consideration of potential *computational limits* on the behavior of organizations. It is known that solutions of some standard economic problems are *not effectively computable*, and that the solutions to others are *computationally intractable*. These fundamental computational limits have strong implications for the theory of the firm, and recognizing their existence and importance suggests new policy approaches for reducing greenhouse gas emissions.

(JEL C6, D2, L2, Q4)

ESTIMATING THE ECONOMIC CONSEQUENCES OF GREENHOUSE GAS REDUCTIONS IS HARDER THAN YOU THINK

I. Background: The Problem.

As governments, economists, and citizens struggle with the variety of difficult issues surrounding the Kyoto Protocol, the problem of estimating the non-environmental consequences[1] of meaningful greenhouse-gas reductions remains unresolved. Leaving aside the predictably pessimistic estimates produced for political purposes on behalf of segments of the fossil fuel industry (for example, see Novak 1998),[2] top-down modelers have begun generating runs that purport to show what the effects of implementation of the Kyoto provisions might be (see, for example, Energy Modeling Forum 1998). At the same time, new bottom-up studies have attempted to answer the same question with different methodology---examining the technologically attainable emissions reductions that appear profitable at current fuel prices or that would be profitable given small increases in such prices.[3] The gap between the two approaches has not been bridged. The top-down studies typically show multibillion dollar GDP reductions (though small in percentage

1. Use of this term, rather than the more conventional "economic costs," is deliberate. Environmental conditions (in particular, the state of the climate) enter directly into individuals' utility functions, and as such are an integral part of the material standard of living. An artificial division between "economic" and "environmental" effects can be misleading, as when environmental externalities are not taken into account in Ramsey-type infinite-horizon optimization models (Brock 1973; Tol 1994; Bovenberg and Smulders 1996; Amano 1997; Howarth forthcoming). With this caveat in mind, it is still useful to ask what the effects of particular greenhouse-gas reduction policies would be on standard measures of output such as GDP.

2. As might be expected, these studies tend to be presented in the form of Congressional testimony rather than as papers published in the peer-reviewed literature.

3. These studies have been reviewed and summarized by the Union of Concerned Scientists and the Tellus Institute (1998). The most comprehensive recent "bottom-up" analysis for the United States is the "5-lab study" conducted by five of the national laboratories (Interlaboratory Working Group 1997). See also the special issue of *Energy Policy* edited by Bernow, Duckworth and DeCicco (1998).

terms) accompanying GHG reductions of the magnitude called for by Kyoto, while the bottom-up studies suggest that very substantial reductions in emissions approaching or even exceeding the Kyoto target could be achieved at close to zero cost (or even at a gain to the economy).[4]

Each approach has problems that are well known. It should be noted first that the framing of climate policy largely in terms of the economic costs and benefits is somewhat an artifact of an "economism" that permeates the debate. It is certainly possible to imagine other powerful justifications for climate protection. *Ethical principles* might be invoked (versions of "sustainable development" requiring that future generations inherit a natural world no less environmentally benign than it is at present, for example).[5] Once it is recognized that risk management is at the center of the climate protection problem, even the assignment of rough monetary values (and probabilities) to the various possible outcomes is quite difficult. There is no way to recover meaningful market-based "willingness to pay" data for planetary risks that fall outside historical experience, let alone outside the narrow range of marginal changes for which current prices can be used to measure marginal utilities. Given the opportunity to make an informed choice, many

4. There are exceptions to these generalizations. The meta-analysis by Repetto and Austin (1997) shows that the models they surveyed would give positive GDP effects if five key assumptions were "best case": (1) non-carbon backstop fuel available, (2) efficient economic responses, (3) increased energy and product substitution, (4) joint implementation, and (5) revenues recycled efficiently. The Council of Economic Advisers (1998) also recently released a top-down analysis (based on the Batelle Laboratory's Second Generation Model) showing very low costs. Most of the emissions reductions in the CEA's analysis were achieved through international trading of emissions allowances with the countries of the former Soviet Union, and with developing countries under yet-to-be-worked-out provisions of the Kyoto Protocol's Clean Development Mechanism.

5. Social policy in many areas is driven by considerations other than cost-benefit calculus. Even where the only issues would seem to be economic ones, there is no guarantee that policy is efficient. For example, federal research policy can hardly be optimal if the social return to R&D exceeds 40% per annum (See Mansfield (1991) and the other references contained in DeCanio (1997)).

(most?) people would be highly reluctant to conduct a one-time, irreversible experiment with the planet which, if the outcome is unfavorable, will have disastrous consequences for their children and grandchildren.

Even granting the premise that it is the dollar costs and benefits that matter most, the gap between the results of the top-down and bottom-up methodologies is very troubling. Such intellectual gridlock can be symptomatic of some sort of extra-scientific agenda, as might be the case if the modelers' differences were being driven by politics or ideology. No doubt politics and ideology do permeate the climate debate, but there is another possibility. The divergence of the estimates may signal a deeper scientific problem that confounds efforts to estimate the economic effects of climate policies. A persistent set of anomalies of this type may be a signal that "normal science" (Kuhn's (1962) term) is not adequate to make progress any longer.

Of course, the top-down studies are known to be quite sensitive to their specifications and assumptions (Repetto and Austin 1997). The ordinary course of research should lead to a narrowing of the range among such estimates, because some of the differences in the models' outcomes can be traced to differences that are, ultimately, empirical. For example, a recent computable general equilibrium (CGE) model with endogenous technological progress shows that a carbon tax that stimulates R&D in energy efficiency can have a positive or negative effect on conventionally measured GDP, depending on whether the supply of scientists and engineers is elastic or inelastic (Goulder and Schneider 1996).[6] At least in principle, it should be possible to estimate the elasticity

6. If the supply is inelastic, the energy-efficiency R&D can "crowd out" other, potentially more profitable, R&D efforts. If the supply is elastic, the energy-efficiency R&D will benefit the economy because of its high returns and spillover productivity benefits, even if other R&D might have had an even greater return.

of this supply curve. There is no question that this sort of normal science will proceed and that it is likely to have a positive payoff, at least by reducing the credibility of the overly pessimistic estimates of non-environmental costs.

The top-down models embody more than particular sets of parameter estimates, however. They all have built-in *a particular theory of economic organizations*. The models uniformly are based on an underlying structural commitment to the "optimization assumption," in which economic agents (whether firms or individuals) successfully maximize their objective functions subject to constraints. In such a framework, the increased provision of any desirable good (such as environmental quality) can only be purchased at the expense of other desirable goods (such as ordinary economic output).

On the other hand, the bottom-up studies also entail a theory of the firm. Even if it is not spelled out, these models require some explanation for why firms do not take advantage of the profitable energy-saving investments available to them. There is a large literature on the "barriers" that allegedly prevent firms from adopting technological innovations that would increase their shareholder value, but this literature does not present a unified account of the origin and persistence of those barriers. Specific examples of barriers are easy to point out (Lovins and Lovins 1997), but bottom-up analysts are hard-pressed to explain why alert managers would not soon overcome them. In fact, the EPA's successful voluntary pollution prevention programs (such as Green Lights and Energy Star) have been built on merely pointing out such possibilities to firms and urging them to carry out the profitable investment projects.

A variety of explanations have been put forth to explain this "efficiency paradox." A large segment of this literature is focused on energy-saving investments, but that is not the only area in which questions could be asked about why profit opportunities are passed up. The answers that have been given in the literature are many and varied, including principal-agent problems, split or dysfunctional incentives, managerial myopia, and institutional constraints, but there has been no unification or definitive testing of the alternative explanations.[7]

Yet by thinking about the firm in a different way, it is possible to see that the efficiency paradox may not be a paradox at all, but instead a manifestation of the normal state of affairs. Suppose that full optimization by firms and other organizations is not automatic and not just difficult, but *impossible in principle*. Then it would not be surprising that profit opportunities are sometimes (or even frequently) missed. Market pressures might screen out the worst performers and the rewards of profitability would still provide positive incentives for managers, but it would no longer be a safe theoretical presumption that the firm's behavior can be modeled by complete profit maximization. The extent to which observed behavior matched the optimization ideal would be an empirical question---if indeed it were even possible for any outside analyst to determine what the optimum might be.

Economists since Simon have understood that there are limitations on the kinds of calculations firms (and other economic agents) can carry out.[8] "Bounded rationality" is a

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7. See Koomey (1990), Lovins and Lovins (1991), Ayers (1993), Jaffe and Stavins (1993), DeCanio (1993), and the special October 1994 issue of *Energy Policy* edited by Huntington et al. for an introduction to this literature.

8. A fine collection of papers on this subject by Simon and his collaborators is Simon (1979).

shorthand for describing these limits.[9] Even before Simon's work in the 1950s, the "socialist calculation" debate that began in the 1920s and 30s had raised the question of the informational and computational requirements for arriving at an efficient allocation of resources economy-wide. This debate still is alive (Adaman and Devine 1996). However, there is nothing intrinsic in the Austrian "impossibility" argument that limits it to economy-wide calculations. Although it is certainly true that there are no successful examples of centrally planned economies achieving anything close to allocative efficiency over the long run,[10] the large private-sector firm exhibits all the features of "localized" knowledge and difficulty of coordination that the Austrians identified as preventing efficient central planning.[11]

Both before and after Simon's initial papers, developments in the formal theory of algorithmic computation delineated bounds on what can theoretically and practically be

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9. See Conlisk (1996) for a recent survey of bounded rationality and its literature.

10. An interesting potential short-term counterexample is the allocation of resources during wartime. In the United States at the peak of World War II, for example, roughly 40% of the economy's output was devoted to the war effort (U.S. Bureau of the Census 1975). The "efficiency" of the allocation of resources is difficult to gauge, but the economy was certainly productive---it overwhelmed the Axis powers in military production---and it must be remembered that the United States was still recovering from the Great Depression when rearmament began. Even inefficiently allocated jobs were likely to have been preferable to double-digit unemployment. See Rockoff (1984) on WWII price controls and the efficiency of the wartime American economy, and Lee and Passell (1979) on unemployment during the 1930s.

11. As Adaman and Devine (1996) put it, "[t]he earlier models...do not address intra-firm organisational problems, since firms are treated as 'black boxes'. Once input is supplied output is obtained, with no consideration of 'shirking' or the costs of monitoring and contract enforcement. More recent models seek to overcome this naïve conceptualisation by recognising that shirking may be partially overcome by supervision and that the 'principal-agent' problems that arise when firms are run by a managerial body rather than by their beneficial owner(s) may be partially dealt with by monitoring and incentive schemes. It is important to note, however, that these problems are structurally the same whether the owner is the Planning Bureau in a market socialist system or the stockholders in a corporate capitalist system" (p. 525, references omitted).

accomplished by step-by-step calculation.[12] Some of these limitations are independent of any particular hardware or software implementations (brains, computers, bureaucracies), while in other cases, the time and cost that would be required to carry out the calculations required for optimization are prohibitive given existing equipment. It is not controversial that individuals and organizations employ rules of thumb, heuristics, and approximation methods to carry out their daily tasks. What has been less clear is that theories acknowledging the limits to computational capabilities would "feel" different from theories based on full maximization. If models of firms, other organizations, and economic agents were required *as a theoretical prerequisite* to carry out their calculations algorithmically, the economic significance of the limits to algorithmic computation would have to be addressed.

Economists have to transcend two deeply ingrained habits of mind in order to appreciate the importance of computational considerations. First, because of the nature of our professional training, there is a tendency to think of the domain of economics as consisting exclusively of those problems that can be solved using the mathematical techniques that form the core of the graduate school curriculum. There is no doubt that these techniques shed light on many interesting issues (and that solving some of the puzzles that can be posed within their framework requires a great deal of ingenuity and creativity), but it is also should be recognized that the tasks faced by real-world economic agents and organizations are not all reducible to standard optimization problems that can be solved in closed form. Second, there is a long tradition in economics that regardless of

12. Simon seems to have arrived at bounded rationality as a generalization from common sense and empirical research in psychology rather than through reliance on the results of the formal mathematical theory of algorithmic computation.

what firms or agents actually do, it suffices for theoretical purposes to pretend that they act "as if" they are optimizing an objective function.[13] Again, this fiction may be usefully applied in some circumstances, but it is far from innocuous. It is in difficult cases such as the estimation of the macroeconomic costs of greenhouse gas reduction measures that it becomes of paramount importance to look inside the "black box" and see how the key organizations (the business firms) really do function. A necessary first step is to examine the implications for economic modeling of the algorithmic nature of actual economic calculations.

II. Computational Limits in Economic Models.

Algorithmic computation is subject to two potential limitations. The first is the possibility that a particular problem is not *effectively computable*. If a problem falls into this class, there is no algorithm at all that can solve it within the mathematical system expressing the model. The second possibility is that any algorithm capable of solving the problem may take too long to be of practical use. This can happen either because the problem happens to be too big for existing hardware and software (including human brains) or, alternatively, because the time and resources required to solve the problem

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13. This practice has been controversial for a long time. Early in the 1950s Friedman wrote: "Under a wide range of circumstances, individual firms behave *as if* they were seeking rationally to maximize their expected returns and had full knowledge of the data needed to succeed in this attempt" (1953, p. 21). In a symposium on economic methodology published in the *American Economic Review* in 1963, the philosopher of science Ernest Nagel noted that "[Friedman] freely admits that as a rule businessmen lack such knowledge and do not perform the intricate calculations required for ascertaining the indicated maximum. Indeed, he declares that 'the apparent immediate determinants of business behavior' could be anything at all; e.g., ingrained habit or a chance influence. He nevertheless claims that these admitted facts do not affect the validity of the hypothesis" (p. 217). In the "Discussion" section of the same symposium, Simon urges economists to discard the false 'as if' assumption and "make the observations necessary to discover and test true propositions....[t]hen let us construct a new market theory on these firmer foundations" (p. 230).

grow very rapidly as a function of the size of the problem. Modern computer science has developed operational definitions of terms like "size," "too big," and "grow very rapidly," but the point is not that economics can be reduced to a sub-field of computer science. Instead, what needs to be recognized is that "bounded rationality" can be quantified, and that limits on what firms can calculate may have an influence on their ability to optimize and other aspects of their real-world behavior.

A. Some Problems in Economics Are Not Effectively Computable.

The theory of *effective computability* or *recursive function theory* emerged in the 1930s in an effort to characterize rigorously what was meant by the intuitive concept of an "algorithm." Perhaps the most famous model of computation is that proposed by Alan Turing in 1936, which was based on an analysis of how a human being could carry out computations mechanically using pencil and paper. Over the succeeding years, a number of proposals have been made for alternative mathematical formalizations of the idea of computation (Cutland 1980). All of the models of effective computability that have been proposed to date have given rise to the same class of effectively computable functions. This unexpected and remarkable result has led to widespread belief in the *Church-Turing thesis* (sometimes called Church's thesis) that "[t]he intuitively and informally defined class of effectively computable...functions coincides exactly with the class of [computable functions that have been modeled formally]" (Cutland 1980). The Church-Turing thesis is a conjecture, not a theorem, although the evidence for its truth is very strong. Copeland (1997) summarizes this evidence as follows:

> (1) Every effectively calculable function that has been investigated...has turned out to be computable by Turing machine. (2) All known methods

> or operations for obtaining new effectively calculable functions from given effectively calculable functions are paralleled by methods for constructing new Turing machines from given Turing machines. (3) All attempts to give an exact analysis of the intuitive notion of an effectively calculable function have turned out to be equivalent in the sense that each analysis offered has been proved to pick out the same class of functions, namely those that are computable by Turing machine. Because of the diversity of the various analyses the latter is generally considered strong evidence.

Acknowledging that there are deep issues involved,[14] standard practice in computability theory is to proceed on the assumption that the Church-Turing thesis is true, and to presume that results derived for Turing machines (or one of the other equivalent models of computation) apply to any realistic model of computation.

The shattering discovery that was made at around the same time that the notion of effective computability was being developed is that there are functions and numbers that are *not* effectively computable, as well as mathematical problems that are *undecidable*. These impossibility results, which include Gödel's incompleteness theorem (1931; see Nagel and Newman 1958) and Turing's demonstration of the existence of non-computable numbers (1936; see Chaitin 1993), demonstrate intrinsic limitations to algorithmic computational procedures. These limitations have implications for economics if one insists that specification of the *method of computation* is a necessary component of the modeling of economic agents and organizations.

To be sure, impossibility results of this type can only be properties of mathematical *models* of economic activity. But that does not weaken the strength of the argument or

14. It is not known whether "[w]hatever can be calculated by a machine (working on finite data in accordance with a finite program of instructions) is Turing-machine-computable" (ibid.) As a recent textbook on computability puts it, "...since the word *algorithm* has no general definition separated from a particular language, Church's thesis cannot be proved as a mathematical theorem" (Davis, Sigal, and Weyuker 1994, p. 69).

allow these results to be disregarded. Models are scientifically indispensable, and specification of the algorithmic processes by which economic actors arrive at their decisions is neither a more nor less stringent requirement than what is entailed in other model representations of economic behavior (such as constrained optimization). The standard neoclassical representation of the firm, with profits dependent on a "production function" transforming input and output quantities and prices into net income is, if anything, a more sweeping abstraction. If insistence that models embody computational routines describing what firms actually must do to carry out their tasks leads to problems whose solutions are not effectively computable or that are intractably complex, it should be a signal that these limits must be respected in our efforts to model economic behavior realistically.

Limits on effective computability are known to exist in economic models. Spear (1989) has shown that in learning environments in which the agents do not have perfect information about the state of nature, "a version of Gödel's incompleteness theorem applicable to the theory of computable functions yields the conclusion that rational expectations equilibria cannot be learned....When agents have incomplete information (i.e. an incomplete signal about the current state), [learning rational expectations is not possible] since agents are required to infer not a function, but a correspondence" (p. 889, 892).[15] Now, it is obvious that any firm wishing to make investment decisions based on the net present value criterion (or any other forward-looking rule) will have to form

15. Board (1994) derives a similar result for agents whose computational resources are polynomially bounded in the relevant parameters. He finds that "it is unlikely that agents can learn even those price functions and economies computable by finite state automata [a class of computing devices less sophisticated than Turing machines]. This result casts doubt on the ability of agents to form rational expectations in complex economies" (p. 246).

expectations about future prices. In the energy-efficiency case, the present cost of the investment has to be weighed against the discounted value of the future energy savings, which depend on future sales, energy prices and discount rates. The actual course of prices over time depends on the actions taken by the other agents in the economy and its future states, hence learning the "rational expectations" needed to make optimal decisions about investment in energy efficient technologies is an instance in which Spear's result applies because it is impossible for firms to have full knowledge of all the factors that will influence prices in the future.

Others have arrived at similar results. Rust (1997) examines literature showing that a number of standard economic problems[16] are not effectively computable. For example, Rabin (1957) showed that there are games whose equilibrium strategies are not effectively computable, and a similar result was established by Binmore (1987). As Rust remarks, "[i]t isn't clear whether these results [of Rabin and Binmore] have anything to say about the narrower and more relevant issue of whether rational behavior is possible in *practical contexts* since they rely on non-constructive arguments to establish the existence of games whose equilibria are not effectively computable. The relevant question is whether equilibria are effectively computable *for the games economic agents actually play*" (p. 6, emphasis in the original). However, he goes on to cite unpublished work by Nachbar (1993) and a series of papers by Lewis (1985[a,b], 1986, 1992[a,b]) showing that

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16. Rust makes the distinction between computations within economic models (such as calculation of the equilibrium of a CGE model) and the computations that are (explicitly or implicitly) carried out by economic agents or organizations. While computations of the first type may exhibit computability or complexity problems, the focus here is on the latter type.

the solutions of standard models in game theory and general equilibrium are indeed not effectively computable.

Nachbar's paper with Zame (1996) proves that "for a large class of discounted repeated games (including the repeated Prisoner's Dilemma) there exist strategies implementable by a Turing machine for which *no* best response is implementable by a Turing machine....in general, there will exist computable strategies admitting no computable best responses, and the problem of choosing a best response (even when computable best responses do exist) does not have a computable solution" (pp. 103-104). Nachbar and Zame point out that their results "apply only to *exact* best responses, and not to *approximate* best responses....Thus, the computability problems we identify do not necessarily have important consequences for *payoffs*. However, these computability problems do have important consequences for *paths of play*, because the path of play specified by an ε-equilibrium may be very different from that specified by an exact equilibrium. In a sense, strategies implementable by Turing machines are intrinsically vulnerable to certain types of 'trembles' which can cause observed play to depart substantially from what one would predict if players trully [sic] optimized" (p. 105).

Lewis makes an even stronger assertion in his paper showing that demand correspondences are not effectively computable (or "computationally viable"):

> "It is obvious that any choice function...that is not *at least computationally viable is* in a very strong sense *economically irrational*. Unless the computation is trivial, i.e. involves no computation at all, and thus is both complete and accurate without the use of any Turing machine, the choices prescribed by a *computationally non-viable* choice function can only be implemented by computational procedures that do one of two things: either (a) the computation does not halt and fails to converge, or (b) the computation halts at a non-optimal choice. In case (a) the costs of computation procedures that do not converge must surely prohibit their

> use, and in case (b) whatever purpose there can be to a mathematical theory of representable choice functions is surely contradicted in such circumstances" (1985, pp. 45-46).

In a later paper (1992b), Lewis shows a similar failure of Walrasian equilibrium and *N*-person non-cooperative games to be effectively computable. These papers are only the tip of the iceberg; Velupillai (1997) in "an attempt...to resurrect the pioneering work of Michael Rabin" cites a "growing literature on applying recursion theoretic concepts to traditional issues in game theory" and arrives at "the melancholy demonstration that very simple effectivized games, even when determined and playable, can contain intrinsic undecidabilities and intractable complexities" (p. 957). He give examples ranging from a form of the parlor game NIM to a standard barriers-to-entry problem in industrial organization.[17]

Problems that are not effectively computable are, as far as we know, beyond the reach of any physical device or human organization to solve, regardless of the resources available. The existence of such problems at the heart of economic theory ought to give one pause; the examples cited above are not artificially constructed perverse cases, but (as with rational expectations, Walrasian general equilibrium, etc.) are very much part of the working equipment of policy modelers.[18] We will see next that even where effective computability is not an issue because finite algorithms capable of solving the problems are known, the *complexity* of the calculations may still provide a significant obstacle to the achievement of optimized outcomes.

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17. It should be noted that Velupillai argues that the consequences of computability constraints are not as severe as suggested by Spear (1989) or Lewis (1985).

18. Barrow (1998) has a very interesting discussion of whether Gödel-type undecidability results have implications for theoretical physics. There does not appear to be a consensus.

B. Computational Complexity in Economics.

Even when it is *possible* to compute the answer to a problem, it may not be *practical*. Computational complexity has to do with the failure of algorithmic methods to reach a solution within a reasonable time (or memory space) bound. Standard introductions to computational complexity theory[19] include Garey and Johnson (1979) or Papadimitriou (1995), but an outline of some of the basic concepts may be useful for those not already familiar with them. The notion of the *time complexity* of an algorithm is grounded in the nuts and bolts of real-world computation. The time complexity of a problem depends on the number of steps required by an algorithm to carry out the computation as a function of the "size" of the problem. The size of a problem is some measure of its dimensionality, such as the number of people making up an organization or the number of parameters needed to fully describe an optimization problem. If the number of steps needed to solve the problem grows exponentially (or faster) as a function of the size of the problem, then the computational burden will eventually overwhelm any physical computer. This property of exponential growth should be quite familiar to economists, because it is akin to the unbounded explosive growth of compound interest.

If an algorithm exists that can always solve a problem in a number of steps that is a *polynomial* function of the size of the problem, the problem is considered to be "tractable" and belongs to the class **P** of problems that "can be solved in polynomial time." For

19. "Complexity" as used here is distinct from the notion of complexity that is often associated with the Santa Fe Institute. For example, Durlauf (1997) in "What Should Policymakers Know About Economic Complexity?" gives the definition that "a system is said to be complex when it exhibits some type of order as a result of the interactions of many heterogeneous objects" (p. 1). The line of inquiry Durlauf refers to

example, linear programming is known to be in **P** (although this wasn't proven until 1979---see Garey and Johnson's 1991 update of their 1979 introductory text). Of course, some problems in **P** might still be beyond the reach of existing hardware and software, but existence of a polynomial time algorithm is reassuring, if nothing else. However, there are a large number of problems for which no guaranteed polynomial time solution algorithm is known. An example is the famous "traveling salesman problem" (TSP) in which one wants to know whether there is a path of length less than B that enables the salesman to visit each of n cities exactly once, returning at the end to the starting point. (The distances between each pair of cities are known.) In the case of the TSP, it is easy to check whether any proposed tour meets the requirement; one only needs to add up the distances between the successive cities on the proposed route and see if the total is less than B. The difficulty of the TSP arises from the very large number of potential tours (which, if the starting city is given, equals the number of permutations of the other $(n - 1)$ cities taken $(n - 1)$ at a time). Problems for which it is possible to *check* proposed solutions in polynomial time are described as belonging to the class **NP**.

A major open question in theoretical computer science is whether **P** is a *proper subset* of **NP**, that is, whether $\mathbf{P} \subset \mathbf{NP}$. Quite a number of problems are known to belong to **NP** that have no known polynomial-time algorithm capable of solving them. In addition to the Traveling Salesperson Problem, these include the Knapsack Problem (in which each of a finite set of objects has a "size" and a "value," and the problem is to pick a subset of the objects whose total size is less than a given bound but whose total value is

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often builds on the existence of positive feedbacks and/or increasing returns. For a survey of "complexity theory" (defined as "the study of non-linear dynamic systems") applied to organizations, see Levy (1998).

greater than a target value), as well as a large number of problems in graph theory, network design, set partitioning, and computer programming. The "hardest" **NP** problems form an equivalence class[20] known as the "**NP**-complete" problems. As a consequence of the **NP**-complete problems forming an equivalence class, if a polynomial time algorithm capable of solving *any one* of the problems known to be **NP**-complete could be found, it would follow that *all* the **NP**-complete problems could also be solved in polynomial time. The inability of mathematicians to find a polynomial time algorithmic solution to even a single one of these problems provides support for the conjecture that **NP** includes many problems harder (in the sense of computational complexity) than those in **P**. This conjecture is so widely believed that papers dealing with complexity issues routinely prove their results contingent on the presumption that $\mathbf{P} \subset \mathbf{NP}$ is true.

NP-completeness should not be thought of as the only possible measure of computational complexity, however. On the one hand, a problem may require more than polynomial time to solve only in worst-case situations; it may be that in the "average" case or most cases, a known algorithm can solve the problem in polynomial time. Alternatively, good approximations to some **NP**-complete problems (including the TSP) can often be found in polynomial time. It is also the case that some **NP**-complete problems are hard to solve only for a range of parameters characterizing the problems. These problems exhibit "phase transitions" as the parameter(s) move from values for which the problems are easy to solve to values for which they are hard (Cheeseman, Kanefsky, and Taylor 1991; Hayes 1997). On the other hand, there are complexity classes

20. That is, it can be shown that any **NP**-complete problem can be transformed into each other **NP**-complete problem by a polynomial-time procedure.

encompassing problems that are harder than those that are **NP**-complete. Indeed, there is an entire complexity class hierarchy in which the **NP**-complete problems are near the bottom. It is an open question whether this hierarchy collapses into a small number of equivalence classes. "In the absence of a proof that **P** ≠ **NP**, there is no hope of proving that the polynomial 'hierarchy' is indeed a hierarchy of classes each properly containing the next (although once again, we strongly believe that it is)" (Papadimitriou 1995, p. 427).

A number of fundamental economic problems have been shown to belong to the computationally intractable classes. Finding the core, the kernel, the nucleolus, the ϵ-core, and the bargaining set for games that are not convex are all **NP**-complete problems (Deng and Papadimitriou 1994).[21] Blondel and Tsitsiklis (1997) have shown that several basic linear control design problems are **NP**-hard, and Papadimitriou has proved that a classical type of decision problem ("games against nature") is in the complexity category **PSPACE**-complete, which is higher in the complexity hierarchy than the **NP**-complete class. These results are important at more than one level, because as Papadimitriou (1995) puts it,

> ....complexity often tells us much more about a problem, than just how hard it is to solve. Sometimes it is more useful to look at a complexity result as an ***allegory*** about how ***conceptually difficult*** the underlying application area may be. After all, if algorithms are often the direct product of mathematical structure, computational complexity must be the manifestation of lack of structure, or mathematical nastiness. From this point of view...playing two-person games is more complex than solving optimization problems, counting combinatorial structures and computing the permanent of a matrix is somewhere in between, decision-making under

21. "Non-convex" games in Deng and Papadimitriou's representation include economic situations exhibiting negative externalities, for example. Of course, negative externalities create other theoretical problems in addition to computational complexity.

> uncertainty and interactive protocols both are as powerful as games, while succinct input representations make things even harder (p. 409, emphasis in the original).

It should be noted that complexity is not limited to discrete problems---Papadimitriou and Tsitsiklis (1986) have demonstrated that both the discrete and continuous versions of certain problems in decentralized decision-making (the team decision problem is their particular example) are computationally intractable (see also Ko (1991)). Nor are the formal computer science complexity classes the only way of describing the difficulty of economic problems. Page (1996) has recently proposed two measures of difficulty that distinguish between the difficulty of solving a problem in parallel and in sequence, with applications to the theory of organizational structures.

Rust's (1997) overview cited earlier also summarizes work showing that a large number of economic problems are intractable in the sense of being subject to the *curse of dimensionality*. Citing Traub, Wasilkowski, and Wozniakowski (1988), Rust lists multivariate integration, optimization, finding zeros of nonlinear functions, finding Brouwer and Kakutani fixed points, solution of partial differential equations and Fredholm integral equations, and the problem of finding fixed points to certain classes of contraction mappings (which includes the problem of solving infinite horizon continuous-state dynamic programming problems) as all being subject to worst-case complexity problems. He states:

> It appears to be a relatively straightforward exercise to translate these general complexity bounds into corresponding complexity bounds for economic problems: utility maximization requires solution of constrained optimization problems, maximizing expected utility requires calculation of multivariate integrals, finding Walrasian equilibria requires calculations of zeros to an aggregate excess demand function or finding a Brouwer fixed point, calculation of rational expectations equilibria involve solutions to

> Fredholm integral equations, computation of option values involve solutions of PDE's, solutions to infinite horizon dynamic programming problems involve computation of contraction fixed points, and calculation of Nash equilibria reduce to the calculation of a fixed point to a correspondence. However to date formal complexity bounds have been established only for a relatively small number of economic problems such as social planning (Friedman and Oren, 1995), Walrasian equilibrium (Papadimitriou, 1994) and dynamic programming (Chow and Tsitsiklis, 1989). However it is likely that this list will quickly grow and formal proofs will soon be available showing that the majority of economic problems are intractable (ibid., pp. 9-10).

Rust goes on to argue, however, that there are a number of strategies or circumstances that can transform worst-case intractable economic problems into tractable ones. These include: hardware and software solutions (the time required to solve an intractable problem is a function of its dimensionality, so even if this function is non-polynomial, it may still be possible for real computers or agents to solve an actual problem of limited size); exploitation of special structure (some restricted problems may be tractable even though the unrestricted problem is not)[22]; decomposition (this works if the larger, worst-case intractable problem can be split into smaller, tractable problems, the solution of each of which does not depend on the solution of the others); randomization (this technique is used, for example, in Monte Carlo integration in which the approximation of sufficient closeness need be found only with a probability, not a certainty); and utilization of knowledge capital (as when a sufficiently close "guess" makes solution of an otherwise intractable problem feasible). He conjectures that "decentralization" (which is difficult to define rigorously) is "computationally efficient" and that this accounts for the undoubted success of many economic institutions such as double auction markets.

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22. Rust cites Nemirovsky and Yudin (1983) who show that the consumer choice problem is tractable if the utility function is concave.

This gives only the flavor of some of the main ideas in Rust's important paper. He makes it clear that he is not a pessimist regarding the ability of economics to understand a wide range of real-world situations. However, his bottom-line point is that determination of the limits imposed by computational complexity is, fundamentally, an *empirical* issue. If this is so, the other side of the coin of Rust's optimism is that there may indeed be cases in which the "computational 'solution' that nature and the economy have 'discovered'" (*ibid.*, p. 38) may itself be only an heuristic approximation to optimality. If it is, then the kind of realistic theory that would enable us to "take our models seriously for forecasting and policy analysis" (*ibid.*) would be one reflecting the actual computations, not an idealization that "solves" the problem by assuming it away.

III. What Do Firms Really Do?

To illustrate and make more concrete the possibilities discussed above, consider just one of the ways the firm's activity can be represented as a computation---specifically as the combinatorial optimization problem of picking the *best organizational form*. Firms (and other economic organizations, such as non-profits or government agencies that provide goods and services) are, first and foremost, collections of discrete *individuals*. The individuals communicate with one another in various ways; in large firms, it is not possible for every individual to be directly in touch with every other one in all matters: some communications must be mediated by other members of the firm. The communications may be two-way (as in face-to-face contact) or one-way (as when a policy is handed down from above through a memorandum or e-mail). It has recently become possible (through message boards and electronic discussion groups) to connect everyone in an organization for some purposes. This suggests that any complete representation of the activities of a firm (that is, one that involves no discarding or stylization of information) must include specification of the communications network(s) linking the firm's members.

Abstraction is necessary when we consider how firms actually operate to perform the tasks they undertake (production and distribution of goods, compliance with tax laws and pollution control requirements, making investment decisions, hiring and firing of workers, making strategic decisions about whether to enter a market, etc.) In the neoclassical model, the firm is assumed to have a profit function that depends on the quantities and prices of the firm's outputs and inputs. The firm may be a price-taker in some or all of its inputs and outputs, but that does not matter in this context. The profit function embodies the state of the art of the firm's technologies, including its "production function." The production function may have nice properties (constant returns to scale, for example), but this is not essential. There may be explicit or implicit time components to the input and output streams, so that the firm's maximization decision can have an intertemporal dimension. Maximization of the profit function may itself computationally complex (see the references above), in which case full maximization would have to be replaced by some algorithmically feasible computation that implements an approximation. Even if the numerical computations made by *individuals* making up the firm can all be carried out in polynomial time, it does not follow that the organizational maximization problem is computationally tractable.[23]

23. In actuality, many of the everyday problems the individuals in firms must handle can be shown to be **NP**-complete when appropriately formalized. Garey and Johnson's extensive list (1979) includes problems in network design, storage and retrieval, database management, and sequencing and scheduling.

The fact that the firm *intrinsically* is an aggregate of separate individuals carrying out their own individual computations means that the firm cannot realistically be thought to operate as a unitary entity with a mind and will of its own; instead, the "decisions" made by the firm constitute a form of *collective action*. This, in turn, implies that there must be some procedure or set of procedures by which the firm processes information and through which the individual agents interact with each other in order to reach a decision. The rules of procedure may be simple (for example, the firm decides to do something when the CEO issues an order) or complicated (there may be layers of review before an action is ultimately taken by the Board of Directors or a top management committee). Different procedures may apply in different cases, and some decisions may be decentralized in the sense that no one above a particular level in the firm's hierarchy ever sees the input to the decision.

The critical point is that the procedures are contingent on the network structure of the firm. The nature and functioning of the procedures are constrained by the possible channels of communication (and command) linking the members of the organization. The network structure is not something that is determined exogenously, so if any of the tasks involves maximization of an objective (shareholder value, say), an essential element of the maximization problem is selection of an *optimal network structure*. Now it is known that the number of possible network structures rises very rapidly with the number of individuals in the firm (Graicunas 1933; Wilson 1985). The problem of finding the optimal structure for carrying out any particular task (or any combination of tasks) according to the specified set of procedures is one of *combinatorial optimization*, and as such is likely to be computationally complex. Explicit models showing how organizations can carry out simple, stylized tasks according to well-defined procedures have been given in the literature.[24] If the combinatorial optimization problem of finding the best structure for some task is **NP**-complete or harder, then the firm cannot be presumed to be able to reach a maximum of its objective function. Levinthal (1997) has shown that adaptation of organizational structures can be represented as an instance of Kauffman's (1993) *NK* model, and Weinberger (1996) has demonstrated that in some cases, finding the globally optimal structure for the *NK* model is **NP**-complete. DeCanio et al. (1998) have developed evidence from simulation studies showing that even in models in which highly stylized firms carry out simple economic tasks, it is unlikely that optimal organizational structures can be found in polynomial time.[25]

It is instructive to see how the organizational structure question arises in the limited context of investment in energy efficiency. At a superficial level, this problem would seem to involve nothing more than calculating on a spreadsheet whether the net present values of investments in particular technologies are positive. But description of the firm's actual behavior would require (at least) specification of (1) who is assigned the job of collecting information on costs and cash flows that goes into the NPV calculation; (2) how forecasts of future costs and savings are obtained; (3) how information on the NPV of different investment possibilities is reported to other members in the firm (in

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24. See DeCanio and Watkins (1998) and the references given therein. For some tasks and rules of procedure, the optimal structure can be determined, as in Radner (1992, 1993).

25. Papadimitriou (1996) reports on several problems of organizational optimization for which decision-making is computationally intractable. For example, regarding the problem of solving a linear program in a hierarchical organization in which the decision-makers have their own objective functions, he observes that "[n]eedless to say, if the hierarchy, as well as the linear program, is part of the input, then the problem becomes **PSPACE**-complete" (p. 563).

particular, to other levels of management); (4) the decision rule by which some subset of managers decides whether to go ahead with projects or not (which may involve some sort of voting, some method of ranking projects, or some way of comparing the information received from different divisions); and (5) a "chain of command" by which decisions are implemented. This last step again involves specification of channels of communication as well as a determination of who is authorized to give directives that are then carried out by other members of the organization. Thus even in the simple case of sifting through potential projects and investing in those with positive NPV, the design of networks of communication, reporting, and command are an intrinsic part of the firm's optimization problem.

It should be stressed again that the actual degree of computational intractability of the tasks faced by firms is an empirical question. Some of the decisions faced by firms (and other economic agents) are of the "no-brainer" variety. It does not take a lot of calculation for managers and employees alike to realize that a firm selling a standardized commodity in a competitive market has very little discretion in its pricing strategy. If everyone were truly convinced that continued emission of greenhouse gases at business-as-usual rates would lead to climate disasters and a large number of preventable deaths, then it would be a relatively simple matter to decide collectively to reduce emissions through a tax on carbon emissions or an emissions permit system. When the scientific evidence of the causes and consequences of stratospheric ozone depletion reached a sufficient level of acceptance, the political and diplomatic difficulties of reaching an agreement to reduce the emissions of ozone-depleting substances were no longer insurmountable.[26]

IV. Conclusions and Implications.

A. Modeling.

The results reviewed here point to a need to replace the optimization assumption as the default in modeling the behavior of firms. But what sort of representation is to replace it? The aim of theorizing is to provide a foundation for scientific investigation and a source of testable hypotheses, not tautological generalizations that are immune to falsification. That in turn suggests two criteria for elements of a reconstructed theory of the firm: (1) specification of the firm's internal processes as algorithmic computations, and (2) adoption of an evolutionary view of economic dynamics.

The first point has already been elaborated upon in some detail in the previous section. The second has only been alluded to briefly in discussion of the adaptation of firms, but it is critical as well. Whatever the nature of the computational problems faced by firms, we know that real firms function and change over time. The fate of economic organizations depends partly on the intentionality of their members, but also on the ebb and flow of market and other (legal, regulatory, cultural) environmental conditions. Given that full optimization along all dimensions is not possible for firms, selection pressure exists nevertheless; no firm that is continually unprofitable can survive (at least without perpetual government subsidies), and firms that are highly profitable have advantages that

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26. As the historian John Lukacs has written in a different context, "[m]en cease to resist when they think that the pressure on them is irresistible; for 'irresistible' is what they no longer want to resist, and 'intolerable' is what they no longer want to tolerate" (1976, p. 525).

allow them to expand to occupy more space in their market niches. The metaphors of evolutionary biology are a natural language for describing the process of organic growth and change, and constitute the basis of an "appreciative theory" of economic dynamics that is closer to what economists know and believe than the formalisms of general equilibrium maximization models.[27]

But as Nelson and Winter understood, the use of evolutionary imagery alone is not enough to create a rigorous and realistic theory of the firm. Darwin and his followers were able to formulate the theory of biological evolution prior to the "modern synthesis" that joined the operation of natural selection and Mendelian genetics, but until that unification was accomplished, there was controversy over the *mechanism* of evolution (Ridley 1996). (Re)discovery of Mendelian genetics was required to formulate laws governing the heritability of traits, and to explain the variation and rates of mutation within populations. Furthermore, it was not until the latter half of the 20th century that the molecular-level genetic code for living systems (the DNA double helix) was discovered. Knowledge of molecular biochemistry increases our capacity to understand biological processes and evolutionary change, even though we are still far from an all-encompassing theory of living organisms.

The theory of human organizations has not yet achieved anything close to its own "molecular biochemistry," or even a passable version of "population genetics." Representation of the inner life of the firm by models of algorithmic computation could be one part of such theories, but obviously a great deal of work needs to be done before the

27. The power of the evolutionary idea in economics has never been expressed more clearly than in Nelson and Winter (1982). For a recent expression of the same line of thinking, see Nelson (1997).

full outline of such models can take shape.[28] Unlocking the code of "organizational DNA" may involve insights from numerical optimization (applications of genetic algorithms, simulated annealing, or neural nets), evolutionary game theory (allowing the individuals within firms to interact repeatedly in self-interested ways), the "new institutional economics" (in which property rights and incentives are emphasized as determinants of institutional performance), or agent-based modeling (in which an "emergent order" arises from the purely local interactions of the agents). It would be a great advance in knowledge if we could devise a scientific description of the "genetics" of organizational phenotypes, even if a computable model of how firms change over time remains beyond our reach.

B. Policy.

To return to the policy issue of estimating the non-environmental consequences of greenhouse gas reductions, the chief conclusion that can be drawn from the findings discussed here is that *estimates based on conventional top-down models should be de-emphasized* in the debate. These models are built on a shaky foundation, and as a result their quantitative cost estimates offer only a specious kind of precision. The behavior of firms (including their invention and adoption of new technologies having different energy intensities) is not now being modeled in a tight and reliable way. Yet a

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28. The starting point for any review (not to be attempted here) of what has been accomplished along these lines is Nelson and Winter (1982; see also Nelson 1995). It is worth noting that Hayek (1988) argued forcefully that the *idea* of evolutionary change in the humanities and social sciences predated its acceptance in biology, although of course the specifics of the theories differ across the various fields. In particular, the biological theory of evolution excludes the inheritance of acquired characteristics, whereas the evolution of culture and institutions must be based on the transmission of learned patterns of behavior.

well-articulated understanding of the actions and responses of firms is an essential prerequisite for reliable top-down model predictions.

This conclusion should not, however, be taken as a counsel of despair. Top-down models can still serve useful purposes (providing a consistent accounting framework, keeping track of the relative sizes of different sectors and the scales of various activities, projecting the consequences of a continuation of historical trends, etc.) so long as too much weight is not placed on their shoulders. The bottom-up models would appear to offer more promise at their current stage of development. These models have the virtue of specifying in detail what *some* of the technological possibilities are. (Of course, *no* model can incorporate the consequences of technological advances that have not been conceived. The world-changing technologies of the past century---ubiquitous computers, nuclear weapons, genetic engineering---were literally unimaginable to "futurists" of the past who were trying to visualize life 50 or 100 years ahead.) These technological possibilities can be incorporated into top-down models "off line" to indicate a wider range of scenarios that could be achieved through appropriate policy intervention (Boedecker et al. 1996; Koomey et al., forthcoming). Where the bottom-up models fall short is in spelling out exactly which kinds of policies could make their putative greenhouse gas reductions a reality.

Even if we acknowledge that we do not have particularly good models (yet) of organizational behavior or policy-induced technical change, it is still possible to be informed by the lessons of recent history. Close attention should be paid to the experience with other major environmental protection initiatives that have triggered innovation and reorganization. The most instructive recent examples include the phaseout of ozone-depleting substances under the Montreal Protocol and the massive reductions of SO_2 emissions being accomplished under the Clean Air Act Amendments of 1990. In both cases, *ex ante* model forecasts greatly overestimated the costs of the policy initiatives (Bohi and Burtraw 1997; Cook 1996; Hammitt 1997). Well-designed, market-based policies appear to have been able to smooth the transition to a less-polluted regime (Economic Options Committee 1994; Schmalensee et al. 1998; Stavins 1998). One of the key elements of both situations was that the policy signal was clear and unambiguous: the reductions in consumption and production of ozone-depleting substances and in SO_2 emissions were spelled out in a regulatory timetable. Within that framework, however, the affected firms were able to exercise maximum flexibility (and exhibited a correspondingly high degree of ingenuity) in meeting the targets.

In the case of greenhouse gas emissions, a carbon tax or emissions cap could provide a similarly clear signal. Within the context of the societal consensus that would be required to institute such a shift in the direction of policy, what additional features of an effective GHG reduction strategy would be suggested by the more realistic theory of the firm outlined above?

(1) Policy could reduce the complexity of firms' decision problems directly. As in the case of the ozone-depleting chemicals and SO_2 emissions, a clear indication that action was necessary would eliminate uncertainty within firms about what strategy to pursue. At the present time (with the Kyoto Protocol unratified, major details of its framework still to be negotiated, and no domestic measures in place to implement emissions reductions), firms are faced with multiple dilemmas: whether to engage in lobbying or political action to influence the outcome (and if so, what coalition to join); whether to extrapolate energy

prices and usage patterns based on historical trends or to plan for a less fossil-fuel intensive future; whether to anticipate that overseas competitors will enjoy competitive advantages (or suffer from disadvantages) depending on the actions foreign governments take; and what kinds of energy-related R&D to initiate, just to name some of the most difficult issues. An unmistakable global (and *national*) commitment to reduce emissions would reduce the range of potential activities related to this issue to the focused goal of reducing GHG emissions while maintaining profitability. As most people (including economists) well understand, there are times when it is easier to have fewer choices.

(2) A more realistic theory of the firm opens up increased possibilities for win-win policy approaches. Because no population is ever optimally adapted to a particular environment, it is possible to create selection pressures that reinforce more than one desirable trait. For example, an environmental protection policy that simultaneously encourages innovation and technical progress may be able to enhance organizations' economic performance and at the same time produce environmental gains. The Porter hypothesis (1991; Porter and van der Linde 1995a, 1995b) suggests that productivity benefits from environmental regulations can be achieved in some circumstances. Change mechanisms capable of non-incremental "jumps" from one local maximum to a higher one are consistent with this evidence, unlike the conventional optimization model that has only a single peak.

(3) It should be possible for policy to impinge positively on the decision processes of firms through mechanisms other than the traditional price and regulatory instruments. The success of the EPA's voluntary pollution-prevention programs is instructive in this regard. These programs operate entirely without a coercive component. In Green Lights,

Energy Star, 33/50, and similar programs, the EPA's activity is confined to providing advice, serving as a clearinghouse for information, and facilitating the dissemination of knowledge about best-practice techniques. These activities can be thought of as speeding up the "rate of mutation" within organizations or as fostering the multiplication and exchange of beneficial organizational "genes." The progress made by such programs does not imply that the traditional policy interventions are irrelevant, only that the range of potentially beneficial policies is much broader than that ordinarily contemplated by economists.

No doubt other policy consequences will emerge as firms' algorithmic computations and evolutionary processes become better understood. The point is that a shift in perspective that incorporates into working models the computational limits faced by firms can have a liberating effect on both theorizing and policy design. The optimization assumption may be adequate for some applications, but in analyzing the response of firms to GHG reduction policies, economists may have come up against problems that highlight the inadequacies of that assumption. Markets work and firms are successful because of the decentralized decision-making, the survival of social structures that have performed better than others in complex environments, and through the management of computational intractability using rules of thumb and heuristics. Why not recognize these realities explicitly and take them as the starting point for theory, rather than trying to force theory into the Procrustean bed of optimization? Acknowledgment of the complexity limits real organizations face would be a fertile starting point for new discoveries and breakthroughs in all branches of economics. Plus, it would enable us to think more constructively about the non-environmental consequences of the measures that *ultimately will have to be taken* to stabilize the atmosphere.

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