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MODELS, MUDDLES AND MEGAPOLICIES: THE IIASA ENERGY STUDY AS AN EXAMPLE OF SCIENCE FOR PUBLIC POLICY

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"The enormous computer systems ... in our culture have, in a very real sense, no authors. Thus they do not admit of any questions of right or wrong, of justice, or of any theory with which one can agree or disagree."

Joe Weizenbaum,
Computer Power and Human Reason (1976)
p. 239.

"Decision makers need a better understanding of the models, their assumptions, strengths, and limitations, and of why they produce the results they do."

Martin Greenberger,
"Closing the circuit between modelers and decision makers",
EPRI Journal, 8 (1977),
pp. 6-13

PREFACE

This paper is a companion to Keepin's *A Critical Appraisal of the IIASA Energy Scenarios* (IIASA WP-83-104). Although it is intended to be self-contained it is better read in conjunction with that paper. In publishing it I want to acknowledge the many valuable conversations I have had with Bill Keepin. I also want to pay tribute to the continual honesty and fairness of his purpose, and to his consistent attempt to be constructive in what is inevitably in many ways a critical task. My reasons for suppporting this work are that it provides an important example of the reasons why more systematic attention to institutional contexts of policy analysis and policy making is necessary in analysis itself.

It speaks something of the quality of scientific debate at IIASA that it should be able to integrate self-criticism into its research curriculum for developing methods and strategies in policy analysis. It has been said that the IIASA Energy Systems Project enjoyed vigorous debate and criticism in its normal mainstream fare, so that these contributions, offered as attempts to promote broader reflection, will take the place in that healthy diet.

The point about Keepin's critique, and of my analysis, is emphatically not that the particular biases of *Energy in a Finite World (EIFW)* are less legitimate than anyone else's, but that the biases: (i) were deeper and indeed of a more subtle kind than recognized; (ii) went straight through the "analytical" process with little or none of the correction that analytical methods are supposed to apply; and (iii) were then obscured by rather extreme claims for the objective control of bias.

My first reaction on hearing earlier versions of Keepin's critique were that while it was mildly interesting to hear that some energy models did little or nothing, that was not itself anything very new, and not worth a lot of effort to document and publicize. However, I was provoked into drafting a paper when I heard reactions to Keepin's criticisms and tested these against other statements. These initial responses were essentially patronizing remarks about a bright but naive young idealist's need for initiation into the "realities" of science. The reactions have been that he has rather pedantically demonstrated his own technical competence like a good graduate student by learning for himself what everybody in the field already knew. If he would now learn the proper protocols of self-expression, the cryptic professional languages which maintain external credibility by muting what would otherwise be explicit, externally visible internal frankness and self-criticism, he would be admitted with honor into the community of analysts. Interestingly, this attempt to initiate the innocent uses the picture of scientific practice as messy, informal, and contingent (not governed by preordained rules of method, etc.), which social empirical analysis of science has contrasted with previous normative images of good practice. The unruly pragmatic reality is being used as a normative framework in the defense of *EIFW* against Keepin, to show him how to be a "*mature*" analyst. He is effectively being told that if he measured his analysis of *EIFW* against a "realistic" view of science and analysis, his criticisms would dissolve.

I tested these reactions in a very simple way - I looked in the literature at what the *EIFW* study said about its models, and what other experts believed. What I found, as is documented here, flatly contradicts the blandishments directed at Keepin that everybody supposedly knew the models were trivial. The whole point, as developed in this paper, is that when Keepin examines iteration, sensitivity analysis, etc., he is measuring *EIFW* against *its own claims about itself*, not against some abstract ideal that he himself introduced. I decided therefore that this was not only worth documenting in itself, but also worth trying to interpret and put in broader, more constructive perspective.

In addition to the collaboration with Bill Keepin acknowledged above I am also grateful to Ernö Zalai, Mike Thompson, Dick Bocking, Alan McDonald, Holger Rogner, Jesse Ausubel, Tim O'Riordan, Gordon Goodman and Val Jones for comments and conversations on this topic. Needless to say, none of them is at all responsible for the errors of assertion, judgement, or expression left in this paper.

ABSTRACT

This paper is offered as a contribution to general methodological reflection within applied systems analysis. It is about several linked questions: (i) the nature of intrinsic structural bias in the very activity of formal modeling; (ii) the pitfalls involved in attempting to be objective by artificially abstracting physical and technical aspects of an issue from institutional dimensions; (iii) the underlying structural correspondence between particular modes of policy analysis and of the policy process itself; (iv) the problems of proper self-representation of policy analysis, given the inevitable conflation of informal judgement and formal calculation involved; and (v) the ambiguous connections between the pragmatic role and practice of policy analysis, and the processes of quality control.

The IIASA energy study happens to be a good example of several general problems and confusions that require further development of

methodological reflection already under way. A major point of this paper is that the necessary acceptance of analysis as a craft skill, like conventional science (i.e., not completely specifiable in terms of its rules of inference, logic, etc.), must not be allowed to justify *laissez-faire* with respect to standards of proper practice in such basic matters as documentation and sensitivity analysis. Although the IIASA ESP suffered problems in these respects and over demarcating the boundaries between formal and informal modes of analysis, it is by no means unique, as this paper shows.* The overall conclusion is that if it is to be meaningful, methodological reflection and change within applied systems analysis requires corresponding systematic attention to the policy process and institutional contexts in which analysis and decision making are conducted. In an important sense, analysis is a symptom of a given policy process, rather than an input to it.

* A recent important paper has come to my attention unfortunately too late to assimilate and discuss here. This appears to contain striking similarities (even to the extent of its independently formulated title) but some significant differences of approach to my own presented here: (The Energy Model Muddle, P. Brett Hammond, *Policy Sciences*, 16 (1984) 227-243.

A NOTE ON REFERENCES

Since there are repeated references to the IIASA book, *Energy in a Finite World, Vol 2 (1981)* as the main account of the IIASA Energy Project, page references to this source are given in the text as *(EIFW, p. xx)*. References to Keepin's analysis of the models are given in the text as (Keepin, p. xx). All other references and notes are referred to by the convention of sequential numbering in the text, and full details appear in the References section at the end.

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I. INTRODUCTION

In the *Usborne Book of Science Fun* for children [1], some elementary strictures are given on "Being a Scientist":

> "When you build a model, or do an experiment, you need to be careful and accurate, as a real scientist would be. On these pages there are some hints on being a scientist. If you follow these, your projects should be successful, though even real scientists sometimes have to repeat experiments because they do not work first time."

We are all grown up enough to know that "being a scientist" is not like this kindergarten ideal. We soon learn that the world is not made up of the artificial entities we create, such as perfect harmonic oscillators, or perfectly elastic solids, or even definitively repeatable experiments [2]. So too with respect to the process of scientific practice, and models of its governance by righteous principles of full communal knowledge; universal access; uniform evaluative standards; (disinterestedness), and organized skepticism towards untested claims. These have given way to more complex and ambivalent accounts [3], and science for policy departs even further from such tidy norms. In policy reality there are no such things as controlled experiments, ideal markets, definitive resource bases or general solutions. Even "the economy" does not exist as an objective entity. Like all other entities around which we construct policies, it is "an extremely high-order intellectual construct ... It is, like the unicorn, a myth - an extraordinarily useful myth nevertheless" [4]. Just as the objects about which science has attempted to create useful knowledge have become more complex, so too have the realities and our perceptions of the social processes of analysis that create that knowledge.

From the earliest days of systems analysis and of its precursor, operations research (OR), there has been endless discussion of its status in relation to 'true' science. The tacit anxiety underlying this soul-searching has justifiably been about the professional status of the field and the public authority of the knowledge it has produced. Inevitably recognizing that it must claim to exercise authority in issues beyond conventional ideas of scientific logic or method, applied systems analysis (ASA) has acknowledged the extra, judgemental or "craft" dimensions of its trade [5]. This has become increasingly significant as the field has evolved: originally from relatively narrow OR applications mainly involving quasi-scientific prediction and forecasting (of, e.g., the effectiveness of a given military operations solution); later moving toward prescriptive comparison of alternative policy options (e.g., within the cost-benefit analysis framework, often for a specific decision maker); and finally in the 1970s to policy analysis in the large, involving conflicting analyses in adversarial settings, often focusing upon institutional and procedural constraints and possibilities, and dealing with problems for which no identifiable decision maker or decision making body exists.

Yet, ironically, as scientific expertise and systematic "objective" analysis have become more frequently involved in policy, their credibility and thus their ability to deliver policy authority have actually decreased. The rate of growth of this policy impotence has been rather dramatic, giving rise to a sense of crisis in many policy circles, yet the dominant reaction has been to repeat (with even more elaboration than before) the attempt to purify the analytical part of policy from the value parts. This has been attempted in both institutional mechanisms such as science courts, or splitting risk estimation from risk evaluation in regulatory bodies [6], and in epistemological principles, such as enforcing more formal precision and specification of rules of inference and decision in policy-related analysis [7].

As responses to diminishing credibility, these initiatives attempt to create and defend a realm of pure authority and *substantive* as well as procedural objectivity. In so doing they present the analytical domain as more intellectually coherent and objectively verified than it is in reality. In a previous paper I have discussed at a general level the false metaphysics of "purification" of science from social values [8]. In a forthcoming paper I will discuss the same theme more practically with respect to toxic chemicals risk assessment.

The underlying point is to draw the connection between this "purification" myth and a central theme of Western social thought that has recently come under long-overdue attack, and then to explore the practical consequences for policy analysis and practice. The misleading theme is the individualistic metaphysics of human behavior and beliefs, which assumes that values are rooted in individual choice rather than sociocultural determination [9].

The complete entrenchment of this voluntaristic or individualistic metaphysics in social thought has recently been soundly criticized by Douglas [10]. In the present context it is enough to point out how the perspective leads to the recurrent false belief that values may "pollute" objective knowledge only through *individuals* with self-chosen biases, incompetences, etc. This presumes the absence of more basic and structural, socially induced perceptions and definitions of reality that may have come to be seen as natural and "objective" by a given culture or subculture, but which nevertheless reflect (unchosen in any real sense) human values [11]. The dominant metaphysics and its associated idea of science misleads us into focusing attention on the control of individual, conscious bias as if this were enough to guarantee an objective, neutral analytical substratum of policy. This diverts our attention from the more complex collective biases underlying even honest attempts to maintain a clean division between science and politics.

In this paper I will show how such fundamental confusions and simplistic ideas about the fact-value relationship in one major systems analysis project for policy, namely the IIASA Energy Systems Program (ESP), led it inexorably towards a deepening policy muddle rather than the factual clarification that was claimed. Except perhaps with respect to scale - it has recently been described as "the most ambitious energy study so far" [12] - there is nothing uniquely delinquent about the IIASA ESP, so it is to be emphasized that this is an example of more general problems. However, the ESP is especially illuminating because:

(i) problems of self-representation have been exposed and accentuated by Keepin's technical critique of the quantitative analysis employed in the study, and the ESP group's reaction [13];

(ii) these problems can be seen to be linked to the *process* of policy analysis, notably arrangements for quality control and peer review of such complex projects;

(iii) confusions in self-representation are related to and are essentially determined by confusions embedded in the very definition of the project, which was to separate the technical dimensions from the political, and thus to conduct a supposedly neutral technical study. This approach would presumably discover "the factual basis of the energy problem, that is, to identify the facts and conditions for any energy policy" [14], and "to provide decision- and policy-makers with the information they need to make strategic choices" [*EIFW, p.800*];

(iv) the problems of self-representation and questions of analytical process are also accentuated by the fact that coinciding with the publication of its major output, *Energy in a Finite World* (1981), a major campaign was launched to communicate its so-called "robust" conclusions to policy makers in all corners of the globe;

(v) finally, as I will argue in the conclusions, all these issues considered together suggest the need to identify different styles of policy analysis, and to link these with corresponding kinds of policy *process*. The model of policy analysis represented by the IIASA ESP is one kind of analysis, implying only certain kinds of policy decision, and of (top-down, centralized, capital-intensive, technology-dominated) institutional arrangements for policy making. This is one legitimate style, but there are others. From its metaphysics to premises, to methods, to conclusions, self-descriptions, and back to metaphysics, the IIASA ESP circulates within the same fundamental bias, encouraged by corresponding epistemological confusions and lack of institutional restraints within ASA generally.

Therefore, using the IIASA ESP as an example, I will discuss some general issues in the development and uses of ASA as policy analysis. I will do this via an analysis of the framing and public self-representation of the ESP. Central to this aim will be an examination of the claims and perceptions about the role of computer models as a basis of authority for the policy "conclusions" that the ESP claimed to reveal.

Originally brought to IIASA to try to simplify the ESP models, Keepin found that indeed they could be virtually short-circuited altogether because their outputs were identical to subjectively determined inputs - there was no dynamic calculation at all. Worse still, when subjected to standard sensitivity analysis the models' outputs such as fuel mixes fluctuated wildly with tiny changes in assumptions.

The responses to Keepin's earlier suggestions and presentations of these insights have naturally enough so far been in unpublished form - informal colloquia and meetings, correspondence, etc. However, their significance is so great that they require examination alongside the published accounts of the IIASA energy study.

A main point of confusion is the centrality or otherwise of the models to the whole IIASA ESP. There was clearly a lot more value in the study than the part surrounding the models. On the other hand, if ESP publications suggest, as they do, that the models are significant in the study overall, the discovery that the models are severely limited will naturally undermine the credibility of all parts of the Program. If they have been oversold in the search for objective credibility, the unfortunate pay off is that valuable aspects of the program - perhaps more fragmentary and modest than comprehensive global claims but valuable nonetheless - may be lost in the general misunderstanding of the real dependence of the study's overall claims on its 'objective' models.

The responses to Keepin's technical criticisms of the models as analytically vacuous and more or less completely constrained by exogenous inputs created by subjective judgement will be examined and interpreted as problems arising mainly from the lack of any coherent and effective peer community for such modeling exercises. The point is not just that discrepancies exist between actual analytical process and public self-description (this is a phenomenon that is well recognized amongst social analysts of science, even in the most academic of sciences [15]); it is more that for the latter there are professional communities that share a tightly knit, self-regulating subculture and informal mutual awareness. Thus, in principle, via their informal knowledge, they can control excessive discrepancies and exaggerated claims without external publicity and avoiding unrealistic demands for complete formal self-representation. In the case of ASA in policy analysis, no such subcultures exist, partly because ASA's defining claim to a rightful place is its trans-disciplinary nature, covering a wider range of questions than a conventional scientific discipline. Thus there are three pressures: to produce more certain-looking knowledge for policy; to exercise more informal subjective judgements because of the broader coverage and the overcomplexity of ambitious models; and the lack of a coherent peer group. These combine to create even greater tensions and inconsistencies between formal languages and inaccessible, informal realities of analysis, to the accumulating detriment of the policy process.

I.1 Analysis, Craft and Authority

The evolving self-image of ASA as craft activity (i.e., science plus intuitive judgement) was accompanied by growing recognition that even natural science itself (let alone scientific statements for policy) is impregnated with unspecifiable, tacit judgements, in the evaluation of data, construction of experiments, recording of observations, defining "adequate" proof and disproof, and so on [16]. Indeed, it is this lack of pure formalism and of complete rule-specificity in scientific knowledge that underlies the ease with which, when subjected to unrealistic formalist standards (most notably legal cross-examination), scientific policy advice has been easily discredited in adversarial situations [17], to the alarm of policy makers fed on an extravagant diet of misleading positivist images of science. Even when conventional science makes inputs to policy, there are crucial and often tacit judgements entangled in the associated technical expressions. The judgements involve, for example, appropriate degrees of uncertainty to attach to variables and relationships; the framing of the problem and the selection or exclusion of different aspects; and implicit conditions attached to the validity of expressed relationships.

Given its express claims to broader scope, yet with continuing claims to scientific foundations, it is hardly surprising that ASA should encounter the same fundamental problems writ large. Adopting the status of craft as (supposedly) distinct from science does nothing in itself to solve these problems unless some further issues to do with quality control and self-representation are more seriously addressed, with more effect than hitherto [18]. As the policy role of ASA has evolved and broadened, from specific prediction through optimization to policy argumentation, its empirical referents have also become more elusive, so that it is no longer adequate or possible to evaluate analytical quality according to empirical tests and relatively solid feedback. (One response to this of course has been the growth of simulative modeling, as discussed in the next section.) As Majone and others have emphasized [19], policy *analysis* has become as much about justification and persuasion as about "discovery" of the best policy [20]. This distinction is complicated, though not contradicted, by the fact that a common means of persuasion is to claim one is not persuading at all, but objectively discovering [21].

There are few, if any, empirical referents with which many key policy propositions may be unambiguously tested. Consequently, in order to evaluate the credibility of any policy analysis there is a need to spell out the analytical ***procedures*** by which any claim to policy knowledge is reached. Thus Meltsner has argued that "knowledge about the analytical process is just as important as knowledge about policies if the effectiveness of public policy is to be improved" [22]. Greenberger, Schelling and others have drawn the same conclusions [23].

Yet as Archibald has established [24], the language of self-description of ASA is ambiguous and confused. In particular, it has often been unclear in central texts of systems analysis whether intuition and craft are to be seen as unfortunate, temporarily inevitable elements of ASA, awaiting their obliteration when formal analysis develops, or whether they will be forever essential to analysis. Much of the agonizing intellectual contortions involved in trying to provide a definitive answer appears to be the result of a perceived need to enjoy the public authority of formal rule-bound thought, accessibility and external testability, whilst recognizing that this could not credibly describe the real process of constructing policy advice. The fact that intuitive processes may be necessary tp scientific analysis can also be used illegitimately, as an excuse for not following the discipline of clear, accessible statements of assumptions and reasons as far as possible.

Threading these debates and those specifically about models, there are the following basic questions:

1. Is formalism superior to intuition?

2. Is intuition or subjective judgement *always* part of formal analysis?

3. Even if it is, should we merely tolerate and contain it, or champion it as a valuable part of the exercise?

4. If there is something more than fully specifiable, externally accessible analysis involved, is it "subjective judgement", which may be recognizable as such to its author, if not to others; or is it socially induced, non-empirical evaluation, which is culturally specific and thus biased, yet so deeply ingrained as to appear to its bearers as objective and natural; or both?

5. If such complex interactions of intuition and formalism do exist as is accepted in modern analysis of science [25], how then should we use science, and ASA, in policy?

The last question is particularly acute because many of the criticisms of specific policy analysis are about their self-description and interpretation into policy conclusions as much as about their substantive content [26] and because a frequent lament from policy makers and analysts alike is the lack of rigorous attention to the ways in which analysis is "transferred" to the practical policy domain.

I.2 The IIASA Energy Study

The IIASA study of the global energy system began in 1973 and took 7-8 years, approximately $10million, and 225 person-years of effort to complete. Its very scale dwarfs other efforts at energy policy analysis. It has been widely taken as the most comprehensive such analysis ever, and has apparently achieved considerable impact. It has been widely described as the most impressive, even unprecedented, and comprehensive study of 'the' global energy problem, "an unprecedented, detailed analysis ... analysing options in a quantitative, mathematical form" [27]. The idea that it had discovered the 'objective' structure of the global energy problem separating these from 'organizational' problems in "an elegant and coherent system solution to a global problem," which had "changed our image of the world and man's place in it," was observed by a US Congressman [27a], and several recent major analyses of global energy and climate issues have adopted it as a definitive frame of reference [27b], thus tending prematurely to leave behind any questions over its origins and validity.

The scale of the project has also been linked to its apparent objectivity. "More than 140 scientists participated in the study, including economists, physicists, engineers, geologists, mathematicians, psychologists, a psychiatrist, and an ethnologist. Thus it is impossible for us to hold an extreme one-sided view" [28]. An impressive network of international bodies collaborated in the project [29], and according to its Director, Wolf Häfele, it has shaped energy policy discussions within several national and international government bodies [30].

One of the benefits of such a long project of course, is that there is in principle time for many points of view to be heard and evaluated on many different aspects of global energy. The project in this sense was more than its final products, and involved scientific publications on a range of issues from fusion, to logistic substitution curves, to carbon dioxide output projections, to risk perception. But although some specialist groups may have focused upon specific scientific papers and subprojects of special relevance to them, the main product for general evaluation remains the book, *Energy in a Finite World*, which in Häfele's own words "presents the findings of the study of the global energy system," and "reflects our work up to this date [1979]" [*EIFW, p.xiii*], and various related summary articles. Whatever may be the complex reasons behind the attempted synthesis of a huge, multifaceted project into such a book, the project put its name to it, and publicized it vigorously and successfully. In this interpretation of the IIASA ESP therefore, there is a clear warrant to take the book as the definitive self-description for policy and other users. Nevertheless part of my task will be to outline the changes in this self-description over time and space to understand some inconsistencies and misperceptions, inside and external to the ESP.

Although the main report, *Energy in a Finite World*, [31] may be studied by relatively few experts, many others will be influenced by the more condensed interpretations of the project and its definitions of "the" world energy problem in the freely distributed *Executive Summary* [32], and in such widely read journals as *Science*, *Scientific American*, *Futures*, and others [33], not to mention the many unpublished summary briefings prepared for practical agencies and bodies dealing with energy policies. In addition, Häfele alone has, in his own account, "given speech upon speech based on our 850-page book, *Energy in a Finite World*, in the last two years" [34].

The IIASA analysis combined mathematical modeling with scenario construction and informal processes of judgment to analyze over a 50-year period the possible transition to what is taken to be a sustainable world energy system. The elements of sustainability were resource supply, excluding environmental, price, technological, or social factors. The study involved "the design of a set of energy models that were subsequently used for developing two scenarios - the principal tool of our quantitative analysis" [*EIFW, p.xiii*] The scenarios were thus constructed with the aid of the models and were the heart of the study, from which certain key policy conclusions were drawn.

The main conclusions of the study, as reported in *Energy in a Finite World*, and in the summary articles, were that a transition to fast breeder nuclear reactors, centralized solar and coal synfuels must be made, and could be achieved beyond the year 2030, if the world acted decisively now, to accelerate the installation of the necessary plants. These "robust conclusions" have been forcefully publicized by Häfele [35].

There have been strong criticisms of the substance of the conclusions and some central premises of the study [36], such as its lack of recognition of diverse, decentralized approaches to energy supply, or ways to reduce energy demand. There is also confusion and dispute as to the status of the models and their role in generating the scenarios and policy insights. Recently, there have been methodological and technical criticisms, notably by Keepin [37], that the models involved are analytically empty; have had no real iteration or sensitivity analysis (despite claims to the contrary); and when so tested are extremely brittle to minor changes in important variables, contradicting the claims for "robust conclusions".

These technical criticisms are more deeply significant than any arguments over substance. Firstly, they do not involve taking sides on the highly emotive policy options themselves; and secondly, they go beyond the question of inputs and their selection to ask how the models controlled such inputs and revised them. The whole point of formal modeling is that it should correct inevitable biases in selection of inputs by repeatedly correcting them against specified and accepted criteria. Thus if this correction process does not work, none of the biases built into the inputs will be corrected by any externally accountable, clearly specified procedure.

An immediate question at issue, therefore, is whether technical criticisms of the modeling within the overall study are relevant to the question of the authority of the conclusions being drawn from it in "speech upon speech". It is into this murky water that the debate seems always to slide whenever argument is engaged about the technical validity of the modeling. For example, the repeated defence of *EIFW* against Keepin's technical criticisms of the modeling has been virtually total acceptance, but with the dismissive rejoinders (i) that everyone knew all along what Keepin claimed as a novel and central point, that the models' outputs were effectively a direct 1:1 "transformation" of their inputs; and (ii) that the models were anyway only a minor part of the analysis leading to the scenarios and conclusions [38]. Yet this assertion is contradicted by the study's own documentation and indeed by the response of other experts who have found Keepin's analysis surprising (at least in the extent of the limitations it reveals) and interesting [39]. By examining descriptions of the models, their use in creating scenarios, and the conclusions drawn from these and associated analyses, I will try to place in perspective the role of the models. This task is made difficult by apparent inconsistencies, such as statements in one place that the scenarios "form a central [part] of the comprehensive account of the group's activities ..." [*EIFW, Vol 2*] and "... were derived by using a linked set of models and procedures" [40], contrasted with assertions elsewhere that "the scenarios constitute only *one* of several levels at which we analyzed the energy problem ... a partial exercise with numbers ... so much emphasis on just the scenarios ... is therefore regrettable" [41]. This examination should help to clarify the sources and kinds of authority underlying the IIASA ESP's policy conclusions. The substantive merits or demerits of those conclusions are of no relevance to this interpretation. The role and representation of analysis in policy is the sole focus of our concern.

II. THE ROLES OF MODELS - WHAT CAN WE EXPECT FROM SCIENCE?

II.1 Models, Analysis, and Intuition

ASA, policy analysis and policy making (or at least its *justification)* have come to rely to a colossal extent upon complex mathematical models. This is despite the paradox occasioned by their broadened role in policy argument, that they are used more and more, but believed less and less [42]. At first sight, the whole point about models *is* their formalism, which should allow mathematically rigorous consistency, discrimination and testability to be achieved, to the benefit of policy. One large symposium on energy modeling was introduced by reference to such models as the policy response to the judicial call for greater accountability and explication of decision and inference rules in science for public policy [43]. A common (idealized) justification is that

> "formal models are first, testable, and second, documented, so that assumptions are clear and you can examine the data being used. Too often in energy policy matters the assumptions being made in a judgemental statement are neither obvious nor testable, also the data cannot be accessed ... judgemental models are models that are not open to scrutiny, their prejudices are obscured" [44].

Unfortunately, however, formal models may also lead to the opposite effect, of obscuring prejudices even from their authors, in a labyrinth of apparently pure technical language.

Indeed the science appeal of formal models has been so great that they have become pretty well a necessary badge of credibility in ASA. Modeling exercises have repeatedly claimed, implicity or explicitly, the authority of formal science. Thus for example one analyst berated his colleagues:

> "We have a list of quotations from Federal public officials including people in the Department of Energy, endorsing models as scientific apparati,...

> The fact of the matter is that these models are presented as the latest in scientific analysis, particularly to the public. Now the fact that you and I know better ... doesn't alter the fact that they are presented that way. The public believes it, *The New York Times* believes it, *The Atlantic Monthly* believes it, *The New Yorker* believes it, congressional staffs believe it, or some congressional staffs. The claim for these as science goes on repeatedly, especially when the heat is on" [45].

Nor is this a weakness only of public officials. According to another analyst,

> "model developers are usually aware of many model limitations or distortions that are never transmitted to other users" [46].

Thus even here the same confusions and conflicts have raged about the characterization of the analytical processes involved and the status of the knowledge produced. This has been especially true in energy policy modeling, where some reputable critics have been so appalled as to call for a moratorium on models [47].

A typical criticism was that produced by the Professional Audit Review Team of the Federal Energy Administration's (FEA) Project Independence Evaluation System (PIES) model in 1977:

> "The credibility of the OEIA's [Office of Energy Information Administration, later Energy Information Administration of the Department of Energy] models has not been established because documentation, verification, and validation have been neglected. Furthermore publications describing the current models are scarce and procedures for public access to them almost non-existent. As as result it is practically impossible for interested parties outside FEA to know whether OEIA's current models have been constructed properly and used correctly and thus whether OEIA's analytical products and forecasts can be used with confidence" [48].

A later review saw some progress from this dismal state [49], but many have recognized that the rate of progress, if any, is far outstripped by the generation of further questionable models, and by developments in the real policy world that often render the founding premises of such models obsolete [50]. Indeed, the immensely cumbersome, costly, and lackluster efforts to subject such models to proper quality controls have been hampered by apathy towards such efforts by the modelers themselves. Even where this has not engendered calls for a moratorium on models, it has stimulated a broad-based demand for critical model analysis to become a distinct institutionalized professional activity integral to model building and use in policy analysis [51]. Goldman, for example, warned that if greater professional self-examination was not forthcoming from modelers themselves, it would only be imposed eventually from outside. He also recognized the legitimate role of analysts' experienced intuition in the modeling process, but added the crucial condition, of truth in labeling the distinction to others:

> "it is simply not enough, in my opinion, merely to assess a model as a mechanical object without regard for what the modelers' insights and expertise can contribute to its performance ... the public and its representatives are (or should be) concerned for the quality of what flows from the entire model-use process.
>
> But please don't misunderstand me; I said that such "depersonalized" analyses were by themselves insufficient, not that they were meaningless or (properly interpreted) misleading. In fact I think they are necessary for an understanding of what the model *per se* does, and whether (as my wine-fancying friends might say) it is likely to travel well. And there may be good reasons ... why a part of such analysis should even be performed at a considerable remove from the modelers.
>
> I agree with those who find that modeling today is still largely an art form. ... Yet this element of artistry does not imply that no useful discussion of the product is possible. For instance, musical scholars and musicologists can and do undertake technical analyses and aesthetic evaluations of Chopin's etudes despite having no recording of Chopin playing them. Part of what composing is all about is the creation of musical works that will continue to display beauty and give pleasure when performed by others, in different places and at different times. ... At any rate I continue to ascribe value to the traditional scientific criteria of reproducibility and portability, while acknowledging that full-scale assessment must extend beyond these properties of the model to include the human elements of the modeling/analysis system. ... It seems more likely to me

that the analyst is bright, has built up a highly trained intuition in the course of working and playing with the model and its data, and should not be forbidden to contribute the benefits of that informed though unformalized intuition to the cogitations of the decisionmaker.

What is essential to maintain is truth in labeling. And so these "extracurricular" contributions need to be labeled explicitly as outputs of the modeler's intuition, not of the model itself. ... Now a more delicate point arises. If the witness giving testimony ... says in effect, like the principal in an E.F. Hutton commercial, "My model says —," when in fact the modeler operating in intuitive mode "said it" in the sense of a confession extorted in the police station's back room, with its inputs twisted and its logic "adjusted" to produce a desired result -- if this is what's going on, does the modeler have a professional responsibility to blow the whistle and try to set the record straight? My own answer is "yes" [52]".

Greenberger has also added his weight to the cause of greater clarity in representation:

"The typical policy model is not designed for ease of communication with the decisionmaker. Even the model builder may have difficulty comprehending fully the essential workings of the model. Ideally, the model should be presented to the policymaker, not as a "black box" with assumptions and data feeding into the left and results coming out from the right, but as an "open box" whose inner workings are sufficiently simplified, exposed, and elucidated to enable the policymaker to trace the chain of causality from input to output on at least an elementary and fundamental level.

Models are rarely presented as open boxes. It would be a research project of considerable intellectual content and practical significance to develop open box versions of selected models of greatest potential interest to policymakers" [53].

"... policy modeling must lend itself to testing and exploration by others than its developers. It must be possible to communicate the rationale of policy models as well as the results.

If a policy model cannot be tested, explored, and comprehended by persons not part of its development, one might expect its future to be brief and its use restricted. Yet, ... policy models have often been objects of blind reverence and admiration or equally blind awe and mistrust. They have been accepted or rejected because of the personal qualities and standing of the person presenting the results--and because of the predisposition of the person receiving the results--more than because of characteristics of the models themselves. And their role is expanding" [54].

In summary, for all their apparently greater analytical sharpness, models still suffer the same confusions as analysis generally, about the actual and proper role of intuition and judgement, and about the proper standards and methods of self-description and use. In the first aspect there are two problems; first, whether a model can ever be usable *in policy* without additional, (but perhaps specifiable) intuitive judgements; and second, whether models can even be constructed and used *technically* without incorporating intuitive judgements and unrecognized assumptions. The appropriateness or otherwise of such judgemental intervention, and its proper quality control, depends on the forums that exist to receive, evaluate and use such analyses, a point which returns us to the issue of institutional arrangements, and the *process* of policy analysis, including its communication to others.

In a later section I return to discuss models further, but next we examine the evolution from modeling to scenario construction in policy analysis.

II.2 From Models to Scenarios

According to Greenberger [55] the difference between science and policy analysis is that models in science produce testable propositions, whereas modeling in policy analysis is more pragmatic - "instruments for comparing alternative policy options". However, how does one compare alternative policy options without first producing propositions about the effects of each policy option? And should not these propositions be testable as far as possible? A common reply is that *comparative* evaluation can be performed without too much dependence upon more fundamental, uncertain or unknown cause-effect processes, because "internal" comparison within the ring of alternatives is the central exercise. However, although this may be true of relatively confined decisions typical of early OR, it cannot be remotely claimed of most public policy issues like energy. Here, "comparing alternative policy options" has perforce to pretend to generate and evaluate different cause-effect propositions: the policy options and the propositions themselves are embedded in very different fields of force that cannot be assumed to form a common background to all the options. Whether modeling in policy analysis is so unlike scientific modeling is therefore still an open question. Indeed the testability of scientific modeling is also less direct and clear-cut than Greenberger suggests; both kinds may be more deeply imbedded in a complicated circularity of correspondence between different pieces of knowledge and presumption than is usually recognized. Within its circle of correspondence, science provides greater opportunity for empirical testing of its models than does policy analysis (but even scientific testing is never completely direct and is often highly indirect). Thus policy analytic models allow more room for the play of socially generated plausibilities.

In any case, as the scope of policy-related models has broadened, so their credibility as a means of forecasting has diminished. The judgement of poets, writers and sages has been evaluated from the historical record to be at least as reliable as formal methods [56]. Thus for large issues like energy policy, the role of formal modeling has had to shift from direct forecasting to more modest functions such as simulation, in an attempt to identify and test "key" policy relationships and variables, to determine the sensitive policy factors, the elasticity of "constraints", etc. Often this "*testing*" has little or no database, and relies upon synthetic data (which in turn often rely upon untested assumptions) or other theories with which results should correspond. Unwarranted credibility has then often arisen because of the lack of awareness of the non-empirical reference points surrounding the whole exercise [57].

One approach that tries to overcome the forecasting problem is scenario construction. Scenarios are not forecasts but thought-experiments or hypothetical projections, the point being to generate imaginative thinking about options and constraints, and to work through the implications of plausible assumptions. For all their emphasis upon imagination rather than measurement or calculation, scenarios usually embody some kind of self-consistency principles as a means of control; otherwise the comparison of projections under different assumptions would not be possible. Sometimes these principles may involve a formal model, even if only an accounting model to add up the cumulative effects (e.g., of primary energy output) resulting from basic assumptions (e.g., about different resources, recovery rates, technological efficiencies, capacities, etc.).

Whether scenarios are or could be an alternative to the problems of models is not at all clear. In the US CONAES energy policy study, for example, scenario construction was looked to as a means of reconciling and synthesizing the conflicting models produced and used by different working groups within the study [58]. Often scenarios are constructed with "plausible" upper and lower bounds of what are thought to be key variables, in the hope that "the real world" will be captured within the band. In a sense this is a kind of crude attempt to specify an all-in sensitivity of the system of interest.

In the IIASA energy study, as we shall see, scenarios were strongly emphasized, and were lent plausibility by the use of formal models to check them. Precisely what scale and rigor this checking entailed, and what relationship the scenarios bore to the "robust" policy conclusions and "factual" bases for policy making that the study then claimed, are a matter of (disputed) interpretation. A serious problem is that the apparent plausibility of a scenario may allow the analysis to leap ahead to the formulation of a further analytical problem for policy, which, in order to be taken seriously, necessarily presumes that the scenario is not merely a thought experiment, but in some way captures the key parts of reality. What begin as hypotheses often end up as certainties without even being properly tested. Thus uncertainties and questions at this earlier level of scenario construction may be inadvertently left behind and neglected as the analysis (and policy making if it is not careful) plunges prematurely ahead into a whole round of flimsily conceived secondary problems and questions.

Goldman's account of formal models as only the tip of a murky policy analytical iceberg, cautioned:

> "Another chunk of the iceberg involves scenario formulation. Policy problems, *in vivo*, do not generally arise nicely formulated in terms of particular settings for the parameters of a model. If I would like to influence what answers a model gives, let me be the one who formulates for that model the scenario describing the decision problem at hand. Grant me that privilege, and I can probably make that model dance to pretty well any tune that is desired.
> But if the input channels to the model are too well-guarded for

> any flimflammery there, then let me be the one who interprets the model's outputs for the user, reading the entrails through appropriately tinted lenses, applying "subjective adjustments" in what Freedman calls "the exercise of judgement through the back door," and standing well-poised to exploit what Arthus describes as the "contrast (of) rigidity of the fact with the plasticity of its interpretation." And this area, that of the provisions for judgemental adjustment and communication and explication of model results, is yet another piece of the iceberg that requires exploration if the model's performance and merits in actual use as a decision-aid are to be well understood" [59].

Scenario construction is thus catalogued as yet another arena for possible "flimflammery" alongside other subjective adjustments referred to earlier. As with modeling *per se*, this does not justify the abandonment of scenario construction, but it is an authoritative expression of the need for rigorous clarity as to how assumptions, models, calculations, scenarios, "subjective adjustments", and inferences to policy conclusions are demarcated, and how they relate to one another.

II.3 Models, Megamodels, and Model Sets

Models are central to knowledge and communication. They entail some kind of attempt to describe and interpret reality [60]. As Box has put it, "all models are wrong, but some are useful" [61]. However, most models are mental models private to individuals or groups with shared values and meanings. As such they are partial, vague, shifting, largely implicit, and thus cannot be subjected to external checking and evaluation. Informal models may act as a valid basis for knowledge within given subcultures that share sufficient experiences and meanings ("values") so that the unspecified aspects, having taken-for-granted validity and authority, can stay tacit. This provides social solidarity and linguistic economy, yet with flexibility to negotiate new meanings and values.

Such "models" are more rooted in concrete social interaction and often have value just because of the ambiguities they contain: they are less recognizable as "models" than those used by science. In such a form they are not ready to act as a valid basis for overall *public* knowledge, which should be accountable, that is, specifiable and in principle reproducible by others.

In the socially differentiated settings typical of modern society, knowledge and language, along with their constituent models, have become increasingly explicit and elaborate, since no shared meanings and values can be assumed [62]. This formalization - explicating underlying models in detail, thus rendering them available for cross-checking - is supposed to reach its most extreme and authoritative form in scientific models. Science is in theory a kind of anti-culture, being supposedly motivated by and organized on pure skepticism and a refusal to share any belief until it has been independently corroborated by "nature" [63]. In the ideology of modern society, therefore, science is taken to be the only basis of overarching authority (in the faith of Eastern and Western blocs alike) not so much *uniting*, but rather revealing and imposing a superior, unified authority over the many subcultures and fragmented, partial rationalities that make up society in general [64]. The claim to such authority is based upon the supposedly completely accessible nature of scientific models.

Usually a given model will be a submodel of a larger model, which in turn may be a submodel of a still larger model, and so on. At some point on this escalating scale, the "model" will naturally become less precise, and more informal and flexible. It will also usually become more speculative, more open to broad value biases, and less accountable. Conversely, at some point on a diminishing scale, the submodel will become a trivial "calculation" such as a simple accounting scale. In the latter case a "model" may still be useful for checking the bare numerical consistency of multiple assumptions, e.g., that national supply and demand projections must balance. But this must not be confused with *verifying* those assumptions - it only means thay are not false in one of several dimensions, measured against other unverified assumptions. This small scale of consistency was confused in the IIASA documentation with larger-scale consistency, implying that a real validation of the assumptions had been achieved.

In talking about models, therefore, especially if deriving authority from them, it is important to be clear about which model level within the nested set one is referring to.

In order to be useful, valid, and to serve as the basis of public authority, such models would be expected to lie between the two extremes of triviality and informality; in other words:

(a) Models are expected to calculate something from specified inputs in specified functions or algorithms representing the essential features of the part of reality in question. Both the inputs and functions can be specified hypothetically for testing, but normally the colossal investment and time involved in constructing the functional specifications of models means that they are inflexible in their internal structure once created.

(b) Models should be designed so that iteration takes place within the model through clearly defined algorithms, in order to evaluate outputs of substages in calculation (submodels) and to revise relevant inputs to obtain consistency. This should be a clearly defined, systematic process.

At a larger level, models should be located in a process where the outputs of the calculations can be used to evaluate and adapt the inputs (and/or functions) for another "iteration" of the outputs against accepted criteria so as to re-evaluate the inputs. This is crucially different from merely ***re-running*** a model. Iteration is a formal process as outlined in Figure 1(a). Re-running involves changing the inputs $\left[I_0 \text{ to } I_1, \ldots, I_n\right]$ without any clearly defined, controlling relationships between the output O_0 and I_1 (O_{n-1} and I_n), see Figure 1(b). This distinction is vital when it comes to perceptions of consistency and credibility of outputs, as we see later.

(a) Iteration

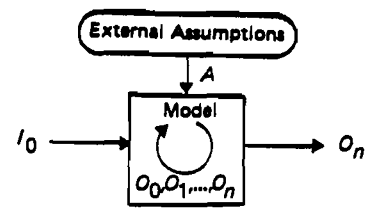


FIGURE 1a Iteration: The model includes an endogenous iteration algorithm. This built-in feedback mechanism updates and corrects O_i, to produce O_{i+1}. Hence, after repeated iteration, the "initial guess" O_0 is transformed into the final output O_n (note that the external assumptions (A) are not altered in this process). This is the kind of iterative process suggested in the documentation of the IIASA energy models.

(b) Re–Running

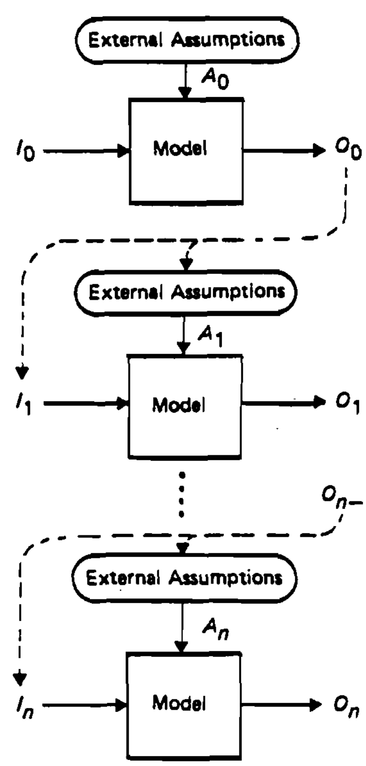


FIGURE 1b Re-running: The first run of the model produces the output O_0V which the user considers unsatisfactory. Therefore, the user changes the inputs (I_1) and assumptions (A_1) and then runs the model again. Thus the model is not iterative; rather, it is run repeatedly until the user is satisfied with the output O_n. This is the undocumented, informal procedure that was actually used to produce the IIASA scenarios.

Iteration is no more nor less than the (idealized) normal process of science, which is supposed to be intrinsically self-correcting through skeptical testing of its hypotheses. The corrective "algorithm" is goodness of fit with empirical observation if possible, and with accepted canons of theory and logic where not. In systems analysis this has enjoyed far less empirical reference, and has usually been computerized due to the level of aggregation and complexity of the systems being "analyzed". Clearly specified iteration is a central and necessary component of any analysis that claims the authority of science for its conclusions. Thus the *prima facie* credibility of an analysis is bound to be questionable if it does not show how its iterations can be reproduced.

(c) The main parameters of a model must be subjected to sensitivity analysis (SA). This is especially important if (as is now usual for complex policy systems) a model is aiming not to predict future states, but rather to identify key real-world parameters and dynamic relationships which will be either constraints, points of policy leverage, or social options. This is another way of systematically checking the selection of input assumptions, since selection must inevitably take place, and is itself a reflection of social assumptions and values. A respectable scientific modeling effort will thus perform and describe SAs in such a way as to allow others to reproduce them. This SA also allows evaluation of the *robustness* of outputs (or conclusions derived from them) to small changes in model parameters or inputs. If such outputs change wildly with small changes in an input parameter whose real-world value could easily vary within that range (or where the uncertainty as to its value is already of that range or more), then those outputs are *prima facie* not ones on which to make commitments.

Thus, to summarize, a scientific model -- that is, one claiming automatic public credibility as opposed to normal bias and arbitrariness -- should calculate something from specifiable inputs and model functions; be corrected by clear and reproducible iterations; and be subjected to systematic, documented, and reproducible sensitivity analysis, in order to test those selected inputs and model features of the real world for their validity and relevance. Although a model set is not used *directly* to derive policy options, but instead underpins scenarios which are so used, this does not mean that the models are exempt from evaluation in these terms. If the models are found wanting, this does not mean that the scenarios are of no use, but they naturally become a less credible base for any policy pronouncement

No scientific endeavors have ever fully achieved the standards outlined above. Nevertheless, they rightly exist as an institutionalized heuristic or normative framework, to guide and evaluate analytical practice.

II.4 Formal and Informal Modes of Authority

Throughout the history of science, but more acutely since its vastly expanded public role after World War II, there has been a tension in the way that science has been portrayed to its public audiences, in order to gain authority. On the one hand, there has been the rationalist, formalized account of science in which supposedly all its determinants can be precisely defined (data, logic, experimental methods, observational criteria, calculation, evaluative criteria, etc.) so that scientific knowledge gains its authority by being thought to be utterly and completely accessible, and *testable*. Even if everyone could not so test every piece of knowledge, the public trust that others competent to do so are regularly doing just that, has given this image of science a powerful role in its public authority [65]. One can see immediately how scientific computerized models would correspond with this ethos.

However, there is another, opposing image of science and its mode of proper public authority that has coexisted with this rationalist image. This has fed upon the realization that scientists cannot possibly give a complete and precise account of all the complex elements of scientific knowledge. In this view there are inevitable tacit components to scientific practice and thought which make it akin to a craft skill [66]. Thus refined intuitions, judgments and faiths that cannot fully be explained and objectively justified, are essential to science. For its authority with non-scientists, this image of science relies upon the idea that scientists alone have been initiated via long and arduous apprenticeships into the esoteric craft skills which differentiate their competence to judge from that of nonexperts. This authority is ironically based upon inaccessibility, but only to the extent that the external audience (e.g., the public at large, or political decision makers) trust the institutions concerned. This requires a pre-existing shared cultural context — a given authority system — like those enjoyed in the reign of other priesthoods, for that is essentially the mechanism of this kind of authority.

This image of science implies authoritarianism and has usually been cultivated by conservative regimes or advocates, whereas the rationalist "enlightenment" image has usually been associated with democratist programs [67].

Both these ideal types are artificially purified images and can be regarded as a kind of rhetoric for public consumption, concealing and giving informal license or authority to the much messier activities and outputs of science that they are supposed to be describing. They are in a sense alternative authority rituals. Each style of self-representation has its own benefits, but also its own disciplines and intrinsic controls against absolute unlimited authority. The rationalist image requires some *real* accessibility and accountability to buttress the public rhetoric; and the priesthood image requires some given authority structure and a previous track-record of *effective* craft skill and trustworthiness.

As we shall see, the IIASA study is deeply muddled over its own nature: the result is to make it look as if it is trying to reap the benefits, yet avoid the disciplines, of both kinds of public self-representation. This is particularly important because of the naturally debatable social assumptions locked into and obscured by the "factual" definition of the analysis.

III. THE ROLE OF MODELS - THE IIASA ENERGY STUDY

III.1 The Logic of Justification

The IIASA study clearly draws upon scientific claims for its public authority. A typical claim is that:

> "over the years more than 140 scientists from more than 20 nations, East, West, and South alike, have for longer or briefer periods joined the programme. ... Amory Lovins ... and others have participated in the study. An explicit attempt was made to incorporate as many views and to be as objective as possible. The idea was to understand the factual basis of the energy problem ... it was not the intent to go into the political or societal aspects"[68].

However, there are no acknowledgments here that "many views" such as that of Lovins were not only not "incorporated", but indeed have engendered mutual condemnation over the study [69].

In order to see how the IIASA models relate to this outdated fact-politics division we need to analyze the relationships between several levels of work involved in the overall study:

The logic of justification is as follows:

1. *There will be a factual approach, excluding all social and political factors.*

2. *However, orthodox prediction is out, because of "the unavoidable uncertainties" [EIFW, p.425] therefore scenarios will be written.* These will aid the rigorous identification of the main real world variables and relationships so as to define policy-specific options and constraints.

   > "Together, the scenarios and the sensitivity analyses should build both a broad enough understanding of the vital characteristics of the energy problem and a set of sufficiently specific facts so that conclusions and

recommendations for the energy transition [to a sustainable future] can be formulated" [EIFW, p.425].

3. *Although these scenarios are not to be evaluated and criticized as predictions, they are nevertheless central to the overall policy conclusions.* To reach its particular physical solution to the energy problem the IIASA team "relied greatly on the quantitative analysis of our scenarios". [*EIFW, p.778*] Furthermore, "these two [high and low] scenarios ... span a sufficiently wide range in order to incorporate the unavoidable uncertainties"; [*EIFW, p.425*] "They are a means of spanning the conceivable evolutions of global energy systems". [*EIFW, p.656*] The scenarios are therefore "central" [70] to the overall conclusions and they define the boundary of possible futures in terms of the factors considered.

4. *In order to have value, the scenarios must be analyzable, and this means that they must be quantifiable.* The key criterion claimed for the scenarios, in addition to their global scope, is *internal consistency* [71]. Models of the system are needed for quantification, so as to avoid, e.g., double allocation of the same barrel of oil (an example used to illustrate how the models safeguard consistency in the scenarios).

It is important to distinguish this limited kind of consistency from that implied by iteration of the model loop. Unfortunately, as discussed later, the IIASA group's own confusion about iteration has led to the belief even amongst other professional modelers that a larger-scale consistency (and thus more real and robust outputs) was being achieved with the models.

The scenarios are supposed to derive their authority from their internal consistency, which is achieved by the models plus the vital control process of full-loop iteration round the feedback loop outlined in the widely publicized Figure 2. It is stated unequivocally that "The High and Low scenarios are the results of applying the ***model loop*** iteratively until satisfactory consistency was achieved" [72] (my italics). The computer models were the "principal tool" used in building the scenarios [73].

Reversing this overall logic therefore: The models are essential to make the scenarios quantified, and thus consistent, analyzable, robust and credible; the scenarios span the conceivable range of futures -- "***our scenarios are globally comprehensive and allow for no escape***" [*EIFW, p.785*] (italics in original); this analysis via the "set of highly iterative models" [74] leads to policy conclusions that are claimed not only to be "robust" [75], but in some important dimensions downright inevitable [76].

Thus, by all published accounts, except significantly, a very recent one [77], the models are claimed to play a key role in the arrival at overall policy conclusions. "The assumptions and results [from MESSAGE, one of the model set] ... represent in some ways the core of the energy studies reported in this book". [*EIFW, p.402*] As we shall see, these accounts of the importance of the formal models in the IIASA study were taken up by other experts. Criticisms of the models as analytically vacuous are therefore far from irrelevant. Patronizing, even insulting dismissals of Keepin are indicators of the quality of the forum of professional quality control itself.

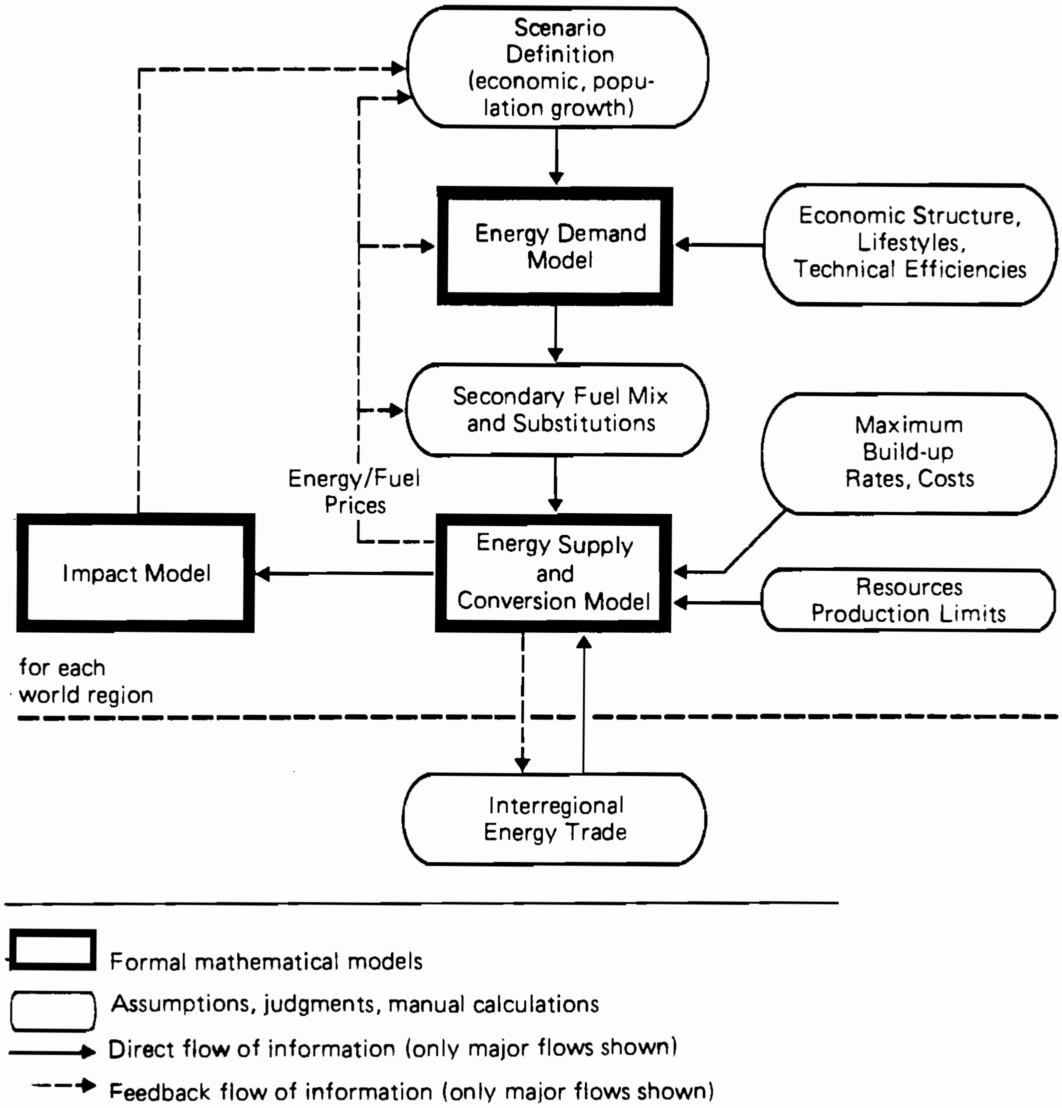


FIGURE 2 The IIASA Energy Group's *foremost* representation of the role of energy models used in constructing the scenarios (from the *Executive Summary)* 1981.

What is important is that the deemed significance of the models appears to have been changed by their users as criticism of them has developed and sharpened. Thus, for example, it was in response to Lovins' critique, that the scenarios (and thus the models) were clearly placed in more modest perspective, as "only *one* of several levels at which we analyzed the energy problem. The scenarios constitute a partial exercise with numbers, the meaning of which is to assist in reaching qualitative robust insights that can be gained only when these numbers are considered together with the results of the complementary analytic levels" [78]. This suggests a more modest status for the models and scenarios than given in, for example, the *Science* and *Futures* articles [79].

The previous claims to scientific authority for the policy conclusions based upon the claimed quantified accessibility of the scenarios to competent peer evaluation, has now been altered to the dependence on the less externally accessible and vaguely described consideration of these with "complementary analytic levels". In a more recent publication this shifting of ground is taken even further. The process of scenario writing is described as in Figure 3. Here, for the first time ever,

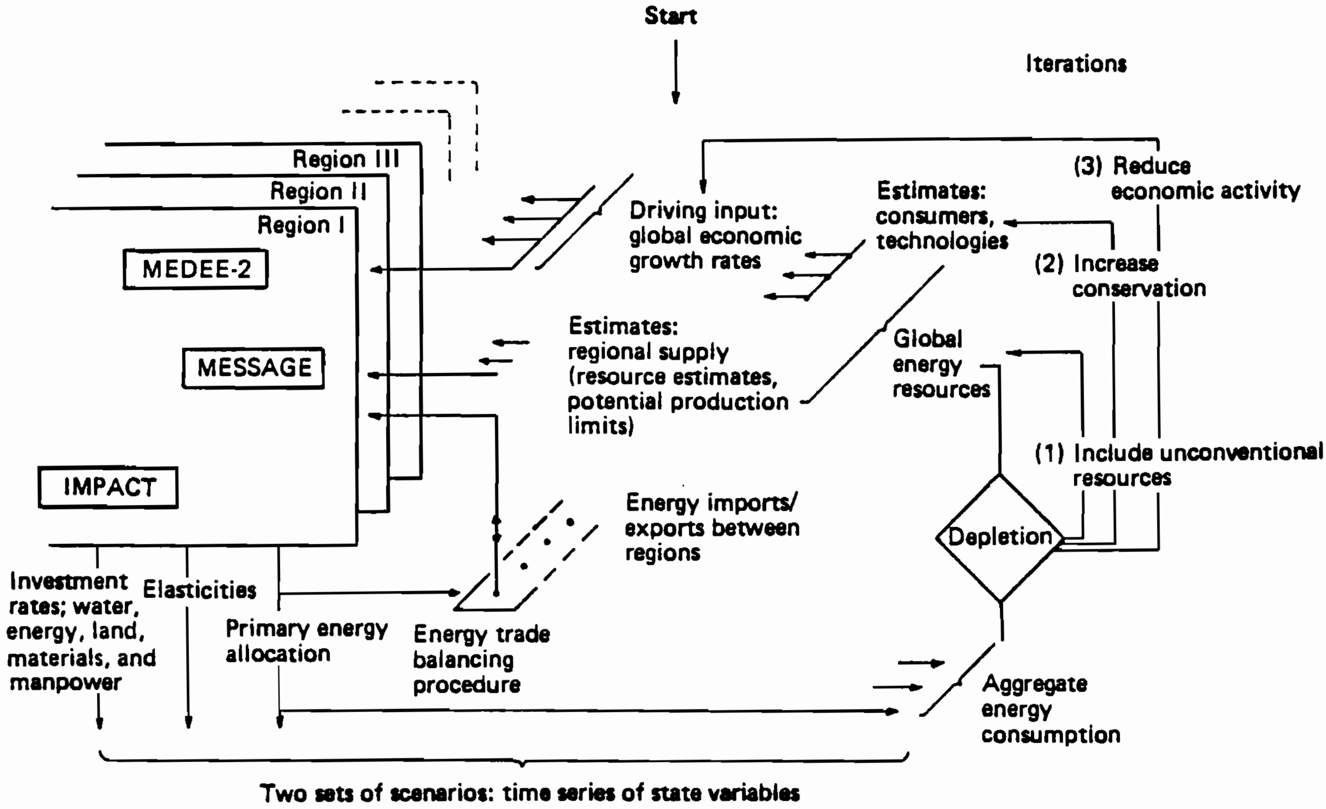


FIGURE 3 Shifting ground: the IIASA Energy Group's *new* representation of the role of the energy models. 1983 (from [77]).

two years after the main study results were published [80], the formal models are seen to play the minor role which the Energy Group claimed that everyone knew they had been playing all along, contrary to the group's own previous public accounts. The burden of credibility has now been lifted from the models and placed on the undocumented informal processes in the private hands of the analysts: largely subjective "judgemental interventions" which were earlier confused with descriptions as full iteration of the model loop [81] and "mechanized" flow of information. [*EIFW, p.400*] What was advanced as analysis controlled by formal iteration now appears to have been dominated by undocumented subjective judgement. This controlled - and as Keepin shows - determined the outcomes of reruns of the models.

Since this is a crucial issue it requires more attention. When Keepin's technical demolitions of the models were advanced, the IIASA group replied that it had never claimed that the models were a significant part of the project's overall means of reaching policy conclusions, and that they had always made very clear the lack of ***formal*** iteration round the model loop. Whilst there are statements about judgemental interventions, even some of the project's own foremost self-descriptions contradict these assertions, and these self-descriptions have naturally been taken up by other experts, as we shall see. First, however, a brief historical review of the descriptions of the models is necessary.

III.2 The Model Set, An Historical Review

The original aim of the IIASA modeling effort was to link at least four main models - MEDEE, MESSAGE, IMPACT, MACRO - in a full feedback loop [82]. This was never achieved, but some papers are, to say the least, ambiguous about the real state of implementation of this full set. Before going on to this however, it is worth recalling that there are two aspects of the implementation of the model set of interest to us. First, there is the question of whether or not a given model was implemented at all, and second, there is the question of how the outputs of one model were fed on to become inputs to the next one. Both aspects of course affect the flow of "knowledge" through the loop and how it was controlled or checked.

In a paper published in 1978, Basile and Häfele described the central core of the model system as follows:

> This network of energy models was largely conceived by, and receives overall direction from, W. Häfele. Coordination and monitoring of the effort is done by P. Basile. These many elements in the energy modeling effort are intended to bring synthesis to the Energy Program. But the synthesis requires - demands - consistency. The modeling recognizes this through its feedback loop design. The central feedback, from the economic impact model to the macroeconomic model, compares the requirements for a given energy strategy with the initial assumptions about economic development, to discover if the economy is able to absorb the energy requirements. Usually this will lead to inconsistencies, and the whole procedure is iterated. It is not our intention to strive for a pushbutton procedure; it is the evaluation and comparison of energy strategies that is the purpose of this exercise" [83].

Several points are worth noting. First, in view of the preceding section, the models are given a central role "to bring synthesis" to the whole Energy Program. Second, this model-based synthesis evaluates and compares energy strategies, which implies some testable propositions about the effects of alternative strategies and the evaluation of what would be a feasible range. Thus, for example, "determining whether or not such [unprecedented] investments [in capital-intensive energy production] can or will be made is a matter for the formal modeling effort at IIASA" [84]. This confirms the argument drawn from other statements (Section III.1) that the study is still claiming *predictive* force, albeit more sophisticated and elusive than direct prediction. Third, the "central feedback" is said to be from IMPACT calculations back to MACRO's calculations of economic feasibilities, as given in Figure 4, which is said to describe "the overall organization, structure, and *implementation* of the models" (my emphasis) [85].

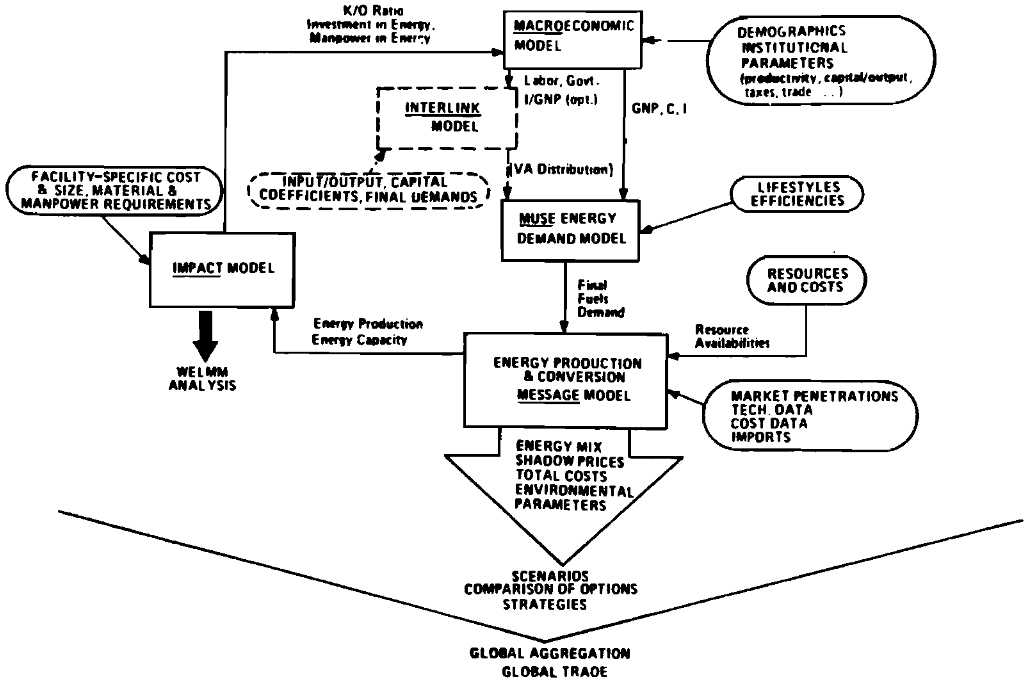


Figure 4 The IIASA set of energy models for a developed region of the world: flow of information. 1978. (from [82]).

There is here a clear claim that consistency via feedback is achieved at the macrolevel embodied in the full loop, and no indication that IMPACT and MACRO were never implemented. The language is quite categorical - "compares the requirements ... to discover if the economy is able ... the whole procedure is iterated". MACRO was said to be "developed ... and further implemented" [86]. Finally, in reference to avoiding a "pushbutton procedure" there is muted recognition that informal judgemental intervention enters into the process to adapt outputs and inputs. However, the discussion presents the benefits of being able to intervene flexibly with "constraints based upon non-quantitative logic" (not "subjective judgements") without acknowledging any of their drawbacks. Indeed, amidst muted recognition of informal judgements there are unequivocal claims for formal clarity:

> "Finally, the formality of computer models or of the analytic frameworks is of high value for a particular reason. All policies and decisions are based on some implicit view of the future or a range of futures. The formal structure of models enables these assumptions to be explicit and subject to audit. This can serve as a defense against bias in decision making" [87].

These statements are difficult to reconcile with contradictory statements about the use of "non-quantitative logic" etc. It also undermines later claims that the models did not play a central role. Although it is claimed that "criteria and procedures" are introduced for handling the "complete interactive feedback" through the models as a whole, these are not described. As Keepin has shown and as I will discuss later, even when results of these "procedures" are exemplified, they produce rather pedestrian "insights" such as that if electricity prices increase relative to other energy prices, market penetration of electricity may slow down [88]. They do not show how much the models' outputs of such factors fluctuate for small relative price changes.

In a closely related paper [89] containing many verbatim passages but authored this time by Basile, and delivered to a more specialist energy modeling seminar in 1980, there are more references to the need for expert judgement of the modelers to intervene between model runs. In this paper also, in the presentation of the model loop (Figure 5) a distinction now appears between "direct" and "feedback" flows of information and the MACRO model, although still in the main feedback loop, is now footnoted as "not yet fully implemented" [90].

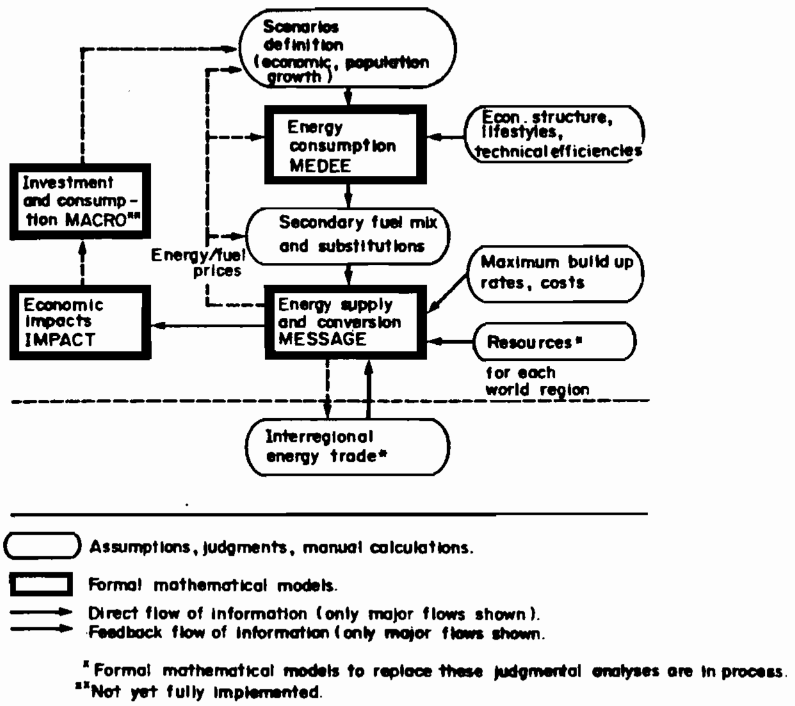


FIGURE 5 The IIASA's set of energy models: a simplified representation. 1980. (from [87]).

Even so, the implementation of MACRO is still clearly regarded as a central requirement for the exercise. After describing what IMPACT "calculates", the paper continues:

> "Finally, a MACRO economic model accepts exogenous assumptions about demographics and institutional parameters such as productivity, taxes, trade, etc., and calculates investment and consumption rates consistent with the costs from IMPACT. This allows assessment of the magnitude of change in, for example, the capital/output ratio if and when energy becomes increasingly capital intensive. This in turn enables a re-check of the original GNP estimates for each region and a re-entering of the iterative process.
>
> This last feedback is one toward which much of the energy modeling design and implementation work at IIASA has been leading" [91].

When we come to *Energy in a Finite World*, we find that MACRO, originally part of the "implemented" central feedback with IMPACT, and towards which the whole modeling exercise was being designed, has disappeared (though to add to the confusion *EIFW Vol. I*, the summary version, printed the four-model loop including MACRO) The language now is that MACRO "*could*" [*EIFW, p.403*] (my emphasis) do all of the things which we were previously told it was already doing. A footnote tells the reader that "it is not yet implemented as part of the modeling loop" [*EIFW, p.403*]. Although there is recognition of judgemental interventions, these revert to to the muted descriptions of the Häfele-Basile paper, and again present only the positive aspects of informal judgement. Again also, the reader is referred to the same trivial examples as accounts of how the criteria and procedures driving such judgemental interventions work. As Keepin shows [Keepin, p.37] some significant judgemental interventions were not recorded at all; indeed, there were so many of these it would have been impossible to give an account of them and render them available for impartial inspection. A further shrinkage in the full model set occurred later when it was admitted that even IMPACT was "a side activity not part of the full loop" [92] whose outputs were used "in a monitoring capacity".

The insight suggested by this historical outline is that there was initially an attempt to link together a multiple-model set of at least four models. Even as late as 1978 through sincere wishful thinking and faith that it was going to work, this was being described as *accomplished reality*. Consequently the role of subjective judgemental intervention was understated, and probably under-recognized. When the originally central models MACRO and IMPACT did not deliver, the role of subjective judgement and "interactive" iteration as opposed to formally controlled iteration was naturally amplified, though its full extent was never clearly and forthrightly stated. Indeed, the *Executive Summary* was directly misleading on the point, as was a later technical document. For example, describing the same three-model figure of the model set (see Figure 2) as that in *Energy in a Finite World*, the *Executive Summary* stated:

> "in reality, as is usually the case with such sets of models, they were used in parallel and iteratively. The object was internal consistency within each scenario, which in turn required several iterations of the model set. The major consistency checks are suggested by the dotted lines of Figure 11 [our Figure 2]" [93].

The lack of any mention of subjective judgemental intervention or even of non-quantitative logic here again firmly plants the belief that large-scale consistency checks round the whole model loop were formally carried out to make the scenarios a factual basis for policy.

A more forthright assertion is made in another (1983) document, this time a 580-page technical description of the tools and input data to the models: "the main model loop is closed with IMPACT", and "the High and Low scenarios are the results of applying the model loop iteratively until satisfactory consistency was achieved" [94].

Yet more recently, as we have seen, the Energy Group has admitted in response to Keepin [95] that no such full-loop iteration was performed, as this term is usually understood, and claimed that everyone competent knew this. MACRO was dropped. IMPACT also did not work properly. For example, although IMPACT was claimed to have calculated manpower, land, capital and other resources needs for the scenarios, key factors such as capital availability and environmental constraints were not included except only vicariously and uncontrollably, embedded in independently and arbitrarily assumed factors such as capacity build-up rates and capital output ratios.

The adjustment and control of inputs (e.g., on economic growth rates or energy efficiencies) from the output of e.g., required capital investments to achieve a given level of energy use consistent with those economic growth rates, simply never happened, even though this is what the claimed consistency on this scale would have involved. As we have seen, other accounts state that such consistency checking *was* achieved in the full-loop iteration. If this large level of consistency had really been achieved, the lower level of consistency referred to (e.g., avoiding double allocation of a barrel of oil) is unimportant. The missing reproducible iteration of the full loop, and the apparent lack of clarity and consistency within the accounts themselves, inevitably weaken the credibility of the scenarios and what was derived from them. It is also important to note how these confused accounts led reviewers to believe that what was actually only a relatively minor technical consistency (part of model validation) was a large-scale formalized consistency in the whole analysis (real-world validity). Clarity has been lent to this situation by Keepin's analysis. As I will discuss in Section III.7, however, this brief historical overview indicates the great influence of institutional forces upon the analytical process, its content and representation including institutional faiths naturalized into "reality". Rather than imagine that analytical designs are planned, implemented and adapted in a social vacuum, it is worth emphasizing that the cognitive substance of analysis is deeply rooted in its own institutional realities and commitments, which therefore need to be examined in order to evaluate the analysis itself.

III.3 External Perceptions

With such self-descriptions as those documented above, it is easy to see why the IIASA ESP modeling could have been taken by others to have achieved a larger-scale consistency and thus a greater status for the scenarios than was actually the case.

A 1982 review of energy-economy models, for example [96], describes "The IIASA set of energy models" as in Figure 6 taken from a 1979 IIASA publication [97].

Here we see MACRO squarely in the loop, and the whole elaborate system is described not as a hypothetical modeling *ambition*, but as operating reality:

> The modeling activity begins with scenario definition, using as key variables economic and population growth, for each one of the seven world regions established by IIASA. The scenario projections for economic and demographic growth provide the basic inputs to MEDEE - a detailed model of energy uses. The output of MEDEE, assisted with some additional assumptions about substitution across secondary energy carriers, is fed to MESSAGE. This is nothing more than a dynamic linear programming model which determines the optimal structure of the energy system, given a set of energy demands and within a given economic environment. The model produces, through a cost minimization procedure, an energy supply mix, together with supporting detailed information about total costs, shadow prices, environmental parameters and energy production and conversion facility requirements. *The MESSAGE model is further linked* to IMPACT, a model which calculates direct and indirect requirements - in capital, labour and other resources - of a given energy strategy. *Thus the basic information for assessing whether or not an economy* can afford a given energy scenario, is provided. Finally, a MACRO economic model calculates investment and consumption rates consistent with the costs of IMPACT, *thus providing for the closing of the feedback loop* with the original economic growth scenarios [98]. (my italics)

Another modeling expert, Dennis Meadows, worked with the Energy Group for three months in 1977, and later spent three months with ten graduate students studying the models, but by 1983 even he was unaware that the Group considered the models to be insignificant compared with the informal judgemental processes modifying their inputs and connections [99]. He was also surprised to learn that the model behaves like a mere identity, performing essentially no dynamic calculations.

Another review partly authored at IIASA compares the IIASA study with several other energy modeling exercises [100]. Although it correctly identifies the judgemental elements influencing model runs in the IIASA study, it criticizes other studies, but not IIASA's for not being grounded in formal models, or for having models play only a small role in the outcomes. The implication is that, despite judgemental interventions, the ***major*** role in the IIASA studies is still played by the models. It is also difficult to understand why these reviews could specifically exempt IIASA's study from being "notably arbitrary" in calculation of fuel shares if they already knew of the extreme brittleness of MESSAGE [101].

In the light of this documented evidence, it is impossible to accept that it was common knowledge that the IIASA models were trivial, and only a minor part of the enterprise. It is hard to avoid the conclusion that even experts have been inadvertently misled by the way the models and scenarios have been represented in publications by the study's authors. If they have been so misled, then what of the vast majority of people less involved and not expert on the subject? As Meadows himself put it, "The models are by no means the sole justification for Häfele's views. But many readers of IIASA reports will assume that they are the source and justification for the conclusions he reports. They are referenced often and described prominently in the detailed and the summary versions of the Energy Systems Program's final reports" [102]. Evidently the past tendency has been to shift the burden of public authority onto the apparently more scientific, formally accountable part of the project, namely the models, whose justification is supposedly their availability to reproducibility testing. When these have failed (as with MACRO and IMPACT) or been successfully criticized (as with MESSAGE) the burden of credibility has then moved onto less firm terrain, namely the expert judgement of the analysts, and their unique access to the "craft" mystique of the analytical process. As already discussed, this presents its own credibility problems even if consistently expressed; and as we shall see when we examine the issue of sensitivity analysis, in this case the "craft" criterion failed even its own test of comparison with accepted formal procedures.

III.4 The Iteration Problem

The issue of iteration in the IIASA energy study gives a good illustration of the problems of accounting and representation. Iteration is central in analysis and science in general as the means by which starting assumptions and hypotheses may be checked and corrected, if not against directly observable empirical phenomena, then at least against other accepted bodies of knowledge. It is the heart of the process of correction and validation which is a ***sine qua non*** of reputable analysis. The process may, of course, be more formalized in clearly defined relationships, criteria, etc., or less so. The less it is formalized does not ***automatically*** mean that the knowledge produced is less true, but it does mean that it is less capable of being checked, corrected, and evaluated. Knowledge produced by informal iteration asks us to take on trust the wisdom, vision and impartiality of the experts involved, because their guiding rules and principles are invisible. When one looks for indications on these counts one is not reassured by: (1) the fact the the early origins of the IIASA study lay in the plan to examine a transition from fossil to nuclear energy systems; (2) the reference to lowered supply-demand projections as "pessimism"; (3) enshrined assumptions that all energy will be produced in future by more capital-intensive means; (4) that when its choice of sensitive parameters was tested against formal methods that were available but dismissed, the craft judgement of the same experts failed, as we shall see; (5) the assumption that the underdeveloped world will undergo inevitable urbanization and industrialization along the model of the present developed world; (6) the fact that all the undocumented judgemental inputs and other quirks that Keepin found in the models (such as the LWR price step) favored advanced nuclear or coal synfuel systems; or (7) that despite claims to have objectively incorporated a wide range of views and to have thus developed scenarios that encompass future possibilities, IIASA's *low* scenario for 2030 is still much higher than other reputable studies of global energy futures [103].

It must again be emphasized - these are not reasons for dismissing the judgements of the IIASA team as wrong. This paper is ***not interested*** in the substantive arguments, e.g., for and against "hard" or "soft" energy futures. But they are reasons for wishing that more attention had been paid to the documentability and accountability of such informal judgements both to other modelers, to policy users, and (eventually) to the lay public.

As we have seen, the received impression that iteration was carried out to give consistency around the full macroeconomic loop was completely false. The aim of this paper is not to judge whether or not this was inadvertent. The fact remains that it was seriously misleading because it implied validation on a scale that, had it been real, would have given substance to the claims for the scenarios. The example of evaluating the achievement of supply-demand equilibrium by either supply expansion or demand reduction is instructive.

In the IIASA model set, MEDEE-2 adds up the total energy demand generated by a set of (exogenously specified) detailed assumptions about technologies, housing standards, transport changes, population, economic activity, etc. MESSAGE then sets about disgorging this amount of energy from its fixed repertoire of also exogenously specified supply options and *relative* costs. The decision rule or objective function is a simple cost-minimization. As Keepin shows, the program runs through the whole of the cheapest option until exhausted, then suddenly leaps to the next (arbitrarily fixed) fuel price category (even if this is only infinitesimally more expensive in the fixed cost assumptions) and exhausts that, and so on, until MEDEE-2's demand is satisfied. Now the consequence of a proper, full-loop iteration would be that all the economic costs in terms of labor, capital, land, water, and other resources including environmental costs, would be calculated (in IMPACT) for MESSAGE's supply configuration. The implications for the rest of the economy (e.g., draining capital from other investment and production including investment in energy-saving technologies) would be calculated by MACRO, as was claimed. This would then adjust estimated economic growth, technological innovation rates, etc., accordingly, and feed these into MEDEE-2, which would in turn adjust its total demand computation. If this were to happen as advertised by the notion of full-loop consistency, then one of the central policy questions would have been addressed - is it more cost-effective to invest in energy-saving devices and actions than in new supply options, in what rough balance?

The implication of the full-loop claims in *Energy in a Finite World* and elsewhere (see for example the quote from reference [98]) is that MEDEE-2's outputs (and its assumptions about, e.g., the feasible extent of energy conservation) are evaluated (and shape the scenarios) by calculating their implications through the full model loop, thus giving them and the full supply-demand scenarios a macroeconomic test. Thus the models are claimed to: " *forecast* aggregate final energy demand; model the evolution of the energy supply, conversion and distribution system and in so doing, incorporate resource, capital cost, environmental and some political constraints; calculate the economic impact - capital, manpower, materials, etc. - of alternative strategies" [104]. It is impossible to see how these could have been achieved without the claim to have analyzed the relative cost effectiveness of energy demand-reducing investments against supply-side investments.

As we have seen the controlling "iteration", such as it was, involved little IMPACT and no MACRO, and instead was driven by many informal judgements (for example, about future capital/output ratios). Yet elsewhere these are described as "findings" [105]. Assumptions are checked by further assumptions. There is therefore no way of evaluating the credibility of the scenar. demand levels (and underlying assumptions about energy conservation, supply mixes, etc.). In particular, despite contrary impressions, no comparison was made of the relative cost-effectiveness of energy conservation versus new capital-intensive supply options. No evaluation of the scenario demand levels was thus carried out, so we do not know how to evaluate the low scenario's claims to have stretched conservation to the limits of credibility. Thus the study's overall claim to have objectively examined "a wide range of views" through its modeling and scenarios stands at best obscure, due largely to the confusions about the scale of consistency checking and the status of iteration [106].

III.5 Sensitivity Analysis

In important respects modern policy analysis is nothing more nor less than sensitivity analysis (SA), for what policy makers need to know is which factors (within or beyond their control) most influence the outcomes they seek. SA is also linked with the initial framing of the analytical problem because a model's outputs may be robust to the variables included within the problem definition, only to be wildly sensitive to variables arbitrarily located just outside it. Thus, for example, if a model or a model set is shown to be sensitive to small fuel price fluctuations when the uncertainties in these are wellknown even to lay people, one reaction may be to draw the conclusion that the modeling could be consistent with a wide range of possible energy futures. Nordhaus's sensitivity analysis is exemplary in this respect [107]. Another option might be by ***fiat*** to define price changes out of the problem domain. This would not provide realistic scenarios, but could still be a useful ***hypothetical*** exercise for some analytical purposes. The IIASA study held prices constant over a 50-year horizon in order to gain some robustness ***for the models*** by excluding the savage effects of price changes. But this ***model*** robustness (even this was not achieved in the case of the IIASA models) is bought at the inevitable price of disengagement from reality, and therefore from the ability to draw realistic and robust policy conclusions. As we shall see, this logic was never followed through; instead, a confusing account of the approach to systematic SA, including the unexplained curtailment of some earlier suggestive work on these lines, is combined with an inexorable arrival at robust, even "inescapable" policy conclusions. One modeling expert asserted that the IIASA study's most valuable contribution was that it identified the main activity vectors in the policy process [108]. This could only be true if extensive, systematic SA of different variables had been carried out, which was not the case. Claims to robustness in conclusions could only be credible if proper SA had been performed.

We should recall that the main purpose of SA is to define the policy importance of the uncertainties involved in the estimation of many variables, in order to diminish the frequency and cost of nasty surprises. Yet complex policy models involve so many variables, each of which needs several runs at different assumed values to perform SA, that some selection has to take place as to which variables to pick. Therefore, despite the use of some statistical techniques here, there is still some room for judgement. What we see in the IIASA case, however, is that variables *already* picked earlier in the study and shown to be in need of further SA work, were dropped and not publicized; and obvious candidates such as price or cost changes were ignored, even though again, work begun at IIASA (and continued elsewhere) had shown the extreme sensitivity of supply mixes and demand levels to different price change estimations.

Although "the sensitivity analysis was done - at length" [109], *Energy in a Finite World, Vol 2* devotes about 10 of its 850 pages to "sensitivity insights" (p.613), dealing with three hypothetical cases. It is acknowledged that these

> "... certainly do not exhaust the conceivable variations in crucial assumptions. Changes in other key inputs can be tested and the sensitivity of results to such changes be assessed" [*EIFW, p.614*]

Only one such test is described in *EIFW*, and as we shall see shortly, even that was misleading. The three"SAs" which follow are expressly

> "... not the formal results of the complete, iterative, quantitative approach described in chapters 13 through 17. Rather they are the judgements of those who, through much experience with the formal analytical approach, have gained some insights into the sensitivity of results to changes in certain inputs. Therefore, these sensitivity analyses are best characterized as informed and experienced opinion, based on other quantitative analyses". [*EIFW, p.614*]

Sensitivity analysis as usually understood *investigates* the sensitivity of outputs to changes in input assumptions in order to discover which are the key input variables. The sensitivity "insights " of *Energy in a Finite World* are therefore circular, since the informed and experienced opinion of the analysts has already decided which are the key variables and how they affect the projected outcomes.

From general considerations and from post-1973 experience, the most obvious variable to choose first for SA is price, in particular variations in the price of energy. There are two aspects of this: (a) overall increases in real energy prices and how these might affect demand and the relative value of new supply versus demand-reducing investments; and (b) changes in *relative* fuel prices, thus affecting fuel mixes and associated supply technologies. This is also a central policy issue. Given that MESSAGE's outputs rely directly upon assumptions about relative fuel costs, and that these have in reality been subject to rapid changes and heated debate, it is surprising that no formal SA explored this. As Keepin shows, [Keepin, p.40-42] one study - the only formal SA described in *EIFW* - did claim to investigate "the possible variations among the ***relative*** price changes of new sources of energy", [*EIFW, p.619*] (my emphasis) only to focus instead upon the calculated energy demand change from increasing costs in ***aggregate*** categories, keeping ***relative*** costs of alternative options constant. Thus the SA finds apparent robustness of electricity demand (an 8% drop) to a doubling of generating costs, but obscures the huge changes between supply mixes which occur when the relative cost structure is varied well within the range of experienced changes in the last decade. (Recall the IIASA costs are projections of fixed real cost and relative cost assumptions for 50 years into the future.) It was left to Keepin to discover these and other sensitivities using standard methods, while the *EIFW* discussion risks obscurantism by instead relying upon informal expert insights and "feelings".

There is no reason why Keepin's SAs could not have been performed and published from within the IIASA ESP years ago. This point is supported by the fact that two different but highly relevant sets of standard SA ***were*** initiated within the IIASA ESP in the mid-1970s, but both appear to have been neglected. In the first set, two 1974 papers describe formal SA conducted on a prototype of the MESSAGE model [110], leading to the discovery of several sensitivity problems related to such factors as discount rates, market penetration rates, and fuel prices. This drew the conclusion that "more work is needed in several directions" [111]. Only one such further work was published, however, namely a 1975 paper

[112] which revealed an immense sensitivity of supply mix, with coal or solar contributions to electricity supply ranging from 0 to 70%, within credible variations in fuel and technology costs. This, in turn, translates into extreme sensitivity for the remaining technologies participating in the same supply mix (e.g., nuclear, hydro, etc.). The importance of these findings appears not to have been recognized, because not only was no further work performed, but these findings themselves are neither cited nor acknowledged in later documentation. The lack of proper treatment of sensitivities is not a "wisdom of hindsight" criticism, because in fact the IIASA ESP seems to have had its own foresight, but then ignored it. A price-rigid framework for the whole exercise seems to have developed from that point.

Given Keepin's discovery of the crucial role of an arbitrary and tiny "kink" in the price of LWR electricity in allowing FBR entry and expansion [Keepin, p.36], it is particularly interesting to note the exploration of the significance of uranium price changes in these earlier studies. Yet again, no further work was apparently performed even on this specific question and these studies were also not mentioned in the section in *Energy in a Finite World* on SA. Keepin's own SA of the IIASA models and his demonstration of their extreme brittleness with respect to minor changes in key parameters (changes that are already dwarfed by existing uncertainties) was entirely unaided by the project documentation. Furthermore, the claims of the IIASA Group, that those "with much insight and experience" of formal modeling could better judge where sensitivity was greatest, are contradicted by the fact that those experts did not identify and analyze the important sensitivity factors spotted by

Keepin.

A second strand of SA relevant to and earlier associated with the IIASA study is that of Nordhaus. He has been concerned more with overall sensitivities of energy demand to price fluctuations, and with the sensitivity of price change estimates themselves to various factors. This work has produced the wellknown spaghetti curves for various sensitivities dealing with price and demand [113]. Given Nordhaus's concerns, since very well developed from his IIASA work, and his involvement with the IIASA ESP at the same time that the other early SAs were being performed, it is unfortunate and puzzling that a more thoroughgoing SA enterprise did not develop integral to, and to the credit of the IIASA ESP.

III.6 Hypotheticality to Hyperthetic ality

The external credibility value of SA and of "iteration" is naturally high, and this is reflected in the prominence given to this control mechanism in the public presentations of the scenarios and the study's policy conclusions. The title of the article in *Futures*, for example, is "IIASA's Energy Modeling" [114], and the abstract describes the scenarios as "quantified via a set of highly iterative models". As we have seen, the models are almost everywhere given a central place by their authors in the whole study. The *Futures* article, whilst issuing the usual modest disclaimers about scenarios not being predictions, moves from its modeling and iteration to utterly unconditional assertions about the future, e.g., that a transition to nuclear and hard solar by 2030 will be "inevitable" [115].

This gradual semantic movement from hypotheticality and scenarios as tentative analytical constructs towards uncompromising assertions is repeated in other articles and lectures. For example, in the *Science* article, the same cautions are expressed about the scenarios, but then the language of objective necessity creeps in ("in our scenario writing we found it necessary ..." [116]), so that we are soon constrained by a "necessity" which comes from the subjective founding assumptions, not from observed objective reality or cross-checked theoretical deduction. Even more, in the same article we are very soon into the realm of absolutely inescapable prescription and prediction -- "What must be emphasized here is the advent of two major technologies in the year 2000: synliquids and the fast breeder reactor ... To have [them] in use by the year 2000 ... means that aggressive action in an overall context is required now" [117]. This transition is, we are told elsewhere, of equal historical moment to the Neolithic transition [118].

This movement from provisionality to supposedly objective inevitability appears to be so natural as to reflect a deep and genuine lack of awareness of the fundamental boundaries of authority and legitimate credibility being traversed in the process. As noted earlier, the more complex, hypothetical nature of scenarios means that they are easily confused even (or, perhaps especially) by their authors, with a "robust" springboard for real policy conclusions. Once one is launched in to the midst of a host of detailed questions concerning, for example, energy investments/GDP ratios to achieve given supply scenarios, and time factors for investment, one is *a fortiori* being asked to take the original scenarios and their assumptions as proven, especially when one is told that they "allow for no escape". [*EIFW, p.785*] Thus all of the questions about *their* realism are left behind. This is especially problematic given what we have now seen about the non-existence of some of the models, the analytical vacuity and "big bang" character of the remaining models, and the confusions sown about their validation through SA and the scale of consistency achieved with them.

This process is encouraged by a confusion between what has been achieved in the modeling analysis, and what is, or was hoped for. This was given some discussion in section III.2. As another example "the ultimate objective" of the model set is "to explore the embedding of future energy systems and strategies into the economy, the environment, and society". The policy question is derived from this; "Is there enough time [and capital] to manage the transition from today's energy system to asymptotically satisfactory systems?" (by which they mean some mixture of FBRs, hard solar and synfuels). The fact that it is acknowledged that "much work remains to be done" [*EIFW, p.399*] never causes the study to waver from this basic conception of the problem and its attendant narrow range of conceivable "findings" and "compelling" policy solutions [119].

Through all this elaborate process, the realm of accountable, externally testable expert authority is fundamentally confused with that of (externally uncontrolled) group and personal value judgment. The boundaries of fact and value are trampled upon, but then the same boundary is broadcast as the guarantor of authority for the study. That confusion is then inevitably propagated with all the force of "science" into the policy debate.

III.7 Summary

Stripped to its bare essentials the representation of the models in the IIASA energy study has been as follows:

CLAIM 1: Macroeconomic consistency is a big feature of the IIASA scenarios, and a major element of their scientific status.

CLAIM 2: This consistency is achieved by models, and their iteration in the full model loop.

Then we find:

ADMISSION 1: The loop does not exist.

ADMISSION 2: The models anyway do trivial things "as everyone always knew".

So, with these admissions, how *could* the models have done any significant consistency control? The claims are contradicted by their own authors' admissions. This underlines Keepin's point that any consistency which may or may not exist in the variables is already there or not there before the computer is ever switched on. Assumptions are controlled only by further assumptions; the relatively trivial control for local *arithmetic* consistency does not in itself avoid this circularity, and does not in itself come anywhere near to validating different options. Yet it was inadvertently raised to the higher epistemological status of having objectively controlled substantive scenario contents and comparative evaluations of policies.

We have seen a slowly-emerging, ambivalent, and somewhat roundabout approach towards full acknowledgment of the limitations of the IIASA modeling effort. Within these historical trends Keepin's discoveries can be seen as a mainstream, albeit somewhat more forthright contribution. They have helped to clarify the full extent and importance of subjective assumptions in the study whilst at the same time clarifying the actual role of the models. Thus MESSAGE, the "core of the energy studies reported in this book" [*EIFW, p.402*] was found not only to be internally extremely brittle, but totally constrained in its outputs, by the externally generated input constraints, some of these undocumented (see for example, Keepin pp.37-38 and his Appendix E). The fact the MESSAGE was completely tied down by its input assumptions was evidently known to the IIASA ESP team but the only references to this important limitation are ambiguous, that they were "often quite tight", or that "these constraints, taken together, are singular characteristics of the scenarios". [*EIFW, p.402,p.527*] The full implications of these hints however are nowhere spelt out; indeed they are obscured by the repeated prominence given to the models themselves. They are referred to as "real world constraints" but the significance of the fact that they are informal expert judgements *and* that they completely drive the model outputs and thus the scenarios, is not made clear. Thus the self-description that:

> "The specific approach is to use an optimization procedure to find the combination of energy sources that satisfies the specific temporal sequence of regional final energy demands, subject to (often very dominant) given constraints, while minimizing the discounted total cost". [*EIFW, p.397*]

does not clarify the fact that there was no optimization within a feasible region defined by the constraints, because the constraints themselves pre-set the "solution" at a single point. It was left to Keepin to clarify this, and to show just how often and how very (i.e., totally) dominant were the constraints, and that some of the more important of these were not documented, even in the "documentation".

Thus the bold assertion in *EIFW* that:

> "Computer modeling has at least one other distinct advantage. With a great many interdependent variables to consider in typical systems problems, it is difficult to know which ones are critical, which ones deserve close attention by planners and decision makers. Models can aid in identifying such parameters through for example sensitivity tests and consideration of alternative scenarios", [*EIFW, p.399*]

does not apply in the case of this study, whether or not it is valid for others. Häfele's earlier advocacy of formalism in analysis and debate as the best way of improving policy [119b] is not easily reconciled with later defenses of informal judgement.

One of the last two remaining formal models, MESSAGE, is shown to have been virtually moribund. Interestingly enough, the setting of exogenous constraints in a model by its authors so as to exclude subjectively unacceptable outputs was identified and discussed by Nordhaus in an IIASA ESP paper [120] on the Bariloche models following up *Limits to Growth*. The lack of credibility of the outcomes of such exercises was emphasized.

This imprisonment of model outcomes by subjective external inputs is particularly acute in the case of large complex models because there are so many more data and judgements to check. The IIASA ESP claims that their models are relatively "simple" and are therefore superior to "large monolithic computer models which often suffer from overcomplexity and rigidity". [*EIFW, p.400*] Yet other experts such as Ausubel and Nordhaus judged the IIASA models to be "extremely complex" and "difficult to comprehend, manipulate, change and verify independently" [121]. Each run of MESSAGE for example, requires the input of approximately

1600 specified constraint variables and 2600 activity variables, even though many of these are zero or constant from one run to the next. Despite having been earlier promised as "the whole set of input data" [121b], the recent supposedly full documentation of MESSAGE is selective in reproducing only parts of these input files [122], so that model reviewers are still unable to gain access to the determinants of the model outputs. Indeed, some of the undocumented parameters involved the most critically sensitive parts of the model. Thus Keepin [Keepin, p.37] found undocumented *ad hoc* inputs which allow an otherwise inexplicable maximum rate of introduction of the nuclear fast breeder reactor (FBR) in one region where it otherwise would not have been introduced. The claimed clarity of the so-called simple models and, as Goldman put it, "their ability to travel well" to other modelers [123] would at least be aided by reproducing the input files themselves rather than a selective description of them. As it turns out from Keepin's scrutiny, the models are analytically simple (indeed analytically non-existent), but still manage to suffer from the "overcomplexity and rigidity" and lack of clarity which the IIASA team thought they had avoided.

The completely dominant structure underlying the lack of any degrees of freedom in the modeling is that, given IIASA's primary assumptions about global population, demographic shifts and economic growth, the total primary energy demand always threatens to far outstrip any feasible supply scenarios from what is assumed can only be more capital-intensive, centrally managed supply systems. Within these primary assumptions there is thus no feasibility space within which to evaluate *optimal* strategies because most resource and supply options are exploited at the maximum feasible rate. This is true even for the "low" demand scenario of 24 TWyr per year. Indeed the range eventually settled upon for the scenarios actually follows a repeated reduction from earlier even higher total demand scenarios [124]. The repeated adjustments downwards, however, remain fully within the framing definition of the whole project set up at the outset, to do with the kinds of technology thought to be realistic (thus for example in 1977 the FBR was assumed to be "feasible not only scientifically but industrially, commercial feasibility being in sight" [125] even though Nordhaus' energy study two years later regarded them as "unproven for large-scale use" [126]), and the kinds of corresponding institutions and values thought to be "natural". With respect to the latter, it is incidentally worth noting that the "large-scale consideration" inevitably necessary for a view of global energy supply, is unconsciously equated with centrally managed processes, which is not an inevitable logical step [127].

IV. POLICY ANALYSIS, SCIENCE AND POLITICS

IV.1 Defining the Problems

Before analysis can begin, a problem needs to be framed and defined. Selection, therefore, inevitably takes place -- boundaries are drawn which exclude some realities; certain factors and relationships are selected, while others are ignored, so as to give a structure within those chosen boundaries. This is of course a socially influenced process, in which unconscious and conscious biases play a part. However, proper analysis is supposed to build in controls for such biases, and should ideally be able to specify both boundary and structure hypothetically, and even adapt "the problem" by the effective operation of such controls [128].

Normative accounts of this unfettered process as an ideal image of science have historically been confused with apparent *descriptions* of real science. Social analysis of science more recently has extensively documented and analyzed this confusion, which nonetheless still dominates public policy. The same analysis has also clarified the basic obstacles limiting this ideal in reality even within academic science, let alone science in policy [129]. But quite apart from these important qualifications, in the interaction between science and public policy a fundamentally important complication arises because scientific problems are not the same as social problems, even though some crucial terms might apparently be shared. Scientific specialties are essentially private subcultures that have been able to develop precision and working consensus by narrowing their defined problems to a set which that subculture alone accepts as meaningful.

It is a problem that for a long time scientists have been used to making routine unconscious social assumptions when they evaluate apparently only physical questions. For example, when the UK Pesticides Advisory Committee announced that 2,4,5-T was safe, it assumed quite unrealistically, that farm workers who used it would always have access to proper instructions and safety equipment for operations, and would not be put under extreme pressure (e.g., threat of firing) to spray in the wrong conditions. The Committee took for granted an ideal world where various unreal social conditions would prevail, and this crucially altered

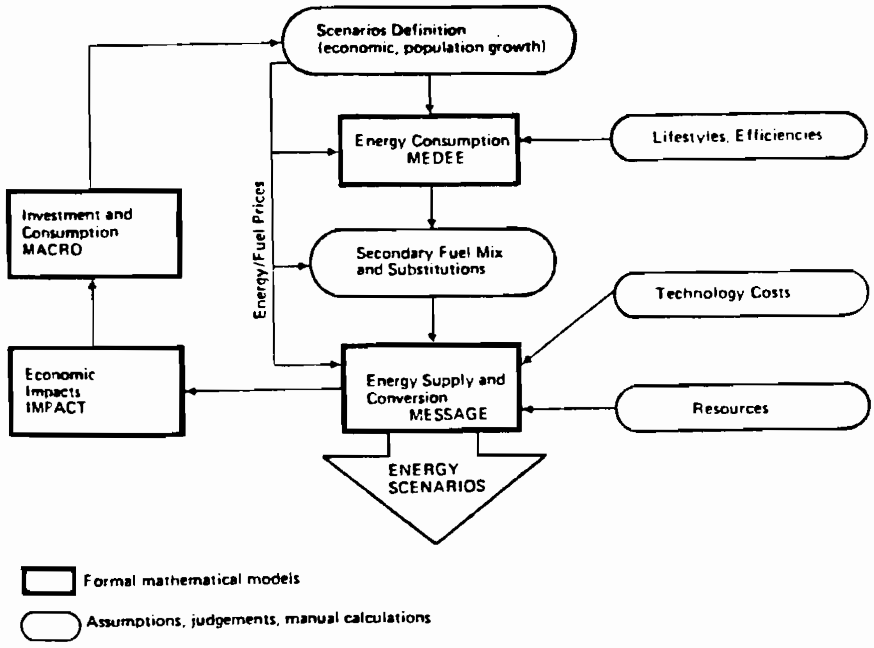


FIGURE 6 The IIASA's set of energy models. 1982. (from [96]).

the evaluation of the risks involved [130].

In using their expertise in policy, scientists do not generally recognize such assumptions, nor that they are significant and often arbitrary. Yet they are effectively prescriptions because technical elaboration of policy within their taken for granted terms makes alternative possible policy based upon different, equally defensible assumptions, simply invisible. This process is usually so deeply ingrained into scientific culture as to be entirely unconscious, which only provokes defensive antagonism towards anyone who points it out.

For example, in the acrimonious debates in the 1960s over race, intelligence, and compensatory education programs, much confusion was created by the misconception that the psychometricians' definition of "intelligence" as the central parameter of the issue was a definition determined by objective reality, whereas in fact it was determined largely by the available measurement techniques (plus the white Anglo-Saxon cultural assumptions incorporated in IQ testing). One can accept that the scientists in this case had measured some part of the reality of the corner of "reality" in question (intelligence) - here at least they could make experimental observations. But this does not make it the only part even of that corner of reality, and thus no basis for defining objective constraints and dominant normative conclusions for policy choices. In this sense the "globalism" of the IIASA scenarios has been confused. It is in fact "globalism" on a single dimension represented by arithmetic consistency, e.g., over allocation of oil; this has been confused with "globalism" in the sense of analytic comprehensiveness, which is not at all the same thing.

Just because it was apparently more precise, the scientific definition of intelligence in the race - IQ debate was no more true than the social definitions of intelligence embodied in policy goals for education [131]. Yet the scientists involved (effectively as policy analysts, though they were not then called that) repeatedly asserted answers to the social problem of how to improve intelligence based directly upon derivations from their private subcultural scientific definition of "intelligence". The precision of their term was gained by gradual subcultural selection, at the expense of valid meaning for the wider social context. Yet because the selection was not conscious, but institutionalized through the subculture as taken for granted, this precision was taken as the grounds for superior *social* authority rather than one (legitimate) input to be taken only with careful qualifications. This undermined the possibility of a mature debate of social values and educational institutions by reducing it to a technical argument (with barely concealed social prejudices) in psychometrics and genetics.

The issue of translation of problems and meanings between science and policy arenas is quite general, and has become more acute as "policy analysis" has developed as a distinct professional field. Because of partisan ambitions and intellectual confusions similar to those outlined above, as early as the 1960s the Operations Research Society of America (ORSA) became embroiled in a major controversy about the proper role and limits of experts in a policy debate which got out of hand on the deployment of anti-ballistic missiles [132]. Quality control of OR professional practice could not be separated from questions of different social values.

The main point here is that bias or ideology is *inevitably* introduced in the way that a policy problem is defined and taken up -- it is not *only* a matter of what external inputs one makes or what social options one attempts to derive from a given analysis [133]. Thus the belief that one can frame a policy analysis problem as an objective or factual problem is immediately and fundamentally problematical itself. It is therefore worth noting that the IIASA ESP claims to make inputs to policy at two levels.

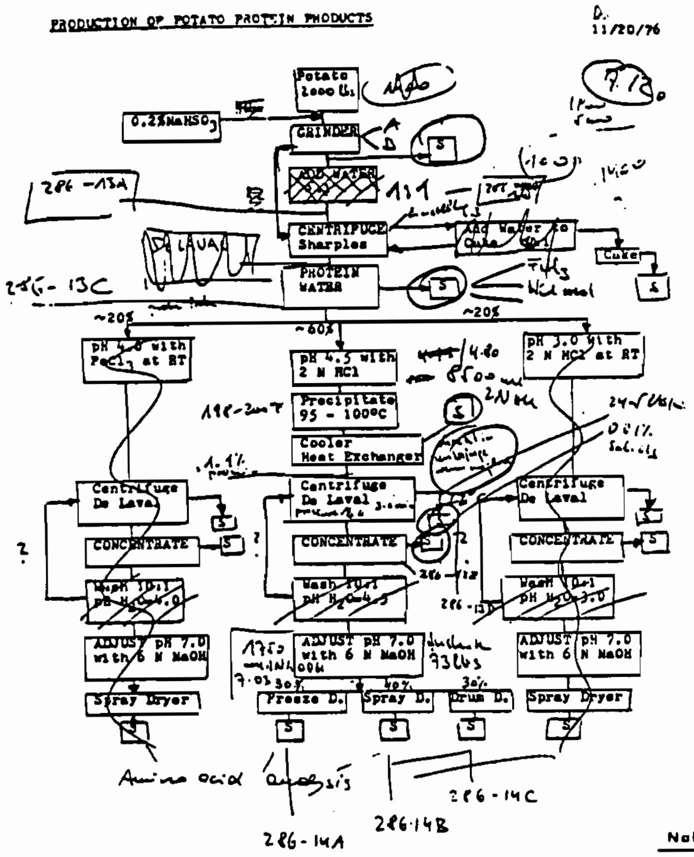
First, it modestly withdraws from the claim to be making specific policy pronouncements, but claims instead to have used a scientific approach to discover via scenarios *the* definition of the policy problem that the world has to solve [134]. As the *Executive Summary* puts it, "we can lay down the basic outlines ... how they will ultimately be filled in is a question that must be left to the future" [135]. Thus starting from a "factual approach", making a strong claim about excluding sociopolitical factors, the study is supposed to have been able to discover not specific policy conclusions. ***but an objective structure and boundaries to the policy problem confronting the world.*** This *is* presented as a neutral, objectively discovered product.

Secondly, however, despite these apparently modest disclaimers, the study and *EIFW* is also repeatedly used as a reference to support very specific, and very strong policy conclusions [136]. One problem with the tendency to overstate the objective underpinnings of such policy conclusions and of the study generally is that the many more modest but valuable achievements of the IIASA ESP are lost from view as a backlash inevitably occurs against inadvertent oversell. This has happened in other cases [137].

The Appendix describes a similar case of a shift from the early disclaimers to later scientific inevitability. This involved earlier policy manouevring in support of the Federal Republic of Germany's fast breeder reactor program. In 1965 a scientific study [138] that claimed to show a clear economic advantage of FBR over LWR was used by the Karlsruhe Nuclear Research Centre to persuade the government to spend DM96 million for building two prototype FBRs. Later it was found that the "input data were tuned" so that the output figures confirmed the economic assessment which the Karlsruhe scientists wanted (see Appendix) [139].

IV.2 Problems of Institutional Setting

It is now widely accepted that informal processes -- intuitive judgments, implicit values, and tacit persuasions (the *craft* element) -- play a stronger role in scientific reasoning and analytical thought generally, than has hitherto been recognized. Scientific rationality and justification is always to some extent a *post-hoc* reconstruction, an artificially formalized, streamlined, and apparently logically inevitable route to the ensuing knowledge than is really the case. "Rationality makes sense of what has been, not what will be" [140]. The knowledge is thereby made to look less human, more objectively verified and more universally certain than it is in reality. The informal, contingent, unruly and socially negotiated reality of analytical practice, is inconsistent with these formal, more public accounts. The example given from Knorr in Figure 7 is typical of scientific practice in general [141]. The public account is an



(a)

(b)

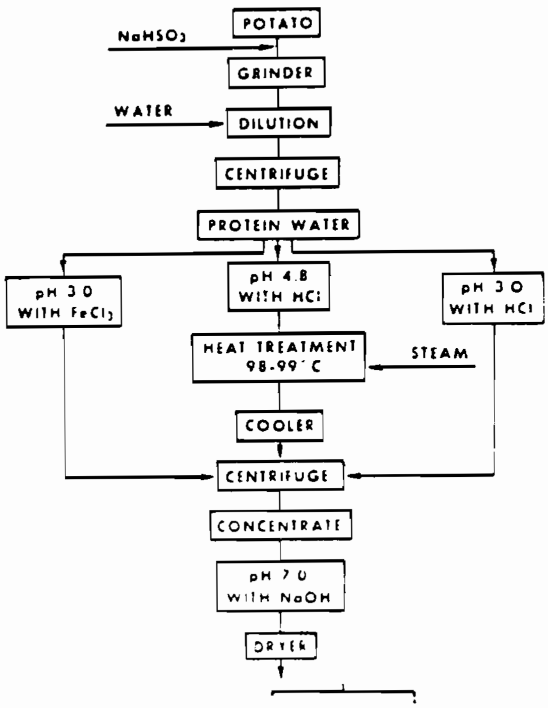


FIGURE 7 The generation of knowledge: (a) Example of flow chart used and notes taken during experiment; (b) flow diagram published in the scientific paper. (from [141]).

emasculated version of the more open-ended, less controlled, real process of knowledge generation.

This is structured not only by the attempt to describe that reality, but also by the desire to *justify* - to present it in a way that conveys compelling authority to the audience.

This has been extensively documented by empirical research within science [142]. There is a rhetorical "public" level overlying the more arbitrary, less compelling authoritative informal level. This contrast is evident in the IIASA study too. It is important to see this quite general inconsistency as something more than some kind of conspiracy to defraud external audiences. There is a functional role within science for a kind of deeply embedded, collective wishful thinking about practice. For the function to be fulfilled, new recruits and practitioners should believe in the rhetorical account as a "real" account of what they are doing. The informal, contingent arena of knowledge production and negotiation does not have to be completely relativistic and arbitrary [143]. Nor does the messy reality have to be championed as a norm to be obeyed. The institutionalized "wishful thinking" or mythology can be invoked by scientists in a given field to maintain morale and commitment; and as a repertoire of accepted norms to test, criticize, or support claims for authoritative knowledge. It thus constrains excesses of bad practice, although some looseness still exists even within a well structured professional peer group. The rhetoric for example, asserts the full, open sharing of all results and methods with a cooperative community of fellow collaborators which is a useful restraint against opposite tendencies.

Formally specified calculative models, with clearly defined reproducibility, iterative controls and sensitivity analyses, are also amongst these norms. Authority can only be assumed by negotiation of (i) the relevance and (ii) the judged fulfillment of these norms by the knowledge claims in question, amongst a given community.

In the case of the IIASA energy study we can describe three such possible communities:

(1) the IIASA team itself, including some, but not all, of the short-term "collaborators";

(2) the modeling community at large;

(3) the policy-making and policy-debating publics.

These communities are badly defined and somewhat centripetal, to a large extent because of the social relationships of analysis and political arenas and interests. Studies such as the IIASA ESP are justified by the first community as means of educating their audience -- the members of the third community. But as a basis for its judgment of the authority of these studies, this latter group has only the public self-descriptions of the first community, as well as the reactions of the second community, to such knowledge claims.

Underlying its move from initial disclaimers to strong and specific policy prescriptions are two central elements of self-description in the IIASA study. The first is its suggestion of a larger-scale consistency, supposedly achieved by controlled iteration and larger-scale modeling, than was really the case.

Secondly, we see that until very recently the energy study relied upon a public self-description which implied a full model set with formal reproducibility as its claim for authority. Nine years after the study started, and two years after its major publication, it has now been clarified, thanks in large part to Keepin, that the real guts of the whole exercise was informal judgement, (inaccessible to other modeling experts), and that the quantitative scenarios are not fully reproducible from the published documentation.

I have shown earlier that at least some energy modeling and related policy experts have been misled by the previous public rhetoric of formal models and quantification. In external reviews, such perceptions as the following are common:

> "IIASA have succeeded in providing a valuable quantified long term framework of global energy futures with considerable detail and adherence to self-consistency" [144].

> "The study is ... an unprecedented, detailed analysis on energy ... analyzing options in a quantitative, mathematical form" [145].

However, equally important is that since it now claims authority via intangible craft judgements, neither the relevance nor fulfillment (or non-fulfillment) of the norms of public knowledge by the IIASA study can be judged by expert peers, unless they have been involved in the day-to-day negotiation of these criteria and of whether they have been met. In other words, the only community to whom the study can satisfy minimal requirements of rational authority is the Energy Group itself. It is therefore *PRIVATE* knowledge. This point raises again the general problem of quality-controlling professional forums for such large and mixed modeling efforts which aspire to the authority that such control would earn them.

V. THE BIASES OF OBJECTIVITY

A world of informal judgement and evaluation has been put forward as a formally quantified exercise worthy of claims to robustness and superior authority. The basis of these claims, the models which "produced" key scenarios, have been shown to be analytically empty, lacking in iteration and sensitivity analysis, and internally brittle to minor changes in some important variables. In addition to contradictory descriptions of the state of the formal models, and the process of analysis, the more recent accounts have moved the seat of authority onto the informal, craft dimension of the enterprise. Häfele himself argued to Keepin that his critique was unfounded because he had not yet understood that systems analysis is neither a science nor an art, but a craft, and that by its very definition a scenario is an assumption [146].

The dilemma flowing from this, however, is that such informal craft dimensions of knowledge generation are by definition difficult to pin down and describe in a reproducible and externally testable way. The elaborate activity woven around the scenarios does not alter their status as assumptions, but renders this hypotheticality extremely difficult to maintain in public communication, and it seems in the minds of their authors. Externally, therefore, such analysts have an endemic credibility problem, exacerbated by the degree to which they rely upon the informal as opposed to the formal elements of their work. But they also have a credibility problem if immodest assertions and impressions are given as to the formal underpinnings of their work when it is clarified that these are false. Keepin's demonstration of the extreme brittleness of the models to projected (20-30 year) uranium price changes (that have *already* been matched), only underlines the inability of this craft skill to identify relevant parameters for sensitivity analysis. It is even more damaging to the credibility of this "craft" that it was advanced as the expert justification for choosing other factors for attention, and thus *actually ignoring* the more relevant parameters that had in fact been indicated already in early orthodox sensitivity analyses.

This paper does not say that the *Energy in a Finite World* group naively claimed their policy analysis to be completely scientific. However, such a claim was implied by repeated reference to the models and quantification. How, for example, could a study juxtapose admissions of informal judgement, many of which were undocumented, with strong statements such as the following?

> "... the formality of computer models or of the analytical frameworks is of high value for a particular reason. All policies and decisions are based upon some implicit view of the future or range of futures. The formal structure of models enables these assumptions to be explicit and subject to audit. This can serve as a defense against bias in decision making" [147].

One does not have to judge between the relative merits of formal calculation and intuitive judgement to recognize that such forthright self-descriptions tend to obscure the extent and importance of informal, implicit views in determining the outputs, and therefore to confuse policy evaluation. Indeed it was implied even in the Preface to *Energy in a*

Finite World, where it was stated that

> "our aim throughout ... has been to be objective. However in adding it all up we recognize the need to take a position and express the views we actually hold. Thus, the assessments and implications of our study, presented in part IV [pp40 of 820pp], cannot be defended merely on an objective scientific basis" [148].

The implication is that the rest, i.e., the study as a whole, *was* based on objective science.

In offering it to the world, the patrons of the *Energy in a Finite World* study recognized the impossibility of complete objectivity:

> "Although analysis strives to be objective, it cannot avoid completely the imprint of personality or the influence of individual or group experience. Consequently this study, like all others, reflects the character and background of its authors. Good analysis, however, tries to make these influences and assumptions explicit, so that the user of the analysis can be aware of and compensate for them. Professor Häfele and his team have taken special care in this report to state carefully the assumptions they have made and to distinguish their visions from their calculations" [149].

According to Häfele,

> "We purposely stretched our thinking to the limits so as to provide the reader with the broadest possible choices of input data and parameters for understanding our quantitative analysis" [150].

Although the motives and ideals behind them are laudable, both these statements misunderstand and conceal the problem. They suggest that bias lies only in assumptions and inputs separate from the "objective" framework and process of analysis: if explicit this bias can be compensated "by the user of the analysis," like compensating for an unbalanced sledgehammer by changing the way one swings it. But the very roots of the analysis, the basic framing of the original problem, can also be biased - indeed it is inevitably so. Thus whatever care may or may not have been taken in making the assumptions explicit, this is by no means the whole issue.

We have seen that the prominent claim to have "stretched our thinking to the limits" did not even incorporate an examination of the implications of relatively tiny changes in fuel cost schedules. Furthermore, central questions were never examined, such as the relative cost-effectiveness of energy efficiency improvements versus expanded energy supply options, even in what was a cost-optimizing model. This neglect was achieved even though several of the wide range of views supposedly incorporated into the study ("to avoid an extreme one-sided view") argued strongly that this was a key issue, and despite the fact that some of the implications of stretching supply-side options were reaching absurd proportions.

A telling footnote in *Energy in a Finite World* [151] fleetingly suggests that, contrary to the claims of the study, the IIASA scenarios may not be opposite ends of a range spanning all the possibilities, but are actually the same basic scenario with only some relatively unimportant parameters changed. It is worth developing this suggestion a little.

The so-called self-denying, factual restriction to technoeconomic and physical factors in the basic shaping of the approach is falsified by the fact that technoeconomic factors take shape within, and themselves shape, a sociopolitical environment. The two arenas are inextricably mixed. Technologies *are* social institutions, around which structured social relationships, commitments, and opportunities consolidate. Thus in the study, some "technoeconomic realities" are taken for granted whilst others are excluded, for no apparent reason, except that selective views of *institutional* realities underpin what is and what is not a feasible technology. Again it is not the point to argue which are and are not justifiable assumptions, but to point out how the attempt to gain "objectivity" by excluding sociopolitical and institutional factors (a) cannot be achieved, and (b) introduces its own fundamental bias. For example, the stretching of energy efficiency improvements (only to levels that others say are ***already*** being achieved) was abandoned beyond a certain point. Thus, for example, it was cautioned that "In the heated debates about energy conservation it is the feasibility and desirability of institutional and political measures that matter" [152], as if this were not also true for large-scale centralized supply options. Yet although strong conservation may have involved the enactment of (supposedly excluded) social innovations, so too would the correspondingly "necessary" extreme expansions of supply, such as the open-cast mining, for example, of vast areas of Colorado for second-grade hydrocarbons, or the construction of at least one new nuclear plant every few days for the next 50 years [Keepin, p.55] etc.

This and other extreme physical and associated social implications were contemplated without demur [153], they were adopted as "normal" because they imply the same social paradigm of centralized supply and consumption-oriented energy institutions and big projects. Yet decentralized supply options, energy efficiency improvements and consumption limitations were effectively excluded from the analytical frame presumably as "social" (i.e., involving unfamiliar and unwelcome social factors, such as diverse local initiatives, localized matching of user needs and supply, etc.).

This kind of fundamental structural bias goes far deeper than explicable inputs and assumptions as usually understood. The honest effort to elaborate the analysis combined with the documentary confusions described in this paper, unfortunately only tends to obscure real policy options rather than clarify them. As Freedman and colleagues have put it,

> "It ain't what you don't know that gets you into trouble: it's what you think you know that ain't so. [Such] large ... models extrapolating from synthetic data bases are likely to increase the stock of things that policy makers think they know that ain't so" [154].

Thus the scope of conceivable policy options which the study claims dispassionately and comprehensively to explore is actually imprisoned by its founding principle - that it should be (in claim anyway) a solely factual study excluding all societal and institutional factors. That it is hopelessly muddled on this point is illustrated in routine ways, such as the fact that even the models, let alone the intermediate informal controlling "iterations", contain even lifestyle factors, as well as other institutional relationships, trade and power assumptions, etc.

By this very foundation the IIASA analysis did not conceive of (even already current) social adaptations that might radically though not disruptively alter the picture of demand -- it could only stretch technical parameters, all within the same taken-for-granted, fixed basic institutional framework. This was not only fixed, but also arbitrary, and with no built-in corrective mechanism. There seems to have been a confusion between the commitment to "surprise-free" scenarios (i.e., presumably, non-disruptive changes) and *alternative perceptions* of which changes already in train are most significant and worthy of (non-disruptive) amplification. The only disruption and surprise here is to cherished worldviews, not necessarily to the world itself.

Energy in a Finite World's problems were indeed inadvertently pre-ordained by the framing of the policy issue and the approach. The claim to scientific authority ironically ends up in a claim to have uniquely discovered *the* political nature of *the* energy problem. Having begun by artificially excluding social dimensions from a "factual" analysis that was actually and inevitably studded with social assumptions, the study then performs an inaccessible technical analysis and claims to discover in objective reality a political energy problem, shaped, of course, in a particular way. ***The crux is not that it is a political problem -- that in itself was hardly a novel insight -- it is the particular shaping of that problem which is important:*** so also is the insinuation of scientific under-pinning for its establishment as a framework for public debate and policy choices. It is no use anyway saying that such a study is only a neutral examination of the factual basis of "the" global energy problem, because the technical and physical dimensions are woven into sociopolitical frameworks, and trying to extract them inevitably biases the implicit model of the latter's values and possibilities. But if the claim to this separation is itself naive, the further claim to have by this means discovered the shape of "the" policy problem is audacious.

The underlying cosmology can be summed up by noting from the IIASA study's own documentation that the conclusions rest upon a simple circularity: The cardinal *assumptions* of the study are a doubling of the world population and a doubling of average per capita income by 2030. But this, along with other lesser assumptions, generates the "inevitable" conclusion that a sustainable energy future can only be reached by an expansion of all energy sources as rapidly as physically possible to achieve a minimum rate of economic growth and capital accumulation needed to invest in the capital-intensive technologies (nuclear, synfuels and hard solar) needed for "sustainability". In order to achieve this, the initial hypothetical *assumption*, that the doubled population will be so much richer, is converted into a scientifically "discovered" ***requirement***, that the population ***must*** become this much richer, to supply the capital for the "revealed" energy technologies! All of this, of course, is premised upon the condition that only capital-intensive, centralized forms of energy count as energy, consistent with the underlying metaphor of institutional order.

Caputo gives an indication of the fundamental discontinuity of relevant world views - one elaborately entrenched and disseminated in the IIASA study, the other passed by. He described his experience as a member of the IIASA study group:

> "In a recent study of possible long-range energy futures for W. Europe, the focus was to identify solar and nuclear energy systems that could power W. Europe. Early attempts to bound the limits of the technical possibilities stumbled upon significant differences of technical judgment. Indeed, it was impossible to scope the range of technical possibilities to include these alternative views of energy experts since, in some cases, they appeared to be mutually exclusive. For example, when on-site and near-site solar energy systems were considered, results clustered into extremes which ranged from solar eventually becoming a marginal contributor ("the 7% solution"), to solar providing almost all of Western Europe's energy needs ("the 100% solution").
>
> The lower and higher numbers that resulted from the early attempts at finding the range of these factors were not what

> they appeared to be. Normally, the higher number is an upper bound and the lower number is a lower bound. Curiously enough, it was found that the opposite was true in this case; the lower number was an upper bound, and the higher number was a lower bound. The range between these numbers was a perceptual void - a perception gap. This gap seemed to represent the views of two particular sets of energy experts. Both apparently were making social predictions and then used their technical calculation of approximate resource potential to verify their vision of future societies" [155].

Thompson has advanced an anthropological theory of basic cultural biasing of perceptions of nature to offer a more general explanation of this state of affairs [156]. The implication is that far from being a neutral analysis, even the elaborate scientific pretensions of such studies (not only their "inputs") are a kind of ritual consolidation of the underlying social visions and biases of protagonists. Their constituent scientific beliefs and technical methods are a kind of mythology, an organized fantasy in Boulding's words [157] (but without science's ultimate empirical points of reference). In such mythologies there is a different relationship between social aspirations and analytical or empirical truths from that embodied in the conventional view.

Myths are both true and false. They are potential truths in the sense that they may, if taken as true, so influence people's beliefs and behaviour as to bring about their own truth. On the other hand, if too successfully promoted they may be taken so literally and inflexibly as to lead their protagonists and all who follow into blind alleys and vulnerable policy commitments.

VI. CONCLUSIONS

VI.1 Some Practical Recommedations

The main point of this paper is more general and fundamental than to propose certain practical standards for the use and control of scientific modeling in policy analysis. Indeed, the main argument is that whatever specific norms are agreed, these cannot be realistically enacted without paying critical attention to the structural institutional relationships between analysis and policy, because this is what defines the existence or otherwise of a quality-controlling professional forum within which those standards could become meaningful. Furthermore, I argue, given the natural interpenetrations of policy and analysis, the question of the proper relationship between analysis and policy, raises questions about the internal structure of both.

Despite these more general aims however, the reader will naturally look for more direct practical recommendations. These are given below not in the claim that they are original, but in the belief that they need to be repeated. If there is originality at all it lies in the point that these substantive norms are in the end undistinguishable from procedural norms and social structural aspects of the policy-analysis relationship, and thus of policy and analysis themselves.

(a) The prior existence of extensive software in other fields should not in itself be allowed to define or dominate the choice of models for policy analysis.

(b) Before commitments are made to elaborate modeling exercises, extensive exploration of a policy field with far simpler, provisional, and interactive modeling (including real policy actors) should be conducted. This may lead to the conclusion that larger-scale models would be useless, at least until certain fundamental processes were researched and better understood.

(c) Although there will always be informal judgements associated with the most controlled of modeling exercises, there is no reason why these cannot be documented clearly as such, and distinguished from more formal data and calculations, which should also be clearly documented as to their origins, margins of uncertainty, etc. Informal craft judgement should be accepted as a legitimate part of modeling and analysis, but it should be clearly labelled for what it is.

(d) Input files should be published as such with no selection or editing, and far more critical evaluation of data should take place before it is allowed to be used. Rough estimates, even guesses of course, have a place, but the effects of cumulative ignorance in such inputs should be acknowledged, and interactions with uncertainties in empirical validity of model structure be systematically explored.

(e) Technical sensitivity analysis should be as extensive and formal as possible on key model parameters (including those which may be defined as outside the model structure but which may crucially affect its validity or relevance). This is probably the most important single norm for proper policy analysis since proper SA should define the scope of given assumptions, the elasticity of "constraints", and the importance of given decision options.

(f) Furthermore, an important extension of formal sensitivity analysis would be to have social scientists, expert in the given policy field and not frightened by the mathematics, go through a model and its inputs systematically identifying behavioral assumptions (and the range of alternative choices of such assumptions) that the technical modelers have made, often unwittingly. Even without quantification of the cumulative impact of such behavioral uncertainties, this would show other analysts and policy actors the relevance of the model and its inputs to the real world.

(g) The distinction between model validation in the technical sense of achieving internal consistency, and model *validity* in the sense of degree of real-world fit, needs to be more clearly defined and more widely understood. SA may be used for either purpose but the distinction is crucial

(h) During the development and running of a model, and not only as a one-off at the outset, there should be much more regular and detailed "revisiting" of the analysis by the policy actors, to keep the modeling exercise related to real world developments and problem definitions. Abstract exercises which exclude key real world parameters (such as prices) should be more clearly differentiated from those which claim real-world conclusions. The interaction between analytical models and non-scientific political discussion needs to be more open and clearly structured.

(i) The writing of executive summaries and of other summary versions of policy analytic exercises needs to be more critically reviewed. All too often, such summaries contain few of the qualifications and caveats born by the analysis itself, yet they are the most prominent public input of such studies. This not only distorts the policy field but usually eventually issues in a backlash against analysis.

There is no real distinction between these technical norms outlined above, and the institutional dimensions. For example, in (c) above, the clear labelling of informal judgements requires that the modeler be self-aware what he or she is doing rather than inserting a "taken for granted fact". This self awareness may be stimulated only by informal criticism and debate, which requires a given social structure of analysis. Furthermore, documentation difficulties are only confronted after a model has been developed and used, when they are not only boring but when pressures to justify particular uses and general model credibility are multiplied by previous publication. The peer review process should restrain publication until more documentation is also available, but the present institutional structure of policy and analysis does nothing to encourage this. In the past, and this applies to the IIASA ESP as well as others, elaborate modeling has run far ahead of adequate documentation to the extent that the analysis may not be supported by the fragility of the input data and assumptions. Decent critical evaluation of input data and founding relationships has not existed as a restraint upon ambitious modeling and analytical claims. The social structure of the field and not the corruption of individual projects alone has allowed this to become a normal feature of the area.

All of the norms proposed above - even the technical ones - imply the need for a given social structure of policy analysis in which accepted technical and procedural standards have real meaning by the award or refusal of credibility and status to given analyses and approaches. Given that practice in policy analysis by definition incorporates political questions and values, professional standards in analysis and representation imply related standards in policy. Thus, how policy analysis is conducted as an institutional- intellectual process acts as an implicit procedural norm for policy itself, and policy analysts have a responsibility for influencing the policy process for better or for worse, independent of substantive issues. Policy actors have a reciprocal responsibility to encourage this procedural model to develop and take hold in the policy sphere.

One cannot evaluate policy analysis as analysis, without also evaluating the processes - intellectual and institutional - that gave rise to it. This is a point of general importance. The more that policy analysis relies upon complex constructs such as models and intervening judgements, the more will procedural guarantees have to replace empirical points of reference as indicators of public credibility. In these conclusions I will also argue that the appropriate "style" or model of policy analysis, including the kinds of problem thought meaningful to pursue, depends upon: who are thought to be the proper audiences for such insight; who are "policy makers" in a field like energy?; what kinds of decisions policy makers are thought to be able to, or should make; what is thought to be a "given" framework within which "options" are conceivable; and thus what kind of policy process *itself* is envisaged as given. In other words, a particular style of policy analysis grows out of and supports as "natural" a particular vision of the policy process as a set of institutional arrangements, or more bluntly, a particular power structure. One of the *policy* options to consider therefore is what *kind* of policy *analysis* is worth doing. Before elaborating on this point however, some brief recapitulation is necessary.

VI.2 What Keepin Found

As mentioned in the preface, this paper originated because of apparent contradictions between reactions to Keepin's analysis of the IIASA energy models, and documented assertions in the literature. As we have seen, Keepin's analysis falls well within normative standards that are generally accepted as basic technical standards in modeling, with respect to standard sensitivity analysis, documentation, etc. Conformity to such standards is implied by the IIASA project's own public claims - that it analyzed alternative energy options, that its conclusions were "robust", and that its formal models were an important part of the whole endeavor, safeguarding overall objectivity. There is no reason why Keepin's analysis could not have emanated from the mainstream of the IIASA ESP; indeed, given earlier, inexplicably discontinued work along similar lines there is occasion for surprise that it did not. As it is, Keepin's work is a sound example of model analysis, the need for which has been widely recognized. It is important to remember that this model analysis has exposed uncomfortable aspects of the IIASA modeling to others in the energy modeling "community", as well as to non-modelers.

Let us review what Keepin found:

(i) the IIASA models were of no analytical significance and were determined by informally generated assumptions;

(ii) these informal assumptions therefore dominated the analytical effort, yet were not fully documented and "made clear and open" as claimed;

(iii) even in the internal structure the central model, MESSAGE, suffers from extreme and critical brittleness that was not identified by its authors;

(iv) a large-scale and more formally controllable iteration for consistency around a much more significant model loop (and thus much larger empirical corroboration) was suggested than was in fact the case. The original four-model loop was reduced in the end to just two, and contrary to impressions given, the feedback iteration (critical for external credibility) from IMPACT to economic structure and revised demand was never effected;

(v) sensitivity analysis on the models with respect to basic factors such as relative fuel price variations was not properly performed, despite earlier work indicating its importance. Thus the claimed robustness of the models and scenarios was not established, indeed their extremely brittle nature was obscured;

(vi) therefore the claimed substantiation of the scenarios used to derive robust and internally consistent policy conclusions was never achieved.

In other words, to use Goldman's term, there was no externally checkable guarantee against "flimflammery"[158] either in the inputs or in the interpretation of the model outputs via the scenarios into policy conclusions, and the scenarios themselves shifted uneasily between hypotheticality ("a scenario is an assumption"[159]) and hyperthetičality ("our scenarios are globally comprehensive and allow for no escape"[160]).

Indeed, one interesting point about the IIASA study is that for all its emphasis upon stretching certain kinds of supply options to meet projected demand levels that were always tending to exceed conceivable supply, it could equally well have been interpreted in its public representations as the opposite - a "discovery" of the urgent need to push all out for energy demand-reducing measures.

In fact, elsewhere extra arguments are brought to bear against energy conservation measures on the grounds that they could "result in reduced productivities" [161]. What the arguments essentially mean is that conservation does not generate more skill and sophistication, i.e., information as a kind of investment capital, whereas the more capital-intensive supply technologies do so. Incidentally, this argument depends upon choosing a particular social definition of "information" as a resource, since conservation could also generate investable information, but of a different kind, held by different "policy actors" and involving different institutional arrangements. It appears to be an attempt at comparative evaluation of capital-intensive supply options versus energy efficiency investments, to act as stand-in for the nonexistent comparative cost-effectiveness evaluation of these options.

This speculative and highly original attempt to create a kind of information theory of energy does not however command the same credibility as an evaluation in more familiar terms of relative net costs, nor does it avoid the crippling lack of responsiveness to energy price fluctuations that was built into IIASA's scenarios. It is ironic that the IIASA ESP justified its neglect of the effects of energy price dynamics by seeking for "data robustness". [*EIFW, p.26*] What this means is that like anyone else they could not foresee price changes - relative between fuel and supply technologies, or overall - so they decided to fix them constant for 50 years. Thus the sensitivity of the scenarios to price is not examined (at either level: overall scale or internal mixes), even though this is widely recognized in energy policy as the key variable and even though very narrow differences in assumed prices were inserted into the models and scenarios, with big bang leaps between fuels when one category is exhausted. Thus it is especially ironic that with this disregard of the major time variable in the system, the ESP should make such a big issue of its discovery that the *time* for investing in the systems it concludes to be necessary is a critical factor. It is also symptomatic of the confusion sown by the inconsistent accounts of iteration and of the models, that at least some external modelers thought that the IIASA study excelled in its careful calculation of fuel mixes, a calculation that would have needed some attention to price sensitivities [162].

Yet again it should be stressed that, like monopoly input control, the monopoly "interpretation control" of the modeling exercise into conclusions is not new. In 1974 the so-called ERDA-48 Brookhaven energy model ran, amongst other things, a "conservation" scenario. Although it showed the largest net benefit of any near-term policy, it was not incorporated in ERDA's policy recommendations and was only reported in a technical appendix. Two years later, a revised version of the model, ERDA-76, produced essentially the same scenario, this time as its central conclusion. The first report had all the materials for the same conclusion, but was interpreted into the policy sphere in a completely different way. Although many critics attacked the ERDA-48 *model*, it was the institutional process in which it was embedded that was more relevant. Again there arises the question of established quality control and external review [163].

VI.3 Reading Policy Analysis

As we have seen, reading of the IIASA study for its policy meaning is made difficult by its inconsistency of self-description, which means that it is hard to see when a "conclusion" is a conclusion or a hypothesis, and when it is based upon formal analysis or informal judgement. Of course with such large and complex projects there are bound to be different emphases in the many presentations to different audiences, and it has to be recognized that the reader's expectations can influence interpretation of what is written. Nevertheless, these do not account for the problems, and one can see three more significant factors, all of which reinforced the tendency to overstate the objective authority of the findings.

(a) the historical inertia of institutionalized ambitions, and a consequent blurring of wish and perceived reality, eventually exposed by the lack of fulfillment of the ambitions;

(b) extreme pressure, given the particular circumstances, for self-justification as an objective example of applied systems analysis;

(c) a propensity common to science in general (and indeed all knowledge), to fuse accounts of how the knowledge was generated with justification of that knowledge, thus rationalizing away its more arbitrary, uncertain, or unruly aspects.

With the possible exception of item (b), and then only in terms of degree, none of these is unique to the IIASA study. Science also suffers from the first-mentioned tendency; indeed, this institutionalized faith that present technical and theoretical commitments will prevail over existing problems (though it does not normally stretch to such categorical claims to have solved them when it hasn't) is central to a given science's coherence and productivity. There needs to be a collective belief that the problems are virtually solved, in order to muster the necessary morale and concentration to even stand a chance of solving them. This does not alter the fact that it is a kind of mythology, balancing truth and falsehood in delicate, continual and creative negotiation.

As I have argued elsewhere, large-scale technological enterprises need the same ambivalent structures of organized fantasy to maintain the commitment and morale to stay the course [164]. Both science and technological paradigms even though they are quite well insulated, nevertheless enjoy the *eventual* discipline of a check against reality - pressure vessels crack before we thought, or enzymes behave as predicted, etc. In science, if overcommitment to a theory is penalized by eventual contradiction, all that might suffer are some reputations and plenty of pride. With technology the penalties may be more costly. Policy analysis however suffers from the same engagements in mythology, but with much less by way of concrete tests to keep faith within reasonable bounds. Yet major social commitments may be advocated, and conceivably made, or justified, by reference to such internally circular analyses misconceived as tested knowledge. Furthermore, such a style of analysis may, through the forms of social inclusion and exclusion it inevitably supports, buttress certain kinds of institutional policy-making structures that are less than optimal.

VI.4 Implicit and Explicit Discourses

Through Keepin's detailed technical critique and this analysis of the ***Energy in a Finite World*** study's public self-presentation, a large gulf between public rhetoric and private reality has been identified. Although greatly variable in extent, such a gap is not itself unusual, even in academic science. It is a form of routine authority ritual, expressing a normatively useful mythology of analytical practice. But the rhetoric should not be presented as if it were the reality. Although the sight of such gaps should not itself lead to moral frenzies about dishonesty, a lack of serious and sustained attention to this problem in the field as a whole will inevitably leave a general feeling of intellectual dishonesty. The point at which legitimate rhetoric (and the strict impossibility of total self-description) overspills into unacceptable misrepresentation cannot be universally defined. In principle, active professional peer exchange and self-examination should constantly negotiate it and thus safeguard against excesses. Fortunately in this paper we do not have to concern ourselves with the inevitable questions of honesty that spring up around the present case as they have for others. The whole point is that, regardless of the answers to such questions, there are deeper structural processes at work in the social process of analysis that will indeed foster a kind of intellectual dishonesty if not expressly acknowledged and counteracted via professional standards and procedures. In creating and enforcing such standards it may be well to bear in mind Marx's point that:

> "the demand to give up the illusion about its condition is the demand to give up a condition which needs illusions" [165].

There is a ***structural process*** in science which produces a fundamental tension. As is now widely recognized, scientists have to shelve some anomalies, evidential gaps, etc., in order to continue to work coherently. There are no ***a priori*** rules to show when an anomaly is insignificant, or when it harbors a major problem [166]. There are always inconsistencies which, if all taken seriously, would paralyze the great concentration of effort needed to progress at all. Scientists therefore progress by socially channeled selection of attention.

The faith that the existing paradigm will work - that it will eventually resolve the problems that are chosen to be neglected for the moment - is necessary, and thus needs constant social maintenance, both internally and externally. But this is itself inconsistent with the self-image deeply embedded in science and projected to the public. The socially supported dogma that things will work out is ambivalent because there is no absolutely identifiable point at which, in relation to existing evidence, it becomes ***unjustifiable*** dogma and arrogance. Scientists are so used to flexibly managing this tension in everyday life that they no longer see that overextended faith and commitment may be legitimately criticized as being beyond existing accepted evidence. They tend not to develop a faculty for self-examination because, partly they *need* to resist this self-examination and get on with articulating and building upon their paradigm and its constituent faiths.

If I have, so far stressed the similarities between science and policy analysis however, it is now time to identify the crucial differences, which have to do with *institutional setting*. Scientists in their own "private" specialities have a shared culture and powerful common symbols of proper practice. This allows the formal rhetoric of orderly method, objective purity, disinterest, skepticism, etc., as regularly repeated in scientific publications, to constrain the actual reality of informal practice within some bounds. Also because of this shared culture, scientists can be expected *amongst themselves* to tacitly understand the relationship between their formal and informal languages without ever having to spell them out as inconsistent.

A scientist who expressly pointed out the potential hypocrisy in this cryptic language would be immediately threatening and suspect. He would have violated a key, unwritten norm of mature practice that we all learnt to live with through graduate school. This practical norm is *not* proper for policy analysis however. Scientists know implicitly when and how to take, for example, the artificial tidiness and logic of a scientific paper with a large pinch of salt. They can more or less maintain by informal cultural self-management the "essential tension" of their mythology as a constructive framework of practice. It is, in this sense at least a condensed code culture [167].

It is very different when science or policy analysis interacts with the public, or policy makers. The same language of ordered rationality is expressed, but the external audience is not educated to take it in any way other than at face value - they are not part of the shared day-to-day working culture with its natural habit of reading between the lines of the rhetoric to see the messier, less compelling reality of how conclusions have been reached. Indeed, policy analysis, especially perhaps that conducted around large-scale modeling, tends to be structured in such a way that each modeling team is virtually *its own* peer group community.

Until recently there was almost no mutual examination in detail of analyses and model structure. Although science projects have the same fragmentary characteristics, most scientific research still takes place within well defined peer subcultures. Thus, unlike science, in the policy-making or debating spheres (and even within policy analysis itself) there is not a well established, close-knit community of specialists to share a common tacit understanding of the messiness of practice. Embryonic elements do exist in the US, as witnessed by the discussion in Section II, but even if they were fully developed policy analysis (unlike much of science) operates in a public dimension - it is only meaningful if translated into the public domain. Here unfortunately, modelers and scientists alike remain embedded in their internal habit of understating the arbitrary or less rule-bound, lack of tidiness of their practice and their products. They are not sufficiently aware of the different needs of the scientific and public arenas.

Thus when given repeated statements about quantified analysis, iterated models, etc., policy people will tend to believe what they hear, not what they should have sniffed out from the interstices of the tidy rhetorical lattice work, had they been part of the professional culture. (At least, if they don't like what they hear they will reject it on "gut reaction" grounds, not discriminating evidence from assumptions, and the controlled from the arbitrary, because this kind of expression does not help them to do so.) The result is that the *potential* for due skepticism or qualification that exists within the common culture of a scientific specialty (i.e., for the education and improvement of practice) does not exist for the policy world.

Scientists whose work does not impinge upon society have less need to be self-aware about the gap between their public accounts of how they achieve knowledge and how they really achieve it, because they have no one to mislead but themselves. On the other hand, policy analysts have a responsibility to develop this professional self-awareness and self-expression because, by definition, they present their knowledge as authority to a wider cultural arena. Even if it is not misled into bad policies by this style of communication, the policy process is never going to be better educated. Keepin's major misdemeanor was essentially that he rendered explicit (and thus made available to a wider audience) the implicit "half-knowledge" of the IIASA team; i.e., the uncomfortably messy nature of the real "analytical" process. The reaction to Keepin was effectively that he had attacked a "straw man" created in the public rhetoric that did not exist in the knowledge of mature experts. Had he been a mature scientist and not a kindergarten one, he would not have taken this public image so seriously and wasted any energy over it. He would then have been playing the unwritten rules of the insider fraternity. His innocence was not of modeling *per se*, but of the tacit conventions of cryptic discourse that not only maintain credibility with and exclude external audiences, but also thereby sustain a particular process of policy analysis (and policy). In creating this explication, whether he knew it or not, Keepin implied the relevance of other audiences, and thus a different kind of policy making and policy analysis.

Indeed, one of the characteristics of the kind of policy process with which such large-scale, immodest modeling corresponds is a chronic undereducation of policy debate, where immodest claims generated by the structural processes I have described (as well as *deliberate* inflation too, no doubt) result in backlash and dogmatic counterclaim, obliterating any discriminating middle ground. This undermines resilience in the policy process itself. A very good example is the Rasmussen Report (WASH-1400) on nuclear reactor safety. This provided a lot of valuable detailed work, but its *Executive Summary* and publicity were overanxious to sell the "proven" safety of nuclear power. In response to the inevitable reaction to this oversell, the American Physical Society's review panel produced a critical evaluation of WASH-1400 which led the US NRC to disown it. Yet WASH-1400 contained analyses of accident sequences, some of which foresaw non-negligible probabilities in the kind of sequence that was to happen years later at Three Mile Island. However, deeply embroiled in the conflict generated by the initial oversell, the NRC never noticed or attended to the practical implications these foresights insight buried in the body of the study [168]. A similar kind of process was also evident in the debate following the Inhaber analysis of comparative risks of energy technologies.

IV.5 Styles of Analysis, Structures of Decision and Policy

The systematic undereducation of the policy arena by modeling is also implied by the remark of one analyst that in many ways even authors of conflicting models seem closer to each other in beliefs than to the public at large [169].

I have stressed that it is illusory to claim a discoverable objective substratum of "the" energy problem. Energy policy as we define it may incorporate everything, since everything we do or use converts energy, or it may be defined on a much more micro-social scale. Thus Greenberger's observation is relevant that:

> "One reason for Federal ineptness in energy programs was the large variation around the nation in local needs and resources much of which could not be accounted for in national policies" [170].

Yet when he asserts that:

> "...analysis is most needed and potentially most useful when it helps in making or changing complex and important decisions" [171],

he does not spell out the implication of the first point, that policy does not *necessarily* have to be made up only of big "complex and important" decisions. This implies yet again only centralized perceptions, management, central channeling of capital, information, and other resources, requiring more and more "scientific" analyses, generating perhaps *more* distance between modelers, policy makers, and the public. A different structure of policy *making*, possibly more responsive to those variable local needs and resources, and thus perhaps less "inept", would involve a correspondingly different structure of decision problems. In turn this would engender different policy-analytic problems and processes, together with different relationships between decision makers, analysts, and publics. These would be reflected in different forms of communication such as self-description to others, and different styles of analysis. By "style" I mean the way a problem is conceived for analysis, its relationship to different possible partner groups ("clients") and to their practical problems; its flexibility towards radical revision, and its openess of self-representation about its own process and cognitive products. It is the nexus of knowledge and practice. Schelling's discussion of implications of possible carbon dioxide climate changes [172] illustrates the connections necessary to keep alive in analysis, where revisiting the problem and the parties allows redefinition of the policy problem, and corresponding redefinition of who are the relevant actors, how they relate to the problem, and how further analysis should be conducted institutionally and intellectually in the light of such social-cognitive changes.

Thus it is interesting that an influential school of thought amongst modelers and model users sees a composite need for: more established external model analysis; more systematic attention to real institutional factors and feasibilities such as interest group structure and interactions, local variations etc.; more repeated interactions ("revisiting") between model users (policy makers), modelers, and those on the receiving end of policies [173] during the development and running of models; more attention to transfer of model insights and limitations to the public policy arena generally; and more concern to re-establish crumbling public credibility of policy making by use of more modest language about models.

All of these suggest a closer involvement of applied systems analysis at the institutional, implementation end of policy, where grand visions of problems and big decisions usually end up as scrap but may avoid being scrapped. The logic is essentially simple, and moves from technical modeling considerations to institutional realities; models should be robust towards real world parameters the most variable and sensitive of which are often institutional (responses to price changes, public perceptions of government competence leading to opposition and large delays, international events, etc.); and the scale of models should not overreach their sensitivity to such potential discontinuities. This means regular "revisiting" of the policy problem and public impacts by the analysts [174] which means more regular contact *in the modeling process* and openness to redefining the analytical problem. Therefore the only viable models are ones which are somewhat interactive, always have implementation on the agenda (the policy maker - public relationship), and never get overly large because they may need to be recast for a new problem definition at short notice. This logic has been put into practice by Holling and others with their adaptive environmental assessment and management [175].

The IIASA ESP has gone in the opposite direction, by formulating a problem which fits no decision making body, requires an utterly top-down capital-intensive, consumption-oriented, science-dominated, centrally organized policy process, in which people have meaning only as passive energy (and policy) consumers and not as policy actors themselves. By assuming a large-scale study within these taken-for-granted constraints, the authors inevitably impose these as normative.

This style of policy analysis has to have big decisions and big decision-making bodies corresponding with the root conceptual and methodological framework employed. It also has to have big pretensions to sustain its exclusion of others from the policy process. In this sense, the reference to the "educational value" of the IIASA study refers to the private education of its authors rather than of the policy community. Although the assumption must be that this is then transferred (suitably digested) to the policy world, the forms of communication do not enlighten policy in the way it may most need. Thus, however "grown up" the analysts and their circle may be, in this style the policy process remains doomed to a kindergarten understanding of science and technology.

A different model of the way things happen in the world - of human relationships and activity - would start from the fact that any policy is dependent on people's behavior and beliefs. Since we do not know the limits of these diverse capacities, we should as far as possible allow those same people to explore and define these limits, rather than preordain and imprison them in the intellectual and emotional commitments of global models and analysis, from whatever particular social bias this originates. One might then explore an entirely different action-based approach to analysis; for example, building up progressive regional pictures of what is possible in locally efficient energy use and supply, and *by one's analytical approach and social setting*, encouraging countless diverse initiatives to reduce "global" energy demand. Frequent "revisiting" might, in this style, be preordained by the closer social relations of the analysts with ordinary people. One could then examine what demand would be left to be satisfied by international trade, megalithic centralized supply or manufacturng consortia, etc., keeping an open mind as to how much would be needed, if any, of any particular energy option.

This kind of policy approach is being practiced, at least in part, by some international energy bodies [176]. The point is that a different model of analysis is being used, consistent with different relationships between analyst, policy maker and people at large, and a different vision of policy making. If policy analysis is to be useful and relevant to the real problems of the 1980s (and especially if it claims *global* status), its standards will have to include a conceptual ability to clarify the social and cultural processes underlying various biases and the incompatible worldviews they engender [177]. Policy analysis can never claim objectivity, not because of some failure but because it has a normative ***responsibility*** to articulate and exemplify acceptable standards of analytical *process* as well as ***substance***. Different models of analytical process imply different policy processes themselves.

Unfortunately, all the sincere scientific and technical huffing and puffing of ***Energy in a Finite World*** served only to elaborate one such partisan worldview and helped to enmesh this worldview deeper in its own particular mythology. I am not interested here in advocating one form of energy policy analysis or process over another, but it is important to recognize the general point that the style of analysis adopted, and the institutions and processes which engender that style are already automatically biased in terms of what kinds of policy could be conceivable, let alone what specific options may be conceivable. Also, since applied systems and policy analysis seem to have fused in the cauldron of broad social debate and interaction, not in the service of a particular decision maker, a new measure of the quality of analysis may have to be its effectiveness in educating the policy debate and the public generally. One central factor in thus providing the policy process with options will be to clarify the social options underlying the kinds of analysis available, and this requires being more clear about the limitations and social value underpinnings of given kinds of methodology and analytical processes.

VI.6 Quality Control

Quality control has been an underlying theme of this paper, which we will now bring to the fore. As Majone has stressed [178], policy analysis needs a professional forum, to uphold norms of procedure and practice like scientific disciplines. The recognition that some elements cannot be incorporated except through intuitive judgement only strengthens the need for a peer community that is involved in the same craft work and is competent to judge it, even in the absence of explicit rules of inference or method.

Yet there are some crucial differences between science and policy analysis as institutional forms. One has already been mentioned, namely that policy analysis has an inevitable external audience, which affects the importance of self-description. Policy analysis is evaluated not only by peers, but also by lay audiences. The latter requires not only the products of policy analysis, but also access to explicit accounts of the *process* of analysis itself. For example, quality control in science is based upon institutionalized commitments including goodness of fit with existing consensus on what is a proper problem to define, a proper method to use, and what is a reasonably plausible result or explanatory framework. Professional consensus on these enables quality control to be flexible but reasonably welldefined for any given scientific specialty. For policy analysis, however, there is no consensus even on what is a proper problem to address, or what methods should be used. The field is fragmented by the fact that no sanction can operate against analysts who take up a particular problem because it is well funded or because of their institutional affiliations, while others take up different ones. This is especially acute when, as sometimes happens, analysts are significant policy actors too. Unlike science there is no accumulated tradition and welldefined social group to discipline this primary process, and little likelihood that one will arise, given the sociopolitical nature of the definition of policy-analytic problems.

Thus quality control and sociopolitical considerations are inextricably connected. Greenberger's account of the rise of countermodeling, running models with conflicting assumptions from those of an adversary, also illustrates this. A model indicating that there was no need for a nuclear reprocessing and fast reactors policy was used by opponents wanting the opposite result, with different assumptions,

> "What became clear from the exchange, was the extent to which the conclusions depended upon assumptions about the availability of uranium, the future demand for electricity, and the use of coal"[179].

In other words, through the roundabout means of attempts to *justify* opposite desired policies and values, a useful sensitivity analysis was eventually achieved. Quality control apparently had to depend upon political conflict, and the forum was inevitably politically structured. But this potential problem can also be seen as an opportunity and a challenge for policy analysis. For if in such circumstances the policy analytic profession can establish good rules of *procedure* including self-accountability, this becomes an effective model for the political process too.

What I am saying here is that analysis is a *social relationship*, in a structural policy process, not merely an independent input to that process. The point is supported in the insights of ethical philosophers, that "telling the truth" is not only merely a matter of stating an objective truth independent of concrete social situations and relationships. They uphold a distinction between thought and speech that we might parallel in the distinction between science and policy analysis. Thought, like science, may refer only to *things*, and truth (or otherwise) in thought or science may be defined in relation only to those things. Speech and policy analysis, on the other hand, refer to things *and* people since they take place in a relationship of speaker to hearer (analyst to policy world or "client"). They uphold (or deny) a social relationship as well as embodying objective truth (or not):

> "Quite apart from the veracity of its [speech's] contents, the relation between myself and another man ... which is expressed in it is in itself either true or untrue. I speak flatteringly or presumptuously or hypocritically without uttering a material untruth; yet my words are nevertheless untrue, because I am disrupting and destroying the reality of the relationship between man and wife, superior to subordinate, etc. An individual utterance is always part of a total reality which seeks

> expression in this utterance. *If my utterance is to be truthful it must in each case be different according to whom I am addressing, who is questioning me, and what I am speaking about.* The truthful word is not in itself constant; it is as much alive as life itself. If it is detached from life and from its reference to the concrete other man, if "the truth is told" without taking into account to whom it is addressed, then this truth has only the appearance of truth, but it lacks its essential character [180].

The point seems to be that the authenticity of self-description of a complex human project is not open to simple unitary evaluation. It may be more or less true or false according to the *significance* of what is said *and what is not said* to different audiences. Thus to other insiders who tacitly know, it may not be "untruthful" to leave out references to failed runs, arbitrary interventions, or whatever. To leave out the same when speaking to a public audience may be open to different evaluations, according to one's view of the proper relationship between analysis and the public, and whether the public "needs" to know such things, whether they would "overreact", whether they are "consumers" of policy or policy actors in their own right, etc., etc. Different self-descriptions presuppose or uphold different analysis-policy relationships and different policy *processes*, because they imply different responsibilities, technical and decision resources and power, to different actors in policy itself.

VI.7 Robust Knowledge and Robust Policy

This leads to a final point that again crystallizes a theme threading this paper. In science, as in policy analysis, the time is past for pretending that *substantive* objectivity can be found and built in to the foundations of policy making. There is the repeated spectacle of scientists claiming objectivity or certainty, only to be unmasked not only to their own discredit but to that of analysis and policy making generally. Policy making, like science for policy, is intrinsically about *justification* of policies as well as objective understanding. This corresponds to the attempts by philosophers of science such as Ravetz to cause epistemology to catch up with new realities, and formulate a view of science as useful and robust knowledge in the policy arena [181]. This naturally pluralizes policy-related science. It comes to be seen as a partisan tool for policy debate, not as an objective nugget lying uncorrodable in the center of all positions.

Of course, since time immemorial, part of the process of justification itself has been to present one's cherished values and beliefs as objective nuggets rather than as partisan tools. The point is that, so overworked, the nuggets become brittle and are soon pulverized into worthless dust. But the central point of this new approach is perhaps that of *robustness*. To produce a robust policy process means that analysts will have to eschew the cheap sale of implicit values as objective policy nuggets, because this false claim that they are nuggets is the root cause of brittleness in the whole policy process. Thus not only can tools be refined as we go along because they can be honestly acknowledged as blunt or underdeveloped, but in so characterizing themselves they sustain a more robust policy *process* in the long run.

We should not underestimate what this might mean. If policy knowledge in its technical dimensions becomes more provisional "in public", so too will public commitments relating to those technical dimensions necessarily become more provisional. This may be an overdue corrective to overcommitment and to the institutional forms that correspond with it. Bearing in mind that the epistemological principle of "robust knowledge" embodies such institutional connotations, we therefore need to encourage more clarity and self-consciousness about the biases, limitations and messiness of our analytical methods and processes even if this has to start through adversarial confrontations, so as to see how really underdeveloped our robust tools are. We then may not reach too soon for "robust" conclusions, and thereby eventually may produce a more robust policy process. And, in the end, a robust policy process is the strongest guarantee of robust policies.

APPENDIX

(WITH REFERENCE TO SECTION IV.1)

The following synopsis is based on Otto Keck's exhaustive analysis of policy making in the fast breeder program in the Federal Republic of Germany. [182]

In September 1964, the General Electric Company (GE, USA) announced its confidence in offering a commercial FBR within one decade. Although this was actually a political move resulting from a domestic skirmish with the US Atomic Energy Commision (AEC), scientists developing the FBR at Kernforschungszentrum Karlsruhe (KFK, FRG) interpreted GE's announcement as an open sales attack aimed at capturing the international FBR market envisaged for the mid-1970s. Therefore, the Karlsruhe scientists perceived an urgent need to jump several years ahead of their own FBR schedule, and immediately begin the design and construction of two prototype reactors. It was necessary to convince the Science Ministry of the imperative for swift action. By mid-1965, under prodding from Karlsruhe, industrial consortia had already been formed for each prototype reactor, and funding applications were submitted to the Ministry. The Karlsruhe scientists strongly supported these applications, stressing not only the urgency brought on by stiff international competition, but also citing a new study that had just been completed at Karlsruhe. This study showed a clear economic advantage for the FBR over the light water reactor (LWR). As described by Keck (p.

108)

> In presenting their electricity-cost estimates to the Science Ministry, the Karlsruhe scientists characterized these not as uncertain guesses but as clear evidence. Their estimates, they said, showed very clearly that according to present knowledge the FBR had the greatest economic potential. Although they recognized that some uncertainty was involved, they stressed that their figures allowed almost no doubt, pointing out that their calculations did not take into account the future development potential of the FBR.

In addition, a cumulative net saving of DM1 billion by 1984 was calculated to result from commercial application of fast breeders (p.100)

However, industry and utility officials did not share the optimism of the Karlsruhe team. "As interviews with a number of participants confirmed, the larger reactor manufacturers and the utilities did not consider the reports about GE's fast breeder plans to be worth particular attention; and they regarded the Karlsruhe cost projections as a numbers game that was not to be taken seriously." (p. 102) ***Nevertheless***, the Science Ministry accepted the Karlsruhe assessment with only minor reservations, and final approval of the applications came in February-March, 1966. In the following November, a total of DM 96.2 million (exactly the amount requested) was appropriated to the consortia for the design of both prototype reactors. "The ministry's justification for the expenditure followed the Karlsruhe arguments referring to the vast commercial potential of the fast breeders in the near future and to the pressing international competition." (p. 96)

One of the major findings of Keck's analysis is that "the dominant influence in the decisions in the FBR program in the FRG was not from the industrial sector but from the government organizations, namely, the ministry in charge of the program and the Karlsruhe laboratory The idea for the fast breeder program was born at Karlsruhe." (p. 225) Indeed, "Karlsruhe's perception of an imminent international competition and its estimates of future electricity costs were taken for granted uncritically by the ministry." (p. 102) However, as events actually unfolded, experience "proved the Karlsruhe laboratory's estimates of electricity costs to be grossly off the mark. Fears about American competition turned out to be unfounded, and the more skeptical assessment of industry warranted. The year 1974 has passed without a commercial FBR anywhere in the world." (p. 103)

The manufacturing industry kept its skepticism about the Karlsruhe calculations to itself and went happily along with the plan. Only later when the government asked industry to contribute did it voice its disbelief in the justifying study.

Leaving aside the clear political maneuverings that must have been involved in the decision to fund the prototype breeders, what is especially relevant here is that a scientific study of electricity cost estimates was used not only to influence, but also to *justify* huge capital investments. Thus it is of interest to take a closer look at this study.

In the fall of 1964, shortly after the announcement by GE, a group of scientists was assembled at Karlsruhe in the Institute for Applied Reactor Physics, directed by Wolf Häfele. The group, consisting mostly of physicists (and no economists), used the available economic data on various types of reactors to produce projections of future costs of electricity generated from various reactor designs. Two FBR designs were included (one each from Karlsruhe and GE), along with a total of six other reactor designs, including LWRs. The conclusion was that the cost of electricity generated from either of the two FBR designs was *exactly the same*, and that this cost was 15% cheaper than the nearest competitor, LWR. However, as Keck describes (p. 110), the fact

> that the electricity-cost estimates for the two FBRs were identical up to the last digit ... was not the contingent result of the input data. If the cost data used for the calculations were compared with the source data on which they are allegedly based, ... one can see that a number of changes were made in the fuel-cycle and in the capital-cost data. As not all these changes can be explained by adjustments for consistent definitions and ground rules, one must conclude that ***input data were tuned*** so as to produce the same electricity costs for the two fast breeder designs. Thus the figures were capable of ***creating an impression*** as if estimates by Karlsruhe and GE coincided for different reactor designs. [my emphasis]

In addition, much was made of the 15% cost advantage (0.3 DPf/kWh), even though the same cost difference was claimed to be insignificant by the same researchers in a later report which compared two different LWRs (pp.110-111):

> Argumentative rhetoric was involved also in presenting a 15-percent advantage of the FBR over the LWR as evidence for the fast breeder's great economic potential. ...
>
> A later report by the study group on nuclear energy reserves included an AEG design of an LWR plant of boiling-water type. Because of an increased burnup, this plant arrived at a cost advantage of 0.33 DPf/kWh over the Siemens design. Whereas in the 1965 publication such a difference was presented as evidence for the commercial superiority of the FBR over the LWR, the 1966 report did not hesitate to deny that such a difference mattered in assessing different types of LWRs.
>
> It is not the task of this study to ask whether the Karlsruhe scientists were legitimate in their rhetorical use of overly precise figures. Rhetoric is part of everyday political life. More important is the question as to the quality of a policymaking process that is receptive to the unrealistic assessments that underlay this rhetoric.

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[27] In various reviews: *Atom*, September 1981, pp.37-38; *Popular Science*, July 1981, p.49. The study was called "perhaps the most impressive of such (energy) studies to date. It was performed by IIASA, in Vienna, using teams of scientists from 20 countries..... It is by all odds the biggest and perhaps the first truly global and long term examination of the energy situation;" *BioScience*, 31(4), April 1981, p.292 - "the first truly global and long-range examination of the world's energy future;" *Industrial Heating*, April 1981, p.4 - "the first comprehensive and professional survey" (quoting Chauncey Starr); *Dawn*, 19 August, 1981 - "unprecedented, detailed analysis.... analysing options in a quantitative, mathematical form".... led by "a

colossus in the form of Prof. W. Häfele,.... a world celebrity;" *Physics in Technology*, November 1981 - "one of the most comprehensive analyses of the world energy dilemma;" *Physics Bulletin*, November 1981 - "(Global energy) constraints are examined in detail by computer modelling."

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[64] This attitude is difficult to pin down precisely, because it has so many forms of expression. A valuable review is I. Cameron and D.O. Edge, *Images of Science*, London, Butterworths, 1977. As the orthodox scientific disciplines and reductionist approaches were criticized in the 1960's and 1970's, wholistic pretenders such as (some versions of) ecology, and general systems theory, took over as the guardians of this scientific morality.

[65] See for example, the great policy conflicts between the "planners of science" and the "republicans of science" in the post war period. The former were typified by the left wing scientist, J.D. Bernal, in *The Social Functions of Science* (Harmondsworth, Pelican reprint, 1968, Vols. I-IV), who wanted to see all scientific progress managed according to good socialist criteria defined outside science. The latter were led by the conservative Polanyi, *The Logic of Liberty*, London, Routledge, 1951, who defined science as governed ultimately by inaccessible craft experience and refined institutions which no outsider could master. Thus scientific progress could not be externally planned and must be left to the scientific elite.

[66] M. Polanyi, "The Tacit Component", in *Personal Knowledge*, op.cit. [2], pp. 69-248. This "craft" element of science, a residual component of scientific progress, has been the source of much debate, for example, over the validation of when it is progressive to ditch one paradigm and switch to a competitor, or, put another way, when it is retrogressive to hold on in the face of anomalies in the faith that those anomalies will eventually be resolved by the prevailing paradigm. Lakatos provided a major advance when he proposed that

this could be done only with the benefit of hindsight. The "objection" that this offers no rule of rationality can be accepted as true but not an objection, and the progressiveness of science seen as based on selection from many redundant options of those theoretical frameworks which work as forms of technical manipulation of nature. Noone guaranteed that this process should be efficient in some overall historical sense and history of science increasingly indicates that it may have been progress, but not necessarily efficient progress according to some universal rational rule. I. Lakatos, "History of Science and its Rational Reconstructions," in R. Buck and R. Cohen (eds.), *Boston Studies in the Philosophy of Science*, Vol.8, Reidel, Dordrecht, 1971; S.A. Shapin, "History of Science and its Sociological Reconstructions," *History of Science*, 20, September 1982. See also S.A. Shapin, "Social Uses of Science," in G.S. Rousseau and R.S. Porter (eds.), *The Ferment of Knowledge: Studies in the Historiography of 18th Century Science*, Cambridge University Press, 1981, pp.93-139.

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[71] See for example, *EIFW* and the references [28] and [33].

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Prepublication Issue, IIASA, April 1982, p.xxx.

[73] *Energy in a Finite World, Executive Summary*, op.cit. [32], p.29.

[74] Häfele, *Futures*, op.cit [33].

[75] Häfele, op.cit. [30].

[76] For example, Häfele, *Futures*, op.cit [33], p.33 ".... a transition to nuclear and solar will be inevitable."

[77] W. Sassin *et al.*, "Fuelling Europe in the Future, The Long-Term Energy Problem in the EC Countries: Alternative R & D Strategies," IIASA Research Report, RR-83-9/EUR 8421-En, March 1983, Laxenburg, Austria, pp.18-19.

[78] Häfele, *The Energy Journal*, op.cit. [36].

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[82] W. Häfele and P. Basile, "Modeling of Long Range Energy Strategies with a Global Perspective," in K.B. Haley (ed.), *Operations Research '78*, North Holland (1979), pp.493-529.

[83] *Ibid.*, p.502.

[84] *Ibid.*, p.522.

[85] *Ibid.*, p.499.

[86] *Ibid.*, p.516.

[87] P. Basile, "The IIASA Set of Energy Models: Its Design and Application," IIASA Research Report, RR-80-31, December 1980. One can follow essentially the same paper in the final *EIFW*, pp.398 *et seq.*.

However, the passage quoted here has been omitted from *EIFW*, and the model set has by then dropped MACRO.

[88] *EIFW*, pp.404-6. See also for example, Basile, op.cit. [87], p.16.

[89] P. Basile, "The IIASA Set of Energy Models: Its Design and Application," in *Energy Modeling Studies and Conservation*, UNECE Seminar, Washington, D.C., 1980, published Pergamon Press, 1982, pp.87-126.

[90] *Ibid.*, p.94. See also [87], p.6.

[91] Basile, op.cit. [89], p.97.

[92] H.-H. Rogner, IIASA Colloquium, May 1983. See also Häfele's account of IMPACT's demise, as quoted by Keepin, p.45. This destroyed the main claim for feedback round the full macroeconomic loop.

[93] *Executive Summary* of *EIFW*, IIASA, 1981.

[94] Schrattenholzer, op.cit. [72], p.xiii.

[95] IIASA Colloquium, May 1983.

[96] J.E. Samouilidis and C.S. Mitropoulos, "Energy-Economy Models: A Survey" *European Journal of Operational Research*, *11*(1982), pp.222-232.

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[98] Samouilidis and Mitropoulos, op.cit. [96], p.229.

[99] D. Meadows, letter to B. Keepin, 26 April, 1983, and personal communications.

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[101] *Ibid.*, p.177.

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[104] Häfele and Basile, op.cit. [82], p.504; *EIFW*, p.405.

[105] Häfele and Basile, op.cit. [83]. p.528, "What are we learning from these preliminary explorations of the range and utility of our models? *We are finding* an increasing capital intensiveness in the energy sector, particularly in the critical years following the end of the century."

[106] Indeed ironically, the fact that MEDEE-2 considered technologies of energy conservation but not their prices whilst MESSAGE involved notional fuel prices but not supply technologies, was made a virtue

in the study's claim for the flexibility of using judgementally linked model sets rather than all-inclusive megamodels.

[107] W.D. Nordhaus, *The Efficient Use of Energy Resources*, New Haven, Yale University Press, 1979.

[108] K. Parikh, IIASA Colloquium, May 1983.

[109] H.-H. Rogner, IIASA Colloquium, May 1983.

[110] H. Konno and T.N. Srinivasan, "The Häfele-Manne Model of Reactor Strategies: Some Sensitivity Analysis," IIASA, Research Memorandum, RM-74-19, Laxenburg, Austria, 1974; A. Suzuki and L. Schrattenholzer, "Sensitivity Analysis on Hydrogen Utilization Factor of the Häfele-Manne Model," IIASA Research Memorandum, RM-74-30, Laxenburg, Austria, 1974.

[111] Konno and Srinivasan, op.cit. [110], p.11.

[112] A. Suzuki, "An Extension of the Häfele-Manne Model for Assessing Strategies for a Transition from Fossil Fuel to Nuclear and Solar Alternatives," IIASA Research Report, RR-75-47, Laxenburg, Austria, 1975.

[113] W.D. Nordhaus, *The Efficient Use of Energy Resources*, op.cit. [107] esp. Chapters 5 and 6; "Thinking about Carbon Dioxide: Theoretical and Empirical Aspects of Optimum Control Strategies", Cowles Foundation Discussion Paper 565, Yale University, New Haven, 1980; and W.D. Nordhaus and G.W. Yohe, "Future Carbon Dioxide Emissions from Fossil Fuels," U.S. NAS Report, *Changing Climate*, Washington, D.C., 1983, pp.87-152.

[114] Häfele, *Futures*, op.cit. [33].

[115] *Ibid.*, p.33.

[116] Häfele, *Science*, op.cit. [28].

[117] *Ibid.*, p.160.

[118] Sassin, *Scientific American*, op.cit. [33].

[119] Häfele, Knoxville lecture, op.cit. [33].

[119b] W. Häfele, "Hypotheticality and the New Challenges: The Pathfinder Role of Nuclear Energy," *Minerva*, *XII*(1), 1974, pp.303-323.

[120] W.D. Nordhaus, "World Modelling from the Bottom Up," Research Memorandum, RM-75-10 esp. pp.9 and 17, Laxenburg, Austria, 1975.

[121] Ausubel and Nordhaus, op.cit. [100].

[121b] L. Schrattenholzer, "The Energy Supply Model, MESSAGE," IIASA, RR-81-31, p.5, Laxenburg, Austria, 1981.

[122] Schrattenholzer (ed.), op.cit. [72].

[123] Goldman, op.cit. [4], p.224.

[124] See for example, W. Häfele and W. Sassin, "A Future Energy Scenario," paper to 10th World Energy Conference, Istanbul, Turkey, September 1977; and Häfele and Sassin, "Energy Strategies," IIASA RR-76-8, e.g. p.27.

[125] Häfele and Sassin, op.cit [124], 1977.

[126] Nordhaus, op.cit. [107], p.xvi. He, of course, is not the only person to have made the same judgement, nor is Häfele the only one to have made the more optimistic one. The point here is not to say who is correct, but to point out the asymmetry of assuming FBR's as proven whilst alternatives are treated as unproven. The asymmetry'

reflects social institutional commitments which happen to correspond with FBR's (and centralized large-scale synfuels production, etc.).

[127] Häfele and Sassin, op.cit. [124], 1977.

[128] This is the essence of the Popperian epistemology of science, continually and open endedly improving via intrinsic self-criticism. The policy analysis equivalent is C. Lindblom's classic, "The Science of Muddling Through," *Public Administration Review*, *19*(1959), pp.70-88. See also D. Collingridge, *The Social Control of Technology*, London, Frances Pinter, 1980.

[129] See for example, M.J. Mulkay, *Science and the Sociology of Knowledge*, London, Allen & Unwin, 1980; Edge and Barnes (eds.), op.cit. [3]; B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, London, Sage, 1979.

[130] B. Wynne, " Redefining the Issues of Risk and Social Acceptance," *Futures*, 20, Jan.-Feb. 1983, pp.1-21.

[131] Y. Ezrahi, "The Jensen Controversy," in C. Frankel (ed.), *Controversies and Decisions*, New York, Russell Sage, 1976; Ezrahi, "The Authority of Science in Politics," in A. Thackray and E. Mendelsohn (eds.), *Science and Values*, New York, Humanities Press, 1974.

[132] P. Doty, "Can Investigators Improve Scientific Advice? The Case of the ABM Dispute," *Minerva*, *XI*(1973).

[133] See for example, Wynne, *Rationality and Ritual*, Chalfont St. Giles, British Society for the History of Science, 1982; M. Douglas and A. Wildavsky, *Risk and Culture*, London and Berkeley, University of

California Press, 1982; M. Thompson, "Among the Energy Tribes: An Anthropological Analysis of the Current Energy Debate," IIASA Working Paper, WP-82-59, Laxenburg, Austria, 1982.

[134] Häfele, Knoxville lecture, op.cit. [33].

[135] *Energy in a Finite World, Executive Summary*, op.cit. [32], p.7.

[136] See for example, refs. [28], [30] and [33].

[137] M. Glanz *et al.*, "Climate-related Impact Studies; A Review of Past Experiences," in W. Clark (ed.), *Carbon Dioxide Review, 1982*, New York, Oxford University Press, 1982, pp.57-93. Also Greenberger's examples in *Caught Unawares*, op.cit. [23] especially the Rasmussen WASH-1400 Report.

[138] H. Grümm, D. Gupter, W. Häfele, P. Jansen, M. Becker, W. Schmidt and J. Seetzen, *Ergänzendes Material zum Bericht "Kernbrennstoffbedarf und Kosten verschiedener Reaktortypen in Deutschland"*, KFK 366, KFK 466, Karlsruhe Nuclear Research Centre, F.R.G. (1966).

[139] O. Keck, *Policymaking in a Nuclear Program: The Case of the West German Fast Breeder Reactor*, Lexington, Mass., D.C. Heath, 1981.

[140] K. Weick, *The Social Psychology of Organization*, Reading, Mass., Addison-Wesley, 1969, p.38. Polanyi's *Personal Knowledge*, London, Routledge, 1958, is an early detailed treatise on this theme from an accomplished scientist. For other examples, e.g. from judicial rationality, see for example Chapter 7, Wynne, op.cit. [133]. See also G. Majone, "Policies as Theories," *OMEGA, The International Journal of Management Science*, *8*(1980), pp.151-162, and IIASA Research

Report, RR-80-17, Laxenburg, Austria, 1980. See also Lon Fuller, *Legal Fictions*, University of Stanford Press, 1967.

[141] K. Knorr-Cetina, *The Manufacture of Knowledge*, Oxford, Pergamon, 1981.

[142] G.N. Gilbert and M.J. Mulkay, "Warranting Scientific Belief," *Social Studies of Science*, *12*(3), 1982, pp.383-408; J. Law and R.J. Williams, "Putting Facts Together: A Study of Scientific Persuasion," *Social Studies of Science*, *12*(4), 1982, pp.535-538.

[143] A. Rip, "Legitimations of Science in a Changing World," in T. Baumgarten (ed.), *Wissenschaftssprache und Gesellschaft*, forthcoming, 1984.

[144] *Atom*, op.cit. [27].

[145] *Dawn*, op.cit. [27].

[146] W. Häfele, letter to B. Keepin, 7 April 1983.

[147] Basile, op.cit. [87], p.4.

[148] *EIFW*, Preface, p.xiii.

[149] *EIFW*, Foreword, p.xi.

[150] *EIFW*, Preface, p.xiv.

[151] *EIFW*, p.426, footnote.

[152] Häfele, Knoxville lecture, op.cit. [33], p.6.

[153] In fact they were acknowledged as important, but not included in the modelling, even though at other points, e.g. *EIFW, p.405* they are said to be included.

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Models," *Journal of Business and Economic Statistics*, *1*(1), January 1983, pp.24-36.

[155] R. Caputo, "Worlds in Collision: Is a Rational Energy Policy Possible for Countries in Western Europe?", *Energy Policy*, 1983.

[156] Thompson, op.cit. [133].

[157] K. Boulding, "Science: Our Common Heritage," *Science*, *207*, 22 February, 1980, pp.831-836.

[158] Goldman, op.cit. [4].

[159] Häfele to Keepin, 7 April, 1983.

[160] *EIFW*, p.785.

[161] Häfele, Knoxville lecture, op.cit. [33]. See also *EIFW*, pp.771-812.

[162] Ausubel and Nordhaus, op.cit. [100].

[163] Greenberger, op.cit. [23].

[164] Wynne, *Rationality and Ritual* op.cit. [133].

[165] Quoted in E. Fromm, *Beyond the Chains of Illusion*, New York, Simon and Schuster, 1962, p.xix.

[166] T.S. Kuhn, *The Essential Tension*, Illinois, University of Chicago Press, 1974.

[167] Bernstein, op.cit. [62].

[168] For WASH-1400, see Greenberger, op.cit. [23]. For the Inhaber-Holdren debate, see for example, B. Wynne, "Nuclear Power - is the health risk too great?" *Journal of Medical Ethics*, 8(1982), pp.78-85.

[169] Goldman, in Gass, op.cit. [4].

[170] M. Greenberger, op.cit. [23], p.340.

[171] *Ibid.*

[172] T.C. Schelling, "Climate Change: Implications for Welfare," U.S. NAS Report, *Changing Climate*, Washington, D.C., 1983.

[173] For example, M. Hollaway, "Dealing with Uncertainty in Public Policy Decision Making: Models and Process," in F.S. Roberts (ed.), op.cit. [44], pp.503-523.

[174] *Ibid.*

[175] C.S. Holling, *Adaptive Environmental Assessment and Management*, Chichester, John Wiley, 1978.

[176] See for example, the work of the Beijer International Institute for Energy and Human Ecology, Stockholm, Sweden, and of the World Council of Churches Energy Project, Geneva, Switzerland.

[177] See for example, the strong emphasis on the need to analyse the social and cultural influences on perceptions of global issues, in the UNESCO Draft Programme for 1984-85, General Conference 22nd Session, Paris, 1983, 22 C/5, pp.25-30.

[178] G. Majone, "The Craft of Systems Analysis," IIASA Working Paper, WP-80-73, Laxenburg, Austria, 1980.

[179] Greenberger, *Caught Unawares*, op.cit. [23], and in Gass (ed.), op.cit. [4], p.27.

[180] D. Bonhoeffer, *Ethics*, New York, MacMillan, 1965, reprint from original translation of 1949. Part Two, Chapter V, "What is Meant by 'Telling the Truth'?", pp.363-372.

[181] J.R. Ravetz, op.cit. [7] and [19].

[182] Keck, op.cit. [139].