Modeling Analyses of the Costs of Kyoto: What Did We Learn?

*By Ronald J. Sutherland**

The Kyoto Protocol requires developed (Annex 1) countries to reduce their greenhouse gas emissions (GHG) to a specified amount below their 1990 level and to achieve this result during the 2008 to 2012 period. The U.S. emissions target is 7 percent below the 1990 level. The economic cost of Kyoto is the cost required to achieve these emissions targets. The Special Issue of the 1999 Energy Journal contains 13 articles that summarize modeling analyses that have the explicit purpose of estimating the cost of the Kyoto Protocol. Indeed, the title of the issue is "The Costs of the Kyoto Protocol: A Multi-Model Evaluation." In this paper, I focus on the question: what do we learn from these models about the cost of the Kyoto Protocol? I address this question by considering three questions. First, what are the conclusions of the 13 articles with respect to the costs of the Kyoto Protocol? Second, do the articles convey a good understanding of the economic analysis behind these conclusions? Third, are the conclusions persuasive and reliable? The 13 articles contribute significantly beyond the main question I address; however, my question is the explicit purpose of the articles. In addition to considering the cost issue, I will comment briefly on the modeling results with respect to the international trading of permits.

Not surprisingly, these models conclude that the costs of attaining the Kyoto targets are high, for the U.S. and for other developed countries. Most economists probably agree with this result. Environmentalists and energy efficiency advocates – the Green Team – argue that we can reduce greenhouse gas emissions in the near term at very low cost. Still others may be uncertain about the economic costs of reducing emissions quickly. What is the likely influence of the Special Issue on the views of the agnostics and the Green Team? My contention is that the Special Issue may not persuade critics and agnostics that the cost of achieving Kyoto is high. First, the modeling analyses do not easily communicate to most readers. Second, the papers typically do not present a conceptual explanation of the results. Third, the models are designed to address long-run issues and not the short-run responses required by Kyoto. On a more positive note, the Energy Modeling forum makes the important contribution of assembling a first-class international field of modelers to compare their simulations under controlled conditions. The modelers make a highly persuasive case that the threat of climate change requires a long run perspective. Further, the optimal path of GHG emissions lies above that specified at Kyoto.

Several years ago the Department of Energy sponsored a modeling analysis intended to project the market shares of various solar energy technologies in the electric utility generation sector. The author, who prefers to remain anonymous, produced the projections using a sophisticated electric utility modeling system. The modeling system included a demand and revenue module, a financial module and a capacity expansion module that selected generating technologies. A published report described the modeling system in detail and included the computer code. The utility modeling system had achieved a wide level of respectability, having provided the basis for numerous journal articles and government policy analyses.

The capacity expansion sector of the utility module used a logit function to forecast market shares, which was the conventional way to model market shares. A single parameter, l, was crucial in projecting market shares. Actually, this parameter determined market shares, with the remainder of the utility modeling system having little influence on the projections. The value of this parameter was merely assumed based on judgement, because there was no credible evidence to do otherwise. The report to the DOE contained the modeling projections of the market shares of the various solar technologies.

How would we assess the modeling projections of this DOE study? One assessment is that they were highly credible, state-of-the-art projections, based on a sound modeling analysis. Another interpretation is that the projections were nothing more than arbitrary input assumptions, disguised by a complex modeling system to convey a false sense of rigorous analysis. Readers of the final report, including the DOE, could see the market share projections, but were unaware of the critical assumption that produced the results. Most readers were unaware of the sensitivity of the results to various input assumptions and they could not determine whether the results were were reasonable. Although the equations of the model were explicit, the model was a black box to almost all readers. This lesson suggests a measure of caution in assessing modeling results.

Quantitative modeling analyses are a primary tool used by economists to provide information about economic behavior. Many energy economists are model consumers, rather than model producers. My impression is that model consumers are typically apprehensive and cautious in assessing modeling results. Model producers are often distrustful of modeling results, especially the results obtained by others. As model consumers, how do we assess the results of energy models? How should we assess these highly complex economic – climate change models? Are they state-of-the-art analyses that provide the most reliable results that we can obtain, or, are they merely mathematical manipulations of precarious input assumptions? The analyses are state-of-the-art. However, skeptics and agnostics will find the results unpersuasive.

To illustrate the application of the above three questions, consider the hypothetical case of an econometric estimate of a short-run price elasticity of demand. Suppose that such an analysis produces a large price elasticity for a particular good. We can readily understand what the conclusion is; it is the large estimated price elasticity. In the absence of explanation, we do not know the economic behavior behind the price elasticity and we will probably not study the econometric analysis to assess the reliability of the results. Furthermore, we are likely to dismiss the results because price elasticities are typically small in the short run. The econometric estimate could achieve credibility if confirmed by some independent evidence. For instance, the author could explain that the particular good has close substitutes and historically market shares are highly sensitive to price changes. Coupled with this explanation, we have a good intuitive understanding of the large price elasticity and we may accept it as a credible estimate. The econometric estimate by itself may not be believable. The estimate achieves credibility when comple-

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mented with an explanation based on economic behavior. I consider the Special Issue articles from this perspective.

What are the Costs?

A brief review of the abstracts, introductions and conclusions of the 13 articles indicates the main conclusions about the estimated costs of the Kyoto Protocol. The following quotes are taken from the Special Issue and are identified by author and page cited.

> "These studies generally show that the emissions trajectory prescribed in the Protocol is lower and the cost of mitigation higher than that required to meet long run objectives that were considered." (Weyent and Hill, p. xli). Note that Weyent and Hill are editors of the Special Issue volume and this quote summarizes several articles.
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> As stated by Manne and Richards: "We find that the short-term U.S. abatement costs of implementing this protocol are likely to be substantial." (p.1). "Finally, and perhaps most important: unless the ultimate concentration target is well below 550 ppmv, the Protocol seems to be inconsistent with cost-effective long-term strategy for stabilizing concentrations." (p. 20).
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> "The marginal cost in 2010…could also exceed $250 per tonne of carbon if the United States must meet its emissions limitations entirely through domestic actions, and if mitigation obligations are not adequately anticipated by decision-makers." (MacCraken, Edmonds, Kim and Sands, p. 25).
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> "First, it appears that the strategy behind the Kyoto Protocol has no grounding in economics or environmental policy." (Nordhaus and Boyer, p. 125).
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> "The emission reduction targets as agreed to in the Kyoto Protocol are irreconcilable with economic rationality." (Richard Tol, p.131).
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> "From a welfare perspective, the major effect of the Kyoto agreement is to produce a large wealth transfer from A-1 to non-A-1, while realizing none of the potential benefits of CO_2 control." (Peck and Teisburg, p. 390).

This sample of quotes from the Special Issue is non-random, but it captures the sentiment of most, if not all, of the authors. The collection of articles concludes that the economic costs of reducing emissions to achieve the Kyoto targets are very high.

Why Are the Costs High? Are The Results Persuasive?

The articles clearly indicate that the costs of achieving the Kyoto targets are high. Therefore, we now consider how these articles account for such high costs. I present a sample of the articles to convey how they explain the empirical results. I then conclude whether the results are likely to persuade the Green Team or those who are uncertain about the costs of Kyoto.

Manne and Richels list four factors that explain why longer term adjustments may be preferable to short term adjustments: 1) allow more time for capital stock turnover, 2) allow more time to develop low cost substitutes, 3) allow more time to remove carbon from the atmosphere, and 4) the effect of time discounting. Manne and Richels use the MERGE model and they use a 10-year time interval through 2050 and a 25-year interval through 2100.

Skeptics of modeling analyses recognize that Manne and Richels identify the factors that produce high adjustment costs in the short run. However, Manne and Richels do not explain the relative importance in their modeling analysis. We cannot be sure whether the capital stock is modeled to reflect accurately the turnover of buildings, transportation vehicles and energy using technologies. Because the model apparently iterates every 10 years, it only iterates once during the Kyoto period. Such a model may be more useful for long run simulations than for estimating the costs of short run market adjustments.

MacCracken, Edmonds, Kim and Sands (MECS) note that the Kyoto target is achievable by capturing or sequestering carbon, fuel switching or conserving energy. In the MECS analysis substituting natural gas for coal in the electric generation sector accounts for roughly 40 percent of the reduction in emissions. Consumption of coal drops by three-quarters, while consumption of natural gas increases by three percent. If the Kyoto targets are anticipated and expected to be permanent, costs are lower ($168 per tonne) than if targets are unanticipated ($250).

The authors provide a good explain of adjustments in the electricity generation sector, but they provide less explanation of assumed price elasticities that induce energy conservation. The MECS model iterates every five years and hence iterates only twice to achieve the Kyoto targets. With only two iterations, we may question how accurately MECS can model new capital additions and capital retirement.

Nordhaus and Boyer present totally negative results about the feasibility of the U.S. achieving the terms of Kyoto at low cost. The authors use the RICE model, which is based on optimal economic growth theory. The model projects optimal paths of emissions and economic variables up to year 2100 and beyond. The model iterates (computes equilibrium values for the endogenous variables) for ten-year periods. This adjustment period of ten years precludes the model from estimating the response of variables as they adjust.

Nordhaus and Boyer do not discuss the adjustments required to achieve the terms of Kyoto. The RICE model is clearly a long run optimization model. We are uncertain in assigning accuracy to the first iteration, which gets us to the Kyoto commitment period. Furthermore, the paper does not convey an intuitive understanding of why Kyoto is expensive. The RICE model appears more useful as a simulation tool for long run (a century) comparisons, rather than as a model that estimates short term adjustment costs.

The Nordhaus and Boyer paper is likely to impress most readers as a first-rate effort that offers several important insights and conclusions with respect to climate change policy. My point is that the Nordhaus and Boyer paper would not persuade members of the Green Team, or even the agnostics, that the costs of Kyoto are high.

The modeling analysis of Richard Tol concludes that the Kyoto targets are political targets that make no economics sense. The model used by Tol iterates annually, which makes it more appropriate than other modes to assess the costs of Kyoto. Tol confirms my point: "Many of the models used for analysis of the Kyoto Protocol…are therefore not really suited to look at issues of when-flexibility before 2012." (Tol, p. 149) Tol is unmistakable in stating his conclusions, but he does not provide a simple intuitive/behavioral explanation in support of his results. Perhaps readers with interest in studying the model documentation can figure out the economic behav-

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ior behind Tol's result. I conjecture that students with this enthusiasm would be favorably impressed with the analysis. For most of us, the model is a black box that confirms what we already know, or, does not persuade us of an alternative view.

Although my sample of Special Issue articles is small, my view is that such models are not the appropriate tools for assessing the costs of Kyoto. Models that iterate every five to ten years are not the best tool for short run analysis. The modelers do not explain why the costs of the Kyoto targets are high. The energy macro models that iterate annually, such as the EIA, WEFA and DRI, are more appropriate to model the short run adjustments required by the Kyoto Protocol.

The Energy Modeling Forum (EMF)

Although my above comments on the Special Issue papers may appear critical, the Energy Modeling Form makes an important positive contribution. Simply bringing together the best of the international modeling teams with a common purpose contributes significant credibility to the findings. Much of the analysis of climate issues is sponsored by an interest group, such as industry, the government or the Green Team. The conclusions of the research reflect sponsorship. The EMF is widely respected for its impartiality, objectivity and high quality analysis. The collection of thirteen modeling analyses, including six from foreign countries, produces a highly credible result.

International Trading of Emissions Permits

The EMF papers conclude that the wider the sphere of international trading of emissions permits, the lower the cost of reducing emissions. Modelers reach this conclusion by specifying a marginal cost function for reducing emissions by region, where marginal costs tend to be lowest in developing countries. If the sphere of emissions trading includes the developing countries, then costs of reducing emissions are minimized.

Although this result is no doubt correct, I offer two qualifications. The important issues with respect to trading include the costs of operating the trading system. These costs include transaction costs of monitoring, measuring, verifying and enforcing trades. For instance, under the proposed trading system, the Clean Development Mechanism allows a developing country to sell a credit for the emissions reduced relative to a base case of no emissions trading. How can we know the base case emissions? Under trading, there is a strong financial incentive to exaggerate base case emissions and difficulty in confirming what would have occurred. The EMF modeling results show large gains from trade. However, the modelers do not reflect these operating costs, which are the main limitations of a feasible system.

International trading of emissions permits has the most potential to reduce costs if the developing countries are not themselves subject to emissions constraints. Kyoto does not constrain the developing countries. By not being constrained to reduce their own emissions, these countries can sell emission reductions at a low price. However, according to Nordhaus and Boyer, p. 104, if the developing countries do not reduce their emissions, global mean temperatures decline by only 0.13 degrees C over the next century. If developing countries were constrained to reduce their emissions, they would no longer have credits to sell to other countries. International trading of emissions permits has the largest potential to reduce total costs when the policy fails to reduce the threat of global warming. If the policy were potentially successful – by requiring developing countries to reduce their emissions – emissions would be less successful in reducing costs. International trading of emissions credits is not a panacea if it only reduces costs when the overall policy fails.

Conclusion

My opinion is the Special Issue papers will not persuade the agnostics and the Green Team that achieving the terms of Kyoto is enormously expensive. Although the quantitative results may be one-sided, the supporting conceptual explanations are not persuasive. I suspect further that Green Team analysts, given the opportunity, could change some of the coefficients in these models and produce the "free lunch" estimates associated with their energy conservation views. A clue may be to look at the capital-energy and carbon-energy coefficients in the models and then adjust the coefficients with a good dose of neoclassical substitutability.

The EMF modelers did not design their models to estimate short run costs. Most of the models iterate only once or twice over a decade. When the modelers look at their first period simulation results, they find that optimal emissions are above the Kyoto targets. They conclude therefore that the Kyoto targets are too costly. The models do not contain disaggregated capital stock by vintage and type, e.g., vehicles, buildings and technologies. Estimating short run costs of achieving the Koto targets should consider the rate of turnover of a disaggregated capital stock. These EMF models are not the best models for estimating the costs of achieving Kyoto. The EIA analysis "*Impacts of the Kyoto Protocol on Energy Markets and Economic Activity* is more appropriate for estimating short run costs.

The models discussed in the Special Issue are designed to simulate long run behavior. The models therefore iterate every five or ten years and simulate variables over the next century or even longer. The modeling analyses make the important contribution that addressing the threat of climate change requires a long-run policy focus. Further, the optimal path of GHG emissions does not go through Kyoto, but instead, GHG emissions decline gradually over a longer period. This contribution by the EMF modelers is persuasive, in my view, and it offers critical policy implications.

Mexican Affiliate Reconstituted

The Mexican IAEE affiliate – Asociación Mexicana para la Economía Energética (AMEE) — has completed the renewal of its Directive Council. It brings together outstanding individuals from the academic, public and private sectors. Linked to the different facets of the energy development of the country at senior levels, this council assures an active participation of the Association in the energy debate to be held in national and international fora.. It is currently preparing a joint Symposium with the private sector Mexican Association of Electrical Enterprises and the University Energy Program of the National Autonomous University of Mexico, to be held next January, and the third AMEE National Congress. It will also seek a closer collaboration with IAEE and the other affiliates.

The members of the AMEE Directive Council are:

President: Dr. Pablo Mulás P.

PhD in Engineering from Princeton University, U.S. (1965), former Director of the Division of Energy Sources (1976-1991) and Executive Director (1991-1996) of the Institute of Electrical Research of the power public sector. Currently Director of the University (UNAM) Energy Program and Regional Coordinator for Latin America of the World Energy Council (e-mail: pmulas@servidor. unam.mx or: pmulas@www.imp.mx)

Vice President (and President Elect): Dr. José Miguel Gonzalez S.

PhD in Mechanical Engineering from the Massachussetts Institute of Technology, U.S. (1972), former Director of the energy consulting firm IPRODET participating in national and international projects (1983-1997). Currently, Director of the Mechanical Systems Division of the Electrical Research Institute.

Secretary: Dr. Juan Rosellón D.

PhD in Economy from Rice University, U.S. (1993), since then professor at the Economic Research and Teaching Center (CIDE). Awarded the Economy National Prize in 1994. General Director of Economic Policy at the Energy Regulatory Commission (1995-1997), participating in the design of the structural reform of the Mexican Energy System.

Treasurer: Dr. Arturo Reinking C.

PhD in Engineering Sciences from the University of California-Berkeley, U.S. (1973), professional experience in General Electric, in the National Institute for Nuclear Energy and as Group Manager of the Investment Bank Division of Banca Serfin involved in financial engineering activities. Since 1998, Technical Secretary of the University Energy Program at UNAM

Officer: Dr. Francisco Guzmán

PhD in Physico-chemistry from Sheffield University, U.K. (1978), professor at the Universidad Autónoma Metropolitana until 1989; since then researcher at the Mexican Petroleum Institute, where later appointed Deputy Director for Environmental Protection (1996-1998) and from 1999 Deputy Director for Research and Technology.

Officer: Ing. Luis Vázquez S.

Chemical Engineer from the Ryerson Politechnical Institute, Canada, extensive entrepreneurial activity in the oil services and gas industry, at the head of several private companies, former President of the Mexican Association for Natural Gas (1992-1996) and member of the Administrative Board of the American Gas Association (1992-1997). Currently Director General of Servicio de Energía de México, joint enterprise with Lone Star Gas International that will distribute natural gas in Mexico City.

Officer: Dr. Javier Estrada E.

Master in International Economy (1980) and Doctorate in Political Economy (1982) from the University of Paris, Nanterre, former researcher at the Norvegian Fridtjof Nansen Institute (1985-1988, 1992-1996), economist in charge of market forecasts and strategic planning at Saga Petroleum, Norway (1988-1992). Currently Commissioner at the Energy Regulatory Commission of Mexico.

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Jane Carter Prize

The Jane Carter Prize is awarded by the British Institute of Energy Economics, the International Association for Energy Economics and the Association for the Conservation of Energy in memory of Jane Carter, former head of the Energy Conservation Division in the UK Department of Energy and a founder of both the BIEE and the IAEE. The Prize for 1999 was awarded for the best paper submitted to the 1999 BIEE Conference by an author under the age of 35 which was relevant to the theme of energy and sustainable development.

Ten papers were submitted for consideration. Several were of high quality. After considerable discussion the judges concluded that the Prize should be awarded to Melinda Acutt of the University of Liverpool and Caroline Elliott of Lancaster University for their joint paper on "National and EU Regulation of Electricity Generation". This paper develops an innovative approach to a major policy problem - the reconciliation of effective economic and environmental regulation of electricity generation. The discussion is based on a theoretical model of the interaction between economic and environmental regulators acting together to maximise their joint advantages.

The Prize was presented at the Annual General Meeting of the BIEE on 1 November 1999 by the Institute's President, Lord Lawson.

David Jones