Climate-Related Impact Studies: A Review of Past Experiences

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In an effort to build on past experience, the Scientific Committee on Problems of the Environment (SCOPE) supported a review of major climate impact assessments from the 1970s. SCOPE, a committee of the International Council of Scientific Unions, authorized "a critical review of existing scientific methods of climate impact study, the development of new concepts and methodologies, and the enlargement of the pool of scientific talent engaged in impact study" (1).

The authors of this essay selected five studies for the purpose of identifying their successes and their problem areas in the hope of assisting those who undertake similar climate-related impact assessments in the future. The readers should keep in mind that the review of these five assessments is meant to be suggestive and in no way is meant to provide definitive guidelines. The three dimensions of our review were to identify the origins of, or reasons for, each study; to evaluate the internal organizational factors; and to highlight the studies' impacts (i.e., how they were received). The five studies chosen met totally or in part the following criteria: (1) each was an attempt at an integrated climate-related impact assessment, (2) each represented a multidisciplinary effort, (3) each had been requested by policymakers, (4) the research activity was completed in the 1970s, and (5) each study was done at a high cost and/or its findings were highly visible, that is, it received considerable coverage by the media and the attention of policymakers. The five studies reviewed are as follows:

- Massachusetts Institute of Technology, A Framework for Evaluating Long-Term Strategies for the Development of the Sahel-Sudan Region (2-13)
- U.S. National Defense University, Climate Change to the Year 2000 (14-16)
- International Federation of Institutes for Advanced Study, Drought and Man: The 1972 Case History (17-18)
• U.S. Department of Transportation, Climatic Impact Assessment Program (19–25)

• U.S. National Academy of Sciences, studies related to the effects of chlorofluorocarbon releases on stratospheric ozone (26, 27)

This essay reviews each study separately, presenting a discussion of the reasons for undertaking the study; internal factors of the study (design approach, time factor, size of research staff, and integration of study components); and the impact of the study on policymakers, scientists, scholars, the media, and the public. It also highlights factors that appear to have contributed to the favorable or unfavorable reception of the study.

While we tried to deal with the studies systematically by addressing similar points for each study, we were limited by the varied amount and type of information available about each study. Our discussions are, for the most part, based on longer working papers on each of the impact assessments as well as on discussions held at the Workshop on Improving the Science of Climate-Related Impact Studies (28) at Oak Ridge Associated Universities, Institute for Energy Analysis, in late June 1981. Given the limitations of time, space, and resources, however, emphasis was placed on important aspects and highlights of each study.

MIT Sahel-Sudan Study

The Agency for International Development (AID) gave $1 million to the Massachusetts Institute of Technology (MIT), Center for Policy Alternatives, between September 1973 and September 1974 to prepare a framework for evaluating long-term strategies for the development of the Sahel-Sudan region of West Africa. The Center’s final report consisted of 12 volumes, two of which were treated as the final report and distributed by AID, while the others, labeled “Annexes,” were not widely distributed.

The report and its annexes discussed such topics as agricultural development; economic considerations; health, nutrition, and population; industrial and urban development; sociopolitical factors; systems analysis of pastoralism; technology, education, and institutional development; transportation; water resources; and energy and mineral resources.

Background

Most scholars as well as foreign assistance donors familiar with the region believed that this particular study was the direct result of concern about the plight of inhabitants in the West African Sahel caused by cumulative effects of prolonged regional drought between 1968 and 1973. However, internal AID documents indicate there was a nascent interest within AID as early as 1971 (inspired by AID’s assistant administrator for Africa, formerly the ambassador to Niger) to address regional problems related to long-term, low-grade, but cumulative environmental degradation. (See, for example, AID’s 1972 in-house report, “Development and Management of the Steppe and Brush-Grass Savannah Zone Immediately
using formal techniques such as linear programming, statistical analysis, and systems dynamics modeling, all of which, according to an AID retrospective assessment, were too theoretical to be of direct value to policymakers in their attempt to resolve the region's development problems (29). Members of the National Academy of Sciences Advisory Panel on Arid Lands of Sub-Saharan Africa in their review of the MIT study voiced concern “over the wisdom of placing too much reliance on computer modeling techniques. . . .” (33, p. 13).

A small group started gathering information on the geophysical and societal aspects of the region and then spent a few months defining the problem and formulating a plan. They recruited additional members for the research team and initiated the component studies. The last groups were recruited with only 6 months or less to complete their work. For example, the hydrology group was among the last to be organized by the project managers, although water resource projects had consistently been given the highest priority by AID representatives. Thus, some of the potentially most relevant development alternatives were examined belatedly and in a cursory fashion (29).

Researchers of the component studies were allowed considerable freedom in defining their research objectives, with each group expected to prepare a report on its part of the Sahelian development problem. The project managers at MIT's Center for Policy Alternatives assumed the role of project facilitators rather than leaders, attempting, for example, to encourage communication between groups, but not providing strong guidance.

Time factor. MIT and AID agreed the research activity was to take place during 1 year. While initially proposing this time frame for the study, MIT managers later expressed their view that 1 year was inadequate for the task. What AID apparently expected and what MIT offered was too much too soon, a view to which AID later agreed (29). Three of the major time-related problems were:

1. No scheduled work plan had been prepared until after the research effort was well in progress. On this, AID commented that it “should have insisted on a work plan earlier . . . and a written explanation of methodology . . . and managed the contract strictly against these measures of progress” (29, p. 27).

2. MIT researchers, unfamiliar with the region, were constantly in need of more information before they could conceptualize the problem (to the point of requiring an extension of the contract by 4 months).

3. Few senior scholars from MIT or elsewhere could be attracted to the project on such short notice. AID's retrospective assessment noted that “AID contributed to the impossibility of the task by adhering to its original timetable even when it became clear that MIT simply could not assemble the level of staff desired and still complete the scope of work in a single year” (29, p. 28).
South of the Sahara.”) The drought’s worst years—1972 and 1973—gave a reason to the assistant administrator to address in a report the underlying obstacles to development related to “the degenerative processes at work across the entire Sahel-Savannah region” (29, p. 2). The national and international news media coverage of drought in West Africa, especially after March 1973, heightened public and Congressional interest within the United States (30) and presented AID with an opportunity to take a leading role in the long-term development of a region formerly within the French sphere of interest (31).

At a U.N. meeting in Geneva held in June 1973, AID proposed the appointment of a major American university to develop a framework for evaluating mid- and long-term development programs for the Sahel-Sudan region of West Africa (32, p. 4). The proposal was accepted, but not without skepticism voiced by European representatives about its chances for success (29).

Massachusetts Institute of Technology was the university selected by AID for the study. According to M. J. Harvey, AID assistant administrator for legislative affairs, “it had experience in systems approaches, especially with regard to water systems. It alone, of the institutions investigated, offered to devote the attention of senior people over long periods to the task, and it had the stature needed to attract French, African, and other U.S. academic cooperation in the task as well as to substantiate the validity of such an approach.” However, at the beginning of the project none of the team members had experience with, or first-hand knowledge of, the Sahel-Sudan region.

In an assessment of the MIT project, AID commented on the origin of the study and the selection of the contractor: “The decision to undertake the MIT study had its origins in work being done before the major Sahelian drought emergency, 1973-74 [the period of relief and rehabilitation], but the timing and nature of the contractor selection was greatly influenced by events surrounding the drought” (29, p. 2). MIT was selected under “noncompetitive” procurement procedures. Although other institutions had indicated an interest, AID did not feel that either their institutional arrangements or their proposed projects met all the requirements for this particular study.

The 1-year period allowed for completing the project was a direct result of AID’s desire to meet a U.N. timetable for a series of meetings (as determined at the June 1973 meeting), a timetable to which the U.N. eventually did not adhere (29).

Internal factors

Design approach. Nine disciplinary teams were established to develop a framework for evaluating long-term strategies for development of the Sahel-Sudan region. A parallel design approach was adopted, with each team simultaneously researching a problem area (such as agriculture; hydrology; or the social, political, and economic context for development) and preparing a report. The researchers selected were associated primarily with MIT’s Center for Policy Alternatives, but there were some participants from other universities as well. Many researchers assessed the problem
Size of research staff. There were 72 people listed as "personnel associated with the project." Of these, approximately one-third were students, a quarter were listed as consultants, and the remainder were professors, research assistants, and advisors (the advisory committee met only once and was viewed by MIT as hostile to the project). While disciplinary subgroups were considered necessary because of the size and the scope of the study, they apparently contributed to the difficulty of integrating the components and, hence, to the isolation of disciplinary perspectives.

Integration of component studies. The lack of successful project conceptualization, in addition to time constraints, appears to have been a major obstacle to integrating the components of the MIT study, in spite of the fact that the study was supposed to have relied on systems analysis methodology. The study groups were allowed considerable freedom to define their research approaches, and the project managers did not provide sufficiently strong leadership to bring them together effectively. In fact, the component studies can be viewed as separate research efforts on different topics for the same geographical area—the West African Sahel. The summary report, written by the project managers, was in essence a compilation of some of the primary findings from each component study, but was not a well-integrated document.

General observations. The experience of the MIT Sahel-Sudan study suggests that an interdisciplinary systems analysis approach to climate impact assessment requires a well-defined focus, careful attention to project integration, and sufficient time to achieve the desired research objectives. Time constraints, in fact, served only to compound problems associated with the study. The fact that the researchers, including the project managers, had little, if any, experience with the region proved to be a major obstacle to the successful development of a framework for evaluating long-term strategies for the development of the Sahel-Sudan region. MIT's response to concern about its lack of expertise in West Africa was that its researchers were able to approach the region's development problems "with no biases" (28).

The impact of the study

When the MIT study was completed in February 1975, AID requested only a few hundred copies of the final report. MIT requested and received permission from AID to disseminate copies of the report at its own discretion and expense. AID noted in its retrospective assessment that "it is clear that the study did not live up to expectations [as well as to MIT's verbal and written commitments]. Its reception has been reflective of that fact. Little reaction to it or demand for further effort by MIT have resulted once the report was distributed to AID, international organizations, and African governments in February 1975" (29, p. 1).

MIT's report was ignored by the media (with very few exceptions) and has seldom been referred to in academic circles familiar with problems of West African development. It did, however, generate general disappointment among scholars involved in African studies who noted that MIT's Center lacked African expertise; that no one on the project
spoke French; that no senior MIT scholars involved themselves in the study; and that African participation as project staff members was, with the exception of a few graduate students, negligible. AID lamented the fact that “most principal appointments went to graduate students or persons with limited professional and no African experience” (29). Eventually, professional African participants were involved in the project (mostly at AID’s encouragement). However, overall African participation remained much less than AID had wished.

The MIT study managers’ conceptualization of the Sahelian problem remained unsatisfactory throughout the duration of the project, according to AID officials. For example, MIT continually treated the Sahel as six individual West African states, each with its own problems, whereas AID sought to develop new approaches to long-term development in West Africa that included coastal and Anglophone states as well as the six Francophone Sahelian states. In a 1974 interim review, AID went so far as to provide examples of what it meant by new approaches.

AID representatives also expressed concern early in the project that the MIT managers might not deliver what they expected: MIT did not convene a proposed senior think-tank retreat, the vital water systems study was unmentioned in the interim report, and the two major alternative development strategies MIT focused on were viewed by AID as “unexciting” (29). Thus, none of the reasons (cited earlier) suggested by AID for its selection of MIT’s Center for Policy Alternatives proved to be valid. Stimulated in part by its dealings with MIT and the Sahel study, AID requested in late 1973 that the National Academy of Sciences appoint an advisory panel on arid lands of sub-Saharan Africa. The panel was to “advise AID with respect to the critical medium- and long-term natural resource management problems of the drought-stricken region of West Africa,” in addition to other activities, and to critique the draft reports (as a panel and individually) of the group at MIT (34, p. 156: see also 35).

Midway through the project, AID stated its concerns in an internal memo regarding MIT’s interim report: “The result is that MIT is not inspiring confidence and thus may do damage to future cooperation of Africans and other academics, and ultimately to the acceptability of the project.” A review of correspondence between representatives of AID and MIT’s Center for Policy Alternatives tends to support AID’s claims.

For their part, MIT study managers continue to believe that AID did not properly advise them during the project (28) and, in a letter to AID’s assistant administrator for Africa, viewed AID’s attempts to keep the research effort on track as interference that hindered the research process. An AID internal memo reviewing the project’s 6-month progress report noted that some changes were so critical to the project’s success that it was recommended that continuation of the contract to completion should be made conditional to implementing those changes. However, few substantive changes either in personnel or in project concept were forthcoming.
AID representatives constantly sought to salvage some parts of the MIT effort that they could present to the international development assistance community, but they felt that only the first two volumes (2, 3) would serve to some extent. MIT has questioned this view, however, noting that many excerpts from the entire 1974 MIT report (2-13) were used in an AID proposal to the U.S. Congress for a long-term comprehensive development program for the Sahel (36, passim). In private correspondence, MIT investigator W. W. Seifert felt that "AID panned us but then presented our results to the Congress without reference [to the MIT study]."

NDU Study: Climate Change to the Year 2000

Research for the National Defense University (NDU) study, Climate Change to the Year 2000 (14), the first of a three-part assessment of the impacts of climate change on agriculture, began in the fall of 1976. The final report was released to the public in February 1978. The second part of this study—Crop Yields and Climate Change to the Year 2000 (15)—was printed in late 1980 and distributed in mid-1981. The third part—Climate Change and the World Grain Economy to the Year 2000 (16)—was released in manuscript for limited circulation in June 1981. Final publication and general distribution of the third part is expected in 1982. A progress report of the crop yields study had been issued in August 1978 (37).

The project represented the first attempt to quantify in a comprehensive way perceptions about climatic change. The task of Climate Change to the Year 2000 was "to define and estimate the likelihood of changes in climate during the next 25 years, and to construct climate scenarios for the year 2000" (14, p. vii). Crop Yields and Climate Change indicated a broadened view of the task: "A secondary goal of this interdisciplinary effort was to advance the art of making climate impact assessments" (15, p. iii).

The entire study was financially supported by the Department of Defense through the Defense Advanced Research Projects Agency at an estimated cost of $100,000. This was given primarily to the Institute for the Future to assist with the study design. This excludes approximately 9 person-years of contributed research by a small, multidisciplinary staff detailed from the Department of Defense, the U.S. Department of Agriculture (USDA), and the National Oceanic and Atmospheric Administration (NOAA), as well as contributions by numerous expert panelists and advisers who received nominal honoraria. Our discussion centers on the report of the first part (14); the remaining two parts have become available only recently and have not been reviewed in the literature. All three reports were cleared for public release by the Department of Defense for open publication and unlimited distribution.

Background

The pressure on U.S. policymakers for information from such a study began in the early 1970s with the debate (among climatologists) about whether the earth's atmosphere would
become warmer or cooler during the next few decades. Supporters of the competing views on global temperature trends selected information to support their view, while highlighting the weaknesses of their opponents’ position. The debate on future global temperatures was fueled by weather anomalies in 1972 that, for the most part, adversely affected food (including fisheries) production and availability in some regions of the world. The debate about climate impacts was intensified with the publication of such popular articles as “Ominous Changes in the Weather” (38), “What’s Happening to Our Climate?” (39), and the Central Intelligence Agency report on climate and agricultural production (40). These publications have explicitly been acknowledged as having stimulated government agencies to consider the climate factor in planning. “Pessimistic views of the future were widely publicized in the popular media. Confusing and conflicting advice was offered to the public and to officials concerning policies and programs to solve the problems or at least adjust to the changed circumstances” (41).

As a part of this public debate on climate, the USDA was criticized for not having considered possible future climate scenarios and their effects on U.S. and global grain production and trade (42). While a National Defense University research fellow from the USDA, J. W. Willet recommended to the NDU Research Directorate that a climate change study be undertaken, noting that it would relate to strategic planning and to the management of resources for national security (41, p. 2).

Internal factors

Design approach. The NDU study managers undertook three separate substudies (called “tasks” by NDU) on climatic change, climate-crop yields, and economics. They used a sequential approach with the output of one substudy consistent with the input needs of the next one. The objective of task 1 was to “seek [through a questionnaire] from those who were thoroughly familiar with the state of research and knowledge subjective probability judgments about the likelihood of occurrence of certain well-defined climatic events in the future.” The responses of 24 climate experts from seven countries were used by the NDU team to develop five scenarios of climate change to the year 2000. The scenarios were designated as Large Cooling, Moderate Cooling, Same as Last 30 Years, Moderate Warming, and Large Warming.

In the second substudy, 35 agriculturalists identified their expectations about how various combinations of changes in annual temperature and precipitation might affect crop yields in 15 country-crop combinations. (For example, how might a 2°C warming combined with a 10% increase in precipitation affect wheat yields in Australia? How might a 1°C cooling and a 20% decrease in precipitation affect U.S. corn yields?) Responses in this survey were used with information from the previously developed climate scenarios to develop climate and yield scenarios. The climate-response model devised for the study predicted changes not only in average yield but also in the interannual variability of yields. In the third substudy, the climate and yield scenarios were used to drive a USDA econometric model of international agricu-
tural demand, production, and trade in order to generate information on the potential implications of climate change for international grain trade and agricultural policies.

**Time factor.** Because the NDU study managers reported to NDU officials and not to an outside agency, the project had only self-imposed deadlines (28). However, personnel attrition at NDU affected the degree of graphics and design support for the second and third phases of the project. This attrition and the return of all but one resident study manager by 1979 to their parent organizations contributed to the long delay between the publication of the first report (February 1978) and the third one (June 1981, in manuscript form).

**Size of research staff.** The size of the core study group was relatively small with apparently well-established lines of communication among the staff. The second of the study's reports commented on those involved in the NDU research effort: “The focal point of the endeavor was a small, interdisciplinary staff drawn from several branches of the Government. Assisted by the Institute for the Future, the resident staff conducted a brokerage operation, planning the study around futuristic techniques for the solicitation and analysis of nonexistent information [sic], and orchestrating advice, ‘data’ and insights from a host of volunteers” (15, p. xv).

**Integration of component studies.** The sequential approach facilitated the integration of information between the component studies. The first study produced climate scenarios, the second integrated those scenarios with information on crop yield responses to produce climate-crop scenarios, and the third study used the latter scenarios to produce projections of global agricultural behavior.

**General observations.** The NDU study was straightforward because of the well-defined scope of the study and because of the survey methodology. With respect to scope, the study was limited in general to the effects of climate change to the year 2000 on agriculture—and more specifically, it assessed only those effects on 15 country-crop combinations. The agriculture panelists were also asked to forecast the influence of technological changes on yield trends, assuming no change in climate. The responses to this inquiry enabled the staff to assess the potential importance of technological change for crop yields relative to the estimated effects of a range of climatic changes. The study managers ruled out consideration of adaptive measures either for countering adverse impacts or for capitalizing on favorable ones. In addition, the study did not include societal or ecological effects of climate change scenarios. In a 1981 internal memo, T. Stewart of the National Center for Atmospheric Research expressed concerns about the lack of safeguards to protect against possible judgmental bias and inconsistency and about the pooling procedure, which ignored individual differences among experts.

**Impact of the study**

**Climate Change to the Year 2000** was released at the 1978 annual meeting of the American Association for the Advancement of Science (AAAS) in Washington, D.C. Recently, insight about the intended audience was gained from the preface to the second part: “In drafting this report we envisioned an inhomogeneous audience of meteorologists,
climatologists, agronomists, economists, futurists, model builders, and policymakers, to name a few” (15). The findings of the report were presented to the press at the AAAS meeting and were subsequently reported by the media in a descriptive way with little or no analysis of the report, its methodology, or its conclusions (e.g., 43-46). During the World Climate Conference convened by the World Meteorological Organization in Geneva in February 1979, the NDU report was unofficially available for conference participants who represented developing as well as developed countries.

The impact of the first part of the study was evident in the Council on Environmental Quality's (CEQ) Global Report 2000 to the President (47) in which the two climate-related chapters were heavily dependent on the NDU report. Had there not been a change in administrations shortly after the Global Report 2000 was presented to U.S. President Carter, one could argue that the influence of the NDU study on policymakers might have been greater. Soviet scientists and policymakers apparently took interest in the first report of the NDU study, which was included as part of a survey article on the influence of climate on human economic activity (48).

In our opinion, four aspects of the NDU study affected its usefulness and, therefore, its long-term impact:

1. The conclusion that climate change would not be an important factor in agricultural production within the next 25 years. The report concludes that “the most likely event will be a climate which resembles the average of the past 30 years...” (14, p. xix). Statements such as this could diminish interest in the final reports of parts two and three. On the other hand, the negative conclusion about the agricultural implications of midrange climate change might also be a useful finding for policymakers. The idea that climate might not be an important factor for agriculture was reiterated in the manuscript of the third part of the study, Climate Change and the World Grain Economy to the Year 2000: “The significance of this study is that we can consider our proper role in the world food situation without great concern that climatic changes during the rest of this century will upset our calculations” (16, p. vi). One of the study's principal conclusions is that “technology, rather than climate, is likely to be the chief determinant of most crop yields in the last quarter of the 20th century” (15).

2. The report's being labeled "science by consensus." The NDU study has been referred to as “science by consensus" (14). Study managers feel this accusation to be unjustified. In their first report they explicitly noted that there was no consensus among the participating climatological experts: "The experts' aggregated subjective probabilities do not reflect a consensus on any narrowly defined climatic issue, but a large majority of the climate panelists were in broad agreement, for example, that the average global temperature is not likely to change more than half a degree Celsius by the year 2000" (14, p. xix). In the methodology section of Climate Change to the Year 2000, the authors addressed the concern about "science by consensus," noting that "the project team has proceeded on the assumption that expert probabilistic judgments, properly qualified, constitute the best available mi
dance for those who must make policy in matters affected by climate. ... In the description of the methodology and the presentation of the analysis and results, appropriate caveats have been introduced to avoid misunderstanding" (14, p. 3). Yet the concern about “science by consensus” still persists.

3. The lag between publication of the first and last two parts of the study. The lag between the publication of the first report (February 1978), the general distribution of the second (May 1981), and the third to be released for general distribution in 1982 suggests a change in interest at the NDU or Department of Defense for this research activity (perhaps resulting from less spectacular climate impacts in the late 1970s and perhaps in part from the impact of the first report). It would seem that this delay—whatever its cause—could detract from the value of the entire three-part study. Changes in personnel involved with the study (as people retired or were reassigned) as well as apparent changes in concern by policymakers about climate (that is, consideration of other climatic factors, such as interannual variability, as being of more immediate concern than climatic change) have perhaps made the reports of the last two parts of the study less important to policymakers (except in the use of the methodology) than might have been the case had they been published closer together.

4. Presentation or “packaging” of the report. Discussions of the NDU research effort inevitably include comments on the “packaging” of this scientific study. Some people have noted (28) that the cover graphics of the first volume were designed to attract attention to the report but were not suitable for a serious, scholarly report. According to T. Crampston, one of the study managers, their “hope was to engage the attention of the busy bureaucrat or decisionmaker long enough for him to read the abstract and summary.” However, the “packaging” of the report has had a positive aspect. Most people recall the study; many have retained it for their library (partly because of its content and partly because of the way it is packaged); and those who have saved it know where it is on their library shelf. This may not be the case for other climate-related impact studies of similar length, no matter how important their research findings.

IFIAS Study: Drought and Man,
The 1972 Case History

The “drought and man” study was sponsored by the International Federation of Institutes for Advanced Study (IFIAS) in Stockholm, Sweden, and was carried out under the auspices of the director of the Food and Climate Program of the Aspen Institute for Humanistic Studies (United States). It was funded by the United Nations Environment Programme (UNEP) (Kenya) and three private foundations at a cost of approximately $380,000 with an estimated $750,000 in contributed research.

The study has resulted in a three-volume report. The manuscripts for the first two volumes were completed in

the regional case studies completed by 1979. Volume 1, *Naturé Pleads Not Guilty*, was published in August 1981 (17); volume 2, *The Constant Catastrophe: Malnutrition, Famines and the Drought* (18), is projected to be published at the end of 1981; and volume 3, *Case Studies*, is scheduled to be published in 1983.

Rolando Garcia (49) was selected senior study author by the director of the Food and Climate Program of the Aspen Institute (he was also project manager for IFIAS) with the concurrence of an international, multidisciplinary steering committee. Garcia began to organize this research effort some months before the official mid-February 1976 starting date. The 30-month effort was to have ended by mid-August 1978, but it received two extensions and was completed in mid-March 1979.

The study examined in depth the impact of the climate anomalies of 1972, specifically droughts in many parts of the world and their impacts on the production and availability of food. The year 1972 was selected for assessment because in that year droughts affected the USSR, China, Eastern Europe, Latin America, and sub-Saharan Africa. In a 1978 mimeographed report, the study author noted that “widespread food shortages and famines as well as serious disruptions in the international food market were considered to be the direct effects of extended and simultaneous droughts affecting various continents.” The study encompassed investigations of both social and physical aspects of the 1972 situation.

**Background**

The underlying reasons for this study were similar to those for the National Defense University study, and to some extent for the MIT study: Until 1972, climate had been viewed as a relatively benign factor with respect to food production, availability, and distribution and therefore not a major concern to policymakers. With the occurrence of the climate anomalies of 1972 (especially the drought in the West African Sahel), which resulted in the first major decline in total food production (including fish landings) since 1945, and with speculation that the established global climate regime might be changing, climatic factors in food and energy issues took on a greater importance than had previously been the case.

In 1972 a new interdisciplinary, international, nongovernmental organization, IFIAS, was created for the purpose of ensuring that science be used to improve the quality of human life. IFIAS established a “climate and the quality of life” project under the leadership of W. O. Roberts, the representative of one of its member institutes, the Aspen Institute for Humanistic Studies (AIHS). A meeting was co-sponsored by IFIAS and AIHS and held at the University of Bonn’s Meteorological Institute in May 1974 to establish research priorities for the IFIAS climate project. The mandate was clear: “The project is not to be viewed as a contribution to physical climate research. The project instead will focus on its main task—the social, ethical, and humanistic implications of changes in global or regional climate” (50, p. 2). A press release announced the meeting as follows: “An international group of scientists assembled in Bonn...
wide pattern of climate that is now emerging will have serious consequences for world food production and may affect the stability of interstate relations. Three projects were formulated at the meeting, of which the "drought and man" study was one. With the selection of the senior study author, the "drought and man" study officially began in early 1976.

Internal factors

Design approach. The senior study author was given total independence for conducting the study, which included reconceptualizing the research problem several months after the project began. This was necessary, according to Garcia (28), because it was not clear to him that the "official version" of the direct cause-and-effect relationship on which his investigations were to have been based (between drought and famine and drought and malnutrition) was, in fact, valid. Garcia not only redefined the research problem, but, after establishing research groups in Latin America, Africa, and South Asia, he also coordinated the research activities, and finally integrated all of the project's research results. Separate case studies of specific aspects of the 1972 droughts were also undertaken by individuals in the United States and Europe.

The study groups were encouraged to follow what Garcia termed "a structural approach," which entailed research at three levels of analysis (17, Chap. 6). Basic to this approach were the interdisciplinary assessments of the events of 1972, particularly in terms of atmospheric anomalies, soil systems, agricultural systems, and social and economic systems. At the second level, researchers identified processes perceived to be responsible for the observed events, including such trends as global increases of meat and cash crop production, urbanization, and industrialization. The third level of analysis involved identifying the causes of the processes observed at the second level.

Some aspects of the study design proved to be troublesome in practice. For example, it took several months longer than expected to establish the regional groups, and the expense of doing so required an additional $100,000 for the study. Some of the regional studies apparently did not meet the expectations of Garcia, who commented that more control over, and involvement in, the activities of the regional groups might have led to more beneficial results (28). While it was not possible to do so, it would have been more effective for the entire group to have worked in one location. In assembling the final "drought and man" study report for submission to IFIAS, Garcia compiled most of the regional contributions for the study's third volume.

Time factor. Garcia has stated that time was not a serious constraint in carrying out the study (28). However, there were two extensions to the study: the first from August to December 1978 to allow for the completion of the regional studies, and the second to March 1979 to enable the senior study author to prepare a document for the 1979 World Climate Conference in Geneva.

The time element of importance with regard to the IFIAS study is related to the publication of the results of the research activity. It is not clear why the first volume of the
report (17) is only just available, considering that it had been presented to IFIAS in early 1979, and the first two volumes were submitted by IFIAS to the publisher in November of that year. The impact of the lengthy publication delays cannot be assessed at this time.

**Size of research staff.** The project involved a small core group under the leadership of Garcia and several regional study groups headed by local study authors who were to submit their reports to Garcia. More important than the size of the project was the fact that the regional study groups were spread over several continents, making communication and the exchange of information between them and Garcia extremely difficult and often delayed.

**Integration of component studies.** Intellectually the study was laid out so component studies formed pieces of an integrated whole. Logistically, however, the project was difficult to integrate. For example, some regional groups were late in producing their reports or produced reports that were inconsistent with the study’s lines of inquiry. The geographical separation of contributors, as noted earlier, handicapped the effective integration of component studies.

**Impact of the study**

IFIAS requested that a select committee review the document prior to publication, a procedure that had apparently neither been expected nor agreed to in advance by Garcia. Late in 1979 IFIAS submitted the report for publication. The first volume, *Nature Pleads Not Guilty* (17), was published only in August 1981. Because of the very long and as yet unexplained delay in publishing the report, there has been no systematic review in the literature of the results of that research effort. However, there are reviews of the study based on earlier working manuscripts (51, 52).

Garcia prepared reports for two U.N. conferences on various topics stemming from the “drought and man” study (53, 54). In spite of these presentations, it appears that his strategy was to publish all the findings at the completion of the research effort instead of piecemeal throughout the project as research results became known. As a result, other authors, undertaking independent research on topics related to those in the IFIAS study, have in a sense preempted (but reinforced) some of the conclusions drawn earlier in the IFIAS study, conclusions that were not published until 1981.

The IFIAS study developed at least two important perspectives. The project staff considered and discarded the hypothesis that we are witnessing a period of profound climatic changes. The study also raised doubts about the validity of what Garcia has referred to as the “official” or generally accepted version of the sequence of events to explain the crisis of 1972 and thereafter. This version maintained that, as a result of the decline in world production, the Soviet Union and the developing countries purchased large quantities of grain, which led to a depletion of stocks and the exceptional price increase of grain and other foodstuffs. Garcia asserted that, in addition to an assessment of the impact on society of “natural forces” such as climate fluctuations, “human structures (societal, political, economical) must be
examined, and their interactions studied to reveal the actual forces at play. It is this set of forces, perhaps triggered by a physical disaster, that determines in the end what will be the effects on man and his structures” (17).

Such challenges to the “official” version often become labeled as radical or iconoclastic assessments and become part of the polarized debate over whether a person (as policymaker) or nature (as climate anomalies) is the primary cause for food crises or shortages that are initially perceived as climate-related. This IFIAS report will, at the least, support the arguments as well as the resolve of those who seek to challenge the “official” version of the food crisis of the 1970s and future food crises.

DOT Climatic Impact Assessment Program

The Department of Transportation (DOT) Climatic Impact Assessment Program (CIAP) was authorized by the U.S. Congress in 1971 and was funded at a total cost of about $20 million, with an unofficially estimated $40 million of contributed research (55). The study was to be a 3-year, multidisciplinary effort and was “to determine the regulatory constraints necessary to safeguard that future flights in the stratosphere do not result in adverse environmental effects” (56). The U.S. Department of Transportation had responsibility for the entire program and supplied the principal investigator and project manager.

CIAP was designed to assess the potential impact of fuel emissions from a large, high-flying supersonic transport (SST) fleet on stratospheric ozone concentrations and of the hypothesized effects of the resulting ozone depletion on the incidence of skin cancer as well as on climate.

CIAP publications consist of six monographs (20-25) and a Report of Findings (19), which summarizes the monographs. An executive summary of the Report of Findings was issued both separately and as part of the Report of Findings. The monographs assessed the following topics: natural stratosphere of 1974, propulsion effluents in the stratosphere, stratosphere perturbed by propulsion effluents, natural and radiatively perturbed troposphere, impacts of climate change on the biosphere, and economic and social measures of biological and climatic change. The final draft of the executive summary and a press release (57) were issued at a press conference in mid-January 1975, the Report of Findings in March 1975, and the monographs from September to December 1975. Additional supporting CIAP material was issued at the final CIAP conference held in Cambridge, Massachusetts, in February 1975 following completion of the CIAP research effort (58).

Background

In the early 1960s, public controversy developed concerning the potential value to America of supersonic transport aircraft. The debate on whether to develop an American SST centered primarily around the economic and political costs and benefits (59) and around two environmental issues—noise
pollution in the vicinity of airports and sonic booms (e.g., 60). At the end of the 1960s, environmental concern expanded to include the possibility of stratospheric ozone depletion (61). Scientists raised the possibility that trace gases (at first water vapor and later oxides of nitrogen) from fuel emissions during SST flights in the stratosphere could reduce the concentration of stratospheric ozone (e.g., 62, 63), thereby reducing its effectiveness in shielding the ground from biologically damaging ultraviolet radiation (UV-B). It was also suggested that there was a link between increased UV-B and the incidence of skin cancer (a 1% reduction in stratospheric ozone was predicted to cause a 2% increase in skin cancer) (64).

In March 1971, Congress refused to fund the Boeing SST prototype program, despite European and Soviet intentions to develop theirs. The ozone depletion aspect of the SST debate appears to have been an "eleventh hour" consideration, used by opponents of the SST to block its development. Later that year Congress established CIAP to investigate more closely the potential impact of SSTs on the stratosphere. Although the American SST debate had essentially been concluded before the CIAP program began, U.S. decisionmakers were still in need of more information on SST emissions and stratospheric ozone because they had to contend eventually with a decision about landing rights in the United States for the British-French Concorde (67).

From the beginning of the study, CIAP managers considered this an international research activity and invited hundreds of scientists from more than 10 countries (but predominantly from the United States) to participate in CIAP conferences to discuss research activities and findings.

**Internal factors**

**Design approach.** CIAP project managers embarked on a "crash" program to combine previous basic research with new research and a climate impact assessment. It was a six-component study, using in part a sequential approach and in part a parallel approach. The parallel approach was taken for the basic research activities in the areas of atmospheric science, aircraft propulsion, and biological science, and modeling activities in the areas of atmospheric, biological, and economic science. The sequential approach was employed in the final year to use the monograph data to accomplish the analysis required to assess the impact of climatic changes resulting from propulsion effluents of vehicles in the stratosphere, as projected to 1990. Concurrent analyses, for which all data collected by CIAP were made available, were undertaken by the National Academy of Sciences (68) and by groups in France and Belgium (69), England (70), USSR (71), and Canada (72).

**Time factor.** CIAP managers were given 3 years to produce their final report. During this period, much basic research was required, particularly on various aspects of stratospheric chemistry and on biological responses to increased UV-B.

Allocation of time among the six research topics encompassed by each of the monographs favored atmospheric research. The first three monographs (20–22) were given
several months more time than the last three (23-25). Two years for designing, conducting the research for, and compiling the result of the biospheric and economic components (monographs 5 and 6, respectively) proved too short for the task.

Time became an important factor at the end of the study, when the findings of the CIAP study were to have been presented to Congress. The project managers issued the final version of the executive summary of the Report of Findings 2 months before they issued the Report of Findings. Whereas the Report of Findings had been circulated among CIAP scientists for peer review, the project manager claims that the pressures of the Congressional deadline for the report caused them to prepare and submit the final version of the executive summary without peer comment. The controversy concerning the timing of the release, as well as the wording, of the executive summary overshadowed in the short-term the impact of the more complete Report of Findings and component monographs. The controversy, in fact, led to Congressional hearings on the executive summary (73, 74).

Size of research staff. The CIAP study involved hundreds of scientists—some estimates run as high as 1000. The large size of the study appears to have been necessary because of the gaps in basic information that had to be identified and the large amounts of information that had to be gathered from a great number of sources. CIAP dealt with the large staff by forming disciplinary subgroups and sub-subgroups. This organization seems to have complicated the task of integrating the component studies, thereby contributing to isolation of disciplinary perspectives.

Integration of component studies. According to the project's manager (Grobecker, private communication), the CIAP study consisted of two types of activity: data gathering and integrative analysis of the data leading to specific answers. The data gathering was accomplished by representatives of six groups working in parallel to assemble all data from all sources that seemed relevant to their particular task. Preparation of each monograph began with a conference attended by 25 to 100 scientists. Existing information was assembled and suggestions for additional research were made. Coherence of the contributions within each major group was achieved under the editorial direction of the group leader, who was responsible for preparing the monograph. The monographs were further refined and expanded through an iterative process that involved the circulation of monograph drafts and the convening of annual workshops.

The integrative analysis of the monograph data, accomplished in the final year of the study, consisted of several steps. Some of these were accomplished under the direction of the responsible monograph chairman; others, by the CIAP staff. In addition, CIAP staff attempted to integrate its study findings post hoc by compiling into a single volume of Report of Findings summaries of each monograph and a complete description of the analyses.

General observations. Although demands for instant information, where a long-term research program was in order, were generally viewed with great skepticism at the beginning of the CIAP effort, general praise of CIAP's
atmospheric research has since become widespread. Its accomplishments appear to have been aided by circumstances in the atmospheric sciences. For example, several of CIAP’s research priorities were at the frontier of atmospheric sciences research. The community of atmospheric scientists needed relatively little incentive to bridge the gaps between various atmospheric disciplines. Scientists not receiving CIAP funding were also enthusiastic about participating in the program. Many links between different studies developed naturally as tropospheric scientists crossed disciplinary lines to work on stratospheric research and as scientists who researched the unperturbed stratosphere also undertook research on the perturbed stratosphere. In addition, interdisciplinary communication was aided by the fact that study structure cut across the deepest disciplinary line within the atmospheric sciences—the break between dynamacists and chemists.

The biospheric and economic component studies were less successful in achieving interdisciplinary coordination or in developing a viable set of research results, in part because both of these studies were under relatively severe time constraints: with only 2 years to design the studies, conduct the required research, and compile the research results. For example, the degree of difficulty of the biologists’ task, especially in the design of growth chambers and other UV-B-enriched environments (and the lack of adequate engineering assistance), proved to limit severely the scientific accomplishments possible in the short time allowed by CIAP’s schedule.

These difficulties with biological research apparently handicapped the researchers involved in the economic aspect of the study. They required, for example, better data than biologists could generate on how UV radiation might affect crop production. Ideally, the economists should also have been able to employ the findings of the biologists on the effects of temperature and precipitation changes on key organisms. However, the disciplinary gap between biological sciences and economics is wide, and the mechanisms used in CIAP (e.g., circulation of monographs and conferences), were insufficient to close the gap. Since the economic and biological studies took place simultaneously, the economists were unable to receive the biologists’ research results until late in their research schedule. Perhaps more important was the fact that the biologists could not provide the information that the economists most needed—the magnitude of the effect of UV radiation on crop plants.

In summary, CIAP’s use of different study designs apparently worked well within the atmospheric sciences, where there was a higher degree of initial momentum, enthusiasm among scientists, spontaneous communication between those working in some component studies, and a relatively generous allocation of research time and funds. The mix of design approaches was less effective, however, in the biological and social science studies because (1) studies were undertaken in parallel under circumstances where one study required the other’s results, (2) little communication developed between biologists and social scientists, and (3) those scientists involved in both studies were required (expected) to conduct basic research but had been allotted insufficient time to complete their research task.
Impact of the study

In January 1975, a press conference was held to announce the completion of the Climatic Impact Assessment Program and to issue an executive summary of the project. The contents of the summary were immediately challenged by some of the scientists who had worked on various aspects of the CIAP reports (e.g., 75, 76). They felt that the tone (77) of the summary disagreed with the tone of earlier drafts of the executive summary and with the tone of the complete Report of Findings (78) in that it appeared to indicate that the report found that the effects of existing SSTs (i.e., the effects of a small number of lower-flying Concordes and Tupolevs) were negligible and those of a large, hypothetical fleet were technologically preventable. Also, the "scientific conclusions" listed in the summary were described in words different from those of the Report of Findings and were listed in an order which seemed, again, to bring out an upbeat opinion regarding the SST. In addition, some topics that were identified as important in the Report of Findings received little or no mention in the summary (e.g., skin cancer effects). Many of the scientists who participated in CIAP activities felt that their credibility had been permanently damaged; they cited news headlines and editorials that appeared after the release of the executive summary such as "Scientists Clear the SST" in the Christian Science Monitor (February 5, 1975) and "World SST Fleets Said Not to Damage the Ozone Blanket" in the New York Times (January 21, 1975). Perhaps typical of the sentiment expressed by these headlines, one newspaper report suggested that "the phony argument has no place in national scientific discourse and no place in the SST debate" (Pittsburgh Press, January 29, 1975). One scientist reacted by issuing his own executive summary and principal scientific conclusions (79).

CIAP study managers declared that they had been misquoted in local editorials based on an Associated Press dispatch issued as a result of the January 1975 press conference. They argued that their summary simply emphasized the positive aspects of technological improvement, such as engine design changes to lower trace gas emissions. They contended that their conclusions were consistent with those in the National Academy of Sciences report on the environmental impact of the SST issued in late March (68) and with those of a number of other studies abroad (69, 70). The controversy over the executive summary eventually resulted in a Congressional hearing on whether the authors of the executive summary deliberately sought to mislead the public on the potential environmental hazards of a large, high-flying SST fleet. After the Congressional hearing, a review of the issue (74) in Science concluded that the summary of conclusions was what might have been expected from a study by engineers who anticipate the best (rather than the worst) that technology might offer for the future.

According to CIAP's project manager (28), the controversy over the executive summary could have been avoided if time had been taken to distribute the final version to CIAP scientists for comment (as had been done with other parts of the study). However, to meet the Congressionally mandated deadline for the CIAP effort, shortcuts had been taken that
led to issuing the executive summary (with peer review only by the NAS Climatic Impact Committee) 2 months in advance of the report it was to have summarized.

The CIAP scientific findings, as an assessment of knowledge at that time and as presented in the Report of Findings, have been generally praised as a good summary of the state of the art (except, perhaps, monograph 6) even by most scientists who vehemently criticized the executive summary. (Since mid-1975, Lawrence Livermore Laboratory has participated in the High Altitude Pollution Program (HAPP), an outgrowth of CIAP. HAPP continues to be supported by DOT's Federal Aviation Administration (80, 81).) Moreover, the CIAP research process has also been praised for having encouraged multidisciplinary interaction on an environmental issue of international concern. In addition, some observers have indicated that CIAP stimulated (or at least greatly strengthened) research on the effects that trace gases from sources other than SSTs might have on the concentration of stratospheric ozone or on climate (82). For example, as CIAP research progressed, there was an increasing number of references (but not necessarily from CIAP-related researchers) to chlorofluorocarbons (CFCs) derived from anthropogenic sources such as aerosol cans, refrigerants, coolants, and cleaning solvents, and to oxides of nitrogen (NOx) from agricultural fertilizers and their possible effects on stratospheric ozone.

Also as a direct result of CIAP, the U.S. National Academy of Sciences established the Climatic Impact Committee in 1972, under contract from DOT, to advise the Department of Transportation on CIAP and to issue a report independent of CIAP, the Environmental Impact of Stratospheric Flight (68). The name of this committee was later changed to the Committee on Impacts of Stratospheric Change (CISC) and it was charged with assessing the impact of chlorofluorocarbons on stratospheric ozone. Thus, one could argue that in spite of the controversy surrounding the executive summary, CIAP mobilized researchers in an area of atmospheric sciences that had been relatively neglected—the stratosphere (83).

In support of this view, Hoffert and Stewart have noted: “Regardless of position on results of the various CIAP investigations, few knowledgeable scientists would contest that the program accelerated the development of atmospheric models and focused well-deserved attention on the photochemistry of atmospheric ozone” (84). Furthermore, one policymaker, involved in research and policy considerations related to stratospheric ozone depletion, noted: “Most of the atmospheric research under CIAP concerning the oxides of nitrogen later became applicable to the oxides of chlorine problem of fluoro-carbons, especially that on the effects of increased UV radiation” (85).
NAS Studies on the Effects of Chlorofluorocarbon Releases on Stratospheric Ozone

The U.S. National Academy of Sciences (NAS) Panel on Atmospheric Chemistry was appointed in March 1975 to assess “the extent to which man-made halocarbons, particularly chlorofluoromethanes (CFMs), and potential emissions from the space shuttle might inadvertently modify the stratosphere” (26, p. vii). The panel issued its report, Halocarbons: Effects on Stratospheric Ozone (26), in 1976 (after a delay of some months caused by the panel’s need to evaluate newly developed scientific information). The panel’s parent committee, Committee on Impacts of Stratospheric Change (CISC), undertook a study to address “the question of biological and climatic effects of ozone reduction and the appropriate policy consequences of both our present knowledge and the knowledge we are likely to have in the future” (27, p. viii). CISC issued its report, Halocarbons: Environmental Effects of Chlorofluoromethane Release (27), at the same time the Panel on Atmospheric Chemistry issued its report. The two reports were produced at a total cost of more than $300,000 and were funded by the National Aeronautics and Space Administration (NASA), National Science Foundation (NSF), Environmental Protection Agency (EPA), and National Oceanic and Atmospheric Administration (NOAA). Computer studies were supported by the Federal Aviation Administration (FAA).

In compliance with the Clean Air Act of 1970 as amended in 1977, which required NAS to prepare a report by January 1, 1978, on the status of stratospheric ozone concentrations, CISC, together with its other committee, Committee on Alternatives for the Reduction of Chlorofluorocarbon Emissions (CARCE), and the Commission on Sociotechnical Systems issued an interim report (86). In 1979 a more detailed report was issued (87). (The administrator of EPA is required by Congress to review biennially the stratospheric ozone issue and has subsequently asked NAS to assist in preparing these reports.) The National Academy of Sciences has prepared two additional assessments, published in February 1982 (88, 89).

Background

Fluorocarbons are manufactured worldwide for use in aerosol spray cans, refrigerators, air conditioners, certain types of plastic foams, solvents, and cleaning agents, among others. As late as the mid-1970s, about 50% of the fluorocarbons in the United States had been used as aerosol propellants, and up to 90% of those were used in such products as hairsprays, deodorants, and antiperspirants. These uses of fluorocarbons have been considered by some (but not necessarily by industry) as nonessential items, especially when compared with the use of fluorocarbons in, for example, refrigerators. The chlorofluorocarbons (CFCs) of major concern are referred to as Freon 11 (CFC_3) and Freon 12 (CFC_2Cl_2).

In the early 1970s, scientific research activities that would eventually tie the CFCs closely to stratospheric ozone depletion were under way in different places and in different disciplines. For instance, James Lovelock in England was interested in developing highly sensitive instrumentation to
measure fluorocarbons in the atmosphere. A few atmospheric chemists (e.g., 90), working independently, focused attention on chlorine in the stratosphere. Molina and Rowland (91) suggested that man-made chlorofluorocarbons, while inert in the lower atmosphere, were diffusing upward into the stratosphere where they would photodissociate, releasing free chlorine to react catalytically with ozone, thereby significantly depleting the ozone layer. They suggested that, given the long lag between the emission of CFCs in the atmosphere and their diffusion to the stratosphere, there had already been a serious atmospheric buildup of CFCs. Estimates of eventual ozone depletion resulting from CFC releases ranged from 5 to 30%, based on projected rates of CFC production.

Interest in the effects of trace gases on stratospheric ozone was heightened by the CIAP research efforts and by concern about fuel emissions from the American space shuttle. In the summer of 1974, while involved in the preparation of the final report for CIAP, the Climate Impacts Committee of NAS (later renamed CISC) determined that the impact of photodissociated chlorine atoms released from CFCs was potentially a more serious threat to the ozone layer than space shuttle exhaust. This concern led to the establishment of an ad hoc panel that met in late October 1974 "to assess the data, to define the urgency of the problem, and to advise the NAS about further action it should take" (26). The ad hoc panel ascertained that the chlorine issue was important and worthy of a more serious investigation. So NAS charged CISC in 1975 with the task of investigating the impact of CFCs on stratospheric ozone depletion and the effects of ozone depletion on the lower atmosphere and at ground level.

Internal factors

Design approach. The two studies (26, 27) assessed the following scientific contentions: (1) CFC releases to the atmosphere can lead to ozone depletion, and (2) ozone depletion might have serious, deleterious effects on life on earth. The studies used both a sequential and a parallel approach. During the first few months of the studies, a 13-member panel on atmospheric chemistry sorted through evidence on the possible links between man-made CFC releases and stratospheric ozone depletion. Once this panel was working, a multidisciplinary committee, also with 13 members, representing various physical (but no social) science disciplines was established to investigate the broader problems related to ozone depletion.

The procedures used in these studies were informal and ad hoc. Panel and committee members were chosen because they had a neutral viewpoint, a high standing in the scientific community, and access to the latest information on questions of concern to the study. Monthly and bimonthly meetings were held to update and exchange information and to work toward a consensus. The committee chairman wrote the main body of the report, which was then reviewed by all committee members as well as by an outside body of scholars.

In 1977, the U.S. Congress called on the Academy to conduct an additional study, covering not only the physical sciences, including biology, but also the "health and welfare
effects," as well as "methods for control ... including alternatives, costs, feasibility, and timing" (92). For these CFC studies (87), the research for the physical and biological sciences section was conducted by CISC, the same committee that had conducted the previous Academy study of CFCs. A second committee, the Committee on Alternatives for the Reduction of Chlorofluorocarbon Emissions, was formed to look at the regulatory aspects of the CFC issue. CISC in its physical science studies used an approach similar to the one used in its 1976 study (27). Stratospheric chemistry was first assessed, followed by an interdisciplinary assessment of climatic, biological, and human health impacts. Small panels (fewer than five) conducted disciplinary substudies, reported back to the full committee, and contributed one chapter to the committee's report (87).

The societal impact component (undertaken by CARCE) concerned possible options for CFC emission control. This component proved to be unavoidably political. Rather than attempt to establish a committee of "unbiased" scientific experts (as they had done with the 1976 report), the Academy sought to include a balance of viewpoints. Committee membership included representatives from corporations, conservation, labor, and consumer groups—in addition to university professors with backgrounds in engineering, economics, and law. CARCE divided its inquiry into topics related to industrial technology and socioeconomic impacts, and panels were selected to investigate each topic.

Both CARCE and CISC worked closely together through informal communication links and a number of joint meetings. Their respective chairmen maintained regular communication, and their executive secretaries shared an office at the Academy. The committees issued a joint report, Protection against Depletion of Stratospheric Ozone by Chlorofluorocarbons (87), that represented a consensus of both committees and was reviewed by an outside panel in accordance with the Academy's review procedure.

The Academy's approach to these studies apparently balanced the treatment of the complex physical aspects of the CFC issue with an assessment of possible options for CFC control, as well as with an assessment of the societal impacts that included discussion of the potential effectiveness of different control options. While covering both policy and physical science considerations, the approach left large gaps between these two areas of research. For example, the paucity of information on the effects of UV radiation on plant growth meant that CISC would be unable to assess meaningfully the nonhuman biological consequences of ozone depletion. As for CARCE, no attempt was made to assess the economic and social costs of ozone depletion. While the U.S. Environmental Protection Agency wanted the Academy to undertake a full cost-benefit analysis, the Academy chose to review critically three studies on costs and benefits of CFC emissions regulation.

Time factor. The NAS studies are different from the other studies considered in that they are part of a series of assessments. Time constraints do not appear to have affected the effectiveness of these studies. While the Academy does have deadlines to meet, its prestige and the potential author-
ity of its reports give it an additional degree of flexibility. As the latest step in this ongoing assessment (biennial review), two new Academy studies related to the CFC-ozone depletion issue were published in early 1982 (88, 89).

**Size of staff and integration of component studies.** The NAS deliberately keeps its committees small (15 members or fewer) to increase the flexibility of its studies and the ease of achieving integrated findings. Where specialized disciplinary study is needed, it forms panels that report back to the parent committee. This format seems to aid multidisciplinary exchange. Integration was achieved through cooperation of those responsible for the study, both committee and panel members. The general report of each NAS study on the CFC issue has been produced by a multidisciplinary group as a consensus position.

**General observations.** The resiliency of the NAS study process as well as the potential importance of its reports were tested when, a few months before the report was due in early 1976, new scientific questions, which needed to be accounted for in the study’s findings, arose. The time needed to evaluate these new inputs delayed the release of the NAS report by several months, as explained by P. Handler in a letter to H. G. Stever at NAS. The panel’s ability to assess new scientific information and to secure agreement on delaying the release of the final report shows a high degree of flexibility afforded to NAS studies that might be difficult to find in most other climate-related impact assessments.

The NAS recruitment policies for the panel and its status gave an image of integrity and impartiality to the panel and the final reports. As noted earlier, the report represented a consensus and was carefully worded to avoid a strong position favoring any side of the CFC dispute. The report asserted that CFCs might pose a serious hazard (i.e., a low probability there was no problem) but also suggested that regulations on some uses and releases of CFCs (specifically Freon 11 and Freon 12) be delayed for up to 2 years. The delay was based on the view that “the impact on the world of waiting a couple of years before deciding whether or not to regulate the uses and releases of Freon 11 and Freon 12 is small although we are uncertain just how small” (27, p. 9).

**Impact of the studies**

The National Academy of Sciences reports appear to have had a major impact in policymaking circles. Interest in them was evident even before the reports were issued. In an NSF internal memo, Bastian commented on the yet to be issued 1976 report, “that a high degree of interest in the ongoing [NAS] study has been expressed by the several [Congressional] committees and subcommittees that have been tracking the issue.” She also noted that the recommendations of the interagency task force on Inadvertent Modification of the Stratosphere (IMOS) (93) were tied to the NAS study: IMOS “set forth a timetable for decisionmaking by the Executive Branch . . . [calling for] initiation of rulemaking for some type of restrictions on fluorocarbon use following issuance of the NAS report.” Noting that the report’s impact was not limited to the United States, Bastian stated: “The
CFM issue is being addressed by many other countries and international organizations...[which] have been informed of the NAS report, are aware of its significance to the U.S. decisionmaking process, and are eagerly awaiting its release."
The study was expected to have a significant influence on regulations of CFC emissions. Bastian noted that agencies such as the Consumer Product Safety Commission and the Food and Drug Administration with existing regulatory authority over products using CFMs "have been petitioned by the Natural Resources Defense Council and several states to take some action, and have not done so, waiting for the results of the NAS study to be available" and that "the various industries potentially affected by any regulation are aware of the importance of the NAS study" (94).

The NAS study confirmed the concern of atmospheric scientists who, since 1975, were looking into the possible effects of fluorocarbons and other infrared-absorbing gases in the troposphere on climate through the greenhouse effect (e.g., 95-98). The Annual Report of the Council on Environmental Quality, paraphrasing the findings of the NAS study, stated that a "stratospheric ozone change may have climatic as well as biological effects and that the accumulation of fluorocarbons in the lower atmosphere (troposphere) would alter infrared (heat) radiation with possible further effects on climate, especially temperature and precipitation" (99, p. 194). There is presently an effort in progress within the World Meteorological Organization Ozone Project to prepare a review paper on the possible impact of ozone variability on climate (100).

Although scientists involved recognized that there is a large degree of uncertainty surrounding the interactions of CFCs with stratospheric ozone and that there is still no general agreement on the scientific issue (100, 101), worldwide research and regulatory activity has been steadily developing since the mid-1970s, when the United States, Canada, and Sweden, among others, became concerned about the potential seriousness of the problem. The series of NAS assessments is the result of this concern and has been an important factor in stimulating additional research in the area of stratospheric ozone depletion. All major CFC-producing nations and many users have taken actions to control CFC emissions by reducing or banning aerosol propellant uses of Freon 11 and 12 and by limiting production capacity for all uses of those CFCs to present levels. The 10 member nations of the European Economic Community, Canada, Japan, and several other nations are considering further emission controls for various CFC uses and are working with industry on the technology required to do so. As another concern outside the United States, one might cite two British reports on stratospheric ozone (102, 103). The British reports are often cited by those who oppose immediate regulatory action by governments, because of the emphasis in those reports on the many uncertainties that continue to surround the CFC-ozone depletion hypothesis. Pointing to the uncertainties, these groups cite such phrases as "the validity of the hypothesis is still in doubt" or "the problem, if it exists..." (103, p. 1) or "there appears to be no need for precipitate action" (102, p. 16).
The international body of broadest membership to be involved in the CFC issue is the United Nations Environment Programme, which in 1977 established a Coordinating Committee on the Ozone Layer to coordinate, report on, and assess the significance of research and other information relating to stratospheric ozone protection and to recommend further efforts. The UNEP governing council has also formally called upon member governments to take actions to reduce CFC emissions. In May 1981, the governing council initiated work on a global framework convention for the protection of stratospheric ozone.

The Environment Committee of the Organization for Economic Cooperation and Development agrees that "CFCs constitute a potentially serious problem which should be reviewed periodically" (101, pp. 4-5) and is preparing reports on the scientific, economic, and legal aspects of this issue.

In the United States, the first phase of CFC regulatory activity ended in 1978 when the Environmental Protection Agency ordered the termination of the manufacture and use of most CFC-propelled aerosols. A second phase in CFC regulations is presently under consideration as the U.S. Congress and federal agencies debate the need for additional regulatory action on nonaerosol uses of CFCs (104).

Concluding Comments

Discussions on ways to improve the science of climate-related impact studies took place at the workshop convened by the Institute for Energy Analysis, Oak Ridge Associated Universities, June 29-30, 1981, in which the principal investigators of four of the five climate-related impact assessments were involved (no representative of the NAS chlorofluorocarbon studies was able to attend). The discussion suggested that while many lessons could be learned from the studies reviewed, each study serves to highlight at least one important aspect of conducting a climate-related impact assessment, from project conceptualization to dissemination of the research findings.

1. All five studies faced delays either in project completion or in the dissemination of results, but the time factor proved to be an insurmountable problem for the MIT study. Since project groups had only 1 year to complete the entire Sahel-Sudan project, from conceptualization to final report, sufficient time was not taken to conceptualize and define the problem. A year after the project had been completed, AID acknowledged that "in one year, no group could have been expected to produce a once-and-for-all conceptual framework for the entire region. The task is much more a gradual one, a learning process systematically pursued over a much longer period" (29, p. 9). Time constraints were even more critical in this specific case because MIT personnel were unfamiliar with the region they were researching, no matter how capable they may have been in their own disciplines. These and other time constraint problems, precipitated by the eagerness of AID to acquire research results and the eagerness of MIT to undertake the research project and to comply with its own 1-year time frame, could have been minimized had MIT convened the retreat of senior scholars and policymakers as
requested by AID. Another facet of this problem was that those negotiating the contract with AID were not directly involved in the project and had no precise knowledge of what research efforts and how much time might be required to complete the project successfully.

- Under certain circumstances, researchers not directly familiar with the topic to be investigated will be involved in a project. While this may be a valuable tactic (i.e., no preconceived biases, a chance for “new thinking”), measures need to be taken to ensure that researchers familiar with the topic are associated with the project from the outset and in a significant way; for example, authoritative (as opposed to nominal) advisers on an advisory council that is to be convened early in the project, one that meets often and to whose advice serious consideration is given.

- At the outset of the research effort, study objectives, problem conceptualization, and design approach must be explicit and agreed to by the client and the group undertaking the research activity. As the project develops, the need might arise to modify the original research agenda including, possibly, the methodological approach or the underlying assumptions. Such a reevaluation some months into the project can be healthy, so there must be enough flexibility (of time, funds, and perceptions of principal investigators) to accommodate such changes in research design early in the project.

- The selection of the principal investigator is of major importance. The principal investigator will be, directly or indirectly, the driving force for the study. For example, he will be instrumental in selecting other project managers and researchers, conceptualizing the research problem, and determining the research design.

2. The NDU study’s methodological approach and the packaging of its final report have attracted much attention. Its methodological approach has been viewed as useful because policymakers often have to make decisions relying on expert judgment, even though uncertainties that surround a particular issue might be very large. Through its methodology, the NDU staff sought to identify and quantify the subjective views of selected climate experts on climate-related issues. In their third and final volume, they suggest that the methodology developed in this study could be applied to CO₂ impact assessments and to other similar problems (105, p. vi). Yet the methodological approach also generated criticism that the study represented “science by consensus” rather than the evaluative judgment of climate experts concerning future climate scenarios.

As for the packaging of the NDU report, few can argue that the report stands out visually from the others. While organizations with established reputations, such as the National Academy of Sciences, need not rely on cover design or graphics to draw attention to their findings, perhaps groups new to the climate-related impact assessment area should consider the potential value of packaging their results to attract the attention of their audiences.
• The strengths and weaknesses of the chosen methodological approaches need to be explicitly identified so the research findings can be judged appropriately. Once a study's methodological underpinnings have been questioned, the credibility of its conclusions—no matter how valid they might be—will be subject to challenge.

• While good packaging cannot replace good science, it can assist study authors in distributing good science.

3. The IFIAS project highlights at least two potential problems that project managers might encounter: a need to redefine the study after it has begun and the delayed publication of the study's findings. Garcia redesigned the IFIAS study after several months because he came to believe that the underlying assumption of the generally accepted view of climate-society interactions in 1972 were based on what he termed "pseudo facts." Garcia challenged the conventional causal link of drought to malnutrition and famine, suggesting that the primary causes of famine lie not in drought but in social structure. Those who have tended to accept what Garcia has termed the "official" view of the impacts of worldwide drought in 1972 on agricultural production will be forced to reassess their views as they consider the validity of the IFIAS report findings.

The first volume of the IFIAS report has only recently been published. One can only wonder what effects the delay in publication by more than 2 years might have on the impact of the study on policymakers. Problems with delay in publication have plagued other studies (e.g., the NDU and the NAS reports). A delay of only a few months in the publication of the 1976 NAS chlorofluorocarbon reports caused concern among those awaiting the panel's findings, but policymakers had to accept the delay. Such might not be the case for other climate-related impact assessments.

• It is important to identify and make explicit underlying assumptions on which a research project is developed. If some of those assumptions prove not to be valid, they can be corrected early in the research process. This, too, reinforces the need for flexibility of the research timetable as well as of the research team to readjust its activities part way through the project.

• Reports need to be published in a timely fashion to avoid long delays between submission of the manuscript and publication and distribution of the final product. In addition, lag time between the publication of multiple volumes from the same research project should be minimized to improve the timeliness of submission of the impact assessment to decisionmakers.

4. CIAP's problem with its executive summary highlights in general the importance of executive summaries. It was with respect to this final stage of the CIAP process (i.e., the preparation and dissemination of the executive summary) that charges of political interference in the scientific aspects of CIAP arose.

Other reports, too, had problems with executive summaries. For example, the MIT Summary Report (volume 1)
provided less information on the West African Sahel than already existed (e.g., 106). The executive summary of the NAS report on CFCs and stratospheric ozone depletion was challenged by industry representatives who charged the report's summary omitted necessary caveats, thereby giving the appearance of a higher degree of scientific certainty about the chlorofluorocarbon issue than was warranted. In contrast, the British reports on stratospheric ozone depletion provided conclusions but not executive summaries. As for the IFIAS study, Garcia noted (28) that he did not produce an executive summary in order to avoid what he termed "misinterpretation" of his report's findings.

- Executive summaries are often more important to policymakers and the public than the reports they summarize. They are often designed for the busy decisionmakers who have little time (and little expertise) to delve into the lengthier technical report from which they would otherwise have to draw their own, probably less informed, conclusions. These summaries must accurately reflect the tone and the content of the longer report as well as the degree of uncertainty that surrounds their conclusions.

- The objectives for undertaking an assessment must match the way the assessment is conducted. Certain research objectives (e.g., to determine the state of the art with respect to a climate-related problem) might require a one-time study, while other objectives might call for an ongoing assessment (e.g., the range of uncertainties surrounding our knowledge is so great that periodic assessments could serve to monitor the state of knowledge as it changes over time). While either approach is valid, both types should be considered and matched to the study's objectives.

5. The NAS studies on stratospheric ozone raised the issue of "term" (i.e., a one-time study) as opposed to an "ongoing" (or series) assessment. It is important to qualify what might be viewed as an ongoing assessment. In the case of the NAS ozone depletion studies, although several studies have been done since 1970 in which the general topic of ozone depletion was addressed, the NAS staff members as well as the scientists involved in the preparation of the individual reports changed from one assessment to the next. The NAS assessment process, however, does seem to have more flexibility than the one-time impact assessments (107). One might argue that an ongoing climate-related impact assessment program can follow up its earlier findings by taking advantage of new developments in instrumentation, data analysis techniques, and enhanced scientific understanding. Term projects that are expected to present definitive results within a specific period may not have this degree of research flexibility. On the other hand, it could be argued that once important issues and areas for concern have been identified in a one-time impact assessment, it is possible to focus on those few issues and to establish smaller but more specific follow-up assessments, such as was done by establishing the High Altitude Pollution Program as a result of the CIAP effort.
There is much to learn from past experience of previous climate impact assessments. In fact, a few reviews of such programs now exist (e.g., 82, 41, and 29). It will be valuable for managers and funding agencies of future climate impact assessment studies if retrospective reviews of each project were to be required in a formal climate-related impact study process. A Chinese proverb is appropriate: To know the road ahead, ask those coming back.

References and Notes


28. Authors' notes from discussions at the "Workshop on Improving the Science of Climate-Related Impact Studies," June 29-30, 1981, at the Institute for Energy Analysis, Oak Ridge Associated Universities in Oak Ridge, Tennessee. Participants included principal investigators of four of the five studies discussed here (no representative of the NAS study was able to attend): Rolando Garcia (IFIAS), Allen Grobecker (DOT), William Seifert (MIT), and William Gasser (NDU).

29. Agency for International Development, 1976, Retrospective Assessment of the MIT study, internal memo. The information contained in this AID internal memorandum is very important to understanding the MIT/AID research. Therefore, it is cited as a reference, although the memorandum has not been published. Queries about its complete content should be addressed to the authors of this essay.


48. G. V. Gruza, 1979, "Fluctuations of Climate and Man's Economic Activity," Hydrometeorology vol. 3 (translation from Russian by the National Science Foundation).
49. Because the IPIAS study report has only recently been published, much of the information and discussion in this section of our essay is based on drafts of the report and on correspondence related to the study. For these reasons and because the involvement of the study manager, Dr. Rolando V. Garcia, was more readily identifiable, we refer to him by name.

64. At the conclusion of the project, CIAP's project manager cited three instrumental factors that in his view led to the development of CIAP. "During the discussion of the U.S. supersonic transport project in 1970, the question was raised notably by James McDonald (65) [who linked ozone depletion to skin cancer], the SCEP (61), and Harold Johnston (62) [who linked a trace gas in SST fuel emissions to ozone depletion], whether impurities resulting from aircraft flight high in the stratosphere could alter the proportions of atmospheric trace constituents, with harmful results in the earth's environment" (66).


72. Atmosphere (Journal of the Canadian Meteorological Society), 1976, entire volume 14, no. 3, deals with findings of the Canadian program on CFCs.


77. On the question of tone, tenor, or mood of scientific reports in general and their potential impact on the effectiveness of a report, see Brian Martin, 1979, The Bias of Science (O'Connor, Australian Society for Social Responsibility in Science).

78. For example, the August 20, 1974, version of the summary ("Executive Conspectus," U.S. Department of Transportation, CIAP, Program, Review Draft) was the last one sent to Harold Johnston for comment (private communication). Johnston was
one of the scientists who strongly criticized the final version of the summary.


94. As the latest example of the importance of NAS reports on this issue, Don Clay, director of the Office of Toxic Substances of EPA, noted with respect to the 1982 NAS reports that "EPA is awaiting the NAS assessment, the NASA/WMO [World Meteorological Organization] report, and other pertinent scientific information, and is reviewing and analyzing the issues raised . . . . I can assure you that further EPA action would be based upon reasonable scientific evidence and the most sound


107. For example, shortly after the CIAP research effort ended, stratospheric ozone depletion projections due to SST flights began to decrease and those due to CFCs began to increase. By 1977-78, the projections suggested that SSTs could have, in fact, increased stratospheric ozone instead of depleting it, a reversal of a major CIAP conclusion. Continued research, undertaken through the High Altitude Pollution Program, resulting from the CIAP effort, however, suggested that (Figure 1) ozone depletion would be on the order of that projected at the end of the CIAP study (80, 81). The projected impact of CFCs on stratospheric ozone has changed as well (Figure 2).
The authors of this essay acknowledge the support, financial and moral, of the scientists and staff at Oak Ridge Associated Universities, Institute for Energy Analysis, for the NCAR/IEA workshop on "Improving the Science of Climate-Related Impact Studies." They also thank the scientists and staff at the National Center for Atmospheric Research for their endless support and encouragement. Special appreciation is due to the scientists who took the time to critique the drafts of this paper. Without their comments it could not have been written.

While I am aware of one similar attempt in the area of large environmental models (1), in general, people do not spend enough time performing retrospective, multistudy comparative assessments. Thus, I view this essay as an important contribution to the climate change literature that should give future study contractors, managers, and participants useful insights and guidance.

Since the format of the Review requires that my remarks be brief, I will resist the temptation to comment systematically on the specifics of the studies reviewed and the conclusions and advice that the authors offer. Instead, I will focus most of my discussion on some broader questions that I believe this essay raises: Why do policymakers commission climate impact studies? How should the field of climate impact assessment research evolve? How can future impact studies be used to help promote this evolution?

Sometimes policymakers or organizations commission studies because they want answers. But, in the case of a problem as complex and intractable as the possible climate impacts of human activities, one can be reasonably confident that a good portion of the studies that have been—and will be—commissioned involve, in varying degrees, a number of other motivations (2). If a problem is too messy or too costly to deal with, a study is one way to show commitment and activity while putting off painful choices. If an organization is trying to strengthen its position or reputation, the support of studies on important global issues may be an attractive vehicle for doing this. In both these cases, the selection of a prestigious institution may be more important to achieving the study initiator’s objectives than the selection of an institution with a well-qualified study team.

Most climate change problems are long-term and will require continued, persistent attention over many years. Such problems are unlikely to be amenable to single policy solutions but rather will require an intelligent evolution of policy as we gain experience and better technical and social understanding. In contrast, political interests and priorities change regularly and are in part stochastic, driven by accidents, crises, and chance (3). I believe it will prove impossible to keep climate change always high on the agenda of public policy issues.

While I argue below that one-shot studies can make important contributions, success requires that climate change

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**Commentary**

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impact research move to a more sustained footing. Interdisciplinary research groups must be assembled and institutional arrangements developed to nurture their growth. Traditions of critical thinking and quality control, including a regular pattern of refereed publications of climate impact and policy studies, must be developed. (Note the relative absence of such material and of Ph.D. dissertations in the references for this essay.) Traditional research paradigms, such as those of economics and decision analysis, must not be uncritically applied to the climate change problem. Climate change is different from most other policy problems, because it can involve time constants that are long vis-à-vis normal human activities, resource requirements and impacts that are relatively large, unequal impacts on many diverse human cultures and societies, and irreversible ecological and cultural change. The unthinking application of traditional concepts such as utility theory, maximization of net present value, and future discounting can easily lead one to problem formulations and solutions which I, at least, view as myopic and ethically unacceptable. To a far greater degree than in other fields, climate change impact research must focus explicitly on the critical evaluation of the research paradigms it adopts or develops.

It is clear to everyone that the study of climate change impact and policy must be an interdisciplinary effort. However, I do not think most observers have yet realized how unusually interdisciplinary this effort must be. A satisfactory treatment of the problems outlined above require the effective collaboration of philosophers, economists, atmospheric scientists, anthropologists, ecologists, area study experts, sociologists, historians, psychologists, political scientists, and probably others. If it is to succeed, such a collaboration cannot be patched together quickly for specially commissioned studies. It requires years of nurturing in a supportive institutional environment.

Technical and scientific uncertainty is an inherent element of all climate impact and policy research, and particular attention to the problems of characterizing and dealing with this uncertainty are required. Subjective judgmental techniques (5) play an important role, but, despite the comments in the essay, the use of such techniques need not involve combining experts or "science by consensus." Rather, one can use the range of professional expert judgments to explore the extent to which the impact and policy conclusions one draws are—or are not—sensitive to the range of expert opinions.

If the public policy agenda is somewhat stochastic and if climate change cannot always be kept high on this agenda, how can we mount the kind of sustained effort that the climate impact problem requires? This strikes me as the central challenge for the field. I do not pretend to have the answers, but I think specially commissioned studies can play a part. If we can begin to build a few outstanding centers of excellence in the area of climate change impact and policy research, we can increase the probability that when special studies get commissioned—even for the "wrong" reasons—the resources will go to institutions that already have good interdisciplinary research teams, will produce results that will advance
the state of the field, and, most importantly, will help to further strengthen and sustain these organizations. Incidentally, I would argue that a few centers of excellence are to be preferred to a single center on the grounds that this discipline will benefit from a diversity of approaches and viewpoints.

Through careful timing and effective packaging, the output of such studies can be used from time to time to get climate change research back on the priority list of the public policy agenda. However, since climate change research will not stay high on that agenda all the time, it is essential that ways be found to institutionalize support for ongoing activities. The results of major, specially commissioned studies can be critically important in this regard. By using the opportunity presented by such studies to make strong recommendations and otherwise promote legislation and other institutional changes that will routinize sustaining support for the field, the participants in specially commissioned climate change studies can have a profound impact on the long-term success of climate impact assessment policy research and implementation.

References and Notes


This essay mirrors the efforts these authors have studied: It has problems with scientific credibility, definition of audience, and biases of the leader. The essay describes five case studies that have little similarity except for the general subject matter. To extrapolate general guidelines for future impact studies from these five studies would defy accepted protocols of science. Seldom would any group of scientists accept five cursory observations of a complex system as adequate data to describe that system. On the other hand, the authors of the essay have independently confirmed observations that have been accepted over the past decade by many other researchers, but the failure of the authors to acknowledge past efforts reflects the general difficulty in disseminating and using existing knowledge.

The issues of interdisciplinary problem solving, problem-focused research, computer modeling, and involvement of clients in research received much attention in the 1970s. In my first attempts to examine these topics (1), I found that even in 1960 Cline (2) had conducted an intensive, quantitative survey of many efforts and had identified the same problems as these authors: the difficulties of completing research with the allotted time or funds, the need to set objectives, the key role of the leader, the use of consensus when experts do not exist, the difficulties in documentation and publication, and the incremental pace of science.

More important issues (3), however, might control the fate of an impact study than those identified by the authors. Three issues come to mind: the experience of the client in purchasing interdisciplinary research, the real goal of the client for purchasing research, and the maturity of the disciplines that contribute to the research.

Most clients who are dissatisfied with interdisciplinary problem solving or research efforts have contributed to the failure of such efforts. They often fail to define objectives; they accept at face value the proposals to conduct work; they lack experience in dealing with particular types of researchers; and they select the wrong groups to conduct their studies. The problem of insufficient time to complete contract research can be traced to inadequate evaluation of the resources required to complete a task, unrealistic expectations of the client, and lack of controls on the researchers. There is no substitution for client experience; for example, the NAS style is well established and participants anticipate their roles. On the other hand, interdisciplinary university efforts have few precedents, and each participant feels he can impose his own rules. Nonprofit or agency research teams usually have defined research processes but lack the highest level of scientific recognition needed for credibility. To place a group of scientists in a situation where no formal patterns of behavior have been established is to invite disaster. The university is not the environment in which to develop efficient problem-focused research (4); instead, it is the environment for basic research where results need not be scheduled.

Often clients have unwritten agendas for sponsoring research or, worse, they are influenced by researchers to aim money in their direction for poorly defined problems. Because the goals of the five projects were not similar, the essay authors lack data to address the generalization of strategies.
to achieve various research objectives. An assessment should be an iterative process consisting of (1) assembling relevant knowledge, (2) assessing the problem based on assembled knowledge, (3) evaluating the marginal value of additional knowledge to reduce the uncertainty in the assessment, and (4) presenting the results to individuals and groups with the power to act. It is not obvious that these general steps are accepted by clients, nor that clients perceive that these steps are iterative and interrelated. Many clients expect answers with absolute certainty and resolution; scientists seldom accept a result as absolute truth. The authors here do not seem to recognize these necessary steps and the different skills required to address each step efficiently.

Not all the projects reviewed here completed the four steps, and each project seems to have had difficulty with steps 1 and 4. Assembling relevant knowledge is a task that depends on the maturity of the disciplines involved. More mature disciplines are characterized by many specialists partitioning the field into subdisciplines in order to limit the literature that needs to be monitored and to create a defendable body of knowledge (5). Assembling knowledge from mature disciplines is relatively simple and experts are numerous, but macro or aggregated views will be considered superficial by those in the subdisciplines. New fields, however, tend to be characterized by "instant experts" who gain national recognition based on a few contributions. This small community of experts tends to be inbred and to use strange nomenclature to isolate its communications from other fields. These characteristics are a real challenge to assembling a common base of knowledge that provides a point of departure for impact assessment. Furthermore, most groups tend to spend a significant portion of their resources sampling the data base before they begin to identify critical issues. Since each discipline has its own standards and style for presenting results, it becomes a difficult task to compile a single document to satisfy all participants.

The difficulty with step 4 (communication) is magnified when each client has its own standards and style of documentation and its own resident experts with their particular perception of a "proper" document. There is little evidence that a single unabridged document can affect a wide audience or be accepted at face value. The concept that one document can effectively communicate to all persons is idealistic. In practice, a network of experts and decisionmakers must interpret and translate the results prior to their wide acceptance. The best an impact assessment can achieve is to stimulate widespread debate or commentary. Most decisionmakers seek input to confirm a position and reject opposing views. Major shifts in positions require a major revolution in the perception of many people. A document must undergo substantial debate and revision before it becomes doctrine.

This essay does not address the characteristics of individual participation in interdisciplinary research. Past studies (3) have suggested that experience with interdisciplinary effects and the interdisciplinary training of an individual can make a significant difference in the ability of a person to participate effectively in interdisciplinary research. Obviously, a person participating in interdisciplinary research
must forego some level of effort in disciplinary activities and might lose prestige in the eyes of peers. Another characteristic that might be important in communicating the findings of assessment studies is that the writer be of the same discipline or background as the reader. Communication specialists are not often members of the study teams.

In summary, the authors of this essay have only glanced at the problem of interdisciplinary assessment activities. They have focused on the researchers and have neglected the weaknesses of the client. In problem-focused research, a partnership between client and research is mandatory if a product must be prepared under severe time constraints. Inability to attract the necessary mix of skills to address the problems is a common fault in these efforts. Biased disciplinary views often exclude the needed disciplines.

Pitfalls, problems, and strategies of interdisciplinary problem-solving efforts are recognized in many of the applied fields and professions, such as engineering. Perhaps the problems encountered in assessment studies are more related to the choice of individuals who lack the skills of cooperative research. Probably the most understated conclusion in this report is the final quotation: "To know the road ahead, ask those coming back." The authors have failed to heed their own advice: They failed to examine the experience of others in analyzing impact studies.

References and Notes


