

**Observing the Assessment of Research Information by  
Peer Reviewers, Newspaper Reporters, and  
Potential Governmental and Non-Governmental Users:  
International Peace Project in the Middle East**

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***Dedication***

*to the pursuit of wisdom and the application of what is true and best for society.*

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## *Abstract*

This dissertation examines whether and how members of a policy network consider using social science research entailing new theoretical constructs and policy reorientation. Assessments of truth and utility were observed in semi-structured, qualitative interviews with thirty-five members of the U.S. Middle East policy network, including six peer reviewers, ten newspaper reporters, seven Congresspeople, eight non-governmental policy analysts, activists, and lobbyists, and four members

of the U.S. diplomatic community. All were presented with a study, published in a refereed journal, that explored a novel strategy drawn from the Vedic tradition of India for reducing conflict in the Middle East (*International Peace Project in the Middle East: The Effects of the Maharishi Technology of the Unified Field*, or "IPPME").

Each group of respondents exhibited a distinctive assessment pattern revolving around IPPME's statistical rigor, foreign policy implications, and cultural associations. Over half of each group decided about the truth of IPPME on the basis of their assumed expertise and political utility instead of scientific evidence of effectiveness. Twelve respondents examined scientific quality only minimally, and nine of those said that science could not contribute to social solutions or was irrelevant to their jobs. Eight evaluated scientific quality and took it explicitly into account.

Respondents likelier to consider further research (mainly those who assessed scientific quality) examined the trustworthiness of IPPME in ways predicted by Carol Weiss' (1980) research. They were able to separate their assessments from the philosophies and practices of their organizations, while other respondents were strongly constrained by views prominent in their organizations and within existing repertoires.

Most were unprepared for the description of reality provided by IPPME statistics. Only one said outright that she believed the results. There were striking differences between those more-likely and less-likely to take the research into account. Those who were less-likely could not reconcile IPPME results with multiple aspects of their reality. Those who were more-likely had similar reservations, but the findings didn't challenge core assumptions or identities as much. Their engagement with science allowed them to suspend predispositions and to inquire further.

## ***I. Introduction***

Success in U.S. foreign policy has to some extent depended on the ability of policy makers to examine and step beyond their assumptions about what is true and practical.<sup>1</sup> Observers over the years have noted, however, that when policy is already set, it may take dramatic events to "get policy makers to reconsider their decisions."<sup>2</sup> These tendencies are cited as reasons why research is so little used by policy makers, and why researchers are counseled to tailor their research to policy makers' needs and assumptions.<sup>3</sup>

My dissertation is concerned with research that is not tailored to the specifications of policy makers, but which addresses their concerns from different perspectives. Solutions arising from different perspectives, cultures, or traditions of knowledge might prove useful in solving recalcitrant social problems, including the more than 60 major conflicts currently simmering around the world.<sup>4</sup> Conditions as surprising as the collapse of the Soviet Union may require new approaches in response or may call for more pro-active solutions. Given that a great concern of policy makers in such a volatile world is that they *not* miss the boat by overlooking salient factors in analyzing, planning, negotiating, legislating, etc., it is important to understand how they confront new ideas, especially those that are scientifically validated.

I gave my interview respondents an item of research information and asked them to consider it as they normally would in the course of their work, and to explain how they would decide whether to consider it in the future. The study used was published in 1988 in *The Journal of Conflict Resolution* (JCR) and was entitled "International Peace Project in the Middle East: The Effects of the Maharishi Technology of the Unified Field" (IPPME).<sup>5</sup> *The Journal of Conflict Resolution* (JCR) is considered "one of the foremost journals in its field."<sup>6</sup> The study concluded that a large group of meditators practicing the Transcendental Meditation (TM) and TM-Sidhi program daily together in one place in Jerusalem appeared to reduce social stresses in the surrounding population as indicated by statistically significant changes in eight different dependent variables. When the group reached a critical size predicted to be sufficient in proportion to each relevant population size, variables such as crime (in Jerusalem), then national crime (excluding Jerusalem), and then war deaths and war intensity (in neighboring Lebanon) also decreased (see Appendix A).

The IPPME study is one of 38 studies conducted over the last 18 years on the same general topic. Fourteen of the 38 have been reported in eight refereed articles. Of these, the IPPME study is the only one published in a mainstream journal within the conflict resolution field.<sup>7</sup>

To the extent that further research might bear out these results, they would be of indisputable social and scientific value. But the findings were generally met with various levels of disbelief. At the time of publication, two reviewers articulated the dilemma: According to their scientific review the study was well done, but they questioned its premises in the strongest of terms.



<sup>8</sup> The IPPME research has stirred considerable controversy because the reported evidence appeared to statistically support premises which run counter to generally accepted understandings about behavioral phenomena (both personal and social) and mechanisms that can alter them.

Professor of government David Edwards, who reviewed research that followed the IPPME research for the American Psychological Association in 1990, wrote that the findings were "startling."

"In study after study they have found that particular types of meditation by small groups of trained meditators has resulted in very substantial decreases in acts of violence, terrorism, and other societally negative phenomena in such places as Lebanon and Israel, and in U.S. Soviet relations."<sup>9</sup>

He pointed out that the concept of mind-over-matter violates an assumption that has conventionally guided social science, namely that "consciousness as such is not causal in human action." He explained that IPPME and related research posed two corollaries challenging these assumptions: "that collective consciousness is itself causal in the social world:" and most problematic of all in the history of science, "that action carried on in collective consciousness ... influences what we perceive to be material reality at a distance."<sup>10</sup>

At the same time Edwards wrote that the research achieved "something most social theories cannot hope for, since they tend to be theories of conflict and violence rather than of their reduction." Even peace research, he wrote, "which focuses on theories of war, arms racing, conflict escalation, intergroup hostility ... evokes the counterproductive dynamic of legitimating ...violence." Noting that he was not affiliated with the researchers, Dr. Edwards wrote:

"The claim can be made plausibly that the promised practical societal impact of this research significantly exceeds that of any other ongoing social-psychological research program. For this reason alone, the research along with the theory that informs it deserves the most serious evaluative consideration by the social science community."

"However, it has thus far been unable to achieve this consideration because the major social science presentation and publication avenues have been largely closed to it by their designated 'gatekeepers' or 'disciplinary mental hygienists' -- the organizers of professional conferences and the editors of major journals."<sup>11</sup>

Though the IPPME research may counter conventional understandings and has not been given serious policy consideration, the study's authors have argued that its premises are not counter to Western thought, but, on the contrary, have been commonly assumed through much of its history. They have suggested that their empirical findings, benefiting from the precision of the methodological tools and meditation techniques currently available, might reintroduce concepts of collective consciousness raised by eminent early psychologists and sociologists such as William James, Gustav Fechner, and Emile Durkheim.<sup>12</sup> They have written that quantum field theorists (including Schroedinger, Jeans, Eddington, Pauli, and D'Espagnat) anticipated the basis of a

fundamental field theory of consciousness in suggesting, as Max Planck wrote, that consciousness is fundamental and matter is "derivative."<sup>13</sup>

According to IPPME co-author John Davies, IPPME and related research presents a new level of analysis, "complementary to conventional behavioral and sociological levels of analysis. It gives policy makers new possibilities for understanding and alleviating societal stress and improving related social factors like health and productivity, thereby enhancing the effectiveness of existing policy efforts."<sup>14</sup> IPPME author David Orme-Johnson explained that with the changed climate brought about by meditating groups he would expect changes in foreign policy to be accomplished "through traditional channels." He pointed out that the "Maharishi Effect" research, of which IPPME is a part, has found that collective TM practice appears to affect negotiations as well as reduce violence.<sup>15</sup> He said, for example, that negotiators recognize the importance of a more cordial atmosphere and reduced hostility for negotiations.

In explaining the mechanics of the TM techniques and their proposed field effect, philosopher Kenneth Chandler has described them as a revival of traditional Vedic concepts by Maharishi Mahesh Yogi. He wrote that ancient Vedic seers

"discovered the capability of the human mind to settle into a state of deep silence while remaining awake, and therein to experience a completely unified, simple, and unbounded state of awareness, called *pure consciousness*, which is quite distinct from our ordinary waking, sleeping, or dreaming states of consciousness. In that deep silence, they discovered the capability of the mind to become identified with a boundless, all-pervading, unified field that is experienced as an eternal continuum underlying all existence."<sup>16</sup>

He noted that techniques for developing the habit of transcending have not been universally available in the past, but that glimpses of this domain of experience have been described in almost every culture throughout history "from Plato to Plotinus and Augustine, and from Leibniz to Hegel and Whitehead." He suggested that scientists like Kepler, Descartes, Cantor, and Einstein "have written of it and seemingly drew insights into the laws of nature from this experience."<sup>17</sup> Several writers have also pointed out that the TM techniques, which require no belief themselves, are fully in harmony with Judaism, Christianity, and with other major religious traditions.<sup>18</sup>

Chandler explained that transcending is not "thinking, theoretical conjecture, or imagination, but on the level of direct experience, which is more vivid, distinct, clear, and orderly than sensory experience --perhaps much in the same way that Newton or Einstein, when they discovered the laws of universal gravitation or special relativity, enjoyed a vivid experience of sudden understanding or a kind of direct "insight" into these laws."<sup>19</sup>

Charles Alexander, Davies, et al. describe transcendental consciousness as a "qualitatively distinct state of 'restful alertness,' psychophysiologically different from adult waking as waking is from dreaming and deep sleep."<sup>20</sup> Researchers have correlated experiences of transcending or

"restful alertness" with heightened EEG (brain-wave) coherence (different from resting controls). They have found coherence "across cortical regions and frequency bands, suggesting an increase in long-range spatial ordering and functional integration of the cortex,"<sup>21</sup> which has also been correlated with "postmeditation behaviors indicative of continued growth, such as fluid intelligence, principled moral reasoning, concept formation, and creativity." <sup>22</sup>

For example, Orme-Johnson, who has published dozens of studies concerned with the psychology and physiology of TM practitioners, found that TM practitioners had 87 percent less hospital admissions and doctor visits for heart disease than the average American <sup>23</sup> and has cited a Swedish epidemiological study "which found that hospital admissions for psychiatric care were 150-200 times less common" than among the population as a whole.<sup>24</sup>

Citing much of this research and TM founder Maharishi Mahesh Yogi's proposition that raising the coherence in even a small proportion of a population would stimulate measurably increased coherence in a society, the IPPME authors, David Orme-Johnson, Charles Alexander, John Davies, Howard Chandler, and Wallace Larimore, proposed that a primary determinant of quality of behavior in society is coherence in collective consciousness and that a sufficient number of people meditating thus has the capacity to achieve "drastic tension reduction" within their society. In their 1983 research proposal, Orme-Johnson and Alexander explained that human consciousness may have a "field character" like those that mediate other action-at-a-distance phenomena; that coherent sub-populations may "generate coherence in an underlying field of consciousness"; and "if the human brain and nervous system are sensitive to the field phenomenon of consciousness, then a number of individuals generating coherence in this field could influence the coherence of others in the environment."<sup>25</sup>

The sociological research program which includes the IPPME study initially grew from Maharishi Mahesh Yogi's prediction that a small percentage of the population -- as little as 1% -- would improve quality of life and lead to world peace. Initial studies involved cities with 1% meditating and examined changes in crime rate, looking at publicly available data. Eventually these studies included more cities, sometimes randomly assigned, and more variables as indicators of change in quality of life, such as automobile accidents, and suicide rates. The research has progressed, in that it has encompassed societal levels including cities, nations, and the world. Scientists have employed more sophisticated techniques for statistically controlling for seasonal and other alternative explanations. Also, in the late 1970's scientists began to look at the effect of the square root of 1% of the population practicing the TM-Sidhi program in the same place. This smaller number of required participants made experimental study easier.<sup>26</sup> Quality of life indices have included increased economic strength, political cooperation, decreases in violent crime, armed conflict, accidents, suicides, infectious diseases, and drug consumption.<sup>27</sup>

As a teacher of the TM program and former student and administrator at Maharishi International University,<sup>28</sup> which has incorporated the TM program into its curriculum, I have had the opportunity to examine the research and explore the techniques studied more closely. Whether or not I consider the technologies to be vaccinations against violence worthy of further study or a description of innate human capabilities will not concern me here. My interest is how people with little or none of my experience and training look at these ideas as expressed in scientific research, in a compressed time frame, and in relation to existing conceptual frameworks.

My concern reaches beyond what the individual respondents told me, to their discussion within the specialized world of foreign policy. Their decision about *whether and how to consider* a specialized scientific inquiry such as the IPPME study is in a sense a group discussion, because each participant referred to what Gregory Clark termed a "functioning consensus," involving the inescapable fact (identified by Aristotelian scholar William Grimaldi) of "large complexes of pre-existing convictions and assumptions" on levels of common sense, "cultural and social ideology," and specialized knowledge.<sup>29</sup>

Clark explained that for Plato and Aristotle, the purpose of rhetoric was the collective determination of what is true and best for the community and suggested that open, communitarian deliberation is ideal. Though it is often adversarial, science provides participants such an opportunity. It gives the scientist and the end-user the chance to test what nature says, rather than what they or the larger functioning consensus expect it to say. It is perhaps even more crucial that community members put aside their predilections when they consider scientific information from new or unusual perspectives, because the new or the unusual is often the source of scientific and practical breakthroughs. But it is precisely the new and unusual that must also be given the most careful scrutiny.

The communication of research information to the policy maker begins with peer review and requires of the reviewers that they balance innovation with quality. The political nature of peer review can also involve competition for resources and give greater weight to the functioning consensus than fresh conjectures. Innovative research may encounter efforts to mark it off as illegitimate, as has often been the case in the history of science. How reviewers approach the innovative research information -- how they weigh what is true and best for the community within the context of disagreements about what is or is not legitimate -- has often determined the course of science and civilization.

Journalists on whom decision makers rely to keep them apprised of research information, as well as the scope of the policy debate and events in foreign policy as they break, sometimes miss the larger story: they get wedded to past assumptions. Those who cover the daily playing out of

aggression in protracted conflict may be hard-pressed to consider ideas that speak of the more abstract aspects of human nature, especially as they are conveyed within scientific research.

Policy makers in Congress must explicitly weigh consensus when considering new ideas and may choose to attend to scientific information only when it pays to do so. It is of interest to understand how they determine whether to do so. Diplomats are constrained by the policy decisions of the past that have tended to be conducted without the aid of science, while at the same time facing expanding demands on their judgment.

Approximately 2300 years since Plato and Aristotle wrote, the ways in which the community decides what is true and best is perhaps even more in need of understanding. Sociologists of knowledge application have been concerned with whether investment in social research is adequately used and how it could better be used. Sociologist Carol Weiss has focused these questions at the juncture where professionals, who work in complex, multi-layered organizations, decide what information to consider. Her research has indicated that professionals value research that challenges the status quo. They tended to draw their conclusions based on their own experience and judgments about scientific quality. My inquiry attempts to extend Weiss' research with exploration of professionals' filtering of research information with premises that many may perceive as far more challenging to the status quo than the research she used. My dissertation is intended as a contribution to the understanding of how professionals judge social science information as well as how they confront new perspectives.

## ***II. Literature and Research Findings that Have Informed my Study***

### ***A. Overview Concerning Research Use***

Why research is little used has been a subject of inquiry since Robert Merton offered a framework for study of the application of social research in 1949. Today scholars are still asking why "connections are missing between the age's best minds and its worst problems."<sup>30</sup>

Carol H. Weiss reported that in the late 1970s, as the federal government was committing unprecedented amounts of money to social research, a series of commissions and reports indicated that research results were "generally of limited use to national policymakers."<sup>31</sup> Seymour Deitchman described the difficulties of doing and communicating research between agencies of the U.S. government, especially when the research involved another culture. He described the impact of such research as nil.<sup>32</sup>

Weiss has also described extensive efforts to understand why research knowledge hasn't been used. Examination of obstacles to research use, characteristics of decision makers, and deficiencies in transmission of research has resulted in a reevaluation and redefinition of research use.<sup>33</sup> Social science research is now understood to be used less in a linear sequence, and instead more incrementally, by professionals who work in complex multi-layered organizations. In this context they accumulate evidence for or against a program or policy and use ordinary knowledge and common sense as well as research.<sup>34</sup>

Nathan Caplan, with colleagues at Michigan Institute for Social Research, interviewed 204 upper-level executive branch members about their use of social research. He identified three reasons for the non-use of potentially relevant studies:

1. the way research is done;
2. difficulties in bridging gaps between scientists and action-oriented policy persons with different frames of reference; and
3. constraints on the application of research imposed by the practicalities of policy-making.

I will explore each of these three reasons and remedies researchers have suggested.

1. *Non-use is sometimes a consequence of the way research is done, techniques involved, or behavior of social scientists doing the research.*<sup>35</sup>

Henry Aaron is one of many writers who have described theoretical, political and practical difficulties that beset social research. The foremost difficulty is the complexity of social problems, which leads to conflicting scientific conclusions. Because of discrepancies and lack of closure in the research, research is likely to have a conservative influence on policy.<sup>36</sup> Aaron asserted that "preconceptions and faiths" motivate policy far more than "reliable information ... or scholarly findings." While he counseled that research will and should be considered within "a broader

perspective," he also warned that "today's faddish theory may turn out to be false, and the chance that some other theory may be correct should not be ignored."<sup>37</sup>

The problem, according to Aaron, consists in "choosing among diverse theories, none of which is refuted by available facts, and among empirical findings that seem inconsistent." Science has resulted not in prescriptions for success but in detection of failures, because scientists, in their search for elegance, simplicity and manageable problems, "separate problems into components producing theory detached from reality." He described gaps in scientific standards, the tendency to develop theory from data not derived from controlled studies and produced for other purposes, the classic problem that facts can conform to a variety of theories, and "disagreements about values often masquerading as disputes about facts." Theories that gain currency are sadly subject to the "mood of the times and will be affected by the persuasiveness or the prestige of their advocates." Ward described the tendency in economics to base judgments of quality on the ingeniousness of attempts to solve a puzzle within the conventional framework even if the problem remains unsolved.

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Charles Lindblom and David Cohen, who came to similar conclusions about the nature of scientific evidence, have suggested more radical recourse. They concluded that so much should not be staked on professional social inquiry, because this kind of knowledge cannot live up to society's expectations for conclusive indicators for solving problems. They lobbied for more discriminating use of professional social inquiry and recognition of the importance of ordinary knowing.<sup>39</sup>

From the perspective of the history and philosophy of social science, D.C. Phillips has described social science in flux, asserting that "for full justice ... to be done to social science research ... some way must be found to incorporate evaluation of the ongoing stream of work plus the basic theoretical orientation that has inspired it."<sup>40</sup>

2. *Failure to use social research results when actors live in two communities or cultures with "conflicting values, different reward systems, and different languages."*

Attempts to bridge the gap between knowledge producers and users have included proposals such as those by Norman Krumholz and Janice Cogger, who urged that social scientists become advocates of their findings.<sup>41</sup> Alternatively, Burkart Holzner has proposed developing a "sociology of knowledge application," including the study of relations between knowledge producers and decision maker/clients, knowledge production strategies, organizational and policy research use, and "societal styles and cultural patterns for knowledge production and use."<sup>42</sup> Holzner observed that there are "reality tests" and "criteria for the certification of information as knowledge among knowledge producers that necessarily diverge from institutionally required reality tests among knowledge users." He suggested that "trustworthiness of different sources of

knowledge" deserves attention and asserted that "frames of reference and reality tests" are not easily revised, because they are "anchored in personal, professional or collective identities."<sup>43</sup>

### *3. Constraints involved in policy making also inhibit use of social research.*

Decision makers access social science information "in abbreviated form" as it comes across their desks in the course of their work.<sup>44</sup> The assessment they make as to whether to give the research further consideration is influential in determining whether they will use it in the future. Such assessment is influenced by practical constraints such as the need for concise information, quick turn-around time, political limitations, etc.<sup>45</sup>

Weiss, whose study approach is central to the conceptual framework of this dissertation, has written that social science research has notable effects despite organizational constraints. This is better understood, she wrote, by looking at officials' actual practice which is a fragmented enterprise, involving many people, high turnover, harried schedules, mismatch of jurisdictions, resource limitations, political boundaries, and a world dominated by compromise, acceptance of things as they are, and precedence given to knowledge derived from experience.

Given these sorts of realities, people who do not rely solely on information, or "view research as not authoritative" are "unlikely to draw upon research in conscious and formal ways."<sup>46</sup> Weiss has explained, "Policy makers use research less to arrive at solutions than to orient themselves to problems ... Bringing new perspectives to attention and formulating issues for resolution ... may be the most important contribution that social research makes to government policy."<sup>47</sup>

The remedy for lack of *direct application of research to social problems*, according to Weiss, is better understanding of social and cognitive learning processes involved in assessing research in policy contexts, and improving research itself, which depends for its explanatory power on its comprehensiveness and breadth.<sup>48</sup>

Weiss asked 255 administrators, professionals, and researchers to assess summaries of two research studies in order to *simulate* the review they routinely apply to social research information - which they usually read in summarized or abstracted form.<sup>49</sup> Using factor analysis, she identified truth and utility tests that these decision makers apply in assessing the likelihood they will use research. Like Caplan and Karin Knorr -- who interviewed 70 government officials in federal, provincial and city governments in Vienna -- Weiss was surprised at the high degree of research use reported by decision makers. Respondents reported that social science contributes to gradual, cumulative processes and to continued education, reduction of uncertainty, confidence building, and legitimation of positions and arguments, altering explanatory frameworks that undergird policy.<sup>50</sup> This also corroborated Caplan's and Knorr's observations that decision makers use research



differently than is commonly imagined, contributing to a theoretical reconceptualization of research use.<sup>51</sup>

Caplan described decision makers' tendencies to be eclectic in their use of information. He documented decision makers' widespread use of "soft knowledge," i.e., a gestalt of "hard information, past experiences, intuition, values and other considerations." Decision makers use soft knowledge to stay current with society and to feel they are operating "from scientifically supported positions" without having "to substantiate the true scientific merit of such positions."<sup>52</sup>

Weiss found that there is little likelihood that results of research "match problems, information gaps and options confronting decision makers with exactness that translates into action," especially when research is conducted outside the agency. Officials are bombarded with information that they implicitly filter. If a report of research fails the criteria of this review process, it is discarded. If it passes, a residue is "incorporated into their stock of knowledge," on which "they draw when action is called for."<sup>53</sup> Research findings percolate into a stock of knowledge, accumulating and gradually sensitizing policy makers to issues. This process relies on assessment of research by individuals, interacting individuals, and ultimately groups.

Weiss used what she calls "the enlightenment model" of research use to explain "jumbled and diffuse processes" of decision makers not taken into account by traditional "rational models."<sup>54</sup> For example, the knowledge-driven model common in bio-medicine suggests that existence of knowledge "presses it toward development and use." The even more common problem-solving model suggests that based on consensus concerning goals, research is used in a systematic process of identifying and selecting "appropriate means to reach the goal."<sup>55</sup> These models involve assumptions about decision making processes that do not necessarily hold, i.e., that decision making grows out of clear goals; involves rational calculation like cost/benefit analysis; entails commitment, decisive action, and consensus; and occurs sequentially.<sup>56</sup>

The enlightenment model looks at research primarily as part of the intellectual enterprise of society -- where information flows are serendipitous and review of research does not result in immediate action, but is more likely to influence the diffuse processes of groups which, after the accumulation of small steps, arrive at an outcome. In this model the individual applies professional judgment to determine whether to give research consideration in his or her work. This *micro-level* process relies on assessment of research by individuals, interacting individuals, and ultimately groups, and may be the most influential determinant of future use of research.<sup>57</sup>

Weiss found that decision makers "invoke three basic frames of reference" when they filter research: relevance of the study "to their sphere of responsibility" (relevance); "trustworthiness of the study" (truth); and "direction that it provides" (utility). The most significant is the truth test, which involves two basic questions:

"Was the research conducted by proper scientific methods?" and/or  
"Are the results compatible with my experience and values?"

Weiss found that the answer to the first question is most predictive of whether decision makers are likely to take a study into account and whether its ideas and information would make substantive contribution to their work. Decision makers tend to discard research that does not conform to their experience and values, but are less inclined to do so if they judge it to be of high quality.<sup>58</sup>

Decision makers also apply a test for usefulness, which asks two further questions:

"Does the study demonstrate how to make feasible changes in things that can feasibly be changed?" and/or  
"Does the research challenge the status quo?"

Weiss found that positive answers to both questions increase the likelihood of a study's application. They also found that if a study provided actionable conclusions, it was better if it did not challenge the status quo. If it challenged current policy, it was better if it did not offer a specific direction for action.<sup>59</sup> In addition, Weiss found no relationship between decision makers' judgments about a study's compatibility with their experience and values and their judgments about its acceptability in social/political work environments, implying that people distinguish their personal views from those of their agency. Decision makers did not discard research that was at political variance with their agency's policy; they often valued it.

This research suggests that, contrary to what social scientists may believe, decision makers seem to weigh social research in light of what they know about science and are open to ideas that counter conventional wisdom. Weiss studied research that had been funded by the government, however, and thus fell within the main stream of social science.

Weiss has also explained that research use to guide change has been only modestly successful because organizations resist radical change and prefer incremental, often insignificant change.

"Organizations' willingness to implement the guidance derived from research appears to be seriously constrained by their limited range of feasible options. An organization is skilled in performing only a modest set of activities, and its traditional procedures exercise a heavy drag on its flexibility to venture beyond accustomed patterns. Even if research should show that radically different strategies have a high probability of working better for both the organization and its clients, many organizations would be unprepared to make the fundamental alterations in staffing, structure, investment, and leadership that such change would require." <sup>60</sup>

Weiss explained further that because research reports rarely provide unequivocal possibilities for change, such organizational behavior is understandable. She wrote that, given the nature of organizational change and the nature of research which rarely identifies "clear causes of social problems or demonstrates the undisputed superiority of new approaches for dealing with them," more appropriate and ultimately useful results of research might involve reorientation of

"perspectives for understanding and interpreting events." Such reorientation may provide "new frames of reference," may "provoke a willingness to think critically about the organization," and "may stimulate internal reexamination, which may often be the best route to organizational renewal."

61

### ***B. How Social Scientists Examine Research within Peer Review***

While some contend that peer review is part of a larger consensus-building effort among scientists,<sup>62</sup> others point out that it is less systematic and more "trial and error, conjecture, chance, competition and even dialectic."<sup>63</sup> Others have described factors similar to those found by Weiss to be most predictive of future research use as central to peer review. Decisions to publish or not hinge on the quality of the research and the need to foster advancement of knowledge. Such decisions are also affected by values, expectations, and social conventions, including political considerations.

### ***The Importance of Research Quality and Innovation***

Peer review has existed as long as people have been concerned about the communication of new knowledge. Since the first scientific journal was published in 1665, scientists' pre-eminent concern across disciplines has been quality.<sup>64</sup> Since the 1920s, the positivistic-quantitative paradigm has predominated in the social sciences, with the assumption that "the unity of all science was to be found in method, not substance." As William Ogburn wrote in 1922, "We cannot have a science without measurement. And science will grow in the social studies in due ratio to the use of measurement."<sup>65</sup> It is widely acknowledged, however, that even though editors and reviewers hold manuscripts to high standards, "error is inherent in research, and validity is always conditional."<sup>66</sup> There is "no watertight way to take a piece of scientific work and decide on its merits."<sup>67</sup> What constitutes appropriate measurement is itself open to debate. As Donald Campbell remarked,

"Non-laboratory social science is precariously scientific at best. But even for the strongest sciences, the theories believed to be true are radically under-justified and have, at most, the status of 'better than' rather than the status of 'proven'. All common-sense and scientific knowledge is presumptive. In any setting in which we seem to gain new knowledge, we do so at the expense of many presumptions."<sup>68</sup>

Several scholars have observed an over-emphasis on sophisticated research technique at the expense of theoretical debate and have described research conclusions as frequently "tentative, hesitant, cautious, and humble to the point of non-belief," this being "foolproof defensive technique," stultifying to disciplinary growth.<sup>69</sup> Morris Zelditch, Jr. defined the problem as methodological exhibitionism resulting from "a good deal of dissensus about what to study." He argued that in sociological journals good methods have tended to drive out good problems because with so much dissensus there is likely to be more agreement about methodology than about theory or theoretical priorities.<sup>70</sup>

According to Duncan Lindsey, the scientific publication system is unable to "adequately assess scientific contributions within conditions of high competition for publication space."<sup>71</sup> The crowding out of good problems is made worse by the pressures upon academics to publish. Benjamin Singer described an explosion of journal publication and subversion of professional standards by the sheer press to publish.<sup>72</sup> What is published has not always been considered useful. A survey of 3,800 scholars by the American Council of Learned Societies (1986) also reported that despite this press to publish, one third of the scholars "rarely find articles of interest in their discipline's [major] journal. Sixty percent said it was "virtually impossible to keep up 'even minimally' with the literature in their fields."<sup>73</sup>

Lindsey has described limited possibilities for social science publication: one half article per year for a working sociologist as opposed to three per year for a working biochemist.<sup>74</sup> Given these circumstances, he argued that creative and productive social inquiry are stifled:

"The creative, questioning spirit that penetrates and reveals has been exchanged for the dogged pursuit of knowledge in the computer printout... I fear the conditions of the publication system in the social sciences have given rise to a generation of scholars and scientists who view the computer, statistical models and the absence of concern with the moral and political questions of the day as the requisite materials for building a successful scientific career."<sup>75</sup>

The issue for today's editor, reviewer and author is the "powerful tension in the history of science, between on the one hand, originality, creativity, and profundity, on the other, accuracy and reliability."<sup>76</sup> The editor in balancing quality with innovation serves as a gatekeeper and as a regulator of the flow of ideas and the advancement or non-advancement of careers.<sup>77</sup>

Malcolm Atkinson has contended that publication greatly influences "competition for status" and therefore decisions regarding funding. While editorial review "exerts a powerful regulating influence on the progress of science," there seems to be no regulation of such review.<sup>78</sup> "The main channel of scientific communication," Atkinson wrote, "is subject to the character and persuasions of the adjudicators," namely editor and reviewers. To the extent that their decisions "pre-empt scientific debate, editors and their advisers assume a heavy responsibility for nurturing fresh conjectures and for maintaining unbiased speedy communication."<sup>79</sup>

The challenge for editors is the promotion of original thinkers, who have historically suffered delay in their work at the hands of scientific communities that resist change.<sup>80</sup> While Atkinson considers resistance and criticism healthy for science, he has lobbied for effective precautions against arbitrary dismissals, namely open and public debate. He has pointed out that there may be an assumption of peer status without detailed evaluation. This behavior confers status on the referees -- i.e., "assumed superiority in the particular topic of inquiry." Even a renowned

scholar "is not necessarily competent to judge a new development." Nobel Laureate Rosalyn Yalow points out that there are "few peers of those with truly important breakthroughs." <sup>81</sup>

### ***Separation of Insight and Logic from Attitude and Competition***

Atkinson has declared that although ideally all parties have truth as a common aim, in reality "entrenched opinion, posturing and irrational rebuttal are normal in human discourse." Intellectual authority may also be exercised to "mollify members of an inner circle." He concluded that "the separation of insight and logic from attitude and competition needs very careful attention," and that we are limited by our perceptions, rarely able to observe "what does not exist [for us] ... as a conceptual possibility." <sup>82</sup>

Arthur Koestler in 1959 wrote that "the inertia of the human mind and its resistance to innovation are most clearly demonstrated not, as one might expect, by the ignorant mass... but by professionals with a vested interest in tradition and in the monopoly of learning." <sup>83</sup> In 1990 Charles Lindblom provided documentation from a "voluminous" and growing literature concerning the ills of the social sciences. He mentioned the difficulty that scientists and advanced students generally have in remaining open to new ideas or in challenging their own beliefs. <sup>84</sup>

Several groups of researchers have explored the scientific reasoning of laymen and undergraduates, and have found that these groups display a weak grasp of scientific reasoning. Michael Doherty et al. and Clifford Mynatt et al. found that when laymen attempted to test scientific hypotheses (some with indoctrination concerning falsification as the optimum scientific strategy), they "shared a strong tendency to confirm rather than disconfirm their hypotheses." <sup>85</sup> Charles Lord et al. studied undergraduates' responses to capital punishment and found that they examined empirical evidence in ways that confirmed their prior beliefs. <sup>86</sup> Richard Nisbett and Lee Ross found that undergraduates who read studies giving fair presentations of both sides of an issue were "likely to strengthen pre-existing opinions" instead of increasing tolerance. People were not swayed by the evidence but tended to read selectively, believing only what conformed to their views and rejecting what did not. They grew surer that they were correct and that those who differed with them "must be fools." <sup>87</sup> Other research shows that scientists are little better at displaying scientific reasoning than the average person. Michael Mahoney and Bobby DeMonbreun compared conservative ministers with physicists and psychologists and found that ministers behaved more like the conventional image of scientists, and scientists "based hypotheses on small samples, and seem to stick to them longer." <sup>88</sup> Leslie H. Kern et al. found scientists only marginally better at scientific reasoning than other subjects. <sup>89</sup> Chapman and Chapman studied clinicians and found that "despite their years of training," they were "just like anyone else in resisting falsification of their ideas, looking for confirmation, and being swayed by the immediate, concrete, isolated, confirming instance against masses of statistical findings." <sup>90</sup> Another study by Amos Tversky and Daniel

Kahneman found that "the gambler's fallacy persisted among psychologists, who are all trained in statistics and should therefore have been disabused of the mistake." <sup>91</sup> Thomas Leahey and Richard Harris, who summarized these studies, concluded about this last one that "scientific sophistication just lets scientists make sophisticated mistakes." <sup>92</sup>

Ian Mitroff's 1974 study of 40 scientists who were involved in Apollo lunar missions suggested that attachment to one's ideas may not always be a liability in the scientific arena. <sup>93</sup> Apollo moon scientists identified three of their colleagues as most attached to their views, "even to the point of infuriating pig-headedness." The same scientists were also considered by the same colleagues to be the most creative and brilliant and therefore important. Less stubborn, more analytical, but less speculative scientists, "who cautiously generalized from large amounts of data, were rated as impartial but dull and unimaginative." <sup>94</sup>

All of the scientists Mitroff interviewed considered the notion of an emotionally uninvolved, objective scientist to be naive and also not an ideal worth emulating. They considered it important to be emotionally committed partly because they considered science to be an adversarial process.

"I don't think we have good science because we have adversaries but that it is in the attempt to follow the creed and the ritual of scientific method that the scientist finds himself unconsciously thrust in the role of an adversary." (Scientist F) <sup>95</sup>

Belief or prior intellectual commitments also appear not to be liabilities when it is understood that evidence doesn't change those commitments and doesn't settle differences. They are changed through an almost adversarial give and take, resulting in a set of assumptions becoming preferred. The scientists explained this process:

"You can't understand science in terms of the simple-minded articles that appear in the journals. Science is an intensely personal enterprise. Every scientific idea needs a personal representative who will defend and nourish that idea so that it doesn't suffer a premature death. Most people don't think of science in this way but that's because the image they have of science only applies to the simplest, and for that reason, almost non-existent, ideal cases where the evidence is clear-cut and it is not a matter of scientists with different shades of opinion.

"In every real scientific problem I've ever seen, the evidence by itself never settled anything because two scientists reach entirely different conclusions. You eventually settle the differences, but not because of the evidence itself but because you develop a preference for one set of assumptions over the other. How you do this is not clear since there's not always a good set of reasons for adopting one rather than the other." (Scientist G) <sup>96</sup>

Mitroff's Scientist B discussed the relationship of the committed scientist to data:

"Most of the scientists I know have theories and are looking for data to support them; they're not sorting impersonally through the data looking for a theory to fit the data. You've got to make a clear distinction between not being objective and cheating. A good scientist will not be above changing his theory if he gets a preponderance of evidence that doesn't support it, but basically he's looking to defend it. Without [emotional] commitment one

wouldn't have the energy, the drive to press forward sometimes against extremely difficult odds.

"You don't consciously falsify evidence in science but you put less priority on a piece of data that goes against you. No reputable scientist does this consciously but you do it subconsciously." <sup>97</sup>

Mitroff has suggested that the norms of Apollo moon scientists changed given the nature of the problem investigated. For example, the more defiantly elusive problems tended to be associated more personally with the scientist who nurtured them. Counter-norms such as intense personal identification with a problem were to be expected when problems were less well-defined. A problem that might be solved in a more clear-cut fashion would involve more traditional norms, more distance, less identification, and more of the universalism that Merton ascribed to science. <sup>98</sup>

Given the resilience of beliefs and pre-conceived notions, scientists who are committed to their assumptions, whether they are publishing papers or refereeing them, may have the most to gain in real understanding when they are also able to risk those assumptions. In his popular writing, Richard Feynman described this as scientific integrity, "a principle of scientific thought that corresponds to a kind of utter honesty -- a kind of leaning over backwards... to try to give *all* the information to help others to judge the value of your contribution; not just the information that leads to judgment in one particular direction or another." <sup>99</sup>

Feynman described the "A-number one experiment" as one "that tells exactly what conditions you have to use in order to be careful and control everything" in another experiment. He wished his readers the "good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organization, or financial support, or so on, to lose your integrity." <sup>100</sup>

### ***Peer review as a social process***

Scientific integrity assumes even more importance in light of the social forces which impinge upon it. Campbell wrote in 1979 that the social nature of inquiry had been most widely popularized by Thomas Kuhn. <sup>101</sup> Kuhn wrote about "incommensurable ways of seeing the world and of practicing science in it." Beyond observation and experience, there is "an apparently arbitrary element, compounded of personal and historical accident, [which] is always a formative ingredient of the beliefs espoused by a given scientific community at a given time." <sup>102</sup> Such arbitrary elements do not detract from the scientific nature of thought, but are

"embedded in the educational initiation that prepares and licenses the student for professional practice. Because that education is both rigorous and rigid, these answers come to exert a deep hold on the scientific mind ... and account for the peculiar efficiency of the normal research activity and for the direction in which it proceeds at any given time."

Kuhn has described research as a "strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education."

"Normal science ... is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost. Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments." <sup>103</sup>

Campbell, writing about the social sciences, has aimed at dispelling the more radical images that became associated with this line of thought. He asserted that the sciences are not "self-deceiving social systems incapable of distinguishing truth from tribal myth." At the same time he portrayed the social science community as having a tribal character -- needing to maintain itself as a vehicle of knowledge even if such maintenance interfered with the scientific mission.

"Before a scientific community can be a self-perpetuating social vehicle for ever-improving a set of beliefs about the physical world, it must first meet the social structural requirements of being a self-perpetuating social system. The requirements of achieving this 'tribal' continuity come first, even if they compete and interfere with the cognitive task of increasing the validity of the image of the physical world carried by the 'tribe.'"

Campbell points out that the scientific community must recruit and find jobs for members, and publish journals. He added, "The requirements of leadership for coordination and continuity may produce leaders whose decision-making power is used to protect their own social positions and their own scientific beliefs against internal challenge from young rivals." Continuing, Campbell describes the occasional necessity for scientific communities to mobilize "hostility and disgust toward outgroups":

"The deeply ingrained social custom of building ingroup loyalty by mobilizing hostility and disgust toward outgroups may be employed as a convenience (and perhaps even occasionally as a necessity) in maintaining group cohesion and continuity. Without meeting these social-structural requirements, there can be no scientific community to serve as the vessel carrying scientific knowledge." <sup>104</sup>

Campbell has defined the scientific outlook that must be evident if the scientific mission is to be maintained:

"I already know enough to insist that the experiment is a ritual of the first type, meticulously designed to put questions to 'Nature Itself' in such a way that neither questioners nor their colleagues nor their superiors can affect the answer. The supplicants set up the altar, pray reverently for the outcome they want, but do not control the outcome... The experimenter provides the galvanometer needle with its limited degrees of freedom to move, its presumptive construction and interpretive framework, but one is careful to arrange things so that one's wishes and expectations do not further control the needle's movement, do not dictate the meter reading. A narrow window has been provided through which 'Nature' can speak, free from the scientist's control. The brilliant historians and theorists of recent years have convinced me that the galvanometer reading is not at all the 'solid fact that speaks for itself' we once imagined it to be. Instead, it turns out to be highly equivocal, interpretable only at the cost of many unprovable and revisable assumptions. Yet, the laboratory scientist's phenomenology is not altogether wrong: these stubborn laboratory facts are not speaking in the experimenter's own voice. Within the degrees of freedom the apparatus allows, they are out of the control of one's own hopes and wishes." <sup>105</sup>



### ***When Research Challenges "Basic Assumptions"***

In writing about paradigm collisions and shifts, Kuhn identified characteristics of the problem of translation of one model to another. He described paradigms in two different ways.

"On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science."<sup>106</sup>

In asking questions about how truth and utility are examined relative to paradigms which are incommensurable, i.e., which have "no common quality upon which to make a comparison," one can begin to understand why scientific communities resist new paradigms.<sup>107</sup> Their examination of the truthfulness and usefulness of the claims may be influenced by the fact that concepts, language, and standards of measurement are not the same. If the competing model begins to gain ground among scientists, then the emotional and social commitments described by Campbell as so integral to scientists' work may be threatened. Livelihoods and ways of thinking and behaving may also be challenged. Rather than truth or utility, the concern may be, "If this new way of approaching the problem is given life, will my old way survive too?"

Maila Walter documented a striking example of this kind of reaction in a biography of Williams Percy Bridgman, 1946 Nobel Prize winning physicist and Harvard professor. Bridgman could not accept the challenge to classical Newtonian physics that Albert Einstein's special and general theories of relativity and then quantum mechanics posed. Walter described an experimental scientist with an "experienced subjectivity, an exquisitely felt self-consciousness" encountering empirical facts that supported a theoretical view that he could not accept. Einstein's theory had overturned the very conception of measurement in which Bridgman excelled.

Seeking "something stable, an island of permanence, in the midst of the change that was so rapidly transforming physics," Bridgman "set out to show where physics had gone wrong and to indicate how such error could be avoided in the future."<sup>108</sup> He invented operationism as a means of rescuing traditional measurement and of saving science from metaphysics. He attempted to apply Einstein's theory, though he misunderstood it. His analysis, according to Walter, was therefore more destructive than helpful.<sup>109</sup>

The Bridgman example is illustrative for this dissertation because it is relatively modern and involves the confrontation of metaphysics by a scientist who was quintessentially mechanistic in his outlook. Bridgman's response indicated how metaphysics itself -- meaning any overarching authoritative principle -- had become abhorrent to the empiricists of his day. Bridgman wanted to "purify science: to weed out what people thought were metaphysical contaminations."

Walter explains the discomfort that many people feel with metaphysics as something "vague and imaginary, which it is not. Metaphysical principles structure our knowledge, give it some unity and coherence." Bridgman was coping with a meta-physics at variance with his own, though he would deny having a metaphysical point of view. Bridgman's metaphysics was inherited from a tradition begun by Galileo.

Bridgman's distrust of metaphysics was natural and almost automatic for an empiricist who had inherited the mechanistic point of view. Since Galileo and Descartes, scientists have made strenuous efforts to distance their observations from the orthodoxy of religion, which at one time dominated most thinking. For Bridgman, disdain for metaphysics was anti-religious and also anti-authority, i.e., any authority outside of himself. Walter described Bridgman's struggles with challenges to the Newtonian physics he had mastered as a moral struggle concerned with his own perceptions about his relationship to the universe.

Like Bridgman, modern scientists might consider anything *not mechanistic, not scientific*. Science historians and philosophers -- including Edwin Arthur Burt, Hugh Kearney and Mary Hesse -- have pointed out, however, that the paradigms overthrown by the mechanists also gave rise to great scientific discovery. Newton, whose theories were more comfortable and familiar to Bridgman, was not a mechanist in his metaphysical orientation but a Neo-platonist who, as Hugh Kearney wrote, "turned to the half-forgotten, out of date, mystical scientist of the court of Rudolph II [Johannes Kepler]. He took over Kepler's three laws and using Galileo's law of falling bodies as his measure, he described mathematically the law of gravity in its application to the whole cosmos." Newton had "swept his predecessors into oblivion," not because of a mechanistic world view, but because of his keen observation and synthesis. Kearney described the hostile reception that his work initially received because it assumed 'action at a distance.'

"But the *Principia* did not receive the welcome which we might expect. The Aristotelian reaction was predictably hostile, and so was the judgment of the Cartesians. The Cartesians, now a power to be reckoned with in Holland and in France, dismissed Newton's thesis on the grounds that it rested upon the assumption of 'action at a distance,' in short, occult forces. Thirty years after the publication of the *Principia* Leibniz attacked it, saying 'that what has happened in poetry, happens also in the philosophical world. People are grown weary of rational romances... and they are become fond again of the tales of fairies!'" <sup>110</sup>

Kearney described Newton as able to make the great leap that he did because he "was in short a second Kepler, whose scientific insights derived from his beliefs about the world as whole. The *Principia* (1687) was not a detached piece of research. It was part of a religious and historical synthesis, the work of a great system builder" who could bridge the ancient and the modern metaphysics. <sup>111</sup> Kearney pointed out that the great insights of science sprang as equally from the Platonic (magical) tradition as from the Aristotelian (organic) orthodoxy and from the mechanists, who are generally believed to have won the battle between the three approaches.

### ***Demarcation of the Line Between Science and Non-Science***

Since Newton, scientists have achieved legitimacy partly by distinguishing their work strategically and publicly from religion. The distinctions that scientists have drawn, according to Thomas Gieryn, are sometimes reflective of the nature of science and the "strains" within the profession to achieve multiple goals. He has pointed out that demarcating scientific boundaries is a highly practical and ideological means of securing "intellectual authority and career opportunities," denying those resources to unwanted contenders, and protecting science from political infringement. Based on his examination of three historical examples, Gieryn concluded:

"If 'strains' *enable* alternative repertoires [within science], 'interests' *guide the selection* of one or another repertoire for public presentation. Ideologists are able to endow science with just those characteristics needed to achieve professional and institutional goals, and to change these attributed characteristics as circumstances warrant." <sup>112</sup>

Gieryn has argued that demarcation of the line between science and non-science is "routinely accomplished in practical, everyday settings," and that "because of considerable material opportunities and professional advantages available only to 'scientists,' it is no mere academic matter to decide who is doing science and who is not." <sup>113</sup> In discussing how scientists and others decide who is and who is not doing science, Gieryn's concern with the intellectual authority of science prompted him to raise the question, "What images of science do scientists present to promote their authority over designated domains of knowledge?" <sup>114</sup>

Larry Laudan surveyed the history of science since Aristotle, including Popper, for whom the question of demarcation was the central question of epistemology. Laudan concluded that distinguishing science from non-science is meaningless. As the history and philosophical premises of science have changed, standards for "partitioning science from non-science" have proven inadequate and insufficiently precise. It is more important, according to Laudan, to ask, "What makes a belief well founded (or heuristically fertile)?" instead of "what makes a belief scientific?"

"The first set of questions is philosophically interesting and possibly even tractable; the second question is both uninteresting and, judging by its checkered past, intractable. If we would stand up and be counted on the side of reason, we ought to drop terms like 'pseudo-science' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us. As such, they are more suited to the rhetoric of politicians ... than to that of empirical researchers." <sup>115</sup>

Laudan recognized that "demarcation criteria are typically used as *machines de guerre* in a polemical battle between rival camps":

"Indeed, many of those most closely associated with the demarcation issue have evidently had hidden (and sometimes not so hidden) agendas of various sorts. It is well known, for instance, that Aristotle was concerned to embarrass the practitioners of Hippocratic

medicine; and it is notorious that the logical positivists wanted to repudiate metaphysics and that Popper was out to 'get' Marx and Freud. In every case, they used a demarcation criterion of their own devising as the discrediting device." <sup>116</sup>

Gieryn wrote that inquiries like Laudan's were ironic, considering the practical demarcation decisions that are made daily in many professional contexts. Laudan, however, has argued that real consequences make careful distinctions even more important:

"Precisely because a demarcation criterion will typically assert the epistemic superiority of science over non-science, the formulation of such a criterion will result in the sorting of beliefs into such categories as 'sound' and 'unsound', 'respectable' and 'cranky', or 'reasonable' and 'unreasonable'. Philosophers should not shirk from the formulation of a demarcation criterion merely because it has these judgmental implications ... Precisely because a demarcation criterion will serve as a rationale for taking a number of *practical* actions which may well have far-reaching moral, social and economic consequences, it would be wise to insist that the arguments in favor of any demarcation criterion we intend to take seriously should be especially compelling." <sup>117</sup>

At the same time that Laudan delivered a forceful argument that "demarcation between science and non-science is a pseudo-problem," he referred to a widely-shared tacit understanding about pseudoscience. In referring to the "shared but largely implicit sorting mechanisms whereby most of us can agree about paradigmatic cases of the scientific and the non-scientific," he estimated that "there is a large measure of agreement at this paradigmatic level, even allowing for the existence of plenty of controversial problem cases." <sup>118</sup> On the one hand, he protested that he was only interested in making the criteria involved in such sorting mechanisms more explicit. On the other, he suggested a set of alternative questions, which he said were far from answered and which when answered would further diminish the importance of demarcation questions: "When is a claim well confirmed? When can we regard a theory as well tested? What characterizes cognitive progress?" <sup>119</sup>

As with peer review generally, discussion of what is and is not scientific involves estimations about scientific standards, and discrimination regarding the responsibility of nurturing innovation thinking. Scientific boundary setting is a social activity which some scholars have identified as inherently rhetorical. When individual scientists engage in discourse about a disputed aspect of science, Trevor Pinch noted, the definition of science "comes out into the open." <sup>120</sup>

Charles Alan Taylor described such an instance: the discourse within the community of scientists following the announcement by Martin Fleischmann and Stanley Pons concerning cold fusion, on March 23, 1989, including the efforts to explain away the errors made by Fleischmann and Pons, and the community's effort to make it clear that the two were not behaving like scientists - thereby placing the cold fusion findings "outside the domain of even *potential* scientific credibility" and thus protecting the "accumulated wisdom of fusion research." <sup>121</sup> Dale Sullivan has examined the rhetorical "excommunication" of Fleischmann and Pons by the nationally televised *NOVA* program. He outlined *NOVA*'s efforts to educate and entertain a mass audience while

reinforcing orthodox views, "creating a sense of communion" and "closing off discussion of [the] issue ... once and for all." <sup>122</sup> Sullivan contended that the *NOVA* program was "an instance of what Gieryn and Figert call a 'de-legitimation' and what Harold Garfinkel called a 'status degradation ceremony.'"

This form of rhetoric was first described by Aristotle as *epideictic*. Gregory Clark wrote that rhetoric was presented by both Plato and Aristotle as

"a dialogical process in which interested people interact through discourse to describe, test, and define what they believe, value, or intend to do as a community." <sup>123</sup>

Rhetoric for both Plato and Aristotle was "knowledge, developed through discourse, of what is true, or best, for a community." <sup>124</sup> Although both conceived of an ideal toward which the community was progressing, rhetoric to them involved dialogue, not preaching. According to Clark, rhetoric meant "sharable, contingent knowledge regarding practical action through a loose process of public investigation." <sup>125</sup>

Clark described Aristotle's rhetoric as incorporating Plato's concern for progress and extending it with a "description of the process by which communities define and redefine consensus." Aristotle's rhetoric was concerned with how members of a specialized community, for example, put issues and arguments before other members "in the full context of their collective knowledge, assumptions, and values for the purpose of enabling their informed [and moral] choice." <sup>126</sup> According to Clark, rhetoric generally is "essentially communitarian," whereas the epideictic, one of Aristotle's three types of rhetoric is "essentially authoritarian," because "it attempts to subvert the dialectical process." <sup>127</sup>

Aristotle, in distinguishing epideictic from judicial and deliberative rhetoric, described it as a means of praising and urging action. As a means of evoking shared values and inspiring, the epideictic has been considered by some as prior and fundamental to other forms of discourse. <sup>128</sup> The epideictic form is also used for disparagement and is often defensive, as Chaim Perelman wrote, involving a speaker or writer who "may be likened to the guardian of dikes under constant assault by the ocean." <sup>129</sup> Clark wrote,

"By presenting values that are collectively held but suppressed or nearly forgotten, epideictic discourse leads a community to revitalize its commitment to those values, thus modifying the direction of its future deliberation and action." <sup>130</sup>

As a rhetorical form evoking past values and abstract convictions in order to influence future action, Clark described the epideictic as "subtly subversive," especially relative to proposed action or truths which don't resonate with those values and convictions. <sup>131</sup> Bernard Duffy wrote that the epideictic "cannot take the place of dialectic as a means of arriving at truth, but it can be implemented to shape an appearance of truth that will be persuasive." <sup>132</sup> In contrast to the

communitarian rhetoric of dialectic normally associated with modern science, the epideictic is used to assert authority and to persuade without dialogue, and "to protect orthodoxies."<sup>133</sup>

Sullivan focused on the epideictic characteristics of the *NOVA* program's ceremonial excommunication of Fleischmann and Pons relative to boundaries that they ostensibly violated between science and public policy, for which their discovery would have had great ramifications; between orthodox and deviant science; and, because they announced their discovery before publishing in a refereed journal, between "internal scientific discourse and the popular media, a boundary described by [Stephen] Hilgartner as lying between genuine and popularized science."<sup>134</sup>

### ***C. How Newspaper Reporters Approach Social Science Research***

For many policy makers an important forum for evaluation of social science research, and one of the only ones which they have time to consider, is the newspaper.<sup>135</sup> As Weiss points out, even those with research staffs and access to specialized information networks rely on newspaper accounts for social science news.<sup>136</sup>

Fay Lomax Cook, et al. found in their study of investigative reporting and "its immediate and delayed impact" on the general public and relevant decision makers, that reporting did influence decision makers' "perceptions of the importance of the issue." They also found that the news medias' affect on policy agendas did not occur because of the airing of the story, but because of "the ongoing collaboration between journalists and government staff members."<sup>137</sup> John W. Kingdon found that news media influenced the U.S. policy agenda far less than anticipated, due in part to the press's tendency to be "responsive to issues that are being aired," and "to cover a story prominently for a short period of time and then turn to the next story, diluting its impact."<sup>138</sup>

Kingdon also confirmed what many others have observed, however, that U.S. policy makers rely on the *Washington Post* and the *New York Times* as forums for quick communication with other policy makers.<sup>139</sup> In the State Department, for example, the *New York Times* is nicknamed "Pravda," and considered vital reading.<sup>140</sup> William Rivers describes an instance where, because of their privileged access to government, *New York Times*' reporters were able to give Kennedy details he had not had about the impending Bay of Pigs invasion.<sup>141</sup>

Policy makers also turn to the media as a means of coping with information overload. Kingdon quoted a Congressional staff analyst:

"Congressmen and senators read the mass media. The big problem on the Hill is the oversupply of information. They have no way of dealing with it. So they don't, mostly. We can write reports and papers and they don't read it. But if the *Times* or *Post* picks up our report and does a story on it, they do read that, and it gets their attention."<sup>142</sup>

Because their descriptions of social science information garner the attention that they do, *how* reporters and editors *use* social science information, including how they decide whether it is truthful and useful, assumes a gatekeeper level of importance. Understanding reporters' and editors'

use becomes even more important considering increases in social science reporting in recent years<sup>143</sup> and the permeation of policy discussion with social science language, concepts, and data.<sup>144</sup> Scholars have judged the relationship between reporters and scientists to be significant enough to assist them in setting aside stereotyped images about their differences and to encourage scientists to work productively with reporters.<sup>145</sup>

Carol Weiss and Eleanor Singer observed reporters' use of social science from several vantage points. They analyzed the content of social science stories "that appeared in ten major media over a five month period in 1982," and interviewed journalists and social scientists involved with a subset of the stories.<sup>146</sup> They also monitored the social science input to the *Boston Globe* and the Boston bureaus of *Newsweek*, *Time*, the Associated Press, and United Press International, in the summer of 1983. They found that of 90 social science inputs in the form of mail, phone calls, press releases, etc. that flowed into the five news outlets that summer, three were used. In that instance and in others, Weiss and Singer were struck by the "considerable latitude that reporters have for selection, ... the newsmaking potential that reporters exercise, [and] the influence of their news sense and their particular interests."<sup>147</sup>

Weiss and Singer paid particular attention to the ways in which reporters filtered the constant confetti of paper that flowed to them, noting that reporters' distinctive purposes, methods and constraints mean that reporters' filtering of research is very different from that of social scientists. Reporters may fragment, over-simplify, over dramatize, and ascribe "too much certainty to provisional findings." They may also be "insufficiently critical toward social science authority." Such tendencies arise, wrote Weiss, from "the structure and values of newswork."<sup>148</sup>

The main consideration in sorting incoming materials for reporters was topic, i.e., topics "important in the larger world."<sup>149</sup> This finding supports a 1969 assertion by *Sunday Times* (London) investigative reporter Nicholas Tomalin that the journalist's "required talent -- is the creation of interest,"<sup>150</sup> meaning interest for large numbers of people.<sup>151</sup> Weiss and Singer's interviews with 127 reporters who had written "big" social science stories within the five-month time frame revealed that the selection criteria followed the news and interests, or issues of importance, controversy or novelty.

Research quality was not particularly important to reporters. Weiss and Singer observed that reporters were not influenced by aspects of a social science study such as the mode of research, reputation of scientists, or the institution's reputation. They found that four of 127 mentioned quality "as their first explanation for why a study was worth reporting" and only eleven gave quality as a secondary reason. One out of 30 editors interviewed considered quality important. Reporters did not judge themselves as competent to evaluate research, but typically invited comment from

other social scientists, especially when the study was controversial. Weiss noted that reporters tended to seek out "establishment types with appropriate academic credentials."<sup>152</sup>

Robert McCall and S. Holly Stocking have written that reporters who have time "often make special efforts to check on a source's credibility." But they are generally more concerned with reliability of the sources, rather than validity of the news item. Fact checker Pamela Ridder wrote that a "checker's job, then, is not a quest for truth, but a quest for substantiation."<sup>153</sup>

Concern for substantiation arises in part from the reporters' approaches, which involve deadlines of hours instead of days. Because reporters are relying on a set of telephone interviews, they are concerned with not being co-opted. Louis Heren wrote, "As a young reporter I had been advised to ask myself 'Why are these lying bastards lying to me?,' advice that had stood me in good stead in many capital cities of the world."<sup>154</sup> Bernard Redmont, another foreign correspondent, wrote that he approached "most stories with more than a grain of skepticism" and passed on to his students "that old aphorism, 'If your mother says she loves you, check it out!'"<sup>155</sup>

Weiss and Singer have described reporters as engaged more with fairness and balance than validity. When reporters are faced with conflicting conclusions they opt for one of several neutral, hands-off approaches: ignoring previous controversies or statements; presenting a balanced view -- meaning both or several sides without interpretation; or "much less frequently ...making controversy itself the focus of the story." They rarely, as social scientists might wish, take a more "active -- and critical -- role ... explaining reasons for discrepant research results and conflicting forecasts" or explaining differences between assumptions.<sup>156</sup> Weiss and Singer have called for more "attention to the assumptions and definitions that underlie conflicting research findings." They recognized that ultimate decisions should not be left with journalists who generally do not have the training to make them, but they pointed to the level of responsibility that journalists assume in their decisions about what is news and how to report it.

Among the tendencies that most influence the reporting of social science, Weiss and Singer found the most pervasive to be "the necessity for fashioning it into a 'story' that fits journalistic story-telling conventions."<sup>157</sup> Reporters seek to correct social scientists' inclination to write "at such a high level of abstraction that [their reports] retain little sense of living people and human institutions." The reporter wants to write about just what the social scientists covers up -- what Merton describes as the messy realities of investigating. And the reporter seeks to downplay what the social scientist tends to "exaggerate" -- "the rational aspects of the work."<sup>158</sup> Since 1937, reporters have been pointing out that they are more comfortable with the "concrete rather than the abstract ... interpreting current affairs... in the context of contemporary trends rather than against the configuration of historical movements or within a frame of depersonalized reference." The aim is for more realism. In fact, wrote Rosten, they must "etch" the personalities they must emphasize



"into sharp stereotypes which the reader can find analogous to the ordinary types of his own experience." In the process they affix labels that "will stand for good."<sup>159</sup>

Weiss described "the traditional repertoire of journalistic forms."<sup>160</sup> She cited Robert Darnton, who described the manipulation of "standardized images, cliches, 'angles,' 'slants,' and scenarios, which call forth a conventional response in the minds of editors and readers." He wrote that the forms of news stories are dictated by "cultural preconceptions of news... probably derived from ancient oral [story telling] traditions."<sup>161</sup>

One of Weiss and Singers' respondents said, "Surprisingly, newspaper people have trouble understanding what you say unless it fits their template." The ten templates for reporting social science that Weiss and Singer found involved

- 1) the reporter's requirement for a timely news peg ;
- 2) association of the story with an event real or conjured up;
- 3) cyclical events;
- 4) use of vignettes from real life to make use of human interest;
- 5) focus of studies being reported in terms of their human interest;
- 6) emphasis on outstanding features of the social science, making "a news peg of firstness and mostness";
- 7) favoring 'man bites dog' type of stories
- 8) conflict( being "the archetypical journalistic value", according to Weiss);
- 9) focus on personalities (although Weiss and Singer did not observe this use);
- 10) "making sense of a complex phenomenon" within the context of a feature.<sup>162</sup>

Weiss pointed out that the feature story is a better forum than a news story in the sense that it allows for the commitment of time and resources to monitor social science more regularly and to make use of it in reporting. She wrote that well done reporting of social science tends to create "informative interpretation" and to "broaden the range of evidence and ideas" to a larger audience. Poor reporting, on the other hand, can misinterpret and distort "social science evidence to suit the case" being made, or use social science as "window dressing."<sup>163</sup>

### ***Diplomatic and Foreign Correspondents***

Among the beats that reporters are assigned, the diplomatic or foreign are the most coveted and are awarded for distinguished service elsewhere.<sup>164</sup> Like most beats, diplomatic and foreign beats are large, demanding a lot of attention and infrequently bringing reporters into contact with social science. The purpose of foreign and diplomatic reporting, like reporting generally, has been, as Weiss observed, to "focus on events, power, controversy, and important people."<sup>165</sup> Since the end of the Cold War, however, *New York Times* foreign editor Bernard Gwertzman has written that while journalists still aim at being "first with the most important news around the world" they have widened their nets considerably. He explained, "Some of the traditional political stories of the past may not resonate as they did before." In a memo to the foreign staff of the *Times*, he also counseled,

"One should look to the foreign report as a forum for all kinds of stories. But the emphasis should be on an interesting mix. It is not enough just to cover elections in country X without providing a reader with a sense of what country X is about and why that election is important, if it is. That is what makes the work so much more challenging. In the old days, when certain countries were pawns in the Cold War, their political orientation alone was reason enough for covering them. Now with their political orientation not quite as important, we don't want to forget them, but we have an opportunity to examine the different aspects of a society more fully."<sup>166</sup>

Because journalistic practices that were in retrospect influenced by the Cold War continue to have influence, studies of them also continue to be salient. David H. Weaver and G. Cleveland Wilhoit, in their study of "the amount, location, and kind of international news" being carried by the four dominant and largest news agencies on sampled days of the spring of 1979, indicated that the main topic dominating Western international news reporting was politics and the main actors in the news were politicians.

They also found that Western news agencies did not report "much about social and economic development as compared to political and military events and that "stories from less-developed countries are significantly more likely than are stories from more-developed countries to be about diplomatic/political activity between states, internal conflict or crisis, armed conflict or the threat of it, military aid, and political crime."<sup>167</sup> Weaver and Wilhoit observed that "even in the more-developed countries, the bulk of the wire service stories we analyzed concentrated on political and military activity and crime. Economic matters, international aid efforts (except for military aid), and social service, culture, scientific and medical achievements, and ecological issues such as energy and pollution were all but neglected in the coverage of both less-developed and more developed countries of the world in favor of the more 'official' news from governmental and military authorities."<sup>168</sup>

Thomas J. Ahern, Jr., writing in the same volume, found that "at the primary news gathering level the universe of events is not a worldwide panorama to be passively scanned, but rather a limited affair brought into being by the assignment of a small number of correspondents to a few overseas locations." Decisions about deployment of correspondents are predicted, according to Ahern, by GNP and trade and political relations with the U.S., but also by "events of extraordinary intrinsic newsworthiness."<sup>169</sup>

David Paletz and Robert Entman summarized the conventional constraints on foreign reporting that contribute to perceptions that foreign coverage is "too brief, too simple, frequently distorted, and often misleading." Constraints are unfamiliarity with culture and language, budgets for limited numbers of reporters to cover vast territories, and "the limited foreign news slot" which is attributed to limited public interest.<sup>170</sup> Paletz and Entman made the case that under these conditions foreign reporters share the same "sources, vantage point, and language" of U.S. policy

makers. They described, for example, 1977 reports of direct, extensive, and secret relationships between reporters, editors, and publishers and the CIA. Paletz and Entman wrote that the sources are main stream leaders, the vantage point is the state's, and the "ways stories are told" are derived from sources, from their vantage points.<sup>171</sup>

Edward Herman wrote that mainstream media are "supportive of government policy and vulnerable to governmental news management" in unevenly covering human rights abuses, government crack downs, airliner shoot downs, Third world elections, and conflicts like the Persian Gulf War. Herman's position is that the media "are themselves members of the corporate elite establishment." Scholars cited by Herman, such as Leon Sigal, describe the reliance of reporters on officials in Washington as arising from beat specialization and routines for news gathering which can easily devolve into dependence and sloppiness. Darnton, who worked for the *New York Times* London Bureau from 1963-64, described such a routine. He wrote that the Foreign Office, in providing an official statement, background analysis and "off-the-record explanation," packaged it so carefully that "it was difficult to unwrap it and put it together in another way" so that "diplomatic stories all sounded very much alike."<sup>172</sup>

Sigal, writing in 1973, cited James McCartney's 1968 conclusion that

"[T]he result in extreme form is that reporters become spokesmen for their news sources rather than dispassionate observers. They become sloppy about recognizing that alternative views may exist and about digging out and including alternative views in their stories. Over a period of time some may well be press agents for those they are covering and, indeed, sometimes perform that role, or something very close to it."<sup>173</sup>

In 1986 Stephen Hess related that reporters working within the "Golden Triangle" between the President, Secretary of State, and the Pentagon are admitted by invitation and may happily "allow themselves to be participants in government." As such they are the most frequent source of insider stories as well as "the envy of lesser journalists."<sup>174</sup>

Foreign reporters assess their position in the 1990s in light of the dramatically changed conditions -- the fall of the Berlin Wall and end of the Cold War. However, they have described similar constraints. William D. Montalbano, Rome Bureau chief for the *Los Angeles Times* wrote that the "correspondent-as-interpreter is more valuable than ever," but that there are more countries to interpret which are "almost generically harder to understand and explain as they wrestle with new economics amid political and cultural uncertainty." He described a "largely apathetic audience," limited resources, danger, and with "fewer correspondents and fewer foreign bureaus," "more reliance on Washington bureaus means more reliance on government and greater opportunity for government to influence both the news agenda and its content."<sup>175</sup>

***Middle East Coverage Dominated by the Israeli-Palestinian Story Line***

Jim Lederman's study of the reporting of the Palestinian uprising known as the Intifada (Arabic word for shake or shake off) which occurred most actively between December 1987 and December 1988 has provided a fascinating history of the evolution of the press corps there. The events Lederman described -- because of the way they unfolded -- entailed paradigmatic-level struggles not always available within an analytic description such as Lederman has provided.<sup>176</sup>

For a number of reasons, including unfamiliarity with the culture and lack of adequate resources to monitor more deeply what was going on in the region, reporters missed this story. Lederman explained that they had confined themselves to a story line that misled them. In relating the details of his study, Lederman provided insight into the added constraints on foreign reporters writing about conflict and a context for understanding how and why they make choices about what fits the story line and what doesn't and why they rarely question the story line.

Lederman interviewed journalists, but based his analysis on a review of tapes of 800 nightly newscasts by the three primary U.S. television networks, 2000 dispatches from the Associated Press and 1500 reports from the *New York Times*, *Washington Post*, and *Los Angeles Times*, and, for comparison, 1000 articles in the Israeli press. He also used his contemporaneous notes and radio broadcasts that he had done for National Public Radio.

He described himself as maturing as a reporter in the region at the same time as the foreign press corps matured generally. At the time of the Intifada he noted that reporters were "operating [technically speaking] under best-case scenario circumstances," i.e., within full time bureaus with "excellent communication facilities," in a country and region with fairly short distances to cover, and with an "established, active, and relatively free press" on which to lean, with well-established support services.<sup>177</sup>

Even so, foreign correspondents could not emotionally relate to people struggling with collective identity. Lederman wrote, "We cannot conceive of the fears and even terrors provoked by tribal and ethnic competition within a single nation-state or territory, and thus shy away from trying to relate to it in depth."<sup>178</sup> The story line becomes a shield against the reporter's personal inadequacies and its addiction is hard to break because it requires asking

"dumb questions and then actually following up on the questions to see if they have real answers. It is not just a matter of questioning the facts being presented... It is a matter of dissecting the assumptions that are being used to tie the facts together. If the resulting pathology report contradicts conventional wisdom, the journalist has to have the guts to say aloud 'The emperor has no clothes' -- not easy when one is up against a powerful editor and the expenses involved in sending the kids through college."<sup>179</sup>

The Arab-Israeli conflict was a major international story for two decades following the Six-Day War in 1967 when Israel first occupied the West Bank and Gaza, after which an Arab summit in Khartoum concluded against recognizing or negotiating with Israel. The story held for the

journalist "well-defined characters, occasional novelty, a strong and dramatic story line..." that reporters could build on every day. He described the story line, much as Weiss and Darnton did, as a "frame, into which a journalist can place seemingly random events and give them coherence."

"It simplifies the narrative thread, reducing it to manageable dimensions by using a single overarching theme so that each dramatic incident can be highlighted as it occurs and each 'chapter' of the ongoing story can be slotted in easily and given a context. It gives all who use it, be we hacks, ideologues, area specialists, diplomats, or scholars, a common reference point, a set of agreed bearings from which to set out into the unknown and through which to communicate with our audiences." <sup>180</sup>

The story line was "Israeli-Arab violent conflict over possession of land." The violent conflict story line, Lederman explained, meant that otherwise non-violent stories could be "'hooked' onto the ongoing narrative. Thus a report about Christmas in Bethlehem would emphasize the security aspects rather than the religious experience..." <sup>181</sup>

Lederman also described the story line as a snare which becomes problematic because it is not questioned. This was illustrated repeatedly by the Intifada. The violent conflict story line which gained credence by mere repetition was hard to buck, especially, for example, when foreign correspondents were dealing with editors 7000 miles away.

"Once in the field, the reporter quickly learns just how difficult it is to carry on a fight over the phone with an editor who is seven thousand miles away. Groupthink is reinforced in the field at the local 'journalists' bar,' at press conferences, at parties and discussions with the local elite, and in conversations with other journalists, where they either justify what they have written or seek reassurance that what they have written is right." <sup>182</sup>

According to Lederman's analysis, the real issue in the Arab-Israeli dispute was cultural identity and the violent conflict or conflict-over-land story line provided a means of escape from the complexities of cultural identity by "reinforcing the stereotypes and the oversimplification of events," the end product of which is "conventional wisdom." Lederman wrote that the single common fault of foreign correspondents working in the Middle East was to "allow ourselves to be sucked in to support and validate these mass Middle Eastern exercises in political escapism." <sup>183</sup>

The difficulties of discarding a story line that no longer fit the reality occurred because the story was event-driven, demanding context which, on short notice, was hard to come by and to explain. Lederman concluded that, while many correspondents got individual stories right they "misrepresented the Palestinian uprising in a major way because the frame itself was no longer valid." The Intifada was

"not a clash between factions or classes or religions, places where journalists had been taught in the past to look for causes of violence in the Middle East. It was between generations within the same class, or the same religion." <sup>184</sup>

Reporters were not only caught off-guard by the Palestinian uprising. Because of their adherence to the conflict-over-land story line, they also failed to air the social issues which exploded

into and fueled the uprising. Adherence to the story line occurred in part because of reporters' ignorance of the culture and consequent emphasis on the

"dramatic and the nonverbal aspect of events, and weak, one-sided analysis based on the ruminations and often baseless or self-centered prophecies of the elites rather than on the thoughts and concerns of real, powerful, but English-mute constituencies." <sup>185</sup>

Without depth of knowledge of the culture, reporters may engage in "displacement journalism," meaning portrayal of events using a more familiar context or lens, which may not be accurate. Lederman stated that despite appearances,

"Israel is not a Western country ... When outside journalists come to examine the society with the preconceptions inherent in displacement, they often are confused, disappointed, and even disoriented." <sup>186</sup>

Lederman distinguished between the kind of information that is demanded by the story line and the frenzy of activity required to pursue it and the kind of information needed for a more penetrating or global perspective -- which he described as likely to arise without questions having even been asked after many tedious hours and shared cups of coffee in the byways of the culture.

*New York Times* reporter Thomas L. Friedman, wrote insightfully about the Middle East reporters' struggle with story lines. He conveyed how particularly gripping the Middle East conflict was for the reporter.

"After spending nearly five years in Beirut, I eventually developed the imagination the city demanded. I came to think of Beirut as a huge abyss, the darkest corner of human behavior, an urban jungle where not even the law of the jungle applied. Experiencing such an abyss not only left scars but also new *muscles*. Life can no longer deal you many surprises or shocks after you've lived in Beirut. The experience leaves you wearing an emotional bullet-proof vest." <sup>187</sup>

Friedman helped his readers to imagine tribal wrath, conditions that were always simmering underneath "constant random violence." An aspect of the violence was the physical intimidation.<sup>188</sup> Another was dealing with government censorship and lies. And a third was giving up some of one's optimism.

Friedman penned his reactions to the signing by Yitzhak Rabin and Yasir Arafat on September 13, 1993 of the peace agreement between Israel and the PLO negotiated in Oslo. Friedman wrote that after 15 years of reporting from the Middle East, it had "seemed as if these people would never -- ever" be ready to "make the fundamental compromises that might make peace possible." He explained that he did not "come by pessimism naturally."

"I was from Minnesota, where America's innate optimism seems most acute. But Beirut and Jerusalem leached away my native optimism. I don't know if it was the bombing of the Marine barracks in Beirut or the blowing up of my own apartment with my driver's family inside or the enraged Palestinian who once tried to throw a stone through my windshield in Jerusalem, mistaking me for an Israeli. It's hard to locate precisely the wellspring of one's

pessimism. Just too many funerals, I guess. Too many sad endings. Too much Hafez el-Assad, not enough Hubert Humphrey."

He remembered observing a Palestinian boy, "wearing a red shirt and shorts and sitting on a small stool," watching as Red Cross workers at the Shatila refugee camp "collected the bodies of Palestinian civilians massacred by Christian militiamen" and carried them one by one into a trench, "pouring white lime over each layer." Watching the crying boy he thought,

"No one, let alone a child, should ever have to watch something this wretched. But I also thought: So this is how it gets started. One generation watches another go to some horrible death, planting the seeds of rage in a new generation. I wonder where that boy is today. I wonder what he thinks of the peace agreement. He would be about 20 now. Guerrilla age."

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In his book, Friedman described his Pulitzer prize-winning reporting of that massacre at the Palestinian refugee camps of Sabra and Shatila in September, 1982 as a personal crisis. He with the other members of the foreign press corps discovered that Israeli troops sanctioned and watched the massacre of innocent civilians, ruthlessly carried out at night with the help of Israeli floodlights. Friedman wrote,

"The Israel I met on the outskirts of Beirut was not the heroic Israel I had been taught to identify with. It was an Israel that talked about 'purity of arms' to itself, but in the real world had learned to play by [the same rules as] everyone else in the neighborhood."

Boiling with anger, Friedman reported what he found, "with all the skill I could muster on exactly what happened in those camps." He wrote that he was driven by conflicting desires to "nail Begin and Sharon," whom he thought were the "true culprits" and at the same time to find some alibi for them. "Although an 'objective' journalist is not supposed to have such emotions, the truth is they made me a better reporter." Friedman describes an interview with the Israeli troop commander at a long table lined with his other commanders. Friedman banged on the table and shouted,

"How could you do this? How could you not see? How could you not know?" But what I was really saying, in a very selfish way, was 'How could you do this to *me*, you bastards? I always thought you were different. I always thought *we* were different. I'm the only Jew in West Beirut. What do I tell people now? What do I tell myself?"<sup>190</sup>

The question arises, how might a reporter who has survived tribal slaughter examine social science, especially research that considers solutions to tribal wrath which seem optimistic rather than pessimistic? How does one evaluate truth or utility, when the bottom has fallen out of basic assumptions about the behavior of one's cherished religious and cultural tradition?

Lederman and Friedman suggested that the answer is determined by collective agreement between journalists, their sources, and especially U.S. policy makers. What is interpretable as also real in terms of peace depends, as Friedman pointed out in his epilogue, on the majority in the

Middle East whose pessimism runs even deeper than Friedman's. What is interpretable as real in the news media depends on readers' interest.

Both authors conjectured about the disproportionate coverage of the Middle East -- as Friedman recounted, one foreign reporter for every 6,100 Israeli inhabitants, "the equivalent of roughly 36,000 foreign correspondents suddenly descending on Washington, D.C." <sup>191</sup> Lederman concluded that the perception that "'Israel is like us' and the anticipation that it will 'find a way and do the right thing'" fuels the disproportionate attention. <sup>192</sup> Friedman wondered if the reason wasn't the potency of the region in providing a lens for the West. He cited Israeli political theorist Yaron Ezrahi, who described "super stories" -- literally super story lines -- as collections of "myths, ideological constructs, tied together by an overall narrative" which "helps us to explain the world to ourselves, to determine the information we will treat as significant," and "whose experiences get interpreted and whose don't." Friedman noted that religions and ideologies are the most popular super stories. The ancient Israelites, he pointed out, are the most widely known characters in the West's oldest super story. News about Israelis is "more appealing, digestible," "intuitively familiar and relevant" as a modern extension of a very old... drama involving God and man. <sup>193</sup>

Friedman explained that the Biblical story has cachet and a place which the Palestinians on their own did not necessarily have. He was interrupted in a hospital by a Palestinian nurse who asked,

"Well, then can you tell me something? Why is it that when the Germans were killing the Jews everyone screamed, but when we are killed by Israelis, the world calls us killers?"

Friedman recognized her pain and thought, "who could blame her?" He wanted to tell her that

"the difference in treatment had nothing to do with the Israeli cause being somehow morally superior to that of the Palestinians and that it also had nothing to do with any conspiracy in the media. It had to do with the fact that the Palestinians simply are not part of the biblical super story through which the West looks at the world, and it is the super story that determines whose experiences get interpreted and whose don't, whose pain is felt and whose is ignored. That is why when it comes to winning the sympathies of the West the Palestinians can never quite compete with the Jews, no matter how hard they try and no matter how much they suffer."

Friedman implied that if everything has a place in relation to super stories, it is the job of a reporter to determine where it is, and where it will continue to be. Friedman and Lederman conveyed a healthy respect for collective identities which help to define their frames of reference.

Other factors influencing the competition for foreign policy space in the newspaper are interest and relevance to U.S. policy makers. These are reflected in Lederman's outline of "unwritten and often unspoken ... Washington rules" that he contended a foreign correspondent -- and all of the other players -- must observe if their stories are to be taken seriously. Lederman's



interpretation of these rules offers a rationale for how government becomes such a powerful player in foreign policy coverage.

- 1) "Wherever possible, any foreign event must have an American angle...no matter how irrelevant," often broadening the content of a story to include comment and/or participation from an American agency, like the State Department.
- 2) "After a comment or interpretation or policy statement has been elicited, it is fed back to the person responsible for dealing with the subject for a counter comment," which invites #3.
- 3) "Each governing elite will try to take control of this flow of information and to use the flow in its own self-interest" ...
- 4) The "single dogma" among these rules: "Contact [between source and reporter] shall not be broken except under the most exceptional circumstances. To break contact is to lose one's position as a participant."
- 5) "Each party can raise almost any item for discussion if the party agrees to the basic premise that the only people with a right to participate in high-level public policy-making are narrow elites who speak to other narrow elites. Those senior reporters in Washington and editorial board members of major media outlets who agree with this premise are considered members of the elite. Gadflies need not apply."
- 6) "If there is any dispute not covered by these rules, precedents set in the application of day-to-day practical politics in Washington shall apply. For example, prevailing U.S. social and political mores are invariably the prime measure used to judge the legitimacy of other foreign regimes." <sup>194</sup>

#### ***D. The Effects of Social and Political Mores and Foreign Policy Decision Makers' Basic Premises on Their Use of Research***

The fifth rule, which, according to Lederman, governs foreign policy players as well as reporters, illustrates the extent to which the culture of the Middle East policy network is dominated by the premises of executive branch policy makers. The "right to participate" usually granted only to "narrow elites dealing with narrow elites" arises from a tacit working assumption that such individuals understand and guard the interests of the state -- an outgrowth of realist or "realpolitik" theory. Michael Joseph Smith has explained that realists have considered the state to be the only important unit of social life, and management of conflict to be achievable, according to Hans Morgenthau, only through "the workmanlike manipulation of the perennial forces that have shaped the past as they will shape the future."<sup>195</sup> Realists assume an "ineradicable tendency to evil...among all men and women," which some, like Morgenthau, also believe to be "the same everywhere and at all times."<sup>196</sup> They view states as inevitably involved in an "unending quest for power." Realists have further deduced that

"the important subjects for theoretical consideration are the permanent components of power, the historically proven methods for its control, and the evolving instruments of its

manipulation. International institutions, networks, or norms are considered significant theoretically only to the extent that they structure or affect the competition for power."<sup>197</sup>

Finally, realists have assumed that international politics can be understood and moderated through "rational analysis of competing interests defined in terms of power."<sup>198</sup> Power has been such a defining factor that realists have not generally admitted other rationales. Alexander George, referring to Kenneth Waltz, who "avoids questionable assumptions, ambiguities, and contradictions...discerned in Morgenthau's writings,"<sup>199</sup> has pointed out that structural realism, as it is also called, is more like a theory of constraints than a comprehensive theory of foreign policy.

In 1988, James Blight engaged in a spirited interchange with fellow psychologists in which he explained why their empirical efforts aimed at assisting nuclear policy makers in de-escalating nuclear rhetoric would continue to be ignored. He pointed to the incommensurability of models held by psychologists and policy makers and argued that they would remain incommensurable unless psychologists began to start from the realist premises of policy makers. Blight, who had already begun to distinguish himself in the policy community with an analysis of the 1962 Cuban missile crisis, argued that the kind of "crisis learning" he had done was the critical need that psychologists could usefully address. He argued that even "the best and brightest psychological solutions to the problem of nuclear risk are remarkably beside the policy-maker's point and thus quite unlikely to affect the policy-making process."<sup>200</sup> He exhorted psychologists to "worry" about nuclear war in "potentially policy-relevant ways."<sup>201</sup>

Blight noted that psychologists' efforts to influence nuclear policy makers had been stalled for 25 years (since the publication of Charles E. Osgood's work in 1962). He explained that nuclear policy makers were too busy to even consider data "derived from one of the most interesting and seminal programs of research in all of social psychology," referring to Morton Deutsch's work "which shows that 'nonpunitive deterrence' is superior as a strategy both to pacifism and to the sort of punitive deterrence Deutsch believed governs the superpower relationship." According to Blight, policy makers not only found psychology abstract and abstruse, but had difficulty admitting their own short-sightedness and changing their approaches.<sup>202</sup>

Blight wrote that the real hurdle for work such as Deutsch's was that it did not start with realist assumptions about how sovereign states behave, i.e., that interests, "especially competing interests and the need for trade-offs," are central.<sup>203</sup> Blight touched on the components of realist theory as he saw them when he took issue with Einstein. Einstein had written, "The unleashed power of the atom has changed everything except our modes of thinking, and thus we drift toward unparalleled catastrophe ... a new type of thinking is essential if mankind is to survive."<sup>204</sup> According to Blight, Einstein's cardinal assumption was wrong.

"Everything has *not* changed except our thinking. Significantly, the two most important determinants of our thinking about international security have not changed at all: namely, the biological drives that we inherit from our distant evolutionary ancestors and the relevant social structures contained in the nation-state system that we have evolved very largely to soften the impact of the aggressiveness and destructiveness inherent in our biology. In short, it is just not tenable to hold that our "thinking" about so basic a question as our collective security simply floats about, like some Cartesian cognitive substance, independent of our biology and culture and radically alterable via willpower. There is simply no longer any doubt that the *form* of our thinking is deeply embedded in organic structures and processes and that the *content* of our thinking is almost wholly dependent on our cultural and institutional contexts.... In short, there are deep-seated adaptive reasons why we think as we do, and these are highly resistant to change."<sup>205</sup>

Psychologist Robert Holt wrote that neither of these arguments could "be taken seriously" because no data existed to corroborate Blight's views.<sup>206</sup> Nevertheless, Blight's comments summarized the thrust of a philosophy that has dominated foreign policy makers' thinking and institutional arrangements, and revealed how severe and pervasive realist thinking had become before the Soviet Union fell. For psychologists to speak relevantly to nuclear policy makers, according to Blight, their work would have to be concerned with a "tag-team wrestling match,"

"but one in which the member of each team who stands outside the ropes, waiting a turn, has a loaded gun trained on both the action in the ring and on the other gunslinger. The two wielding the guns deter one another as the two in the ring jockey for position and advantage. Of course, neither one in the ring will want to try to push the adversary too far, for fear of causing the opponent to take desperate action that might transform the competitive match into a winnerless slaughter."<sup>207</sup>

Interpreting the situation as psychological would not alter the conditions of the match for nuclear policy makers. Survival still depended on their judgment, so they remained interested, according to Blight, only in information and research relevant to those realities.

Holt concluded that Blight's argument might better have been put thus:

"As long as national security policy is in the hands of people like Kissinger, Brzezinski, Schlesinger, Schultz, and Weinberger, the work of psychologists who do not share their world view or operational code will be considered 'policy irrelevant' and will be ignored, no matter how clear its actual implications may be for national and international policies."<sup>208</sup>

Nuclear policy makers' dismissal of research that Blight considered of high quality may have also reflected a disdain for "scientism." Morgenthau, a contemporary father of realism, had concentrated "on the inadequacy of 'scientism' as an approach to politics." Politics was art, not science.<sup>209</sup> He wrote that the true realist who did "justice to the true nature of things" was not the scientist, "who derives conclusions from postulated or empirical premises and who in the social world has either nothing but facts or nothing but theories," but the statesman, "who recognizes in the contingencies of the social world the concretizations of eternal laws."<sup>210</sup> Former Secretary of State Henry Kissinger expressed this same view, which has influenced his protégés and a

generation of diplomatic foreign service officers, analysts, and other policy makers.<sup>211</sup> Kissinger considered his own "correct 'intuition'" about a situation "to be his greatest strength"<sup>212</sup> and referring to his own study of Bismarck, considered history "the only true policy science."<sup>213</sup>

Empirical analysis of history has been important to diplomats, according to historian Alan Henrikson, because they are "men and women of the occasion." Henrikson reported that a former State Department historian told him that the typical Foreign Service Officer, who may have had to represent U.S. interests in outposts around the world, tended to consider him or herself to be a "great historian -- his or her *own* great historian,"<sup>214</sup> not tending to consult or contribute to official history, a fact which poses difficulty to projects attempting to centralize historical descriptions or their use. Henrikson noted that diplomats have also tended to be skeptical about generalizations, beyond a few instances.<sup>215</sup> They would tend to doubt, for example, generalizations in the applied field of conflict resolution, which have "attempted to draw ... upon the kind of *descriptive* data... produced to generate *prescriptive* recommendations for how the sometimes contradictory requirements of peace and security might actually be met."<sup>216</sup>

Historian John Lewis Gaddis enumerated several reasons for "disillusionment" with this kind of data, which echo the concerns about research use by policy makers addressed at the beginning of this literature review. Gaddis wrote that "too much was promised"; that the scientific approach "became preoccupied, to the point of paralysis, with debates over methodology;" and that data bases were confined to "the *public* manifestations of international relations, which often amounted to whatever appeared in the pages of *The New York Times*," whereas policy makers often discounted what appears in "open sources." Gaddis pointed out that many policy makers "believe that nothing is worth reading unless it is stamped 'top secret.'"<sup>217</sup> He suggested that conflict resolution scientists had given more attention to technique and not enough to issues and have tended to communicate with each other in jargon that "prevents them from communicating with anyone else."<sup>218</sup>

Alexander George more recently has written about how and why policy makers have tended to dismiss quantitative social science. He explained that social scientists limit the number of variables they consider, while a President must weigh them all, often at once. A statistical correlation may indicate a trend, but will not predict where the point of interest to them falls relative to the trend. A social science study "typically ignores domestic political and decision-making variables" over which the policy maker has leverage.<sup>219</sup> Policy makers need short, precise answers on short notice and they favor "actor-specific models that grasp the different internal structures and behavioral patterns of each state and leader with which they must deal."<sup>220</sup> Policy makers suspect academics of "political or ideological biases" and therefore tend to rely on studies that fit their own assumptions.

They also attend to intelligence requirements, and to question the adequacy of academics' information sources.

Finally, George cited Richard Goodwin, a former policy advisor for Kennedy and Johnson, who criticized Thomas Schelling's 1966 book, *Arms and Influence*. Goodwin argued that military policy couldn't be subjected "to systematic theory" and that generalizing from history was dangerous, "almost guaranteeing error." He argued that valid generalizations were not possible.

"And even if some sort of generalization were somehow derived, its relevance to any immediate policy problem could not be determined, since in almost any situation policymakers must act without knowing all the facts, and the variables in the situation 'are so numerous that they elude analysis.'"<sup>221</sup>

Goodwin warned, "The most profound objection to this kind of strategic theory is not its limited usefulness but its danger, for it can lead us to believe we have an understanding of events and a control over their flow which we do not have."<sup>222</sup>

Scholars have since acknowledged the difficulties inherent in relying on historical analysis. Reflecting caution expressed by fellow historians Richard Neustadt and Ernest May, Michael Fry wrote, "Learning from comparison and analogy" does not "invariably" promote understanding. "Learning in that fashion may confirm established views that are unsound and alter views that are sound. Knowing the past never inoculated anyone from error."<sup>223</sup> Gaddis also noted that historians, unlike political scientists, resist thinking about methodology and "seem determined to treat history as an 'art,'" rather than science.<sup>224</sup> He noted an "absence of comparative focus," a tendency of historians to "preoccupy themselves with the particular, and to ignore "relationships that extend across space or time" that concern political scientists. At the same time, historians have sought to improve historical data bases and their availability to policy makers.<sup>225</sup> Neustadt and May, by virtue of their own historical analysis, have developed suggestions for how history might be used.<sup>226</sup> Gaddis noted that political scientists have effectively undertaken crisis management and perception/misperception studies (such as those favored by Blight).<sup>227</sup>

Looking at policy makers' use of research from the academics' view point, George wrote of their

"hope that policymakers do not take at face value Morgenthau's dubious contentions that power is and ought to be the dominant objective of foreign policy."<sup>228</sup>

Academics are concerned that policy makers who hold such views, besides being too simple and outdated, "tend to have a fixed body of knowledge that is relatively impervious to outside influence and challenge." Furthermore he wrote:

"Academics are skeptical of claims that the intuitive judgment and experience of policy specialists suffice to ensure sound foreign policy decisions, that they need not make use of what academics refer to as theory and systematic empirical knowledge regarding the uses and limitations of the various instruments of statecraft. Academics also point out that in fact,

whether policymakers realize it or not, they do use theory, though usually in the form of a variety of implicit assumptions, beliefs, and maxims that are seldom raised to full consciousness and examined critically."<sup>229</sup>

The difficulties involved in appropriately applying history and coming to a judgment that in hindsight bears the mark of correct intuitive judgment are illustrated in a profile of Lieutenant General Brent Scowcroft, President George Bush's National Security Advisor. John B. Judis described Scowcroft as "profoundly influenced by Kissinger's view of the world and of diplomacy." Scowcroft told Judis, "What I learned from him [Kissinger] is what it is like to have a really strategic mind."<sup>230</sup>

While Judis acknowledged some of Scowcroft's notable successes, he wrote that in a number of instances Scowcroft had "fixated on a bygone balance of power" and was "unwilling to adapt his realism to the new reality of an emerging post-Cold War world." He wrote that Scowcroft "inspired or seconded some of the [Bush] administration's most dubious calls" because he mistook the present for the past.<sup>231</sup> These calls included, for example, "not anticipating the collapse of the Soviet Union ... [and] postponing the START negotiations that Ronald Reagan and George Shultz had virtually concluded," which delayed initialing of the pact by three years. The delay meant that thousands of warheads fell "into the hands of newly independent governments at odds with Boris Yeltsin's Russia." According to Judis, Scowcroft also reinforced Bush's June 1989 "determination not to allow the massacre at Tiananmen Square to disturb America's relations with China's brutal gerontocracy." Judis wrote,

"Scowcroft argued that the United States faced the same situation in 1989 that it had faced in 1971 when it initiated talks with China while the Cultural Revolution was still raging. Now as then, Scowcroft argued, the United States should base its diplomacy on the two nations' mutual interests and not on the Chinese leaders' internal practices. Scowcroft and Deputy Secretary of State Lawrence Eagleburger (another Kissinger Associates alumnus) even traveled secretly to China twice later that year to assure the Chinese leaders that the United States was not serious about the ban on high-level diplomacy that Bush had announced after the massacre.

"Scowcroft's conservative realism once again led him to mistake the present for the past. 1989 was not 1971. With the Soviet Union edging toward collapse, the United States no longer needed Chinese support against it. And where the administration needed Chinese support--stopping arms sales to unstable Third World regimes--it could not get it before or after Tiananmen Square. The only thing the United States got out of its conciliatory attitude was China's grudging abstention in the Security Council debate over Iraq's invasion of Kuwait. But the administration's acquiescence in the Beijing massacre may have contributed to Saddam Hussein's conviction that he could get away with murder in Kuwait."<sup>232</sup>

In the Persian Gulf War, Judis pointed out that Scowcroft "also took the lead in arguing that the American military not intervene after Saddam began slaughtering the Kurds and Shiites that the

administration had incited to revolt." Judis attributed these and other misjudgments to Scowcroft's "conservative realism ... stragg[ing] behind events."

George undertook a constructive effort to understand the strategic failures involved in the Persian Gulf War, in particular its aftermath. He offered "suggestions for better conceptualization of ... strategies of resocializing and reforming an outlaw state, appeasement, reassurance, and aspects of war termination."<sup>233</sup> George wrote, "Although realist theory identifies important constraints on statecraft, it provides limited guidance and very little of the knowledge required for the conduct of foreign policy."<sup>234</sup> This point was later touched on by Richard Haass, who had assisted Scowcroft as former senior director for Near East and South Asian affairs on the National Security Council and special assistant to President Bush. Haass outlined possible intellectual structures with which the U.S. might address "numerous, if limited, challenges to its interests around the world." In January/February 1995, he wrote that the "principal weakness of realism is that it pays scant attention to the internal evolution of societies." He explained,

"Realism provides no guidelines for dealing with important (if less than vital) economic, political, and humanitarian problems within states, arguably the potential source of most post-Cold War instability. Not all interests need be vital to be worthy of American protection. In part as a result of this narrowness, realism, with its emphasis on 'stability,' lacks popular appeal."<sup>235</sup>

Haass concluded that a more coherent American foreign policy would ideally involve "augmented realism -- 'realism plus'," which takes into account market reforms and promotion of democracy or human rights as appropriate.<sup>236</sup>

George also concluded that some of the strategic failures involved in the aftermath of the Persian Gulf War were due "to an imperfect understanding by policymakers of the nature and general requirements of the strategies that were employed." He saw a need to "at least bring to the attention of policymakers in the future better conceptual understandings of these strategies and better generic knowledge regarding the conditions on which their successful employment depends."<sup>237</sup>

George's effort incorporates some of the policy makers' own presumptions. He acknowledged that policy makers require many other kinds of information and ultimately must exercise their own judgment, and he outlined the forms their judgments might take. George also identified three types of knowledge that might aid foreign policy makers: "abstract conceptual models," "general knowledge of the conditions that favor the success of a strategy," and "actor-specific behavior model[s]" of adversaries.<sup>238</sup>

George sought to provide policy makers with information that resonated with their analytic and organizational repertoires.<sup>239</sup> These remained conditioned -- if not dominated -- by Cold War realism, which in Blight's words, called for a "thickly textured, hands on, complexly informed view

of the situation. Blight called such a view necessary "to affect the world of affairs." Blight explained, somewhat sarcastically,

"then one may call oneself a realist. The alternative is to stand aside from the complex crawl of daily life as it occurs, to spout solutions to whatever problem interests one, and to meet no greater need than one's need to appear brilliant."<sup>240</sup>

***E. The "Creep" of Research into Formerly Hostile Policy Making Processes: the U.S. Congress and Public Interest Groups***

Throughout his analysis of theory and practice in foreign policy, Alexander George acknowledged that policy makers might not use the store of "conditional generalizations" and "rich theory" he proposed. Despite efforts to meet the needs of policy makers more directly, the force of habit might mean that they would not seek out the research.

Although efforts such as George's to increase understanding of policy makers' needs by analysts and valuing of research by policy makers are worthwhile, actual use of research also appears to depend on a more indirect, slow, but steady accretion of social science perspective. This perspective is brought into agencies in the morning newspaper, in verbal presentations, and in concepts, approaches, and language introduced by the growing number of agency members trained in the social sciences. For example, in 1977 Pio Uliassi described the flood of information that State Department officials take in yearly. He asserted,

"commissioned studies -- and especially the face-to-face encounters associated with research -- somehow help [policy makers] to be better informed, more open to alternative ways of looking at the world, and more judicious than they would be if they remained more tightly enclosed within the intellectual confines of their own bureaucratic environment."<sup>241</sup>

The policy makers described by George and Uliassi resemble those described by Weiss in the U.S. Congress in that their research use is often severely constrained by their institutional missions. At the same time, according to Weiss, "the regularized practice of evaluation and analysis has become embedded in government structures."<sup>242</sup> Weiss explained that this has been so in Congress partly because members have distrusted executive department information and have increased the size, scope, and activity of Congressional support agencies.<sup>243</sup> Members have hired "more and better education committee staff, fewer of whom hold law degrees than in the past and more with training and experience in substantive specialties." Staff members are inundated with analyses from executive agencies and support agencies on the Hill. Support agencies also encourage staff to think in analytical terms, to submit research requests and use their data bases of outside research.<sup>244</sup> Staff thoroughly scan newspapers, which include research, and attend seminars given by professional associations and think tanks with primary emphasis on "imparting results of research that the association believes is relevant and important in the context of the legislative calendar."<sup>245</sup>



Congressional staff make use of research information because they and their members don't want to be taken by surprise or to look ill-informed because they don't have a sophisticated grasp of salient facts. Weiss reports that staff try to keep up to date on research findings to be more effective. Research lends authority that might otherwise be difficult to find. Influence or research on Capitol Hill has also increased because scholars have attempted to adapt to the realities of political decision making.

Like executive branch policy makers, however, members of Congress are less likely to use research because of the nature of Congress itself. Daniel Dreyfus wrote that this was surprising because Congress is "primarily a policy analysis mechanism." He reasoned,

"The functions of the legislature are to sense the needs of society for policy initiatives, to define and articulate the options, and to determine and assert the will of the collective social decisionmaker. These functions in the broad sense include everything that policy research can encompass."<sup>246</sup>

But Dreyfus pointed out that these functions are "done in ad hoc ways," with problems and options being "nominated by the executive, the media, and interest groups;" and selection of options "based on nebulous criteria and instinctive, almost mystical determinations of the public will." Dreyfus explained that "the whole process is frequently obscured by rhetoric designed to rationalize rather than to explain the decision."<sup>247</sup>

Dreyfus further explained that research does not fit easily within the range of Congressional tasks -- that is, not within the "incremental adjustments in bodies of existing policy" involved in "legislative oversight and policy adjustment" and not within "fundamental revisions of existing policies, programs, or activities; important reorganizations of federal agencies; and very large increases or reductions or outright terminations of ongoing activities." In considering oversight and adjustment matters, members do not have the time to consider facts that are "conveniently available," much less to seek and absorb further research. Dreyfus cited Senator James Buckley of the 93rd Congress.

"In session and out, a Senator is constantly on the run, trying to keep abreast of the most immediately pressing matters through meetings, briefings, and staff memorandums, his attention drawn in a thousand different directions.

"As for trying to do all the 'necessary reading' --the bills, reports, studies, background material; all that [is] required to develop in-depth personal understanding and knowledge -- there is only one thing to say about it and that is that it can never be done. The amount of reading necessary to keep a Senator minimally informed on matters of maximum importance is always double that which he can possibly accomplish in the time allotted."<sup>248</sup>

Dreyfus wrote attention to major national and controversial issues which are thought to be amenable to analysis are not usually open to significant debate unless "two legitimate, credible, and

reasonably well-defined options exist." Such issues will normally have received "examination by a broad spectrum of interested analysts."

"Commentators within and outside of the government will have verified and criticized the principal arguments for all of the options and viewpoints. Even the political positions will have been defined, at least in general terms."<sup>249</sup>

Dreyfus maintained that in these instances the Congressional task is to

"evaluate political strengths of the various viewpoints, to assign political measure to the intangible and otherwise unquantifiable factors that make the decision controversial, and to arrive at either a political victory or a compromise that will result in a feasible and legitimate policy."<sup>250</sup>

He concluded further that members of Congress are not then interested to "discover new factual knowledge or previously overlooked alternatives.

"The likelihood of anything really new emerging at that state of issue development would be remote even with the most exhaustive search, and the nature of congressional decision making severely limits the search that is possible."<sup>251</sup>

Dreyfus noted that major issues are generally not formulated within the Congress, but result from intense outside pressures. These issues may not easily be anticipated and they often demand speedy resolution, with the result that the time needed for research is not usually available.

Writing with reference to the 99th Congress, Weiss indicated that new structural constraints had developed since Dreyfus wrote about the 93rd Congress. Proliferating subcommittees were making claims on "some piece of the action" and no committee had "the authority or interest to look at analysis of the issue as a whole." The tendency of members of Congress to seek committee assignments pertinent to their constituents' interests also raised the question of how much they were in the market for objective, neutral research and analysis. Because they have had to extend tenure on sub-committees, members also may have developed stable positions on issues. Weiss quoted a staffer who said, "Members have their own gut feelings of where they think the programs should be going. They've worked with the programs for many years and they have strong feelings." Weiss explained that staff "rarely want to hear about analysis -- unless it is supportive" of their boss' views. Weiss noted that there are "few rewards" in this setting for "dispassionate analysis."<sup>252</sup>

Between the 93rd and 99th Congresses interest group activity grew steadily. In 1978 Hugh Heclo described growth in the "sheer mass of government activity and associated expectations," a "peculiar, loose-jointed play of influence ... accompanying this growth," and "layering and specialization" even in political leadership of bureaucracy."<sup>253</sup> Intermediary groups proliferated which "helped to diffuse the focus of political and administrative leadership."<sup>254</sup> Specialized subcultures "composed of highly knowledgeable policy-watchers," some of whom had advanced professional degrees, cultivated "detailed understanding of specialized issues that comes from sustained attention to a given policy debate."<sup>255</sup>

Heclo wrote that "unlike the past where doing what was right was important, knowing what is right has become crucial." He pointed out that since knowing was so unclear, dealing with those who do know had become crucial. "Instead of power commensurate with responsibility, issue networks seek influence commensurate with their understanding of the various, complex social choices being made." Heclo pointed out that issue networks tied together "what would otherwise be the contradictory tendencies of, on the one hand, more widespread organizational participation in public policy and, on the other, more narrow technocratic specialization in complex modern policies."<sup>256</sup> Within these networks, "policy issues tend to be refined, evidence debated, and alternative options worked out--though rarely in any controlled, well-organized way."<sup>257</sup> Within policy networks incentives work "against ... decisive closure."

"New studies and findings can always be brought to bear. The biggest rewards in these highly intellectual groups go to those who successfully challenge accepted wisdom. The networks thrive by continuously weighing alternative courses of action on particular policies, not by suspending disbelief and accepting that something must be done."<sup>258</sup>

In 1992, Heclo asserted that history had born him out: Issue networks were more extended with even more issues.<sup>259</sup> In 1987, Edward Laumann and David Knoke wrote that government was more in pursuit of such groups than being pursued by them. They explained that "contemporary politics is an activity for corporate actors rather than dedicated individuals or amorphous mass movements" -- and that within large policy domains, or networks, the core populations are large numbers of private and public organizations. "About three-quarters of the hundreds of key actors are nongovernmental collectivities ...[with] sufficient political clout to ensure they are listened to."<sup>260</sup>

Weiss wrote that interest groups are a major source of analytic information, which Congressional staff take seriously because the analysis has already been melded with political positions, applied to legislative provisions, and linked to committee business.<sup>261</sup> Weiss pointed out that information from one interest group will compete with others, which staff will continuously test. Weiss reported,

"Their usual premise is that both sides are exaggerating and that truth lies somewhere in between. They see their task as ferreting out enough about the strengths and weaknesses of each side's arguments to get a good purchase on the actual situation."<sup>262</sup>

Testing and counter-testing such information is part of the larger task of discovering "what each interested party wants" and how intensely. Compromise is achieved as a result of screening many different kinds of information, consultation with experts, and application of political criteria which vary depending on the nature of the committee.<sup>263</sup>

This kind of political give and take actually permeates the government, according to one official who was interviewed as he left the executive branch to return to academia. He said that the

main thing he had learned in Washington was, "there is no clean mechanism here to resolve conflicts." He explained, "everything is negotiated."

"There are all these power centers, all these lobbying groups. When there are conflicting arguments, it is very difficult for anyone to add them up and weight them, and more times than not, you get stalemates."<sup>264</sup>

He compared his life at Harvard with his position as an under-secretary. In the academic world, he explained, "the longer and more complicated the logical chain, the more people like it." In Washington, however, "People... are so busy they don't have a tolerance for great complexity. So you have got to find a simple melody. People can't hum complicated tunes. It's got to be simple enough that they can remember it." He also found that top officials were "not expected to read or write, but got their information through oral briefings."<sup>265</sup>

Analysis also becomes a dispensable luxury in the heat of legislative activity, according to Weiss. When Congress is in full session and numbers of bills are being considered, members and staff have little time to read. Weiss cited a House report indicating that Representatives averaged 11 minutes per day.<sup>266</sup> The pace of work and incentives for "very different behavior" caused staff to abandon their intentions to examine analysis. The oral tradition in Congress sets "the context for staff activity." Staff members told Weiss that staff

"collect information through personal interaction, and ... take pride in their ability to 'read people' rather than...reports. They filter the information in a largely intuitive style and develop political judgments."<sup>267</sup>

One staff member said,

"I don't have time to read things. Someone has to come in and show it to me or tell me. I have to be communicated with verbally."<sup>268</sup>

Weiss noted that because committee staff constitute "such an uncoordinated aggregate of people, hired by and responsible to an array of members," they are not as able "to do and use policy analysis." Weiss described fragmentation along party lines and between the House and Senate, very little sharing of information, and "dispersion of effort and duplication of work."<sup>269</sup> Weiss noted that a good deal of effort is siphoned by staff to coordinate themselves at important moments.

Weiss pointed out that the most important constraint on Congressional use of research is its mission -- "to represent the interests of constituents." Because hammering out compromises and passage of bills are rewarded, members and staff become promoters of *their* bill and are then loathe to examine analysis "that seeks an objective 'right answer'" that might not support the bill. Right answers might also harden positions and diminish sought-after compromise. Weiss quoted a Congressional analyst who said,

"The attitude most typical of committee staff is 'Whose side are you on?' If you are not for us, you must be against us. The Hill works in a partisan sort of advocacy style. The

objective analyst, whose job is to illuminate alternatives and not make recommendations, finds a communication gulf right away with most committee staff because there is no room for neutrality."<sup>270</sup>

Weiss found in the Congress, as George also did among executive policy makers, that use of policy research depended on policy makers' needs. In Congress, research use hinged on "the type of committee," "the stage in the legislative process at which analysis is introduced (earlier is better), the degree of uncertainty about the issue under discussion (more uncertainty leads to greater reliance), the extent of conflict, and the qualifications of the particular members and the staff to deal with analysis."<sup>271</sup>

Weiss observed that, despite constraints, receptivity was increasing. She outlined four functions of research in the Congress: support for existing positions, warning, guidance, and enlightenment. Enlightenment refers to the capacity of research to present "new ways of thinking about an issue," and "a new frame for considering a problem."<sup>272</sup> Weiss concluded that the "most common form of legislative use is support for preexisting positions." She observed use of research for highlighting the seriousness of problems (warning), and use of research for guidance, which tended to occur in relation to detailed issues rather than grand policies. Weiss examined her interviews carefully after discovering that "relatively few people talked about the influence of analysis on reconceptualizing problems." She wrote,

"I conclude that the paucity of discussion of enlightenment is more than a semantic matter. Although analytic enlightenment undoubtedly takes place on the Hill, it is a slower and more muffled process than in the executive branch. It seems to happen elsewhere, offstage as it were, and to come to the Congress as a distant echo of debates waged in other arenas. The new discourse is transmitted to the Congress along with all the other messages that pour in. But since the volume of incoming messages is so high, it seems to take multiple repetitions before a new perspective makes headway."

Though she wrote that there were exceptions, "it is hard to avoid the conclusion that enlightenment uses of analysis are infrequent in Congress."<sup>273</sup>

### ***III. Exploring Reconceptualization: Research Questions***

Carol Weiss' 1977 study of decision makers' research use suggested that policy makers valued research that challenged their "comfortable assumptions."<sup>274</sup> Weiss wrote that her study lent support to the *enlightenment* model of research use which includes a role for "research as social criticism" or research "based on variant theoretical premises."

"It implies that research need not necessarily be geared to the operating feasibilities of today, but that research provides the intellectual background of concepts, orientations, and empirical generalizations that inform policy. As new concepts and data emerge, their gradual cumulative effect can be to change the conventions policymakers abide by and to reorder the goals and priorities of the practical policy world."<sup>275</sup>

Weiss' research suggested that a remedy for lack of application of social science research -- besides tailoring it to the goals and assumptions of policy makers -- is a better understanding of how decision makers might use it to reorient their thinking. Understanding of real but subtler impacts of research might also encourage broader conceptualization of research.

Weiss' 1983-'87 research found that members of Congress and their staff occasionally sought out "paradigm-shifting" analysis, but tended more generally to use research to buttress pre-existing positions. She noted that reconceptualization took place elsewhere.<sup>276</sup> This raises a couple of questions: Where within the policy making process does reconceptualization take place and under what conditions? Is it reasonable to expect Congresspeople and foreign policy makers to consider new perspectives? Her results invite more exploration of how, within policy communities, research offers new intellectual constructs.

This dissertation seeks to take up the broader question of whether and how a range of participants in a policy community consider an item of research whose premises lie outside the ongoing debate. My inquiry assumes that alternative perspectives may provide crucial insights, and asks how people responsible for filtering ideas, reporting them, and orienting and executing policy, might approach research that produces or requires reorientation.

Weiss' research suggests that the most productive approach is to consider the frames applied by policy makers and others involved in evaluating research, with the aim of exploring the processes they use to weigh what appear to be powerful constraints and pervasive assumptions against concepts presented in the specialized and precise language of statistical mathematics. Examining each respondent's assessment patterns provides an opportunity to explore this as a reflective process.

The IPPME research provides an example of viewing relevant problems from an unorthodox perspective that has been explored with acknowledged scientific rigor. While IPPME is part of a larger research program applying an ancient Vedic perspective unknown to most foreign policy makers for the purposes of this dissertation, it was the only study presented to respondents.

Innovative research like the IPPME study may well be expected to receive greater scrutiny and skeptical inquiry than less innovative research. Lakatos has pointed out that change in science results from the evaluation over time of *ongoing* research programs rather than single studies, because there is no "instant rationality" and no "watertight way to take a piece of scientific work and decide on its merits."<sup>277</sup> Weiss has observed that, in policy arenas, action is considered unlikely as the result of a single research report.<sup>278</sup>

The value of observing a fairly informal, incomplete assessment of one study for this dissertation is that such an assessment appears to simulate the *actual process that policy makers go through on a rather constant basis*. They don't seek out research directly, but assess it as it crosses their desks. If it meets whatever criteria they apply, they may seek evidence of replication. My concern is how they determine whether to look at more research or to consider more evidence as they may encounter it.

In considering assessment by a range of policy community members, I have chosen to consider those who are most influential in the communication of research, from its publication to its eventual (potential) application in policy. The dissertation, therefore, compares the in-depth assessment by scholars who actually reviewed IPPME and related research with the more immediate assessments of potential end-users (diplomats), plus those in between (journalists, officials outside of the government, and staff and members of Congress).

My research question is: **To what extent do individuals in the Middle East policy-making community apply "truth tests and utility tests" to research in determining whether to pay attention to it in the ways Weiss describes, particularly *when the research information in question involves unorthodox assumptions?***

This over-riding question suggests several pivotal sets of questions:

**How do members of the Middle East policy community determine whether research information is trustworthy? Do they:**

**weigh the research premises and results against their experience, expectations and values?**

**ascertain whether and to what degree the research information conforms to scientific standards?**

**How do they determine whether the research information is useful?  
Do they:**

**examine whether the research makes feasible changes in policies and other things that can be changed?**

**assess whether the research challenges the status quo?**

**How important is each of these factors in determining whether they will give the study further consideration or whether the research ideas and information would substantively contribute to their work?**

**In what sense does the unorthodoxy of the IPPME study influence policy makers' response to it -- and how does this interact with the other considerations they take into account?**



#### IV. Research Design and Methodology

##### A. Selection

As Weiss and others have found, understanding the context in which research is actually assessed is integral to understanding how it might be used. According to Hugh Heclo and Edward Laumann and David Knoke, analyzing the context of policy decisions would be, in Heclo's words, "disastrously incomplete" without taking into account their makeup -- the "specialized subcultures composed of highly knowledgeable policy-watchers ... [whose] detailed understanding ... comes from sustained attention to a given policy debate."<sup>279</sup> These domains are dynamic, including many participants who share a common base of information and understanding, but who may not share commitment or necessarily agree.<sup>280</sup>

In addition to selecting respondents from different nodes of a policy network, I sought to include *significant* nodes from the standpoint of the research information -- junctures where crucial decisions are made about a piece of research as it proceeds from author to end-user: journal referees, reporters, and governmental and non-governmental policy makers.<sup>281</sup> Respondents were selected from within these sub-groups based upon their *positions* as people who reviewed research and their *reputations* for influence within the policy community.<sup>282</sup> In selecting a diverse set of respondent categories -- even with a small number of respondents within each category -- I sought variation in assessments, which might yield somewhat greater explanatory power.<sup>283</sup>

Given my past professional interest in the Maharishi Effect research and my decision that the IPPME study would provide a valuable means of probing responses to unorthodox research and perhaps would extend the context of Weiss' findings, the natural choice of an issue network was the Middle East conflict resolution and policy community. I interviewed 46 respondents and analyzed 35 of those that were completed.<sup>1</sup> The completed interviews included

- 6 Scholars responsible for the peer review of the IPPME study;
- 10 Journalists who might be called upon to report about such a study;
- 7 Members of Congress and staff, 2 members and 5 staff of members of Congressional committees responsible for Middle East policy;

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<sup>1</sup> I included all preliminary interviews that were complete and within selection criteria (4 reporters and 2 non-governmental officials). In addition to three incomplete interviews that did satisfy selection criteria, I left out preliminary interviews with eight who did not meet the criteria: a philosopher/bio-chemist, a political science professor who did not formally review the study and was not mentioned by Congressional respondents, an Israeli engineer involved in the peace process but not involved more broadly in policy, an American author with interest in the process but without policy experience, a Jordanian official whose interview was not complete, two graduate students, and a State department official who did not complete his interview.

- 8 People outside of the government who work to interpret or influence U.S. policy through their writing, grass roots and legislative activities; and
- 4 Members of the U.S. diplomatic community responsible for carrying out U.S. policy.

I contacted academic reviewers of the IPPME study from among those identified in print and from references to anonymous reviewers given to me by editors. All but two gave me complete interviews. (One agreed only to a brief telephone interview covering his responses to IPPME. The second originally agreed to an interview, but later declined.)

My analysis includes four preliminary interviews with reporters from two newspapers described by Hess as "middle" rather than "inner ring" newspapers. While the inner ring newspapers "set the tone" and serve as "the reference point of Washington journalism," Hess has described the middle ring newspapers as serious in intent with substantial resources committed to the Washington bureau.<sup>284</sup> I also selected four reporters writing for papers considered the pace setters by policy makers and media professionals. Three of the four are considered among the inner ring.<sup>285</sup> The four newspapers selected were among those with highest readership, but were considered most influential according to four other criteria described by Hess: scope or national orientation, resources, characteristics of readers, and purpose, i.e., "seriousness of function."<sup>286</sup>

I telephoned the four newspapers and asked who in the Washington D.C. bureau covered the Middle East, and then interviewed those reporters. I also sought and conducted interviews with journalists with more direct experience in the Middle East. I was referred by a respondent and a colleague to an American with 17 years experience in the Middle East and a diplomatic correspondent covering Washington for an Israeli paper.

I selected and interviewed members from two Congressional subcommittees concerned with the Middle East: the House of Representatives Foreign Affairs Committee and the Senate Foreign Relations Committee. I put the names of each subcommittee's members in a separate hat and drew out two names for each committee. I interviewed one Senator and one Representative. The other two declined the interview. A staffer for the Senator I interviewed suggested that I call another Senator whose responsibilities included the Middle East but he declined. The Chair of the Senate subcommittee also declined, but suggested that I interview his staff assistant, which I did. The Chair of the House Committee also declined and suggested that I interview a respondent whom I had already interviewed and then suggested that I interview another relatively new staffer, which I did. I also called another Congressman with seniority on the committee and was directed to his assistant on the Middle East, whom I interviewed. I met with that Congressman, but the interview was interrupted and he did not have time to follow up. The final roster of respondents included a Senator (R), a Congressman (D), three senior committee staffers (one who worked for a Chairman

as well) and a relatively new staffer, and two staffers who worked for members of the subcommittees (one for a Chairman).

In order to select non-governmental decision makers, I asked staffers and members of Congress and reporters whom I interviewed to tell me who they read, talked to, and sought out in addressing Middle East policy. I selected my respondents by scanning my interviews for people and organizations mentioned most frequently:

- Scholars
- Principals from the Middle East, including ambassadors, foreign ministers, embassy representatives
- Institutes and think tank consultants, lobbyists and directors
- Former U.S. policy makers
- Congressional Research Service personnel, and
- Philanthropists.

I included a preliminary interview with a scholar/reporter and interviewed a scholar/presidential advisor, a foreign policy analyst and scholar who worked for a think tank, two lobbyists, a human rights research director, and an activist and organizer involved in the Middle East. I also included a preliminary interview of a human rights attorney.

Finally, from among the diplomatic community I selected four senior U.S. policy-makers with Middle East expertise who had policy related Ph.D.s. Two of the four diplomats interviewed were selected by using media accounts which described them as people who had been active in the area. A third was identified by these two as an expert in the area. The fourth was selected from among diplomats dealing with the region.

### ***B. Preparation and presentation of the research***

The interviews depended on the respondents' reviews of the *International Peace Project in the Middle East* report and/or a summary. Co-author John Davies prepared a summary describing the hypotheses, techniques, and findings. He also graciously condensed the summary into two and a half pages with footnotes.<sup>287</sup> In assisting Dr. Davies, I observed the challenge inherent in presenting the technical and conceptual breadth of the research in an easily digested and sufficiently concise format even without making efforts to tailor the summary to the respondents' needs.<sup>288</sup>

All respondents were given a copy of the *JCR* report and a copy of the summary in Appendix A, with several exceptions. Reporter Rohan reviewed the entire article (and no summary) and reporters James, Berman and Nolan and human rights attorney Kaplan were given an earlier draft of the summary with a copy of the article. Reporter Gaines reviewed the research in the course of writing two articles. Scholarly reviewers were invited to review the summary, but focused more on the review that they had given the IPPME study as journal referees.

Excluding the scholarly reviewers, twenty of the twenty-nine other respondents reported that they read the summary. Of these, one reported reading two paragraphs (Roach), while others read it quite thoroughly (Paris). Four others reported reading the summary and skimming the longer article (Banks, Cox, Krachon, and Tasman). Five read the longer article (Gaines, Rohan, Roth, Golden, and Hall). Hall read the longer article twice.

### ***C. Preparation for and conduct of the interviews***

In preparation for my interviews I collected correspondence between the IPPME authors, journal editors and referees; kept a journal including background information and memos about interviews and their analysis; attended seminars and lectures related to Middle East policy; and monitored the media. My intent was to learn what was meaningful to ask; to get background; and to listen to interviewees' frames of reference and communication patterns and norms.<sup>289</sup> I also conducted preliminary interviews including interviews with IPPME co-authors and other Maharishi Effect researchers. I analyzed four preliminary interviews and explored the comparisons that I would make across categories.

Even though reporters noted in 1992 that peace talks had brought Israel to a position that they had sought for 43 years -- direct conversations with neighboring Arabs<sup>290</sup> -- in August of 1993 (just after the historic Oslo accords, but before the formal signing of the declaration of principles on Palestinian self-rule in the Occupied Territories by the late Prime Minister Yitzhak Rabin and PLO leader, Yasir Arafat, on the White House Lawn) one of my respondents told me, "Until there is a dramatic break through or a dramatic collapse, this is a dead story." By the time I completed my interviews in December, 1994, the events in the Middle East had become one of the top stories of 1994 around the world.<sup>291</sup> My interviews were interspersed with indications of historic breakthroughs in negotiations as well as increased terrorism and controversy in response.<sup>292</sup>

I conducted the final interviews using interview guides prepared as the result of preliminary interviews (Appendix D). Respondents were generally assured that their responses would be kept confidential due to the nature of the study.<sup>293</sup> I have therefore attempted to describe respondents without identifying characteristics and assigned all but one a pseudonym, selected at random from the Boston telephone directory.<sup>294</sup> These pseudonyms were assigned without regard to ethnic or other identifying characteristics, except in the case of respondents from a specific country. (Appendix C lists respondents with brief biographies to aid the reader in keeping track of respondents). Also, I have not cited the writings of respondents in this dissertation, but instead have paraphrased them.

The focus of the first part of the interview was the respondents' use of information in their professional contexts, including an exploration of how respondents decided which information to consider or not consider in the course of work. I asked each respondent to consider IPPME

summary, which I referred to as a piece of information, as they would in the course of their work. I also gave them a copy of the entire article, but made it clear that I was only asking them to read the summary. The second part of the interview generally occurred on another day and involved the respondent's responses to the summary.<sup>295</sup>

Within each group and often across groups, respondents' descriptions of their information use tended to overlap, complement each other, and give more breadth to my understanding of their contexts. Each group's descriptions reflected their institutions and roles. For example, scholarly reviewers' discussion focused primarily on the editing and reviewing process rather than on scholarly activity. Diplomats instead focused on the relatively specialized area of the Middle East and their use of intelligence and other information in relation to it.

In conducting the second part of the interview, I avoided a yes/no, reject/accept approach. In our brief interview -- usually lasting no more than a half hour -- I realized I was asking respondents to consider something they had most likely never considered. In addition, even though they usually had had a day or more to review the summary, I asked them to articulate responses that they may not have fully formulated. I therefore sought to ask questions that allowed the respondent the possibility of experimenting with a range of answers and to then respond legitimately. I also wanted to avoid either the conventional answer or knee jerk or visceral response, but instead to get a reflective answer. In opening the interview, for example, I referred to the context that the respondent had described and asked them to imagine how they might begin to think about such a piece of information, should they encounter it.<sup>296</sup>

Within the context of the inquiry I sought to follow the respondent's line of thinking. The second interview was a simulation which I didn't want to direct. I gave the respondent opportunities to bring out what was naturally occurring to him or her in response to the IPPME summary. I later found it valuable to examine the interviews, noting the sequence of respondents' reactions as they developed. As much as possible I raised questions that would test the implications of what the respondent was saying. I sought to keep my language neutral and never to introduce my opinions. I returned to points that had been made -- in the words of the respondent -- in order to encourage him or her to go beyond code phrases or thoughts. I tried not to jump to conclusions and to explore ideas introduced by the respondent.

#### ***D. Analysis and Interpretation***

In order to reach accurate and reliable conclusions that are not over-generalized, I attempted to keep track of the interview context, while considering the research questions. All interviews were transcribed and I monitored the process carefully to ensure that the transcripts were true to the tapes in the smallest detail.

To analyze each group of interviews within a category, I used the summary format and posted the material on a wall, organized according to the research questions, and then compared interviews. Analysis of scholarly reviewers involved not only interviews but also correspondence, referee reports, and some published essays, which I integrated into the charts.<sup>297</sup>

In drawing conclusions from the summaries and wall charts, I took care to refer to transcripts and to respect the integrity of meaning of each interview. I used memos to reflect on the conceptual process involved in my interviews and analysis as they unfolded.<sup>298</sup> I referred often to my computer journal to refresh my memory about aspects of the analysis, to follow up on themes, and to explore them, often using tables to explore the ramifications of a line of thinking across a set of respondents or groups. I discussed the patterns that I found with colleagues and my doctoral committee members as a means of stepping back from the data, testing preconceptions and seeing the data in new ways.<sup>299</sup>

Based on the wall charts and the within-group analysis derived from them, I graphed responses to the research questions and related themes for all 35 respondents. From this I derived the charts. Each of these graphic displays involved careful comparison of respondents within the context of their whole transcript.<sup>300</sup>

### ***E. Validity of Findings***

The validity of my findings reflects my selection of respondents, documentation, my skill and integrity as "the research instrument," and my appropriate drawing of inferences from data.

My selection of respondents represents a broad range of perspectives, and was based on objective criteria, as described above. I carefully documented interviews using techniques suggested by Miles and Huberman for data reduction, display, and interpretation. In drawing inferences from the interviews, I attempted to avoid any tendencies to strengthen my own preexisting opinions. I arranged for two colleagues, Susan Markowitz, Ed.D., a Boston high school teacher, and Rose Zimering, Ph.D., a clinical psychologist, to read a total of 10 interviews, including a common interview. Dr. Markowitz read four and Dr. Zimering read seven. I selected every third interview transcript from the interview roster which was listed the interviews in the order they were conducted by group. I gave Drs. Markowitz and Zimering a form to guide them and instructed them to either complete the form for each transcript or to mark the transcript directly. They each also provided a note or letter about the results of the transcript audit, i.e. that they found no evidence of bias (Appendix F). Dr. Zimering also monitored my analysis for explicit or implicit effects that my views may have had on the plausibility of my inferences. In a letter included in Appendix F, she described her audit of my analysis of randomly selected participants, which she also found to be free of investigator bias.<sup>301</sup>

I have not generalized from my interviews to other populations, but have attended to the factors and conditions relevant to each particular interview in relation to the findings described by Weiss.<sup>302</sup>

**V. *Analysis and Interpretation -- Assessment of the International Peace Project Research by Respondents Within Each Group***

**A. *Scholarly Reviewers***

When I interviewed six scholars who reviewed the IPPME research, four were working as tenured professors, one was an emeritus professor, and another was a think-tank consultant. Two of the six lived near each other. The rest lived between 500 and 3,000 miles apart. Two had been professors of two others. An editor identified three as "trusted associates" of *JCR*. They were part of a network of quantitative conflict resolution scholars, most of whom appeared to be so well aware of each other that one of them said that although his peer review is "allegedly blind ... I can usually guess the author(s)."

Our discussions about the respondents' use of information in their scholarship revolved around peer review, with the exception of the think tank consultant, whose discussion of information use was more akin to that of government officials, even though he said that he often wrote for technical journals.

Asked about *JCR*, one scholar noted that it was "one of the top three (journals) dealing with international relations." Another called it "an absolutely key journal for quite a wide range of people working in this area," concerned with "the conditions that produce conflict, especially but not limited to international, ... ethnic conflict within countries... labor management relations ... basically conflict at every level but [within] a social political context." He said that *JCR* was interdisciplinary and emphasized the empirical, quantitative methodology.

**Lawrence Moore** had been a professor of political science at a state university for 11 years. Moore's academic focus was initially physics, but he switched to philosophy for his B.A. His doctorate in international relations focused on statistical techniques, especially time-series statistical methods, in which he considered himself "somewhat competent." He was experienced with Box Jenkins, the specific time-series approach used by Orme-Johnson et al., and he had completed a paper that he was submitting for publication using Box Jenkins. He planned to present another time-series paper within a few months.

Moore described the review process as a "two-read process" involving flipping through the manuscript "to see why I'm reviewing it... what I think of the article" and then a second read "to build a case based on that." On first reading, he said he would examine whether the paper was in his substantive area. If Moore considered himself competent for theoretical review he would

"analyze it in terms of its research and statistics, its methods... then, I'll sit down and I'll read the whole article in order to understand what the person is trying to say. Having read it, I will sit down and write the review, going back through it to make [my] points."

Moore described the decision "about accept and reject" as

"actually surprisingly easy.... because papers are either fairly polished, good presentations, or they're fairly rough efforts. And there's not as much ambiguity as it would seem..."



[Reading] maybe hundreds of articles, published articles, you get a good sense for what publishing level performance is -- writing skills, presentation, how smooth it is stylistically and then if you know the area, how well it fits into the substantive body of literature."

Moore viewed statistics, not as a tool, but as

"formal conceptualization of the questions being asked with some rules about how you decide the answer... When you ask a question using statistics, you've structured a set of statements about the world that if they are true, then this result will happen. And so that statistical test gets the mathematical structure that verbal argument requires."

Moore described the kinds of mistakes in conceptual and statistical logic that can be made. He said that, "frequently, a reviewer acts as not so much a judge, but both an educator or teacher as well as kind of advancing the state of performance in the discipline." Moore also characterized limits of statistical modeling:

"I will say that uncategorically virtually all the analysis that we do is incorrect. We use real simple structures, mathematical structures to try to answer very complex questions. And because the setting up and testing of the statistical model is difficult, mathematically, statistically, computer-wise -- it is a labor intensive process of doing it all-- they do take on a life that's disproportionate to how well that method represents the idea. That's just kind of the nature of the discipline."

Moore explained that he was as intrigued

"as much by the fact that we do our empirical models and can't find what should be obvious, as I am by [the particular substantive area]... So I'm generally much more interested in the question than I am in the real world substantive importance of whatever the issue is. And I tend to look at research questions as 'What is the puzzle you're trying to examine and what picture of the world does this provide?'"

Although the other reviewers that I interviewed were not *uninterested* in statistical techniques and puzzles, they explained their overall scholarly pursuits in terms of substantive areas. Two described their choice of substantive areas referring to comparative advantage within their profession.

**George Fisher** received his mathematics training to the Master's level with a doctorate in political science. His minor was religion. Most of his professional work

"has been dealing with quantitative models of international behavior -- in some cases abstract models, that is models without data and in other cases database modeling. So I basically come at it as somebody who works with a lot of data about international events."

He was known as a rigorous mathematician. He has taught international relations, mathematical modeling and other courses for 18 years, and was very interested in using mathematical modeling and computer technology to predict the outbreak of war or famine. He said that he examines time-series analysis, but does not use Box Jenkins. Fisher said that one important consideration for him in choosing an area of focus was his competitive advantage.

He described his interest in the Middle East as arising from

"the usual set of sort of accidental things...a couple of professors in college who were very interested in the Middle East. I subsequently became involved with some human rights organizations, in the Israeli/Palestinian issue. One of the professors ... where I was first teaching was Palestinian. And then you sort of gradually accumulate information. You suddenly find yourself with a comparative advantage in that area. And the areas that I'm looking at, international conflict, that's as a good an area as any."

**John Harrington**, an economist and political scientist by training, was interested in interdisciplinary approaches to war and peace. He has been a political science professor for over thirty years, known among his colleagues as a "barefoot empiricist." He said, "I'm an old fashioned quasi-positivist," approaching "most of my professional life ... as a fairly hard-nosed social scientist, looking for evidence in a more or less standard scientific mode." He was not an expert in time-series, however.

Referring to competitive advantage, Harrington described his intellectual context in examining research. He described a "gut feeling or guiding intuition" about ideas that ran counter to the realist power-politics model that dominated his field for many years. When a manuscript presented a data set that spoke to his intuitions, he worked with other scholars to explore it further. He said his ideas had gained visibility within the scholarly and policy communities.

"Exactly what the transmission valve is, I couldn't tell you. But it depends surely on novelty, but not too much novelty, because it has to have a root somewhere -- the Wilsonian liberalism. But novelty -- window of opportunity so to speak -- the end of the cold war, people are looking for new ideas. Simplicity and relative consensus."

Harrington also indicated how distinct such a substantive interest could be from an article examined in peer review. Although as a reviewer he said he scanned scholarly work for "a clever new idea, an interesting hypothesis, a new theoretical perspective, a refutation of what we had thought was true," he indicated that finding a manuscript that supported "in a more rigorous way some of the things I had been thinking about and indeed writing about" was relatively rare.

**Kevin Gurney**, a retired social psychology professor who continues to be involved in a dozen research interests, wrote a dissertation in the 1950's which involved time-series data. Gurney said he came to decisions about a manuscript via intuitive tests, e.g., "Something feels wrong or implausible." He also described applying a more analytical test.

"On page 14 it says this and so, you look back and on page 3 it says something different. You infer from what it said on page 6 that they wouldn't come to this conclusion. Or being statistically trained..., I like to scan tables for funny appearances or implausible looking figures or data or so on. So that's another which is both intuitive and analytic."

**William Walsh**, another social psychologist with a distinguished career, said he could not participate in the interview regarding his peer review and use of information, but is described in social science literature as having creatively used alternative research approaches; as being interested

in macro as well as micro issues in political science; and as having a non-reductionistic approach to social science. He said he had found that in order to understand behavior, one must understand its social and political contexts.

**David Lambert**, a policy specialist at a U.S. think tank, described himself as by training an applied mathematician dealing with large-scale problems. He has published in "numerous technical journals," but said that he dealt "on a daily basis with scientists, politicians, sociologists, political scientists, and regional experts." He said he had been involved in shaping U.S. policy in several areas.

### ***Assessment of International Peace Project Research Information***

Scholarly reviewers assessed the "International Peace Project" research prior to its 1988 publication. Several critiques have been submitted since then, which have involved responses by Orme-Johnson et al., as well as review by editors and peers.

In 1985, when an earlier Maharishi Effect study was reviewed and declined for publication by the *Journal of Conflict Resolution*, an editor commented in the rejection letter that he was intrigued by the findings and wanted to be kept apprised of further research. Within about a year David Orme-Johnson and his co-authors submitted the *International Peace Project in the Middle East* manuscript, which *JCR* editors sent to four reviewers, two more than usual. One of the four, Lawrence Moore, concluded that the study examined an hypothesis on the scientific fringe in a professional manner. When a revised paper was submitted a year later, the manuscript was reviewed again by Moore and by a trusted associate of the journal, William Walsh. Based on the detailed technical review, an editor decided to publish IPPME, even though he and Moore wrote that they considered the premises unorthodox and probably objectionable to many others.

Within six months John Davies and Charles Alexander, two of the four IPPME co-authors, submitted a follow-up study to the *JCR*.<sup>303</sup> One of two anonymous referees deferred to editorial judgment, calling the analysis rigorous while raising reservations about such "ethereal explanations." The other referee favored publication, and stated,

"The author(s) have designed the studies and analyzed the data in ways that answer all reservations I have had, and more. And the results are internally consistent (across assemblies, across indicators of the dependent variable, and across time) as well as being congruent with the theoretical argument."

However, the paper was rejected after *JCR* editors received a call or letter from someone who claimed to have been a former associate of Orme-Johnson et al. accusing them of data mining - in other words conducting many studies and only presenting those with positive results.<sup>304</sup> This concern was brought up in the letter from the *Journal* declining publication, and was answered in detail by Davies, who pointed out that his study was specifically designed to address this issue.<sup>305</sup>

About a year after IPPME appeared in *JCR*, a critique of the IPPME study was published along with a response by Orme-Johnson et al. About that same time, *International Studies Quarterly* rejected the Davies/Alexander paper on the basis of two anonymous referee reports, including Fisher's.

In 1993, the *Journal of Conflict Resolution* received a fresh critique of IPPME. Several peer reviewers recommended that the critique be published. Orme-Johnson et al. requested that they be allowed to publish a rebuttal. Publication of the critique was declined. In an effort to stem further critiques and rebuttals, another trusted associate of *JCR*, Kevin Gurney, was asked to look at the study. In turn he submitted what was thought to be a definitive final critique. That critique had been accepted and scheduled for publication at the time of my interview with him, though he later withdrew it based on a detailed response from David Orme-Johnson.

I interviewed three of the initial referees, Moore, Walsh, and Harrington<sup>306</sup> and Gurney. I also interviewed George Fisher, the author of several other reviews and critiques and a sixth respondent, David Lambert, because he had reviewed the research as a member of an independent advisory board organized by Orme-Johnson et al. in 1983, when they conducted the research. In addition to my interviews, I read and analyzed published and unpublished reviews and critiques as well as correspondence between editors, reviewers, critics, and IPPME authors. Fisher, Moore, and Gurney have written more about IPPME than any of the other respondents.<sup>307</sup>

In the following analysis I will describe the likelihood of further consideration of IPPME and related research by scholarly reviewers and the truth and utility tests they applied. First I will outline a pattern that I observed within the analytic processes of three of the six reviewers. I will explore the assessment made by two reviewers who were not critics, but who also gave cursory reviews, and who arrived at different conclusions than the other three. I will discuss the incommensurability of models and preferred methods of measurement that explain some of the differences in assessments. Finally, I will describe the effect of efforts to rhetorically mark off the IPPME research as illegitimate.

### ***Likelihood of Use-- Further Consideration for Publication and for Scholarly Inquiry***

None of the reviewers saw a likelihood of *using* the research in their scholarship or of recommending the research in a practical way. Harrington and Moore advised that the article be published in 1988, but would require more stringent tests before considering it further. Fisher and Gurney would not be likely to give further consideration to the research. Walsh, who originally recommended against publication, said he was "glad that the paper was published, because it highlights... foundational issues" regarding whether people like Orme-Johnson et al., with premises like IPPME's, should be published. In the future, he would be "really, really skeptical, to the point of dismissive." David Lambert's response to whether he would examine the research in the future

was, "I'm not totally masochistic" -- referring to the reactions of his colleagues on the Foreign Relations Council and the Trilateral Commission, whom he said "would just inherently be skeptical of any signs of different techniques at all and would be even more skeptical of the [Maharishi Effect] experiments."

***Common Truth Test: 'Something Must Be Wrong'***

Moore, who after two reviews recommended that *JCR* publish IPPME, Fisher, who wrote an essay critical of the editors for publishing it, and Gurney, who wrote what was considered a final critique, had very different purposes, yet they described strikingly similar analytic processes which reflect the difficulty that the research posed for their conceptions of reality. Determining at the outset that they could not accept the IPPME premises, each also determined that 'something must be wrong,' and searched for rationales for the IPPME findings consonant with their views of the world.

**Lawrence Moore** had reviewed a previous MIU study and had rejected it because it had a clear design flaw. When he read the IPPME study he said,

"The first thing that surprised me was that what I would consider a state of the art statistical tool was being applied in an area that I would write off as largely pseudo-scientific. I didn't know anything about TM and I haven't really learned anything about it since, but my kind of limited, lay knowledge of it was that it was a vestige of sixties counterculture pseudo-science. And, so the application of the statistics intrigued me. They laid out their argument very well in terms of where their theoretical structure was coming from and I will acknowledge that they do have a theoretical framework. It rests upon some assumptions that are unobservable, unmeasurable and therefore I don't accept. But nonetheless, they presented a theoretical case and they do as much as they can to bridge from what is unmeasurable to what can be measured, trying to make that connection as well. So, I found all that interesting."

Moore's examination of impact assessment, the type of time-series analysis that was used, suggested that "the way they coded it makes the assumption of causality." He asked for additional analysis, characterizing himself as "smugly" anticipating that cross-correlations would reveal that meditators in the study were responding to conditions rather than influencing them.

"Since I don't accept the theoretical framework of the article and I don't believe their results are accurate based upon my world view, I see a flaw in their design... Their measurement structures it in... The impact assessment does not adequately measure this ...and they really should do transfer functions. The transfer function tests for the lag. And so I wrote back in my first review, reject: that if they really want to answer this question with this data, then they should do transfer function models. And I believe that what they will find is that the correlation is negative."

The results of the ensuing transfer function model analysis, however, showed lags consistent with the original hypotheses. Though Moore couldn't believe the results, he felt that the case had been sufficiently made for publication.

"And so, [when] it came back, they implemented the statistical methods as I suggested that they do and reported all lags as either simultaneous or positive... I still do not believe that a

number of people practicing TM influenced conflict. I am willing to believe that the two are associated and that what occurs is that people know that there is going to be conflict across the border or there are enough warning signals, etc., that everyone goes 'UH OH, better hurry down to the hotel and meditate tonight or tomorrow' and so that there are enough cues in the environment... It's my belief that this behavior really does cause this variation ...I've held them up to as tough a set of standards as anyone ... And then they present information which is acceptable from my scientific criteria... Why should I hold them to a higher standard of research performance than I would hold someone else?"

In an anonymous referee report, he wrote that he was "in the uncomfortable situation of having my earlier objections to the piece in great part mollified."

"I am by no means convinced of the basic thesis, but at the same time, much of my objections have been adequately resolved. I cannot at this point offer a strong case against the scientific merits of the research. Now I must claim that the research offers some interesting empirical observations for which I might have some explanation, but am not perfectly confident that the results are overtly spurious. In short, the evidence presented is of the type that I would call a 'prima facie' convincing argument. Hence the author is left with the problem that the hypothesis falls outside the normal peace research/conflict studies paradigm and paradigms are difficult to budge." <sup>308</sup>

In 1994 Moore was still convinced, as he asserted four or five times in the interview, that there is an alternative explanation. He based his knowledge on his "sense of what's happening in the world," from his dealings with international relations and from talking to "people on the street." He explained that as rigorously and carefully as Orme-Johnson et al. were in laying out their theoretical framework,

"It appeals to certain things that we cannot measure, [and therefore] I will remain skeptical. In other words, the unified field of consciousness or unconsciousness or whatever, no mechanism for measuring that and knowing that's what it is exists. It is simpler to believe that these people were influenced by the events around them, than that their practicing TM is likely to influence these events."

He explained, however, toward the end of the interview that he had not yet found any explanation for how the other dependent variables in the experiment were related to his alternative explanation.

"It's possible that peoples' auto accidents and fire and burglaries are influenced by the social environments around them. Days that are more stressful, people are less cautious in traffic and they have more traffic accidents or they leave the iron on or whatever else. So, I'm willing to believe that the social environment around can influence those behaviors... That's actually where the research gives me much harder problems than the conflict. It's the little stuff."

Moore also discussed IPPME in the context of phenomena like UFOs and telekinetics and parapsychic phenomena. Some instances he knew to be "fake" and others he would be "willing to concede could occur" though he didn't believe they did.

"One day we'll know more about the way the world works ... I'm willing to acknowledge that significant paradigm change in the ensuing centuries might want to see the world entirely differently than we do now. I go with convention, acknowledging the possibility that

something like this may well-- I'd give it one chance in 100,000. But I won't deny it as impossible."

Moore had determined, in spite of his beliefs, that "this is ... publication quality research," but cautioned that he would hold further research to a higher standard, i.e., randomized design. At the end of our interview, he succinctly described how his thinking progressed.

"My perception of what the world's all about-- how it works-- plays a great role in how I've processed the information. I believe that human beings are very poor information processors... Everything we know is based upon inference, based upon our own experiences. My experience with the world leads me not to want to accept that [referring to the IPPME findings] ...I think if we confronted what our world view really was, we would find all kinds of interesting paradoxes and conflicts... So it's very natural that as I read it, I go, 'this is wrong. Now why is it wrong?' Or 'I don't believe this.' And therefore, 'What is wrong and why are they saying this?'

"What's the difference between my world view and that world view? I have a filter that processes it all. And I probably have enough skill at developing argument that I can now look at that and go, 'OK, their fundamental premise is one that I won't accept; it's something that's unobservable.' And so I'll construct that. I understand that's where they're coming from and so I can hinge that as being the difference between my world view and theirs. And therefore I can dismiss it based upon that world view. And now all I have to do is integrate the observable information in there into mine and what explains that observable information in my world. And so, causation is not controlled or the design doesn't control for causation."

**George Fisher** said that he took it upon himself to critique the research when the wife of a colleague asked him to "rescue" her husband because he "was taking this stuff seriously." Fisher's examination of the research started with the presumption, like Moore's, that "something's probably wrong with this." He reasoned,

"The claims of this article were so extraordinary, so inconsistent with how social behavior has been viewed for the last 300 years, that if true, I would have to overturn my entire world view which, like most humans, I'm unlikely to do. The probability that something was messed up statistically, which happens very frequently, seemed much higher than the probability that my understanding of social behavior was wrong or that the Western world's understanding was wrong."<sup>309</sup>

Approaching the study as "an interesting little intellectual puzzle," sure that "there's a problem in here," he started digging, he said. He concluded that Orme-Johnson et al. had misestimated the size of the meditation group needed in the '88 study, because they left out cities in nearby Jordan and Syria.

The second puzzle for Fisher was similar to Moore's. Fisher asked,

"Why did they find what they found? ... Can I find something consistent with my belief system that would explain why they would find this result?... That's where I sort of built a little story that would explain why, to me at least".

Instead of asking, "Should this be published?" as Moore did, Fisher's critique asked, "Why was this published?" He concluded its publication was "slothful." What fundamentally bothered Fisher was that he considered TM a religion.

"I am skeptical of what I view as a religion trying to claim to be scientific... That kicks off a buzzer saying, 'Hey, wait a moment, these dudes are trespassing on my turf so I'm going to go after it.' ...If somebody says, 'I've got a lot of stress in my life and I just can't cope and I've got a friend that Transcendental Meditation helped,' I say fine, because... it's been good for Hindus for the last 2500 years. That doesn't bother me at all. It's when they're taking these faith statements and saying, 'We want to take our faith-based theories and get the same credibility of your testing-based theories and we want to have a body of knowledge or belief that is religiously derived to be given the same credibility and funding that science is given in Western society.' And I, as a scientist, if only a political scientist, am saying, sorry, we can't do that."

In his concern that the IPPME scientists were "trespassing on his turf," Fisher described a utility test: "Does this research challenge the status quo?" His determination that it did and that he was "going to go after it" then influenced the way he examined the truth of the study.

Fisher constructed an alternative explanation for the findings. He suggested that the research was deliberately deceptive, though he said he had no proof. His aim appears to have been to authoritatively write off the research as unworthy of publication in credible political science journals; to perform a scientific demarcation excluding IPPME; to get social science "out of this mess," as he indicated in one of two lengthy anonymous reviews that he sent to *JCR* and *International Studies Quarterly*, where Davies' and Alexander's subsequent paper was submitted and rejected.

Fisher's critiques and correspondence about the IPPME research contributed to highly skeptical reactions to it. A *JCR* editor referred to accumulated critiques by Fisher and others as a reason for not considering follow-up research. Fisher, invoking religious imagery, in effect described the IPPME, all successive research, and its authors as heretical to orthodox standards and assumptions. He described his conversation with the colleague whose wife felt he was taking IPPME too seriously.

"You [who lead a highly stressful life] want to start meditating for half an hour a day. Frankly, you'll probably live five years longer, probably the best thing you could do. And if you want to shave your head and sell flowers in airports, fine with me, I don't care. But don't call it political science."

Fisher had earlier explained his interest in the predictive potential of statistical applications for political science, giving the example of modern polling which has become very well developed. He explained that one of his rationales for his approach to the IPPME study was to guard such hard-won legitimacy for social science.

"If [Orme-Johnson et al.] are doing this as a social science, which I know they want to do, the social sciences have imposed upon themselves imperfectly but still have imposed upon



themselves certain criteria that are derived from the natural sciences which in turn gained their legitimacy -- really starting with Galileo and that crew, in the Renaissance and later in the Enlightenment. Well, we earned that and [they're] not gonna just be able to just sort of transport this over."

Fisher wrote that it was important that reviewers take IPPME and succeeding research seriously in order to prevent further publication of this type of research, which he felt would blemish social science in the eyes of natural scientists.

"The natural scientists, the physicists, and the biologists who usually aren't too fond of the social sciences in the first place, will come back and say, 'well what you guys are doing is lousy and here's evidence thereof.'"

Fisher insisted, however, that if IPPME authors met criteria of random assignment, he and others would be more receptive. "We'll still be skeptical, but it's like cold fusion. At least the issue will be over," he said. The IPPME authors have strenuously argued that demanding random assignment is in practical terms an impossibly high standard for macro-level social experiments -- one that has not been demanded of other macro-level experiments.

In 1993 when debate over IPPME resumed, an editor of *JCR* asked Gurney to compose a critical essay that would finally and effectively close the debate. In the essay he had described fundamental flaws in time-series analysis and a mismatch between chart and tables that "made the randomness of the intervention seem more random than it was." A *JCR* editor described this review -- before it was withdrawn -- as showing that "there is nothing extraordinary happening in this data set at all." Gurney later retracted his essay when Orme-Johnson et al. wrote a response indicating that he appeared to misunderstand the methodology. This occurred after the interview discussed below.

Gurney's confrontation with the premises of the IPPME article was like Moore's and Fisher's in that he said he had "predispositions" and "a whole series of attitudes" about the nature of the premises. He reflected on how these operated and how, having a semi-mandate to find a way to end debate on this one article, he looked for a rationale, for "particulars." Unlike Moore, however, he was not "reviewing" IPPME. Like Fisher, he was seeking simply to debunk it, to quash further discussion. Gurney said an important factor in his decision was his "long history" of skepticism about matters that he deemed similar to the IPPME report, such as belief in UFOs, reincarnation, and ESP.

"I approached this article with a good deal of skepticism already. An arbiter from outer space could say, 'Well that was unfair. [The responsible editor] knew' -- he knew that I was a skeptic. He's a skeptic too. So, you can view the matter as finding a way to justify rejection [but] it's more complicated than that. [The *JCR* editor's] crisis was another critique of the original article that was on his desk. And he had thought he had the lid back on the box. It's not just the Maharishi Effects but it begins to be very tedious when people keep [debating] the same piece for year after year. So, he thought if he published the critique that the

Maharishi people would want a rejoinder so there would be two articles where he wanted none. So he appealed to me in the implicit, almost explicit request, 'Advise me, but I would be most happy if you would find a way to turn off the spigot.' I've never [before] had someone ask me to review something with a mandate to find a way to stop debate on this. Although, he left a large loophole saying, 'whatever you come up with, [Gurney], I'll trust you.'"

Later in the interview, Gurney expressed strong feelings and assumptions about the IPPME premises and authors:

"I don't for a moment believe that they're on to something. I think it's mischievous of them to be proposing this, because... everyone is going to think, 'Well, to solve the world's problems, all we've got to do is sit around and think.' ... That's highly misleading. They are doing a bad thing. And I'm glad I was able to find a justifiable criticism of them. I would have been disappointed if I hadn't."

That he assumed that Transcendental Meditation was a sort of thinking or, worse, wishful thinking, was clear in his comparison of IPPME concepts with student gatherings in the late 'sixties and early 'seventies:

"Where they all prayed, the idea of think peace, there was an instant where they were going to stop an atom bomb explosion by thinking and by preventing it with the power of thought. And I don't like this. It's a magic solution, it's a magic pill."

About his predispositions regarding such phenomena as ESP, he identified people's belief in such things as "a core interest."

"When people want to believe in something, that makes me a little skeptical because they may believe it because they want to, rather than because of any compelling pattern of evidence."

Summing up several interviews with subjects who believed they had ESP, he said,

"The belief spoke to a personal need. Which is another way of saying that a belief, the force behind the belief... was extra-evidential... Well, that's not quite true. They usually had a story they put credence in, but a critic could find other ways that the story could be accounted for. I've long been interested in this because of my interest in beliefs... [and] how people come by them and how they defend them."

After describing his predispositions about ESP and like matters, he described his process of analyzing in more intuitive terms:

"Maybe more of it is intuitive and the analytic follows up the intuitive. I say, gee, it doesn't sound right. I wonder why I'm having this reaction? It's not merely a Yum and Yuck test. It's called intuitive evaluation or something but Yum and Yuck is kind of part of it. And you see whether the Yum and Yuck come from some particular advantages or disadvantages when you look at it analytically. **[Interviewer: Well, when you looked at this particular piece, I have to ask you in terms of that particular aspect of the review, how it came out?]** Well, I was frankly pre-disposed to find Yuck because there was a long history to my skepticism about these matters. I think, I may be fooling myself, I'm ultimately open-minded about a lot of things in that territory but every casual study I've... looked at, has always come out with flawed evidence."

When I asked Gurney where his sense of what was "yucky" or "yummy" came from, he mentioned taste, professional judgment, consensus in the field, and personal predilections, and always the tendency to ask, "Is this fair?" "You learn that certain things are yucky, you shouldn't cook them this way and so on and so on, so you get to be an expert on food. OK." he said.

He explained that an expert in social psychology develops sensibilities like a food connoisseur develops tastes.

"Part of it is training in the discipline, training in reliable and unreliable, trustworthy [and] untrustworthy methods. You come to be familiar with the people who write... That is, yums and yucks... would be agreed upon by almost everyone in the field. On top of that, there's no way to avoid some individual predilections. I say 'Oh, God, here comes another one of those what's their name.' But always, always with yucks you have as well a tendency to dismiss it, you have the tendency to say 'Well, wait a minute, I'm yucking this, is that fair? How can I explain this to the editor?'"

Regarding the IPPME piece and his mandate, Gurney said,

"There was a predisposition toward yuck. ... But I knew that it would have to be backed up by some particulars. If there was anything that [the responsible editor] didn't want MORE than turning off the spigot, he didn't want gratuitous judgments. So I saw my task as 'examine this situation and if you can see and defend any way that I can argue to turn off the tap on the debate about this particular article, then let me know that. If you can't find anything, tell me what else to do.' And this wasn't entirely yucking the original article. I should make that clear."

In summary, each of these reviewers recounted a confrontation with another world view with suspect claims, and then how they felt compelled to find evidence consonant with their own world views that would explain away the reported results. Interestingly Fisher's and Gurney's own behavior -- holding to their own beliefs as precluding the need to take contrary empirical research seriously -- mirrored their criticism of Orme-Johnson et al., whom they assumed were doing the same thing.

### ***Practical distinctions between referees in their assessment of scientific quality***

Comparison of these reviewers, however, prompts a question: Given the interplay between their philosophical and other predispositions, their disbelief in the premises and findings of the IPPME study, their training, consensual influences, purposes, and strategies with respect to analysis of IPPME's quality, how would a truth test be delineated for each? Were they concerned with the study's trustworthiness and if so, how?

They were each concerned with IPPME's trustworthiness. Fisher focused his argument on the related issue of legitimacy. He said he was concerned that IPPME threatened normal social science. Gurney was concerned that the research might wrongly influence gullible people. Neither inquired more deeply into the truth of IPPME once he had chosen his angle for an argument with which to end debate. Moore was more deeply concerned than either Fisher or Gurney in IPPME's scientific validity and reliability.

Fisher and Gurney were significantly less thorough in their analysis of the study's scientific quality. First they judged that Orme-Johnson et al. and the IPPME findings were illegitimate and untrustworthy, then tried to justify that assumption with negative assessments of IPPME's scientific standards. Both Fisher and Gurney set out to construct not only a story about what else might explain the data, but a story to right a perceived wrong -- namely, the original publication of the piece -- and to find a way to stanch further debate about it.

This kind of activity in science, known as debunking or demarcation, is as the philosopher of science Laudan wrote, a weapon of academic warfare. It is off-putting in its detail, and the internecine twistings of the pursuant debate are taxing to follow. It tends to be, as Laudan points out, emotional and political. Certain aspects of the fight, however, illuminate the larger discussion of how people in a policy network handle extreme disbelief when examining scientific quality.

Moore attributed his differences with Fisher to different standards:

"It's simply a professional disagreement. I will hold them [Orme-Johnson et al.] to the same standard that I would hold anyone to. But I elevate on the next round of it. That's why I said, I would now hold them to a change in design [involving random assignment]."

In fact, there is more than professional disagreement here: Moore, asking "What's wrong?" examined the entire statistical analysis *twice*, writing as a result of the second round that the findings met his minimum requirements. He had constructed an alternative explanation and when the data analysis that he then demanded did not support that explanation, he wrote:

"My two major points in the earlier draft have been, if not completely resolved, at least softened considerably. The discussion of the experiment indicates that the likelihood of conflict (incoherence) influencing the behavior (number) of practitioners of Sidhi-TM is less than I conjectured. The second major point, the introduction of transfer functions, serves to further emphasize the hypothesis that the number practicing TM influences conflict/coherence, and not the other way around."

Fisher, on the other hand, did not perform a detailed quantitative analysis. His discovery of what he judged to be a discrepancy between Orme-Johnson et al.'s theory and their execution of research was for him sufficient evidence of the study's fallibility. Fisher wrote that the square root of one percent of the population, being key to the MTUF [Maharishi Technology of the Unified Field] theory, was applied to cities in Israel and Lebanon, but not to the cities of Syria and Jordan, though they are closer geographically than Lebanon and some other places in Israel. He argued that using population figures that included omitted cities -- if one is using geographical distance rather than political boundaries -- would mean that the square root of one percent threshold for an impact in Lebanon "was never reached!"

Fisher charged that Orme-Johnson et al. changed their theory in response to his finding, enabling them to find results when experimental conditions had not been reached. He equated the IPPME research effort with a notorious example of parapsychological research wherein a crucial

measuring device was removed, while the unsuspecting observer continued to measure phenomena and get results.

In what was considered by other reviewers to be "a quite respectable-- though not perfect" response to Fisher, Orme-Johnson et. al. objected to the 'paranormal' labeling emphasis, and reiterated that their model, which describes coherence rates and states of collective consciousness, requires specification of population figures based primarily on socio-political realities, such as the fact that Israel controlled the occupied West Bank and Gaza and half of Lebanon, and only secondarily on geographic distances.

Because Fisher felt he had shown that Orme-Johnson et al. were seeing an effect where they shouldn't have, he also believed that "on a standard scientific eriterium (sic), the results would be spurious. So, in that sense, I had shown what I needed to show." After he located the discrepancy in population figures, he set out to "construct a story" that would explain the IPPME results. If the Maharishi Effect was not responsible for the results that are described in IPPME and even more clearly in subsequent research, what would explain them?

"And then the question is, 'Well, can we come up with a mechanism, a social mechanism that would explain why?' I mean there's clearly some relationship between their two series and it doesn't come out as clearly in the original article as some of the stuff they've done later... And that's the second puzzle."

In his first critique, he discussed the likelihood that TM participants in Jerusalem would have believed in participating "for the good of humankind," commenting that participation was costly in time and lost income. It would be rational for them to participate when others were likely to do so -- when they judged they would be successful. Therefore, he reasoned, the "rational strategy would be to watch the dependent variables themselves" and participate accordingly.

Fisher continued to consider this alternative explanation as more viable than that offered by the IPPME research even though Orme-Johnson et al. argued that an important point was overlooked in this alternative explanation, i.e., that the TM and TM-Sidhi group size and war intensity had an *inverse* relationship. If participants were watching events and participating accordingly, they would have ungenerously stayed home when they noticed that the war intensified and rejoined the other meditators when they saw that the war had decreased again. The authors argued that even had the meditators been so motivated, this scenario was unlikely for five additional reasons, including the fact that drop-in involvement was small; meditation occurred in the early morning and late afternoon and most reports of fighting in Lebanon were not available until later, in the late afternoon and evening; and broadcast coverage of crime, accidents, and fires was too minimal for influencing meditator participation. Orme-Johnson, et al. said that the logic of this critique overlooked the 13-day experiment within the experiment which involved participation numbers empirically-raised on a predetermined schedule independent of fighting. These were

correlated with a mean number of war deaths of 1.5 for the experimental period compared to 33.7 per day for the 13-day periods before and after. Finally, the authors asserted that they had addressed this critique with their use of transfer functions or cross-correlation analyses, which yielded results directly contrary to what would be expected to occur according to Fisher's explanation.

Fisher also chided editors and reviewers for publishing the research when the statistical techniques used were suspect. In several reviews, Fisher wrote that application of Box Jenkins time-series analysis allows for flexibility in adjusting for noise and is thereby susceptible to finding significant results by chance. In our interview he spoke about Box Jenkins being a tool he would not use, that he did not teach, many times referring to its potential flaws;<sup>310</sup> but he said that he had not examined the IPPME study's analysis closely enough to have found direct evidence of such flaws in it,

"because I didn't have their original data and it wasn't worth my trouble to ... I mean, my original suspicions were raised because I knew the Box Jenkins was a very dodgy technique to use. The most obvious flaw in the article was something much more basic: Having the population figures wrong."

Given his observation regarding the population figures and his opinions that Box Jenkins is "very difficult to use properly and it's very easy to just do total nonsense on the basis of it," he presumed in all of his critiques, and led his readers to presume, that Box Jenkins was used improperly. He made this charge without ever looking into the actual analysis.

Fisher criticized Orme-Johnson et al. for not performing simple t-tests. The latter replied that in fact they had done so and found strong correlations leading them to employ Box-Jenkins in order to test that their t-statistics were not inflated.<sup>311</sup> Fisher accused Orme-Johnson et al. of seeing results where none existed, "a tendency only enhanced by Box-Jenkins." Orme-Johnson et al. responded that the raw data alone were striking in that one could observe two graphs representing the size of the meditating group and the composite index as almost identical.

By 1993 Fisher suggested another story to explain what he said was "clearly some relationship between their [Orme-Johnson et al.'s] two series," referring here to the same raw data. He refereed a new critique of the original IPPME study. The new critique sought to delineate events in Israel and Lebanon during the IPPME which would explain the Orme-Johnson et al. results. In our interview Fisher concluded as he did in similar terms in a written review:

"The authors (of the critique of IPPME submitted to *JCR* in 1993) have done an excellent job of identifying what I regard as the 'smoking gun' in how the results of the Lebanon experiment were manipulated: the meditation experiment was linked to an Israeli withdrawal from the Shouf Mountains, an event that was known in advance."

Fisher charged Orme-Johnson et al. with manipulation, though the critique did not. The critique called for a more detailed analysis from Orme-Johnson et al. to account for such facts as the Israeli troop withdrawal from the Shouf Mountains surrounding Beirut, intended for Sept. 1, and postponed until Sept. 4, at which time fighting predictably erupted. They also mentioned the abrupt resignation of Prime Minister Begin on August 28. Around this time the numbers of meditators peaked and the authors of the critique claimed that Orme-Johnson et al. had not explained why, e.g., whether it was planned or in response to these events.<sup>312</sup>

In their response to this critique Orme-Johnson et al. argued that in the context of many sharp irregular fluctuations in independent and dependent variables over two months, mention of these two "military/political events ... would hardly seem sufficient to account for data gathered and analyzed on a daily basis for a 60-day period." They suggested that even a qualitative appraisal would need many more examples to be persuasive in explaining the detailed co-variation of the independent variables with multiple dependent variables -- including crime and accident rates in Israel -- which was higher than the correlation of dependent variables among themselves.<sup>313</sup> They also pointed out that these two events in themselves did not explain what was needed to constitute "any part of the correlation between group size and the war." Begin's resignation did not seem, from the data, to predict immediate increase or decrease in fighting. The military withdrawal did not seem to explain the increase in group size.<sup>314</sup>

Despite Fisher's accolades for the critique, the editors of *JCR* decided not to publish it, but to eventually publish a final critique by Gurney. Gurney located what members of the IPPME research team had always recognized was technically a weakness of the study as with virtually all large macroscopic studies: that the intervention was not assigned randomly. He judged the peak intervention dates to be less randomly distributed than claimed, and discovered what he thought were difficulties in the time-series analysis. He also found what was later acknowledged by Orme-Johnson et al. as a typographical mislabeling of a chart specifying upper and lower boundaries of meditation group size quartiles -- which Orme-Johnson (having checked the original data) explained did not affect the mathematical analysis and accompanying diagram.

Gurney, true to his description of his approach to research, scanned the study and found the labeling error. The description of quartile distributions did not precisely match the diagram (Figure 3 in Appendix A). He said he was "pleased" to have found "the discrepancy in the diagram," which he said "related to the non-randomness ... the crucial argument." He had noticed "that the series looked very systematic." Consistent with his explanation that he did not undertake a thorough review, he missed several key elements of the analysis. When they were pointed out by Orme-Johnson, Gurney withdrew his critique.

Orme-Johnson pointed out that peak intervention dates were never claimed to be randomly distributed in the original paper and it was only the binary variables that were described as effectively randomly distributed, a point which Gurney missed. Orme-Johnson wrote that no flaw existed in the research, noting that Gurney's methodological issues were addressed in the original paper and that IPPME had analyzed the data in three ways: using "the continuous series of numbers representing the daily group size as the independent variable in cross-correlation and transfer function analyses" and an additional analysis in which "the independent variable was a set of four binary variables representing the quartiles of group size in the impact assessment analysis." Orme-Johnson observed:

"[Gurney's] comments relate to the latter formulation of the independent variable as four binary independent variables, which he misunderstood. In addition, he did not acknowledge that the two primary analyses used in the study (transfer function analysis and cross-correlation) directly address the basic methodological issue that he raises. Moreover, he did not seem to be aware that JCR had already published a parallel robustness check with pseudovariables ... which he recommends as a strategy for checking time series results." (unpublished reply, Orme-Johnson, 2.)

Gurney's argument revolved around the pattern of participation levels of meditators which appeared to him to involve a cyclic series and a monotonic trend. He argued that correction for either would mis-estimate the other half of the data. Orme-Johnson wrote that Gurney either hadn't read or hadn't acknowledged that trends and cycles were removed from both the dependent and the independent variables "before assessing the relationships between them" in the cross-correlation and transfer function analysis. Orme-Johnson also pointed out that "only one of the dependent variables had a weekly cycle to begin with." (p. 5) He wrote that Gurney mistook the four binary variables Orme-Johnson referred to as one non-binary variable and didn't understand that given the binary nature of the variables, they had been correctly described and analyzed.

Gurney had constructed an argument to explain 'what was wrong' with the IPPME data. As he saw it, "there are any number of artifactual factors which could create this relationship and it's up to [Orme-Johnson et al.] to rule them out," which he didn't think they had. To introduce his argument, Gurney placed dots on a copy of the IPPME diagram to indicate quartile boundaries as they were (mis)labeled and not as they appeared in the IPPME graph. While he told me that he realized that Orme-Johnson et al. may not have intentionally mislabeled their chart, Gurney implied in his writing that they had. He explained that his 3-page discussion of the discrepancy between the labels and the chart was a ploy.

"[My focusing on] the dots being in the wrong place is, at the most uncharitable, it's a cheap trick. At the most charitable, it's a diagnostic of their being sloppy about their data. I did it as a ploy, really, to be frank with you. How can anybody in a journal make such an elementary mistake? They've been very sloppy about this."



In his rush to indict, Gurney used the dots to rhetorically imply fraud, even though he said he didn't consider that part of his analysis "definitive" and wouldn't have used it if he hadn't decided that the series was non-random. His decisions about how to convey the results of his cursory examination of IPPME quality reflected his emphatic disbelief and suspicions.

### ***Two Additional Reviews***

Respondents William Walsh and David Lambert also gave the IPPME study cursory review. As a "trusted associate" who had used time-series analysis in his own work, Walsh had reviewed IPPME for *JCR*, but did not consider that he should give it a formal or thorough review. He said, "Looking at it from a purely technical point of view -- in terms of the internal analysis, [IPPME was] not of discriminably different quality from your average *JCR* article." But said that his problem with the paper was

"first and foremost... a problem of source credibility. This was not an organization that had any track record of doing serious scientific work, and I was not persuaded that the organization nor that the investigators had a commitment to the self-correcting norms of science. I thought that their first commitment was to a set of religious beliefs revolving around the Maharishi. So that immediately made me nervous."

Walsh indicated that he had more specific knowledge about Maharishi than many respondents, particularly when he later referred (albeit imprecisely) to Maharishi's theories about higher states of consciousness. He also displayed his own pre-commitments. Walsh assumed no track record for the Orme-Johnson team, though several of the authors have published extensively in a wide range of peer-reviewed journals. He, like several other respondents, assumed that Orme-Johnson et al. were all from Maharishi International University, when one was at Harvard University at the time and another, Larimore, was an independent statistician who designed an advanced method for time series analysis.<sup>315</sup> Walsh assumed that Transcendental Meditation involves conversion and belief, both of which are widely acknowledged to be untrue. He also presumed that suspension of loyalty on the part of Orme-Johnson et al. was more difficult than for other scientists. He said,

"I come to this with some pretty classical prejudices. I think that one's track record as a scientist is extremely important, and one's demonstrated commitment to the norms of science is extremely important. And I think that a commitment to religious organizations, working within religious organizations, or quasi-religious organizations interferes with the commitment to science. ...When you're testing claims that bear on central components of the religious belief system, and that's what you're doing here, it's like asking a fundamentalist Protestant to test the efficacy of prayer.

**[Interviewer: "And what is religious about these particular people? Or their claims?"]** Well, what you have is a central prophet-like figure, the Maharishi, who posits a set of stages of consciousness. He doesn't posit them on the basis of scientific evidence. He's positing them on the basis of some special, personal knowledge he has of the structure of the universe. [He] is a charismatic figure, who attracts lots of supporters, who has a

meditation technique that he spreads among his supporters who go around trying to show that the meditation technique brings about desirable results. I mean that, it looks like a pretty classic prophet going forth and converting people who in turn convert people, and the movement spreads.

**[Interviewer: "What I'm interested in here, is, how that hits you as a scientist.]** Strikes me as dangerous. **[Interviewer: And why is that?]** Because the norms of science have to do with a commitment to norms of objectivity. And I think norms of objectivity are compromised by proselytizing on behalf of a quasi-religious cause... Whenever you have people whose primary commitment is not to science, but rather is to a religious or to a political movement, whether it's Marxism-Leninism, or fundamentalist Christianity, or Islam, or TM, you have a problem. Science is a demanding enterprise. And social science is an extraordinarily demanding enterprise, because it requires a suspension of loyalty and commitments to causes that are influential in our daily lives."

Walsh, citing Stephen Jay Gould's *The Mismeasure of Man*, explained that "when you combine politics and religion into science, we know that data fabrication is quite common." Actually Gould's book gives examples of scientists justifying unsound theories with "fudged" and otherwise unsound data, but he by no means locates the source of such behavior as solely political or religious.

Walsh echoed Fisher's concern regarding IPPME's challenge to conventional science.

"It would require a major transformation of our understanding of the workings of human psychology, human physiology, and physics. Basic laws within those disciplines would have to be revised to accommodate these findings. So, I would have to have an awful lot of confidence in these findings in order to consider revising laws that are that basic within a number of disciplines."

Walsh's ontological skepticism was expressed a little later, more sharply, when he said,

"I don't believe that there is a shred of reliable scientific evidence anywhere in the literature that human beings are capable of influencing, via their thoughts alone, the actions of other human beings at a distance. These results don't make sense unless you assume that some mechanism of that sort did occur."

Unlike Fisher, however, Walsh explained that he would not "go to the mat" regarding whether IPPME should or should not have been published.

"I don't feel intensely that it was a great violation of scientific standards to publish this article. I think that in many respects this article does pass the normal scientific tests, within social science."

Having decided that something must be wrong with IPPME, but that it also appeared to be statistically robust, he posed a question similar to Moore, Fisher and Gurney. That is, 'What would explain Orme-Johnson et al.'s findings?' The answer was not conclusive, but enough so for him to suggest that *JCR* dismiss the paper. He suspected the authors.

"When you confront something like this, one obvious possibility is they're just making up the data. That's an extreme charge, but it goes on in biological and social science. A second possibility, which I think is fairer to the investigators, is that there's a capitalization on

chance. But to the best of my understanding, that didn't particularly fit. I mean their results did look reasonably robust, statistically. A third possibility is that there's an enormous file drawer problem. That the Maharishi people at the Maharishi University are regularly doing studies of this sort, and occasionally there is a correlation between what a meditation group is doing and outcomes in the world, and they choose to submit those results for publication; but when there are no correlations, they ignore the results, or they assume that the meditation was not done properly, or what have you. And that comes back to my original concern about the credibility of the source. If they had a long track record of doing serious scientific work that proved to be replicable by independent investigators, I'd have been much harder pressed to, to reach a negative judgment on this paper."

In the comments he sent to *JCR*, Walsh wrote what he called the "nub of the problem," his epistemological and ontological problems, instead of couching his disagreements in methodological terms.

"This paper poses an epistemological conundrum for me. What we have is a logically and methodologically coherent effort to test a set of hypotheses that, to be blunt, I regard as absurd. To be sure, it is possible to raise technical objections concerning the research design and data analysis procedures used here, but such objections are skimpy camouflage for my true objection: I do not accept the metaphysical premises of the study (that small groups of mediators [sic] can initiate major societal events via changes in the field of collective consciousness). And, a related point, I do not trust a quasi-religious organization (such as the TM group) to conduct fair and impartial tests of the predictions of the founder of the organization. All this leads to the following conclusion: I'd be willing to consider seriously the current research for publication if, and only if, it were conducted by an independent, scientific body such as the National Academy of Sciences. It is, in my opinion, important to set a particularly high threshold of proof in this case. Otherwise we run the risk of opening up social science journals to a potential flood of pseudo-scientific religious demonstration studies (e.g., fundamentalist groups purporting to show the power of prayer to heal.)"

In a published comment, a *JCR* editor observed that as implausible as the IPPME premises were, social science journals should not refuse ideas on the basis of their source. He understood Walsh's uneasiness with the IPPME authors, but observed that independent researchers, like those at the National Research Council, were unlikely to undertake such research on any unconventional theory they saw as implausible. Not publishing until they did so would consign the research to never being published.

When I first contacted Walsh he expressed an interest bordering on enthusiasm about research that debunks parapsychological research. His remarks conveyed a sense of community with the various scholars that he knew who study such phenomena and cited a recent example of such an article in *Psychology Bulletin*. He also referred to several Harvard professors who are involved in debunking, and explained that he tended "to sympathize with the skeptics."

Lambert, too, considered himself a skeptic, and used the term in explaining how he came to be on the advisory board set up to monitor the IPPME research.

"What he (referring to IPPME author Charles Alexander) suggested in terms of meditation and the statistical effects of bonding with lots of people seemed counter-intuitive. I have a

tendency to believe this was more random than not. But he was a professor at Harvard and couldn't be all bad. And he wanted, I guess he wanted a skeptic on his show."

Recalling his involvement in the 1988 time period, he said he didn't mind his name being used because he wasn't "drawing conclusions." He remembered the research as "seriously analytic" and "surprisingly good [analytically]," though he was "just not convinced." He said, "The effects of mass meditation, for example, may have no effect whatsoever on reducing violence whether in the Mideast or in Washington D.C." He saw a "real problem because of the experimental design and no matter how good it is [IPPME would] have many critics." And he concluded,

"And I would have no objections to see it go on but whether I would prefer to invest in fusion energy or come up with a new subway system compared with the amounts of money that have been available for this... I'm not sure what to call it -- mass meditation. I'm not certain that I could make a judgment."

Thinking about the reactions of his colleagues, he imagined they would find IPPME "ludicrous." He would not find it so, but would be hard put

"to define parameters well enough to do the experimental design and then once one has done, to prove that the successful results are exogenous not endogenous reasons. I think it's a hard sell."

After reading the IPPME summary again, Lambert continued to grapple with two factors: not being able to conclusively assess the research technically because he had not applied "due diligence" to it and his estimations about how the elite members of the policy community would react. He explained that to become more convinced himself, he would "have to give it a lot more study," and "be willing to take on the accusations."

His reading raised more pointed skepticism. He said he found it interesting

"that you can make these measurements. There's no real way of isolating phenomena. I mean you really have a hodge podge of culture, cultural differences, violence, in the Middle East, so much so that you need some clear, clear experimental variables. And in dealing with those that are not very clear or not very well defined although the experiment itself may be well defined, it leads to ... how do I put it, confusion. It may all be right. But I would have no way of telling."

Lambert explained that while some people might consider some of the things that he had done "daring and interesting," they were "really mundane" and "reproducible." His colleagues on the Trilateral Commission, on the Council on Foreign Relations, and elsewhere, would

"just inherently be skeptical of any signs of different techniques at all and would be even more skeptical in my view of one of [these] experiments ... for the purposes of analyzing foreign policy. The people who drove policy very rarely ever look at mathematical models except when there is direct operational and tactical connection such as aiming errors with missiles to prove one way or the other that you can destroy a target."

Lambert explained that a group had contacted him to put the research down and he had declined. He said his colleagues not only did not use science explicitly, but were also "curiously involved in being God-fearing and defensive religiously." Referring to former White House aide Oliver North, he said, "faith is an inherent aspect of policy making too. You have to have the gall to believe that you're right." Lambert said that given that the people he was describing "are not very kind," he expected that their reaction to IPPME and to those who took it seriously would be "derisive."

***Decision to engage or debunk the research and subsequent truth and utility testing in relation to perceived incommensurability of paradigms***

Whether to engage with a model like IPPME's appears to involve a level of comfort with its premises so that one can accept the language, approaches and practical consequences as legitimate. Part of what distinguished Moore and Harrington from Fisher, Gurney, Walsh, and Lambert was the fact that Moore and Harrington credited IPPME enough to give it a more thorough evaluation. Moore and Harrington rated the scientific quality of IPPME highly and were therefore not able to dismiss it. Neither of them accepted the IPPME premises, though both showed more flexibility based on their ordinary knowledge towards what Moore characterized as not impossible, but "highly, highly improbable." Fisher, Gurney, Walsh, and Lambert on the other hand could not credit the research.

All of the respondents were operating within a different scientific paradigm from Orme-Johnson's team. In assessing IPPME, they were confronted with what Kuhn described an "incompatible" mode of "community life," like a different political party.<sup>316</sup> Fisher, Gurney, and to a large extent Walsh were among what Kuhn described as "those who refuse to step into the circle" posed by a competing paradigm, where "the premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive."<sup>317</sup>

In often colorful language, Fisher expressed reservations which may have been shared by many conflict resolution scholars. He also articulated the depths of his ontological distress with this research. In his review of the study by Davies and Alexander that followed IPPME, Fisher referred to the

"bombshell... dropped [therein, which] loosely translated, says in a couple of paragraphs, 'Oh, and by the way, we're now proposing a mechanism which runs against every single tenet of modern scientific practice since the Enlightenment.'"

In this excerpt, Fisher provided a fascinating picture of some elements of the Maharishi Effect research that violated his sense of *what is possible* and *what kind of explanation is permissible*.

"It is about time that someone calls the bluff of these dudes on quantum physics. There is a great deal of semi-mystical gobbledygook arising from the simple fact that physicists

writing about quantum theory use natural language words in very context-specific ways that do not correspond to their meanings in everyday discourse. To take the most common misunderstanding, there is the issue of how the 'observation' of a phenomenon affects it. If one simply uses the everyday language meanings of this discussion, it gives an impression of a mysterious, mind-over-matter phenomena which cannot be explained by our ordinary understandings of things. If one looks at the basic physics, it is not mysterious at all: 'observation' involves the interaction of particles and energy, and those interactions obviously affect the state of the system being observed. If I observe a ball rolling down a hill, my 'observation' actually involves my eye receiving photons which previously bounced off that ball. At a macro level, the photons on the ball don't affect its behavior, but if we get something sufficiently small (this is where Planck's constant comes in), such as an electron, the impact of the photon can potentially change the state of the system. Nothing mysterious about that -- in fact it is just common sense once one has figured out the properties of things like electrons and photons. The 'mysteries' of quantum physics are almost entirely the product of scientific illiteracy and Snow's 'two cultures' problem: humanists like to use words to mean whatever they want at the moment; scientists use them technically and the context is very important. Scientific illiteracy being a sufficiently big problem as it is, I don't think *ISQ* [International Studies Quarterly] needs to compound the problem."

Fisher registered distaste for what constitutes a different -- and not necessarily threatening - level of discourse and metaphysics different from his own. He portrayed Orme-Johnson et al. as out of the main stream of science, using concepts and words out of context. He did not acknowledge the vigorous, varied, and voluminous debate among physicists about the broad implications of quantum mechanics, which involves many interpretations that differ from his and that in some cases appear to be consistent with the IPPME model.<sup>318</sup>

In discussing how observation is really not mysterious, Fisher explicated aspects of his philosophy of science. Things affect things: photons affect particles. He was not comfortable with discussion of abstract field dynamics affecting human behavior.

Two fundamental components of each of Fisher's critiques and referee reports further illustrate the disjunction between the model of scientific discourse with which Fisher was comfortable and the IPPME model. Because Fisher couldn't imagine the world the way the IPPME scientists described, he couldn't imagine that their approaches to measurement were valid. The IPPME scientists conceived of the mechanism in abstract quantum field theoretical terms. While they were not able to outline in detail how the mechanism operated, they considered that an effect of specifiable intensity (relative to the group size) generated through collective TM-Sidhi practice in one place could measurably influence the trends within a surrounding population of proportionate and specifiable size. They posited an effect with concrete behavioral repercussions, measurable on the societal level, regardless of the particulars of individual behaviors within the population. Fisher, on the other hand, was concerned in his science with modeling precisely such behavioral particulars as potentially causal.

Fisher argued, as stated above, that Orme-Johnson et al.'s theory was concerned with population estimates at its core, and that the study ignored cities that should have been included in their calculations for the IPPME study, arguing that the theory was therefore not testable or falsifiable. Orme-Johnson et al. responded that he overlooked the fact that they had made consistent reference to political/social factors in their theorizing, and misunderstood their hypotheses regarding the nature of geographical closeness, which was posited in relation to political/social factors. They said that Fisher also misunderstood how they reasoned that more coherent consciousness might influence social and political interactions in Lebanon, and the nature of field effects, which Orme-Johnson et al. had posited to vary depending on local conditions, such as population density and social and political boundaries.

Orme-Johnson et al. also said that the formula for calculating the extent of impact was not a theoretical absolute but a beginning point suggested by previous data.<sup>319</sup> They wrote that they were more interested at this stage in whether they would find the predicted effect reliably, rather than the more complex issue of how precisely the formula could predict its geographic extent. They also did not say that no effect could occur below their threshold of the square root of 1% of the population; only that any such sub-threshold effects (such as in Damascus or Amman) would be expected to be less pronounced and reliable. They said, however, that as group size increased, the coherent effects generated by the group of meditators would first be felt in the Arab quarter of Jerusalem (where the group was located), then in Jerusalem, then in Israel within conservatively defined boundaries, then in the larger territory which Israel dominated, including the Occupied Territories, and then the neighboring country, Lebanon, which Israel was still largely occupying. No statistical evidence of a significant sub-threshold effect was found prior to group size reaching the level projected to affect each successively larger population area. In contrast, significant effects were found above this threshold.<sup>320</sup>

Fisher's rejection of Orme-Johnson et al.'s logic stems in part from his distrust of their assumptions. The way he understood the theory was geographical and mathematical. His understanding of causality, that 'things affect things,' his skepticism about mysterious mind-over-matter phenomena, and perhaps his own approaches to mathematical modeling were in line with his expectation that if a Maharishi Effect were to exist it would be *measured* as an even radiation of the effect from the group meditation location. The problem that he confronted is linguistic, conceptual, and ontological: How much consciousness is necessary to influence war deaths at a distance?<sup>321</sup> Fisher's critiques and his statements in our interview cry for a mechanistic answer to this quantum problem. At the same time, he said he knew in advance that the results must be "spurious."

Fisher's stated distrust of Orme-Johnson et al. because of their unwillingness to randomize demonstrates further how different basic assumptions may lead to differing interpretations of the

same methodology and evidence.<sup>322</sup> Orme-Johnson et al. agreed that randomization is sound from a logical standpoint. But sociological experiments are generally not randomized, so saying that the study was not randomized makes no sense in the context of common practice, Alexander said.<sup>323</sup> In response to Fisher's first critique Orme-Johnson et al. wrote that their review of the methodologies used in 98 articles from *JCR*'s March 1984-March 1990 issues revealed that roughly half were concerned with international conflict and they were not experimental or quasi-experimental in design. The only experimental studies among those examined were laboratory simulations, only ten of which used random assignment. They concluded that the IPPME study may have been unique in its experimental investigation of a conflict resolution strategy and that it was being held to a higher, impractical standard.

Randomization also raised an ethical dilemma for Orme-Johnson et al. because their data indicated, according to Davies, that

"the presence of groups significantly reduced the number of deaths in a social situation. If you pull them out, bring them back in on a random schedule, this increases the cost enormously and also reduces greatly the number willing to participate because of severe disruption to their lives. So the total number of days you can get for group meditation is drastically reduced. Therefore you're increasing the risk to human life. That sort of logic cannot be allowed to prevail -- in any science -- in any moral community -- in isolation from the potential risk to human life. It is no accident that you never see random assignment used in non-laboratory prospective studies of conflict and war alleviation."<sup>324</sup>

In our interview, Fisher explained that he didn't think that he was "imposing a particularly high cost" in asking for randomization. He said that because Orme-Johnson et al. haven't done a controlled study, "the more I think it's that they know it doesn't work... I originally thought they were innocent and I don't any more."

In spite of the fact that Fisher is renowned for his mathematical and methodological skills, he didn't examine the science beyond what he considered "a basic error on the population figures." IPPME clearly violated his sense of what is possible or permissible within his interpretation of the history of science since Galileo and Descartes -- although Descartes, like Newton, functioned within metaphysical assumptions that acknowledged the inner dimension that made Fisher uncomfortable.<sup>325</sup>

Fisher's determinations were also politically motivated. His utility test involved the question: "If this were true, would it overturn our concept of what else is true?" Fisher feared the impact that accepting IPPME and related research might have on social science and his political science niche. He said that if IPPME were considered feasible by anyone else to replicate or use, it would be challenging to the larger status quo and to his place in it. Fisher appointed himself as critic of IPPME and related studies and defender of political science, of his colleagues and his turf.



"TM is unusual in that it's what most people would call a religion that is claiming to be a science. It is not unique in that regard; Scientology is like that, Mary Baker Eddy's Christian Science is like that. And I'm not trying to place myself off as Mr. Great Scientist saving the world, but as somebody who takes that scientific view, I am naturally skeptical. That dispute tends to be buried nowadays in the West. But if we went back to the 17th, to the 18th century or even the 19th century, that was the big issue, faith versus science or whatever. That was what the Enlightenment was about. And so, yeah, that, that kicks off a buzzer saying, 'Hey, wait a moment, these dudes are trespassing on my turf and so I'm going to go after it.'"

Davies claimed that characterizing the Maharishi Effect research as religious and therefore unscientific is grounded in cultural misunderstandings. He noted that the Vedic tradition or Raja Yoga, from which TM is drawn, takes an experimental, empirical approach to knowledge in contrast to approaches based on devotion, belief, or acceptance of authoritative texts as being beyond testing. He explained that it is natural for TM to be associated with a scientific approach in the West: "Only the emphasis is on the study of consciousness rather than behavior as being fundamental."<sup>326</sup>

In his first critique Fisher called for the *banishment* from political science of any models lacking independent empirical evidence, a plausible mechanism justifying the model, and on-going, replicable empirical testing. Fisher thus proposed a tough and broad criterion which is ultimately based on a subjective standard -- is the research plausible? But the question remains, who in the scientific community should decide?

John Harrington spoke specifically to this point:

"Sure it's an absurd premise, but they're playing the game. I don't dismiss it out of hand. I may dismiss certain kinds of manuscripts that are basically opinion-pieces that don't attempt to play by these rules. But this is a different matter."

He explained that he considered it absurd,

"in the sense of everything I thought I knew about society. That is, I could not see a mechanism whereby this would operate. There is the Maharishi people's general field theory. But that is -- shall we say -- abstract, to say the least. This didn't fit in with anything I know as a social scientist that could explain the mechanism, OK.

"And when doing a piece of social research, presumably one ought to have, at least at some point in the process of doing research, some specific hypotheses about the process by which (A) produces (B), OK. And to me the process by which (A) produces (B) wasn't here -- and it was extremely hard for me to imagine what that process might be, OK.

"Nevertheless it is also true that not all these things happen at once, in the same mind or set of minds. *JCR* had published in the past interesting, more or less inductive results, that didn't fully lay out the mechanism.

"Well, this is at an early stage. I certainly don't know what is going on here. Nevertheless, as a piece of research it meets standards as achieved by a lot of other stuff that [is] published, with the sole exception this is an off-the-wall hypothesis, premise if you will. **[Interviewer: Off-the-wall, meaning?] Off-the-wall meaning quite outside of anybody's standard**

scientific paradigm, OK. That's a huge statement. Nevertheless, why not put it out once and let the normal process of social science go at it and let's see if there's anything there. Or whether it is indeed a one-shot coincidence that they got something here. In which case the normal processes of replication can address that question."

Harrington and Fisher described different approaches to social science. Fisher's call for banishment of research like IPPME would exclude the normal process of social science that Harrington described: in effect *any* knowledge capable of breaking the limits of the current paradigm would not be admitted, entrenching the limits of the current paradigm.

### ***The Influence of Exclusionary Rhetoric Among Social Scientists on Truth and Utility Testing***

Peer review in this case involved a rhetorical climate that began with a serious effort to treat the IPPME study as any other would be treated, despite its unusual claims. Harrington, for example, said of his approach, "My instinct was one toward fairness and a John Stuart Mill view of free speech and an open scientific process." But accumulated critiques created a rhetorical climate which tended to make an open and thorough assessment of IPPME or the paper that followed it less and less possible. When the purpose of the reviews was not to ascertain whether or not the conclusions were true, but to "go after" the research, examination of scientific quality was limited, at best. Fisher and Gurney each built a story to explain away the IPPME results. Part of the story involved projecting themselves as authoritative and depicting Orme-Johnson et al. as not scientific.

Authority was partly claimed on the basis of the *JCR* decision to publish their reviews (though Gurney's was later withdrawn). Fisher made authoritative statements about the time-series methodology, not because he had "gone to risk" as Feynman might put it, but on the basis of opinion about such methodologies. He gained in authoritative status when two editors requested his review of articles that followed IPPME.

Fisher raised the cost of open, fair-minded debate, enabling other critics and editors to cover their bases by calling on him. In this process, critics were put in touch with each other. Anyone who might publish the IPPME research was also put on notice that this would entail the risk of being painted as slothful, unscientific and irresponsible.

As he continued to write referee reports, Fisher also claimed authority as watch-dog of Maharishi effect research, on the basis of having written prior critiques and referee reports. Delegitimation of Orme-Johnson et al. was approached through disparagement of their methodology and depiction of them as people who played loose with their own theory and post-hoc explanations of their results. Fisher's and Gurney's arguments were that science is over here and *they* are over there. Neither did this on the basis of open and thorough peer review, but by repeating their claims and excluding and/or ignoring evidence to the contrary in more formal correspondence, referee reports, and articles, but also informally.

An example of Fisher's efforts to make a case that Orme-Johnson et al. did not approach their science legitimately occurred with his discussion of what he said was the central flaw of IPPME (specification of the number of meditators needed based on the population size).

"Now there is conveniently -- and perhaps predictably -- an escape hatch for this problem: page 21 [of Davies and Alexander's paper] refers to the possibility of 'possible unevenness in the pattern of spread in predicted coherent effects due, for example, to changes in geographic, national or ethnic population boundaries defining localized units of collective consciousness.' This will doubtlessly be used to justify the choice of boundaries; it also has the potential for rendering the model unfalsifiable -- not a good thing."

This quote is fairly damning, but was taken out of context by Fisher. Davies and Alexander were explaining the difficulty of estimating *precisely* the effect of the 4,180 people meditating. This was the group size which had been predicted to affect a population of 1.75 billion "of the total population at that time of the total area at least as close to the assembly as Lebanon." Davies and Alexander then stated, "It was not possible to take into account the number of individuals practicing TM outside the assembly." Davies and Alexander were trying to delineate how and why they were not able to be more precise in dealing with large populations sizes (1.75 billion). Within "normal" paradigms this would be an opportunity for possible further refinement of measurement and specification, a possible set of factors to take into account for more understanding. Instead, Fisher accused them of loose science. He did so partly because his world view, which would not allow a quantum concept like collective consciousness, would also require a more mechanistic measurement of predicted effects.

Fisher also argued that TM is a religion because its adherents wouldn't test core tenets. He said that testing the Maharishi Effect was a first step in that direction, but insisted that only randomized design would indicate to him that Orme-Johnson et al. were really willing to test their core tenets. Asked to identify those tenets, he referred to John Hagelin's writing:

"Hagelin has got the opening and closing articles and they're his whole connection of TM with grand unified theories, blah, blah, blah. He's got pages of equations that are nonsense basically. Those are givens. I've never seen any evidence they are willing to test those theories. Nor even in the case of Hagelin's stuff. Hagelin doesn't publish in physics journals. Hagelin publishes in psychology journals."

Fisher's writing on this topic in his '93 review of the IPPME critique -- shared with colleagues -- was more emphatic and emotional:

"Again, one could critique Hagelin endlessly -- the short critique is that he has never published this stuff in a refereed scientific journal and therefore it is no more scientific than Uri Geller, David Koresh or Bozo the Clown."

It is noteworthy that Fisher would so ridicule Hagelin, who has published 69 articles in physics journals -- including widely cited articles on his interpretation of grand unified theory --

and has also formally presented the Maharishi Effect findings to physicists around the world.<sup>327</sup> Fisher displayed a surprising willingness to make accusations without fully investigating or crediting the actual facts, including the published articles by Hagelin on grand unified theory that were cited by Davies and Alexander.

In his '93 anonymous review, Fisher asserted that

"TM has crossed the line into the realm of actively doing physical harm about three years ago when they got into the business of selling worthless medical cures and getting people to forego conventional medical treatment (see *Journal of the American Medical Association* about a year ago). They are killing people. While I am not suggesting the *JCR* is morally responsible for that activity, *JCR* is unwittingly being used to help legitimate the scam, and therefore I think that the *JCR* has at least some responsibility to continue to rebuke the original TM article if that can be done on valid scientific grounds."

The *JAMA* article Fisher referred to was an ad hominem argument written by a free-lance journalist without M.D. or Ph.D. credentials who neither critiqued the science involved nor purported to review research on merits.<sup>328</sup> The piece, while full of innuendo, did not however assert that anyone is killing anyone else. Several published research studies have found some of these alternative medical remedies to be helpful.

Fisher's expressed opinions about Orme-Johnson et al. became more emphatic, as he suggested they should in his anonymous review of the '93 critique. Referring to his first critique, he wrote, "If the original critique did not fatally damage the TM research, more criticism is needed."

My interviews and analysis suggest that the decisions by Fisher, Gurney and Walsh against Orme-Johnson et al.'s legitimacy did not stem from their examination of scientific quality, but from a determination that the questions they asked, the nature of the Maharishi Effect premises, and the solutions suggested could not be allowed "in."

Moore and Walsh, who used Box Jenkins time-series analysis in their own work, found IPPME scientific quality appropriate. For Moore the question of what explained the data remained open, but for Walsh the result was that either Orme-Johnson et al. were lying, overlooking some element of chance, or the unthinkable was true: Science could be overturned on the basis of one study. In contrast, Orme-Johnson et al. proposed that their study offered another level of analysis to complement, rather than contradict, existing levels.

The case that was made in the critiques and in the correspondence between reviewers and editors was intended to end deliberation. Fisher was allowed to do the dirty work: namely the degradation in status of Orme-Johnson et al., not on the basis of methodological inquiry, but with a rhetorical reassertion of the boundaries of science, identifying Orme-Johnson et al. as an outgroup. He did what Walsh would not do: He expressed his disagreements methodologically and presented

his arguments as deliberative, when they actually appealed more to values like the rhetorical form of disparagement described by Aristotle.

In this instance, to quote research use scholar Holzner, "frames of reference and reality tests" were fully anchored in personal, professional and collective identities.<sup>329</sup> Collective and professional identity was so central to Fisher's assessment of IPPME and his rhetorical choices that he appeared to combine truth and utility tests. He saw IPPME as challenging the collective identity of political science, which Fisher and others described as embattled. Political science, according to Fisher, needed to be protected from encroachment from what Fisher considered to be a religion. Something that so fundamentally challenged political science threatened his "turf" and could not be true. Therefore, the question of trustworthiness of IPPME was settled before he examined it; and he never did examine it in depth.

Fisher's assessment was also at heart political.<sup>330</sup> He and other reviewers and editors created a rhetorical case for ending deliberation about the Maharishi Effect research, which discouraged other reviewers (or journalists, or decision makers) from crediting the research enough to take on the task of testing its scientific quality. As Lambert pointed out, the skepticism inherent in the policy world about science in general and the potential for derision, were intimidating enough. When Fisher's earliest draft of his IPPME critique was widely circulated to conflict resolution scholars it had a dampening influence, discouraging any debate on the merits of the research.<sup>331</sup>

Harrington's response five years after he recommended that *JCR* publish IPPME illustrates how influential the critiques of IPPME were. He observed that Orme-Johnson et al. managed to answer critiques (up to the last one, which had not yet been answered when we talked, and which was eventually withdrawn) "reasonably well." The weight of controversy and of critiques -- including the last critique which he thought was devastating -- prompted him to conclude, however, that the research was "fundamentally bogus."

"I think there are just too many accumulating critiques that collectively make it clear what I originally thought, that this was highly unlikely -- that it was a very peculiar or even absurd premise."

Harrington's comments made it clear that he had reflected on and was affected by Fisher's rhetoric, for example, when he referred to potential harm done by Orme-Johnson et al.:

"There is a difference between subjecting an idea to the intellectual give and take of science and legitimating it for activity in the real world where people may get hurt. The ways in which this work may be presented as legitimate and ... at the least persuade others to put resources into it and at worst conceivably do some policy harm -- though again I repeat I have no evidence on that."

Part of what bothered Harrington was what he referred to as "misuse" of the *JCR* publication of the IPPME study for publicity. As a prominent reviewer and associate of *JCR* he was

often contacted and he, like Fisher, came to wish that IPPME would "go away." He felt that Orme-Johnson et al. breached scientific boundaries by publicizing the study. Given the weight of the critiques, Harrington said that he would have to give a new study "special scrutiny," though he would be unlikely to recommend publication. In fact he recommended against publication of the Davies/Alexander paper.

Five years after IPPME's publication, a seventh respondent, the colleague whose wife had alerted Fisher to IPPME, also appeared to back away from his original assessments of IPPME. He had originally written in his review of the Davies/Alexander paper,

"This is an extraordinary study. I read the previous Lebanon study in JCR with some skepticism because, although the methodology could not be faulted, the theoretical premise of an underlying field of collective consciousness contradicts one of the most basic epistemological assumptions of behavioral science, namely that all interpersonal influences are transmitted by behavioral or symbolic interactions.

"There is another, equally fundamental epistemological assumption in the sciences: if a body of evidence consistently and strongly challenges a theoretical position, we should be prepared to discard or modify the theory. After reading two versions of this manuscript, I think that is where we are at. It reports six discrete replications of the same social experiment -- assemblies practicing TM -- all of which coincided with positive effects, not merely statistically significant but substantively significant effects, on a variety of indicators of conflictual and peace-making behaviors in Lebanon. I can find no flaws of any kind in the methodology employed. The author(s) have designed the studies and analyzed the data in ways that answer all reservations I have had, and more. And the results are internally consistent (across assemblies, across indicators of the dependent variable, and across time) as well as being congruent with the theoretical argument."

Originally agreeing to interview with me, he later said on several occasions that he could not.

The Davies/Alexander paper has not been published. A third quantitative conflict resolution journal had apparently been inclined to accept it, based on their own reviewers' comments, but finally rejected it, stating that the authors would have to meet more stringent requirements. In light of the critiques circulated following IPPME, the journal's editor felt it necessary to refer the decision to his editorial board (of which Harrington was a member). The board split: some thought it should be published because it conformed to normal scientific standards; others thought that it should not be published because the premises were implausible. This led to the decision to reject.<sup>332</sup>

### ***Summary -- Scholarly reviewers***

When scholarly reviewers examined the trustworthiness of IPPME, they wondered what was wrong with it. As Edwards' writing predicted, they didn't believe that behavior had been influenced at a distance, or that collective consciousness existed and could mediate an effect on social behavior, though Moore and Harrington each said that they wouldn't discount the possibility of such a thing happening. Scholarly reviewers looked for an alternative explanation. Five out of six of them were skilled in statistical and mathematical modeling -- the language of IPPME. The other

was skilled in interpreting social science research methodology. The research presented a relationship between the dependent and independent variables which didn't make sense to them. They sought alternative explanations because if the relationship were interpreted correctly in IPPME, their *basic* assumptions and approaches would be overturned.

Of the five reviewers who said they had the expertise to assess time-series analysis, however, only one reviewed it in detail. Moore suggested publication, though he still didn't believe the research. This is the pattern that Weiss described as most predictive of future consideration of research. Moore's weighing of scientific quality caused him to recommend publication of IPPME in 1987-88, despite his disbelief. He was still ambivalent in 1994, but had not closed his mind to the research.

The other four researchers with time-series skill -- Fisher, Gurney, Walsh and Lambert -- gave the research less than detailed review. Fisher and Gurney authoritatively cited methodological problems on that basis. They used rhetorical means for marginalizing Orme-Johnson et al., which were fairly effective, as illustrated by Harrington's statements about the weight of critiques.

Fisher and Gurney were so uncomfortable with the IPPME premises that they could not accept the language, approaches, and results as legitimate. Even though Walsh, who used Box-Jenkins methods in his own research, found that the internal quality of IPPME met his criteria, he also looked for another explanation for the results, and decided that they might be explained by the authors' lack of competence. This is an extra-scientific factor in the sense that journals, as Moore pointed out, normally grant competence to researchers whose research meets methodological standards.

Walsh and Gurney questioned whether people who believed in the premises of their research could carry it out properly. As Mitroff's study suggests, attachment to one's ideas may not always be a liability in the scientific arena; as his subjects, who were among the most creative and effective in science, noted, such an expectation is also naive.<sup>333</sup> The antidote to being swayed by biases is scientific integrity and open and thorough review. On the contrary, several scholarly reviewers described assessments and overall peer review of IPPME overwhelmed by their own emotion, fear, pre-commitments, and suspicion. Assuming the stance of protectors of science, Fisher and Gurney went after IPPME. Maintenance of tribe -- as Campbell put it -- overwhelmed fair and open examination of trustworthiness.

### ***B. Journalists***

My interviews of newspaper reporters included six who had worked as reporters for 20-plus years, including **Paul Nielsen**, who had been a reporter for 32 years. The remaining four had written for 13-20 years. All had covered foreign policy beats, except **Christopher Rohan**, who

was leaving to become a bureau chief when I interviewed him. Reporters **Judith Gaines** and **Kingsley Taft** both had masters degrees, while the rest had the equivalent of bachelors degrees.

### ***Assessment of International Peace Project in the Middle East***

Aspects of the IPPME premises -- particularly its metaphysics and counter-intuitive description of causality were so off-putting to seven of the ten reporters as to provoke assertions that it was untestable and unscientific. They would discard it even before they applied what to them is a more bottom-line consideration, i.e., whether the research is relevant to the psychological, personal, and tribal nature of conflict in the Middle East. The study would have been in the wastebasket before it could fail on the even more stringent requirements that it have authority and power. Referring to his time and priorities Nielsen said that IPPME "struck out very, very early in the game."

I will contrast the distinctions made by these seven journalists with those made by two reporters who didn't necessarily find the research believable, but who would and did consider its scientific claims. I will describe another reporter who felt it was improper to weigh the research against his own beliefs and unnecessary to consider the science because he was only concerned with whether it pertained to the diplomatic debate.

### ***Use of Research***

Nine of the ten reporters interviewed were unlikely to consider research like IPPME in their future work, while Gaines, who was selected because she had already written two stories about the Maharishi effect, would consider the research further. Four of the nine who found it unlikely were fairly vehement in their decisions, using phrases such as "never in a million years" (Jennifer James); asserting that the research "spit on the reality of the Middle East" (Bruce MacBain); and "it's pseudoscientific nonsense" (Nunzio Nicosia).

**Paul Nielsen**, who was fairly vehement, mistook Maharishi for "the fellow who had a commune in Oregon a few years ago," whom he had determined was a con man in conversations with "people in the government who investigated the Oregon operation." Nielsen asked himself, "Who is the man? What are his credentials to deal with a miracle like this?" He admitted,

"I mean I bring my own bias to this but I'm skeptical about the magic properties and nature of Transcendental Meditation, yogi-ism, things like that. There are people who believe in it; I know people who think it's the best thing since sliced bread and cheese, but I don't. But even if I put my innate prejudice aside, it's getting into an area that is not really a scientific area; it's an attempt to mix science and religion together and it just doesn't work... Somebody might fault me, tax me on the grounds that I am prejudiced and I wouldn't argue with it, but TM is not something that I place a great deal of faith in. I don't have a mantra; I know something about it; I am not speaking from total ignorance; I've read about it. I know people; I've talked to people who think it's the cat's meow. ..."



Trying to analyze why the IPPME piece would strike out "very, very early in the ball game," Nielsen said, "All these things are like depressing a button in my head." He said, "In evaluating whether I have an interest, whether something should be of interest to me, what I am doing for you is a calculus that is not consciously in my mind. It's a subconscious calculus that I am going through."

For him, the calculus involved his innate prejudice, the unworkability of mixing science and religion, the estimation that it was "just too wooly, and by my standards, off-the-wall" for a general interest newspaper, as well as virtually impossible to test. Finally he came to what he called "the bottom line" regarding the attempt to apply Maharishi's technologies to foreign policy: "You have to look at something like this and say, 'Is this something that's in the real world?' And the answer from my point of view is, no."

Of the journalists who said they were unlikely to give this research further consideration, **Rubin Klein** was one of eight who simply did not believe it, partly because of its metaphysical nature. He said that he realized that he was reacting emotionally:

"Very marginal. I would say that since I am ...reluctant to get enthusiastic about all this Maharishi stuff. I'm very skeptical about this so I would be very cautious. **[Interviewer: And what about it makes you feel that way?]** I'm not sure even if it's very rational. I mean, it's more emotional than rational... Well, what comes to mind is what I know and what I read about those gurus previously. Because I think that I am superstitious about superstitions, and since I'm secular, I don't believe in gurus. I don't believe in gods. I would be very hesitant to even waste more time on getting more stuff and writing about them... What comes to mind ...is all the stories about misery, people who got their life in a mess, when they got involved with gurus like this and stories about corruption, exploiting people, exploiting their naiveté and immaturity. So, I give them very little credit... I can write about millions of topics and I've got all kinds of papers like this one, lying on my desk. So, if I want to make my priorities, I think this would not be very high on my agenda. I wouldn't even bother to ask myself those questions that you are asking me because ...of, primarily, priorities."

**Francis Nolan**, his voice conveying his discomfort with the research, reminded me that the "main test that I have on something is whether it seems to fit within the universe of likely stories."

"And this, to cut to the chase, is one that falls outside of that... For one thing, we're talking about an area with about 3\_ millennia of stories of divine intervention in politics so I suppose that this sort of story is not unlikely. I require considerably greater terrestrial corroboration than seems to be available here... It's just ...the metaphysical explanation that I have the problem with... I have run across other similar claims... People were telling me that a group of men in North Carolina prayed for something for two or three weeks running and the result was what they had been looking for, which may mean that God decided to grant their prayers... I have no trouble really with the people who were engaged in the meditation altering their behavior in some way. That's well within my experience and something I have no trouble believing. It's the spill-over effect of this claim that I simply doubt on its face."

For Nolan the TM-Sidhi results described in IPPME involved "divine intervention" because he was unable to conceive of a mechanism causing the effects:

"to assume that the fact that a few people are meditating affects the actions of everybody or large or substantially large numbers of people who not only are not engaging in the meditation, but don't even know that other people are doing it, that is by definition a divine act because ...there is no direct cause."

He could only relate it to fanciful ideas that he had encountered, "things that I have trouble believing and my readers are going to have trouble believing."

"They just fall outside of the area of what I'm going to handle. When I was on the intelligence beat a few years ago, we got lots of reports [that] ...Czech intelligence [had] put in transmitters in [people's] teeth that controlled their lives, and this may even be true but it is so unlikely that it didn't require more follow-ups. At the same time, the Church of Scientology was coming up with stuff that they kept giving you which was plausible; none of which ever checked out. I mean, I was never able to get any of it proved to the point that I could do something with it but it was all stuff that **could** have happened so you have to look at it."

The metaphysical aspects of IPPME did not bother **Kingsley Taft**, perhaps because of long-time knowledge of TM, but he considered the effects on large populations and on conflict implausible.

"I'm not saying that I don't think that meditation isn't valid on an individual basis. I'm only saying, I don't think it can alter behavior in a society unless you have 180 million people doing it -- not as the latest trend, like racket ball, but really seriously getting involved with it; that's a whole different thing. But it would have to be a lot of people and it would have to be proven to me that these people were ...really going to hold onto this thing and it was really going to affect their lives long term."

Describing friends whom he considered tragic figures and a friend who practiced Buddhist meditation, he continued,

"Now, I'm sure it's made him a calmer individual ...and changed his outlook, etc. but all I'm saying is ...'Can you multiply that by 100?' No, I don't think so. A hundred people he comes into contact with? No, ...which I think is what they're saying here. Relatively a small number of individuals practicing [reading] 'combined coherent influence is enough to alleviate built-up stress, as reflected in reduced violence.' I just don't believe that..."

Another unbelievable aspect was whether

"something like this actually ...works because I think that when you have profound external factors pressing against you, ...meditation is only going to take a society so far. I also think that certain societies may be more attuned to this than others. What might work in India isn't necessarily going to work in Bosnia."

Reporters from the region or who had spent considerable time there were especially dubious of such an intervention. **Bruce MacBain** reacted strongly:

"Solving the Middle East crisis by meditation and ...subjecting the entire population of Israel and Gaza to ...that kind of stuff is really so fanciful, ...kind of Disney world stuff... It's not something I spend a lot of time analyzing... It's so esoteric. ... It's nonsense. That's all... I don't have to analyze it. All I have to do is read it... It's completely unrealistic; it has nothing to do with the real world out there... This is somebody sitting somewhere

fantasizing about what he or she thinks is an ideal solution for a conflict that's very real, that involves very real issues about which this person clearly doesn't know anything."

For MacBain, who associated Maharishi with Buddhism, "Asian philosophers" could not usefully address a conflict that was more than real, that required long, hard solutions:

"It's a real conflict over land, between people who feel very strongly about this issue. To come and tell them all you have to do is to sit down and meditate and cool down and everything is going to be wonderful, is like Disneyland... It has nothing to do with apples and oranges... It's wonderful in the days of the sixties, during the Flower children... But...this is a real conflict where people are killed every day, where sons and daughters and grandsons and granddaughters feel strongly about the land on both sides, where they hate each other, where they are Christian and Muslims and Jews, especially Muslims and Jews. They are not Buddhists to begin with and have no Asian philosophies that are esoteric. It's kind of preaching in a religion that is not at all theirs. It has nothing to do with the Middle East. "

**Christopher Rohan**, who expressed his disbelief much less vehemently, offered a couple of alternative explanations of how conflict might be resolved:

"As a reporter, instinctively I'm skeptical at any suggestion that changing behavior of the square root of 1 [percent] of the population could have any demonstrable effect on the behavior of the population as a whole... What I would be more disposed to believe is possible is: If you could take as many of the elites as you could find in the Palestinian and Israeli society ...and bring them together -- and do this. If you take, for instance the governing body of the Likud and the PLO, leadership of the occupied territories, and you had them do TM together, then I could see that there might then be some reason to believe that there would be some demonstrable effect on their behavior and their perceptions of one another. But by the same token you could say that if you just get them to sit down and talk through their common differences, ...or if you could sort of lower the barriers between Israelis and Palestinians and get them to see each other as human beings, interact that way, then on a wide scale basis, not necessarily using TM -- you might have the same thing."

Though risky, Rohan found the concept interesting and potentially newsworthy. He asked six times whether anyone had covered the research, and wondered why not, if it was valid. He said he would need a social scientist to explain it:

"I just find it ...hard...to believe in it. I mean I would immediately rush around with this and say, call up some psychologists, sociologists, and ask them if they read this and send over the study and ask them what they think about it. Because I think it's clearly something that I'm not equipped to reach judgment about myself." <sup>334</sup>

### ***Assessment of Science***

Seven of the eight reporters who couldn't believe the IPPME premises and conclusions didn't believe it was a testable or scientific scenario. Four of the seven had examined the data and referred to the variables during their interviews. However, understanding the level of control imposed by time series analysis or that the pattern of days that alternated with intervention days served as controls as well, would have required a more careful reading of the study as well as greater competence, which most of the reporters volunteered that they lacked.

**Francis Nolan**, who interpreted the study as "divine intervention," nevertheless understood one of the subtler implications of its causal premises: that the IPPME model entails an explanation of behaviors in society, which from outside of the model may themselves be considered causal.

Regarding what he would need to be convinced, Nolan said,

"I would guess a considerably longer study and one in which the other variables were controlled in some way or another --none of which is going to work because one of the things that this postulates is that people are acting differently than they would have otherwise, and that becomes one of the variables. 'People acted differently than they might have otherwise.' That's the explanation. There may be other explanations as to why they acted that way. I would probably have had a great deal of trouble covering Moses crossing the Red Sea, or the Resurrection, both of which I now believe occurred but which I would have had some trouble with at the time. It's a business in which skepticism is important. ...If it's not useful on every occasion, that's the way it is."

While understanding some of the study's implications for measurement, Nolan's objections indicated that he had read the summary very quickly or had forgotten details. He had trouble with the length of the interventions, which he took to be two weeks instead of two months, and he said that he would take it more seriously if more experimental evidence were available, which the summary had described briefly. He also mentioned that he had been in the region at the time and hadn't noticed these results being reported, which made him skeptical. He was also skeptical about the time and length of the intervention.

James similarly mistook the lack of explicit controls as an indication that there were none and that the theory was untestable and unprovable:

"So the problem is they don't, they can't control for other factors, so it's not a testable theory. I mean, when they talk about lowering the level of violence in the Middle East (I think it would be the '82 war they're talking about here?) they don't talk about anything like negotiations going on, diplomacy going on, the war goals having been achieved, and therefore the level of violence diminishing, they don't talk about any of that. I mean there's absolutely no way to prove what these people are saying."

Her belief that IPPME was untestable was practically one with her estimation that it was unlikely. In colorful language she asked,

"They're claiming, as I understand it, that the reason the level of violence lowered is because of this meditation effect that they had. Well why should one mean the other? Why is there any provable cause and effect relationship? I mean if I say that a little yellow dog peed on a dandelion every day for two weeks and the level of violence lowered in the Middle East and therefore it's because the little yellow dog peed on a dandelion since the level of violence lowered, does that prove a cause and effect? One happens; the other happens. Is there any necessary relationship between these two things? Does that control for anything else? It's totally absurd. I mean why would you waste a moment of thought on this? It's so patently absurd."

Despite giving attention to the evidence, James and MacBain concluded, as did Nolan, that they couldn't "find a lot of science in it." MacBain asserted,

"To begin with, this is pseudo-science. ...This is not science, really. **[Interviewer: What does that ...mean to you?]** You know, 'positive index of quality of life' and so on... I don't understand the dependent variables here ... [reading] 'Stock market, national mood derived from news content analysis.' That's lumping an awful lot of stuff into very broad categories. I don't think this qualifies as a scientific article. I'm no scientist but certainly from what I know in the Middle East (and I have been to college) ...that was not a serious study. Not to mention that I do have an initial hostility to anything that is talking about cults and that strikes me, anything that says Maharishi Technology of United Fields ...leads to suspicion.

Several reporters judged the study unscientific largely because they distrusted social science in general. Kingsley Taft explained that as a journalist he saw his province as not the scientific but the anecdotal, and saw his skill as understanding what's going on in a city:

"I'm always kind of turned off by stuff like this. ...I consider a lot of it pseudo-science. ...It's trying to make something scientific that doesn't lend itself to being scientific. I don't think you can put people in a box and conduct experiments with them and come up with any. I just think there are too many variables. And they mention here accounting for variables and stuff like that. I think there are just too many variables. Who's the guy I'm thinking of...who did the experiment with the, I'm associating the word ducks with this for some reason. Is there a guy named Mallard or something? I just think this whole notion of conditioning, that you can put people in a box and stimulate them **[Interviewer: Was it Skinner, that you're thinking about?]** Yeah, Skinner box, you're right. Well, I don't know enough about Skinner but it's conceivable it was Skinner. I guess that I'm just skeptical ...Put me in a city for a week and let me just kind of wander around, and talk to people and, ...I'll have confidence that I'll have a pretty good sense of what's going on there, what people are concerned about, what their behavior is like, ...what their fears and aspirations are and I just have more faith in that, than laboratory-type experiments...in the social sciences. ...We're not talking physics here, or chemistry. And if this came across my desk, I probably would not do anything with it."

He further explained,

"It's tough to do a story on it because it's tough to prove journalistically that meditation reduced the number of car accidents in Jerusalem ...where they drive like maniacs. I just wouldn't believe it! And I would assume that you could also correlate during the same thirty day period, maybe, there were fewer clouds in the sky, why not attribute it to that? Or maybe it didn't rain as much, which is a much more concrete reason why there would be fewer car accidents. I just don't have faith in this. ...To be quite frank with you, ...if I were going to use this at all I would see this as something to gently poke fun at; I mean, it's ...like writing about EST... In fact, I did that once; I wrote about Werner Erhardt [who] started a new group in the 80's for yuppies... And I did it ...to sort of gently poke fun at it, that corporations were paying this guy a lot of money for what I thought was just really a bunch of bullshit. ...I got to know him; ...he's actually... kind of a charming guy because he realizes that it's (chuckle), it's why I'm prejudiced but-- You sort of get the impression when you talk to Werner Erhardt ... he's one of these charming psychopaths. He always had this twinkle in his eye as if he would say to you, 'I know this is all a game but I'm pretty good at it aren't I?'"

An IPPME story would have limited play in Rubin Klein's paper, except through an occasional column that he writes. The column features subjects that he judges to be "relevant to

Israeli readers; because it's fun to read it; it's unique, it's interesting, it's exclusive; [or he considers] whether my partners here, my colleagues, my competition are going to write about it." He compared the IPPME study to one that correlated hot dog consumption with development of childhood leukemia, which he considered evidence "of bizarre stories of all kinds of bizarre researches that people are doing. If you are going to spend a few years on research and somebody is financing you, you have to come up with something." Regarding IPPME, he said, "There are a lot of figures here and graphics and references and it looks relevant to begin with, but..." If he did cover it he 'would worry about his reputation.

"When I say that I'm not very good at [style stories], I think that I'm more worried about my image as a serious correspondent. ...If I write about hot dogs or this stuff, I will write it in my personal column ... with some flavor or some humor. ...I won't take it too seriously, so we want this one to fit into a hot dog story, see? Say the Americans are doing all kinds of bizarre researches, here is the one that we Israelis are involved [in], shows that maybe ...the solution is there. So I won't give my name to a serious story on Maharishi technology, because I know that ...the reader is someone that probably thinks about the Maharishi technology what I think. So I would like my name to be attached to a serious story about-- Like my hot dog story, I won't write a news story about hot dogs and leukemia. Because to me and to my judgment and what I've learned about statistics is enough to be very skeptical about it." <sup>335</sup>

Klein notes that social science poses a challenge technically and in displacing other priorities:

"Perhaps it's ...how much time it will consume because if I do this one, I won't do something else, so again it's priorities. And in order to *really* understand the technology and since I found it a bit vague, I would have to go into it to get some more material and read and perhaps talk to some people. It's relative proportion; how much time this is. And what do I know about it... But what I know is from a few articles that I read and perhaps speaking to ...other people in the past and not being an expert on this one, I would have perhaps to speak to more people and I would ask myself whether at the end of the day it will be worth it because I could use this time in better ways."

**Judith Gaines**, who would tend to look at IPPME research in the future, described her own difficulty checking out the science for her second story about the Maharishi Effect:

"How many readers care about what times series analysis is? If I had been doing it for science or if I had more space, ... it's important, but I have to be honest here. In journalism you don't really have time ...to analyze somebody's methods in depth. It's not like academia. ...My editor did not want me to do this story ... and only gave in because I came back the second time and said that I thought it was important and I wanted to do it. ... But it was clear that she was never going to give me a lot of time. ...I basically had a day to research the story, which was Sunday because that was my own day and Monday to write it. And that was it. ...I did a lot of interviews on Sunday. ...Journalists often work at that kind of pace and you don't have *time* to go behind somebody's arguments and really look at how solid the research is."

However, she judged the research to be crucial. "That's the validation of the idea," she said. Like three other reporters, she said she would rely on the judgment of social scientists or people that they trusted.

"People like [the editor] found it to be internally consistent. ...I am not a mathematician. I'm not really in a position to evaluate, ...'Did they do the regression analysis or time series stuff-- did they do that properly or not?' I can't do that kind of evaluation. Somebody who can had looked at it and said that it seemed to be done well. That's enough."

Like Gaines, Nielsen consulted an expert, a psychologist, (who he later said was his wife):

"...Given what I do know about psychology, I know that ...bona fide psychologists would question this. ...You're taking what some would call a spiritual or religious precept, what others would call mumbo-jumbo and trying to fit it into a scientific framework or straight jacket. ...It comes from, where, the Department of Psychology, Maharishi International University. ...So right away, it's outside the main stream. ...I am aware also, that there are breakdowns and divisions in the psychic-psychology/psychiatry field toward the behaviorists, the Freudians and so forth and some Jungians and things like that. I mean, a psychologist I know, what I would call a mainstream psychologist, would look at this in the same way that a hard scientist, biologist, physicist, chemist or geologist would look at creationism biology. He just doesn't accept its validity."

### ***Skepticism about research integrity***

The fact that "International Peace Project in the Middle East" was conducted by people associated with Maharishi International University raised, according to Rohan, an immediate first question: are the authors biased?

"Well, I'm intrigued by it and interested enough in it to put my reporter's hat on and ask a number of critical questions. The first is, what independent outside verification has there been of this research? By that I mean... that Orme-Johnson and Davies and Chandler are from the Maharishi International University, which is where? **[Interviewer: It's in Fairfield, Iowa.]** As a reporter, [the] immediate instinctive question is: are they unbiased or are they in the business of promoting the Maharishi's views? And if they are in the business of promoting the Maharishi's views, then perhaps we ought to have independent, outside academics take a look at this phenomenon to see if in fact they get the same results. So my first question as a skeptic is to immediately wonder about the source of the research and whether in fact it can be verified that the correct controls were used."

MacBain expressed the question in more emotional terms.

"Something else aroused my interest immediately. It's the credits. We have here ...David Orme-Johnson, Dept. of Psychology at Maharishi International University. I have no idea about Maharishi International University, where it is, but I suppose it is tied to the Maharishi, wherever he is. Then we see Charles Alexander who borrows the name of Harvard University, right here, Dept. of Psychology and Social Relations, Harvard University, but there is a star there. Down at the star, it says that he is currently at the Maharishi International University. In other words, he joined the cult. And then John Davies, also Maharishi University and Wallace Larimore, Computational Engineering, Inc., which can be absolutely nothing. I mean anybody can create a company. So, the lineup immediately is very interesting. Putting Harvard in there is an attempt to give it additional credibility. Thank God there is the notation at the bottom. It would arouse my suspicion. It is attempting to use the name of Harvard to give this credibility. Which clearly is, and this guy clearly is no longer at Harvard. Harvard to me is a sacred institution. That has credibility. Maharishi University has zero credibility. **[Interviewer: Well, if these people had come from Harvard University--]** Had come from Harvard University, not good enough. If they were still at Harvard University, that would be a whole different ball game. I would take it more seriously. I would certainly have called somebody at Harvard and said, hey, what the hell is going on? What is this Maharishi business? But they are not. Obviously this is somebody who used to be." <sup>2</sup>

Taft's assessment was that IPPME's statistics were "cooked," and that his readers shouldn't waste time reading about the study. At this point in the interview he was incredulous about the levels of results and described the difference between IPPME and the statistics he had cited in a recent story:

"They're telling me how much the GNP increased last year. That's a whole lot different statistics than [IPPME]. It may be wrong but I can assure you, it's not wrong by a magnitude that this is wrong... It's just instinct. ...If they're telling me to use those two examples, if the U.S. Embassy in Damascus is telling me that the Syrian economy has increased by 7.2% last year, after adjustment for inflation, maybe it was 6.1% or maybe it was 8.2% but it wasn't 93%. When I look at, when these people are trying to tell me that auto accidents in Jerusalem *decreased 34% because of this*, which is what they're saying, then I say, I doubt it! ...The statistic is very, very suspect to me. Very, very suspect."

**Nunzio Nicosia** judged the statistics suspiciously after reading a newspaper report of Maharishi Effect research conducted in June and July 1993 in Washington, D.C. *The Washington Post's* Laurie Goodstein said in the article that her scrutiny of the police blotter showed different results than those described by the Maharishi International University scientists: a 9% increase in rapes and a 50% increase in homicides compared with a 13% decrease in violent crime described by the scientists. She also wondered how the scientists accounted for the gunman who fired a

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<sup>2</sup> Co-author Charles Alexander was a Harvard post-doctoral fellow at the time the study was conducted, but later moved to MIU.



semiautomatic handgun at children in a Southeast swimming pool two weeks after the Maharishi effect was supposed to be kicking in.

Nicosia was the only respondent in the study who reported reading the article, which prompted his swift decision to discount IPPME and any other examples of Maharishi Effect research.<sup>336</sup> He said,

"There's just no way to quantify that kind of stuff. ...You can't say that because 600 people all meditate simultaneously and the murder rate goes down, that there's a causal relationship there. There was study done ...in Washington, where the same technique, the TM people tried to get a bunch of people meditating simultaneously in Washington and then... take a look at the crime statistics in Washington during that period. And the murder rate went up. And the TM people lied about it. They said it went down. And people then said that's not true. Here are the police figures. It went up. It was just nonsense. It's like saying ...if we all sunbathe... the murder rate will go down. I'm not saying that TM is not good for you: I do it myself. And I find it personally very useful to relax me and to help me go to sleep and... to make me feel rested on a personal level. But there's no connection... between that and a political, social, tribal situation that is so complex. ...When you have an occupation that's been going on for 25, 26 years... and the daily humiliations that go on, the violence and the passions that are part of that situation, ...meditation is irrelevant. It's totally irrelevant."

Nicosia relied on Goodstein's report, her checking of the police blotter, and especially her interview with the police department's researcher, rather than checking the MIU scientist's data and methodology. He explained,

"...They said that the murder rate went down during a certain period<sup>3</sup> and the spokesman of the Washington D.C. police, said this is nonsense, the murder rate went up. So who do you believe? Do you believe Maharishi Mahesh Yogi? Or do you believe the police? I believe the police.... As a journalist who has to look for credible sources, I see that they cook the data, why should I believe them? If I can't believe them in Washington, why should I believe their data in the Middle East?"

The anecdote about the semiautomatic handgun provided graphic emphasis to the overall picture Goodstein painted, but she did not report that the meditators needed to create the predicted effect were still being assembled at that time, and their numbers did not reach the highest threshold until two weeks later. Such a report would have required a more serious examination of the theory's fine points. She also did not note that, while six D.C. children were injured and everyone terrified, no one was severely injured or killed.<sup>337</sup> Goodstein, perhaps as skeptical toward social science as some of my respondents, did not understand the difference in statistical significance between one incident and the overall trend that the MIU scientists were examining.

The press releases that Goodstein was likely to have received from its organizers over the course of the experiment did contain different figures from those described by a police

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<sup>3</sup> The scientists did not claim that the murder rate went down, but that the rate of violent crime was substantially less than what had been predicted by time series analysis.

spokesperson. But they were not as different as Nicosia recalled (i.e., the increased homicides were acknowledged), and they cautioned about "drawing any firm conclusions before the independent scientific review board releases its findings."

The final report of the analysis was based, not on the police blotter, but on "Radio Runs provided by the District of Columbia police department." The radio run data were found to be reliable "when compared with official DCMPD Database for previous years and the official FBI data." The report concluded that toward the end of the Washington assembly, "when the group was largest, actual crime decreased sharply below the predicted level. This approximately 18% decrease was highly statistically significant ( $p=.003$  for daily HRA crime analysis;  $p=.0008$  for weekly HRA crime analysis)."<sup>4</sup>

Nicosia, however, was not likely to consider the final report or revisions of the police spokesperson's position unless his boss asked him. He did not question Goodstein's report and he did not mention scrutiny of the IPPME report whatsoever, except in the context that the MIU authors were not to be trusted. Goodstein's story served to reinforce his estimation, based on 17 years of reporting from the Middle East, that such an effect would be impossible.

"TM is like I said, I do it, it's very, very useful for me, I use it to relax, ...it lowers my blood pressure, it ...lowers my pulse, it helps me sleep at night, it's a wonderful, wonderful thing. But to think that you can take that wonderful thing and apply it as a tool for conflict resolution in this kind of situation is ...not possible."

### ***Relevance***

Like the other reporters who have lived in or are from the Middle East, Nicosia's skepticism was grounded in his experience there. He described the real story of the Middle East as "psychological," "tribal," "wrought," and "personal," that is, having to do with whether "if I exist or if you exist, do I cease to exist?"

He described the problems of Israelis and Palestinians as "so massive and so chronic and so systemic that TM is less than a band-aid. It's nothing. It's totally and utterly irrelevant." One reason that Nicosia judged TM as irrelevant was that he said the people there were not interested in conflict resolution, but in obliterating each other.

"Events on the ground have a tendency to outrace the deliberations of the diplomats, and what a lot of people miss ...is that players over there are not interested in conflict resolution, they are interested in victory. ...There is an assumption here that everyone wants to work out their differences. Look at Bosnia, this is the perfect case, look at Rwanda, you work out, you resolve the situation by killing the entire other tribe. You don't resolve differences and shake hands with the enemy. You obliterate him. ...In a tribal conflict, ...that's how things are resolved."

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<sup>4</sup> Final FBI data, released in the summer of 1995, showed a decrease of at least 20%

Relative to the level of wrath involved in the Middle East, Nicosia didn't distinguish between meditation and social science, but saw both equally ineffectual to deal with children being shot.

"The core issues are political and personal and matters of identity and dignity and political independence and economics. ...Social science stuff and ...a bunch of people meditating simultaneously does not address that. All it does ...is it relaxes the individual. But when he comes out of his restful state, his problems are not solved. ...He still has his house blown up. He still has a kid who just got shot in the spine. None of that is addressed by that. ...It doesn't encourage or maneuver the situation to one where the two tribes feel unthreatened by each other. These are deeply religious Biblical things."

The reporters I interviewed who determined that they didn't buy the concept, its origin, or the causality described, also dismissed the science. The bottom line, especially for the people living in the Middle East was: 'is it real?,' a question asked on two levels. One was the level of conflict described by Nicosia. The second was concerned with the political nature of the problem in the Middle East as seen in Washington. Nielsen noted:

"First... is the question of whether or not this is an idea based on a legitimate premise. I don't accept that it is. So, I mean, it's a stillbirth before you even get to that point, in terms of being something that would be seriously considered. But if you were to leap that hurdle and find some way of establishing the fact that there is or might be some validity to this, then it is still something that anybody who understands the realities of the situation they are trying to apply it to, knows is impossible of implementation. So, it falls on that score even if it clears that first hurdle."

MacBain asserted that political problems cannot be solved scientifically:

"You raise a red flag if somebody comes and tells you, I have a scientific solution for a political problem. You have a scientific solution for the Bosnia problem? What is a scientific solution? You have a scientific solution for the India-Pakistan war? What is the scientific solution? There are no scientific solutions to sociological historical problems. There are sociological and historical and other kinds of political solutions. Science resolves problems of the body, problems of the mind, problems of medicine. Science--except ...sociology--does not address social and political problems. ...Secondly there is no track record of scientific solutions to social problems. I don't know of one. Do you know of one? I never heard of one. ... People hate each other, ...for different reasons. Reasons that perhaps I can relate over a hundred years. If the scientific problem is to change the character of an entire people, ...I'm certain this is not feasible. What are you going to do? Prescribe Prozac for the entire population of Israel? And the West Bank and Gaza? That's a scientific solution. It will calm them down but is that a solution? Or for that matter, ...some Scientology type [of] esoteric solution like...calm down and meditate? ...This is a contradiction in terms here. This is as I said oranges and apples. You are not dealing with the same thing. So ...no matter how this is packaged, I don't see how it could sell."

### ***Primary test, relevance***

**Barry Berman** dismissed future consideration of IPPME because he considered it irrelevant to "the debate."

"I don't think that reporters are there (although sometimes they are perceived as doing so) ...to define the political debate or necessarily to steer it one way or another."

Berman said that there were many ways that one could analyze it but it wasn't worth analyzing. He explained,

"If it's in the debate and if it's something top level policy makers are talking about: ...if others beyond the folks who may have an ...immediate interest in it are talking about it; then it's ...in the middle of the debate and then it's in the game."

Whether or not he found it reasonable personally was not important for him to consider, "any more than religion or ethnic priorities or prejudices or anything else should... be a major determinant."

### ***Challenge to the status quo***

Nielsen described a very high threshold that he has to observe in his job for determining who actually gets coverage. He explained that there are many theories, many possibilities, and that it's really the status, the credentials of the world statesman that determines who he will study:

"I have a speech that's being delivered today by Jim Baker down in Texas with some interesting ideas... about how this administration should be conducting its foreign policy; I would like to do something with it but I've got some more immediate things. Jim Baker, if he was making the speech while he was Secretary of State, would have been interesting and important but he is now one of yesterday's men.... He still has stature and credibility but not as much; ...he doesn't have the authority and the power. So, he moves into a secondary kind of a category. ...If he was still Secretary ...I would drop the other things I'm doing to give that primary consideration; instead it... has to fall into the line behind."

That kind of consideration definitively took IPPME off the map in Nielsen's mind. Rohan articulated another fairly automatic question: "Why haven't I heard about this before?" MacBain said that someone would have had to hear about it before for him to cover it. He explained that everybody is selling something and "It's a very dangerous responsibility to take something like this and to tell your average, basically innocent reader, 'Here, I nominate this to be taken seriously.' Big responsibility." MacBain continued,

"You check out an idea that is already there. You don't check out some esoteric idea, with a slant that you generated in your own mind. ...Some people have a completely wrong idea of what reporters do; they think they create things. We don't create things; we report. The name reporter is literal. You report what you see. If you are a good reporter, you report what you really see."

MacBain explained that the idea would really have to exist already and it would have to be fairly pervasive in society. It would have to "catch fire." Commenting on IPPME, he observed, "This is not a good idea and won't catch fire." He said that he would be stunned if anybody took it seriously, and that if he took it seriously, "I would make a fool of myself." These sentiments were echoed by several of the reporters. For example, Nolan discussed the risk involved with a new idea and the balance one has to strike between whether something is news in the first place or, if it is, then if it's too hot to cover.

Reporters also discussed how their editors would respond, i.e., that the idea simply would not sell. However, as Rubin Klein explained, he would have dismissed the idea long before considering whether his editor would consider it feasible.

***The Exception: Reporter who chose to use the International Peace Project research***

Gaines had written two articles in which she mentioned IPPME related research (3 paragraphs in the first and 7 in the second.) (See Appendix H.) She submitted the second as a newspaper staff member just before I interviewed her. In it she described some results of the IPPME study and a follow-up study that has not yet been published. Gaines ran into difficulties in selling the stories to her editors, succeeding the first time but not the second. Later, she included the Maharishi Effect in a story about the Persian Gulf War. Gaines exhibited an unusual independence from worry about what other people thought, and had no reluctance considering new ideas. She was aware, probably from the very beginning, how others might perceive this idea in her profession: "The automatic tendency is to flip off," she said. Space in a newspaper is tight; not everything can be published. "The problem with this kind of story is that people don't make a quick connection. They need to build confidence. They need more background." When she wrote the first of her two stories (which was printed in 1983) she asked the editor, 'did you think it was a little far out?' and was told no-- that it was interesting and topical.

Gaines weighed the plausibility and even believability of the Maharishi Effect ideas against the quality of the research, which impressed her. Her main impetus, however, involved a test of relevance. She decided it was a lively idea. Also, she felt that the people involved were credible and consistent.

Gaines' "offbeat" beat and her working philosophy distinguished her from the other reporters. In 1983, she was looking for stories in the Midwest that she knew were not being covered by other reporters. Her philosophy guided how she approached a new idea. Her approach to the Maharishi Effect research was similar to Berman's, who said that as a reporter on principle he would not judge the research "pro or con." She said,

"I don't think my personal opinion of whether it's actually true is relevant. But if you asked me, do I actually believe in the Maharishi Effect? ...I would probably say no. ...But ...from my point of view it's not relevant to ask whether I think the research is true. What I would say is that I believed that it was internally consistent. I believed that it was serious. I believed that it was an argument that deserved to be presented in a straightforward way so that people could make up their own minds, and that's what I tried to do as best as I could. ... When I'm covering an election, it doesn't matter what I personally recommend. The issue is what is this guy saying."

Gaines' thinking about her judgment went beyond principled reflection on her role as reporter and into the realm of whether such judgments can be decisively made at all. She recognized how different her views were from most of those in society.

"But then if you ask me a few other questions: Do you believe Jesus Christ is the Son of God, I'd say no. Do I believe in heaven and hell? No. Do I believe in cause and effect relationships period? I would say no... You have to understand how deep my whole thing about this is. I'm probably never going to reach that point where I say A caused B. ...If you really pressed me about what do *I* think, not me the journalist, but me the private judge, who never wants to judge... If you pressed me to the wall, I might say in the end, 'There seems to be a high likelihood that there is a relationship there.' I'd never say A caused B. I think in general what's happening in the world is that cause and effect relationships are appearing to be much more problematic than we thought they were. ...Things are more complicated and subtle than we realized. ...Think about it in your own life. What causes you to do anything? Do you really know? ...Maybe you had blueberry muffins and you liked them and so therefore you decided to write a thesis. ...You felt like writing that day. Maybe you were mad at your husband. Maybe ...a whole range of stuff. Who knows why anyone does anything? ...I think I live in a world where most people believe in Jesus Christ and I live in a world where most people believe in cause and effect relationships and a lot of other things. What is a big deal to me -- a really big deal to me -- is I want to do them justice."

Gaines explained that she approached the Maharishi Effect story as she would any other, i.e., not asking what might be wrong but why people think what they do.

"I decided ahead of time that it was interesting enough to merit taking a look and however it came out was how it came out... Honestly, in my soul, ...I really don't care whether its right or wrong. I don't even think it is enlightened to ask that question...I look on it as a lower level question. ...What do we really care about? We care about understanding each other better. That's all. And we can argue any question. ...The more you work as a reporter and the more you live, you see that any issue will have people on both sides... And they are often well intentioned and they have reasons for what they think. And getting involved in that whole level of who's right and who's wrong is not ...in my opinion an important thing to do. What's important is to understand: Why do people think the way they do? What is it that they think? And then, what context are they in that it makes sense to think that way and come to those conclusions? And the job of the reporter is to try to communicate that... The basic question: Is it something worth writing about in the first place? Is it a topic that people are interested in? Is this an answer that has a certain amount of credibility?"

Gaines' first consideration in weighing the Maharishi Effect story was that it seemed to be a reasonable idea. Aside from her ideas about causality and her approach to truth, she had a background, framework and a perspective that allowed her to be comfortable with something like this idea.

"I've been interested for a long time in ... maybe mental answers is the best way to put it. I ...played around with Christian Science for a while. I'm generally interested in the way attitudes, just plain old attitudes, affect real concrete realities, in a way that people would normally say wasn't possible. ...This is actually what I learned from [a former editor]. I would tell her various situations that I would be in and she had the ability [to] drop the thought, just a thought, not good, bad, anything, just drop a thought that would completely transform the situation for me -- like the pebble in the water that makes everything different. And I have seen that a lot. In journalism you are always pressured. You have three hours to

get some place and do the story, and write it. You're always under pressure. And in that kind of situation, I think you're particularly suggestible. I don't know quite how to say it exactly, but you can see how something that works at that level of thought can change everything ... much more dramatically than the difference between being with someone [who's] in a good mood, bad mood. So the idea of people meditating and sending the thought that transforms the whole scene ... seemed logical to me from that point of view. And, I'm generally interested in the way --I don't know quite how to say it, but-- the way vibes sort of work."

She also voiced interest in ideas that were not intuitively obvious, such as the square root of one percent in IPPME. John Hagelin, a physicist associated with the IPPME researchers, clarified it for her with an analogy:

"We see this phenomenon in all kinds of situations. For example, a beam of light -- we only need a small number of photons marching in perfect step to make the whole beam coherent, so coherent that we can shine it all the way to the moon and back and measure the distance - - and that's how we measure the distance from here [to the moon]. I thought that was a convincing analogy. I thought it was *real* interesting. In fact I was talking to Jim, the TM spokesperson, and it made me think again about the whole idea of Ayurveda, because given that the brain, according to Jim, is about 5 or 10% of the body, not the square root of one percent-- If you can get the brain waves orderly you ought to get the whole body orderly. ...I suspect that a lot of things in the world work the same way. ...I don't think human beings are all that different. ...I'm quite prepared to accept that there are a lot of basic principles that have that effect -- things like order, disorder."

Once she had decided to do the story, Gaines found the research crucial for her own understanding, although she did not go into it deeply in the story because she didn't have time.

### **Summary**

In Nielsen's description of his "subconscious calculus" in confronting the IPPME research he identified steps or elements that other reporters also used. He spoke of his innate prejudice and his immediate (and incorrect) associations of Maharishi with the Oregon "con man." Associations like this were not as off-putting for some, but for others, were decisive. The other association Nielsen mentioned, the feeling that TM was a religion, compounded the discomfort felt by four other reporters who thereafter would not then continue considering the research. Other reporters did not accept the idea that a small group might affect such a large population.

The journalists generally had difficulty accepting or even discussing an idea which challenges basic premises normally left unchallenged, e.g., action at a distance. The exception, Gaines, had a framework for investigating such a possibility, at least to the point of asking questions about it in order to determine whether it met her criteria for consistency. Others did not have such a framework. Rohan proposed an alternative explanation.

Kingsley Taft, who knew of the associations that others could not overlook, was not so affected, probably because of his long term awareness of TM and meditation among his friends. He and two other reporters were stopped, however, by the sheer improbability of a small group affecting a population at a distance. Taft concluded that the numbers were "cooked." He was one of

seven reporters who arrived at the conclusion that IPPME premises were neither possible nor testable.

Rohan didn't believe the results, but would ask experts to look at the science. Gaines, though she didn't necessarily believe the outcome, considered the research premises to be possible and testable, largely because she had a different kind of beat and no anxiety about exploring the concepts. In fact, she found them lively and topical, and had a framework and contacts for exploring them.

Berman felt that a reporter's job was to report on what is relevant. In other words he applied what Nielsen called the bottom line for a Middle East diplomatic reporter: "Is it relevant to the debate? Also, is it a serious story to the *players* in high diplomacy?"

Three other reporters, a meditator (Nicosia), a reporter familiar with TM people professionally (James) and MacBain, all passionately felt that the research was irrelevant because of the nature of the Middle East and the realities of the centuries-old conflict. MacBain's passionate response was cultural, based on the negative associations of gurus, cults, etc., coupled with his feeling that such ideas had no chance of solving long hard struggles. Nicosia didn't have the negative associations with TM itself, because he meditated. His passion arose from 17 years in the conflict and a judgment like MacBain's -- that an intractable conflict was beyond measures like the Maharishi Effect, and what struck him as lies by the TM people.

While he found meditation beneficial, his practice did not give him a framework to see the IPPME research as plausible; he didn't consider himself a cult member. The overwhelming factors in his consideration were the reality of covering the Arab/Israeli conflict and the influence of a fellow reporter's investigation.

### ***Types of Truth/Utility Tests Applied by Journalists***

Determination about truth and utility involved themes predicted by Weiss (1980). Specifically, were the concepts emotionally acceptable? Was the causal explanation offered believable? Was the source of the research credible? Would such ideas be effective in the face of horrific levels of conflict, or culturally acceptable in the region, much less acceptable among the principals in the political game? An additional consideration raised by MacBain was, 'Is the idea pervasive enough to be reported' -- i.e., is it even capable of "catching fire" in the general population, U.S. or Middle Eastern? Is it too controversial and therefore too risky to report?

### ***C. Congresspeople***

The following interviews represent views from the offices of three Senators and three Representatives (four Democrats and 2 Republicans), four of whom were chairs or ranking members of committees or subcommittees concerned with U.S. foreign policy in the Middle East. A fifth was a ranking member in a past Congress. Nine interviews were conducted: one with a



Senator, two with Representatives, and six with staff members from the House and Senate. Two staffers were personal legislative aides (one for a Senator who declined the interview and the other for a Representative who was interviewed). Four other staffers worked for foreign policy sub-committees.

An interview with a committee chief of staff responsible for a hundred people was incomplete because he lost the material between the first and second interview. He didn't have enough time to both read the summary and participate in a full scale interview. His comments which followed a quick reading, will be noted. He did not, however, comment in sufficient depth for comparison. Similarly, one of the Representatives, a ranking member of a sub-committee relating to Middle East foreign policy, had no time. When his aide and I met the Congressman, we spoke about his experience with foreign policy, and then he quickly glanced at the IPPME summary and declared that he didn't think that TM would be helpful with foreign policy decisions. His committee staffer explained that this was typical of the amount of time that he would give to written material. My comments below are therefore confined to seven of the nine interviews.

### ***Overview of Responses to the Research***

Three of the seven respondents within Congress were unlikely to take IPPME research into account in the future. The remaining four would take the research into account if and when it became less challenging to constituents. The guiding factor for Congressional respondents were relevance and feasibility. Relevance for most respondents meant subjecting the information to a demanding yes/no test that caused most staffers to toss 80-90% of the mail they received without looking at it, and which admitted maybe five pieces of mail per day. In Congress, constituents' perceptions were integral to the question of relevance. Two respondents also questioned the legal relevance of the Maharishi effect programs.

All of the staff members discussed the importance of presentation of ideas in Congress, given the enormous flood of incoming information and the percentage which can actually be read. This was discussed by the two most experienced staffers and was emphasized by the Committee Chief of Staff, Kevin Dunfey, whose interview was cut short. Pointing to the graph included in the summary, he said,

"My audience has no time for this kind of stuff; absolutely none. I mean, partially because I don't understand it but partially because it's just -- it's on a different kind of level... I mean, I think that the packaging is terribly important ... It's GOT to be packaged an awful lot differently than it is here ...it's got to be a lot shorter. It's got to look at the problem in the macro sense and it's got to state very succinctly and very quickly how that kind of working with individuals can have an impact on behavior and conflict. And how this as one of several approaches to conflict resolution, how this has it's place. "

Scientific or academic presentations, according to two of the seven respondents, Anna Roach, legislative director for Representative Jack White, and Patricia Thomas, Committee Aide to

Representative Noah Berger, should be "avoided at all costs." With White, they were the most dismissive of IPPME and also did not consider the scientific argument to be significant or persuasive. Most Congressional respondents did not engage fully with the scientific argument that was presented. The four who were more likely to take the research into account again, should they encounter it, took the scientific argument more seriously, but did not refer in detail to the scientific concepts or language.

Among the four who would look at the research again, the two who appeared most likely to ever consider it had direct personal knowledge of meditation; Senator Marcheselli meditated regularly (whether TM or another form was not discussed) and committee staffer Brian Banks' parents had practiced the TM technique. Two senators represented by staffers also had more direct experience: Committee staffer Rumar's boss, Senator Toland, had a personal interest in "different types of religions and different types of meditation." Committee staffer Glaser's boss, Senator Shiff, had been professionally and extensively involved in U.S./India relations (which may have given him more opportunity to know about meditation). Rumar and Glaser each expected that the senators would keep their views private. Independent of their Senators, the aides would be the second most likely to consider the research, should they encounter it. More direct experience of meditation may have given Marcheselli and Banks a better handle on the study, but they were also skeptical.

***A Senator and a Representative -- Truth Tests & Perception of Relevance: Overriding Consideration Political***

**Senator Ralph Marcheselli** had served in the House and Senate for twenty-one years; **Representative Jack White** had been a Congressman for four. The overriding consideration for both was political. As the Senator put it, one might be "tagged as a weirdo" or "cuckoo" for trying to "allocate national resources for rather extensive programs in this regard." White mentioned concern for the opinion of his committee chair. He had much less experience than the Senator. He may have rightly considered himself more politically vulnerable and was also unfamiliar with the TM technique. His first test of IPPME was for utility, specifically how others might judge his association with ideas that challenged the status quo.

"I approach things more from the level of am I going to be asked to help *pay* for it or in some way promote it? If the answer to that is yes, then the test I use is well, does this make sense from the point of view of the people who sent me here? At first glance, this is the kind of thing that people would use as an example of outrageous examples of misspending of their money."

White had earlier explained that when confronted with information, he decides *whether* to decide, and estimates the cost and benefits of making use of his staff's time. He described his working philosophy and principles. He said he had learned over his first term to "try to pick a few

things and really do them, rather than pick a multitude of things and put out a lot of press releases and not do much at all." White explained that one begins to know intuitively which activities will prove to be an unproductive investment of time and resources, and which "will pay off in terms of a viable product," relative to legislative agendas or constituents' concerns.

White reiterated several times that the basic decision he made regarding IPPME was whether there was reason to justify examining it more closely -- for example, getting more information about scientific validity. He decided, "There's no reason to justify my doing it," because it "isn't relevant to what I have chosen to do." "Even if a constituent with whom I have a long-standing relationship" -- who could perform appropriate analysis -- "said you really need to look at this because this is terrific," and White looked at it more carefully, which he said he would do in that case, and even if that constituent recommendation removed some of his skepticism, "It will ultimately come to the relevancy test." IPPME held "a smaller bit of relevance" in White's "universe of priorities" than even the interest a constituent had expressed the day before in having a "law changed so that people who investigate [certain crimes] in one state can be certified to do it in every state." In that case he assigned what he estimated to be 12 hours of staff time to drawing up a small piece of legislation. In the case of IPPME, he said,

"The thing is, [would] knowing more about this enhance what I'm trying to do? If the answer is yes, I try to learn more about it; if the answer's no, I try to get a conceptual understanding of it, I [don't think I] should reject it, but I sort of shut off the planning mechanism in my mind and say OK I'm just going to absorb a couple of facts."

Having decided IPPME was not useful, White decided to not take the time to find out more. He decided not to apply a truth test. His response to the scientific aspect of the study was,

"I don't know much about probability theories and statistics, but I know enough to know that there could be literally thousands of other explanations as to why that happened. And before I would want to invest any credibility in the proposition of this, I would want to know a lot more about it and see a lot more research."

According to his aide, White laughed at the IPPME information. He was relatively guarded in speaking about his reactions, describing only toward the end of the interview how crazy his constituents would consider the concepts.

In contrast, Senator Marcheselli approached new ideas more openly and had a framework for taking IPPME more seriously. He explained, "First of all I think you have to have an open mind and an inquiring mind ..." He referred to his own experience with an Eastern discipline which involves meditation and his involvement in college with a study of "ESP sort of things." He concluded, "So I understand meditation a little bit better than perhaps some do. I'm not alien to understanding and believing in people's ability to utilize their mind in such things as transcendental meditation ... as a way to get people to be able to react appropriately to stress."

Marcheselli's experience with meditation may have allowed him more comfort with the concepts. Toward the end of the interview, he explained that his participation in martial arts was covered by his State's top newspaper. His staff had great reservations, but he took the risk and said it turned out positive politically because the public agreed with his aims.

"You have to be confident in what you're doing; to have some goal in mind that you want to attain. And to me, to try to re-establish a sense of self-esteem and self-confidence and discipline in young people who have no structure around them is very important and that the utilization of the techniques of the Orient cultures to accomplish these things is seen [even in the popular media to be of value.] ... We can borrow from other cultures ways to help resolve conflict other than through violence."

The newspaper reported that 'without meditation daily he could not accomplish the overwhelming work of the Senate' and quoted the Senator's suggestion that 'East and West could benefit from each other.'

Marcheselli did, however, apply a truth test. Though he was comfortable with meditation to the point of identifying himself with it in the press, IPPME described a phenomenon beyond his experience. Referring to his training as an engineer and scientist, he questioned whether such an approach would be practical in dealing with such things as a crowd frenzy, which he had witnessed when he was in the military. His most reiterated concern was that one would have to "examine the desirability of generalizing from relatively small group studies to a larger situation."

"I mean when you see the people just out of control, it's a little hard. I suppose if you've done the preliminary work with the groups and try to train them how to handle stress and those kind of things. But how you can generalize that to a large population in the sense of how it's going to work ... I think it would take a lot more of a demonstration of the efficacy of it on a broader scale to be able to convince that it ought to be something followed up with resources."

Marcheselli's truth test was posed within his political context. He said, "It's a little hard to think if it's an attainable goal to try to utilize it for [larger groups]. He was referring to feasibility of implementation and political feasibility.

"Well, when, would you have to start? Could you start with an adult population? Or would this kind of a program have to start with young people, so that in their years of schooling, they are informed as an appropriate, acceptable, in fact recommended way to, to reconcile their differences and to attain the state of mind where they could be receptive to working out things rather than through violence. My own feeling is that if we try to start with the general population now with people who are not only probably not receptive but even alien to this kind of an approach, it would seem very difficult to have a workable plan to put into effect."

Marcheselli's questions about how the technique would work for groups and how it would find wide acceptance were at the heart of the primary judgments made by Congressional respondents. The Senator had only read the summary "to an extent," but not enough to fully grasp the concept of the group intervention; to him the summary did not explain why such an intervention might be expected to work.

While Marcheselli is known for political risk-taking, he explained in the context of considering alternative approaches for U.S. foreign policy, he checks to see whether it can be demonstrated, so that he can confidently communicate with his colleagues.

"There is a tendency of all of us to see things through the eyes of somebody who's lived in a certain type of culture and so you've always got to make sure that you don't get over there, you're telling them what they ought to do and that has no relationship to what reality is. Because you've got to have some way to convince your colleagues that the change that you want to make has enough sufficient credibility from others that are knowledgeable and know what is going on that you can convince them what you think ought to be done."

Though Marcheselli recognized the political risks involved in IPPME, he did not judge it to be irrelevant, as did White. He considered the ideas potentially relevant for the resolution of conflicts -- either in the Middle East or in core inner-cities. "Certainly the ultimate goal is one which is very desirable and meritorious and would be very helpful. The question is how would you go from their studies to a broader [application.]" Extrapolating from his experience that the public can become more receptive to ideas like meditation, Marcheselli suggested that political problems would not always be insurmountable; that once they had been overcome, "some sort of funding to do some demonstration projects of a much larger size or for an even longer period of time" might be obtained from an alternate conflict resolution group.

Marcheselli reached conclusions similar to those of other Congressional respondents: that the IPPME information posed political risks and that funding such programs, once certain questions were answered, would most likely come from outside of the Congress. In considering its feasibility, however, he admitted the study to the realm of possibilities worthy of examination.

### ***Time Considerations for Members of Congress***

Legislative director Anna Roach explained she had spent the prior three years honing her skills in presenting the most information in "the most succinct way before I lose his attention ... I probably have a good two to ten minutes... to present a new idea or an argument [each day]." As White explained, though, Roach screened all of his information.

"The only flow of information coming to me is from her. The biggest entrustment that I give to my staff people is to screen information for me. I trust that they will give me the universe of information, it'll be accurate, it'll be readable and current."

Senator Marcheselli also described being overwhelmed, sometimes getting "a little suffocated" with information. He characterized the Senate as being even more reliant on staff than the House because of the smaller numbers of Senators.

"We have broader responsibilities in different areas so we have real problems in the sense of time to do things... A lot of the preliminary work in sifting down things is done by staff and they will prepare briefing books for you. So we rely pretty heavily on staff to do a lot of information collection as well as the analysis and they will give you types of questions they think you should ask. They generally know how you think and what you're feeling on so you can have a pretty close relationship."

Concerning his ability to look at information daily, Marcheselli said that he can read "nowhere near as much" as he wants. "You don't have any time. The hardest part about this job is you don't have time to do your job."

### *Assessment by Legislative Aides*

Working within the personal offices of Congressional members, Legislative aides' first loyalty and responsibility is to their member. In this sense, **Anna Roach** and **Alan Glaser** were each constrained by principles outlined by their bosses. The principles reflected political judgment and past experience. White said that if his task were a vote, he would rely solely on Roach

"to frame the question that a particular vote raises for me -- what it will do, what it will cost, whom it will affect in what way, to advise me of who thinks what about it, which interest groups, which constituent groups, which members of Congress line up where. And then to engage in a sort of running dialogue in which she will anticipate my general philosophical approach to something and then try to make it fit and not fit, challenge, question, cajole, whatever."

Roach said she spent 10% of her time on Middle Eastern issues, which were important to the Representative's constituents and crucial to his sub-committee work. She managed a staff of six full time aides, a part time press person and a couple of unpaid interns and managing trade and women's issues. She started as an aide after college and had been working for the Representative for almost three years.

On occasion she might disagree with the Representative when controversial votes occurred. She also said that Representative White "pretty much makes up his mind" and that she had not seen him "change his mind about things very often." So if she would disagree, she would

"spend extra time if I feel strongly, really boning up on things and, using the same intuition I use to filter information that he wants anyways. I feel like if there's some real controversy there and I think that there's some worthwhile perspective that we're just not agreeing on or he's misunderstanding, it really is my duty to be most prepared to argue with him about it. So, I might spend half a day preparing for something, and I probably will lose anyways because he pretty much makes up his [own] mind. I have seen him change his mind about things but not very often or not instantly at least. Then if I still lose, I go and represent him the way he wants me to."

Roach's central concern with IPPME was like her boss' and involved the political utility of such challenging information for him.

"Something like this,...would end up on CBS's 'It's Your Money' in two seconds if someone found out about that; it's a political liability... I mean it would have been in the garbage after I had read like the first two paragraphs, I wouldn't have finished reading it."

In rapid-fire, Roach summed up more objections: that it was out of her job description because of unemployment in her district; Congress' impact on U.S. foreign policy was already limited; that interjecting solutions into Israeli culture was not part of the U.S. role; and suggesting that it is would loose her credibility for future endeavors.

"Israel in our view is a country that, thank God, does most of what it needs to do for itself. And fights its own wars, doesn't ask us to come over there. Israel has never asked for us to go over there and it's just so contrary to the way we conduct our foreign policy in the Middle East. They've been more successful than anybody in constructing a society out of nothing; they don't need us to tell them that they need Transcendental Meditation. And the other thing is, what is this going to do to my credibility with my colleagues too, if I choose to devote publicly any amount of resources to an idea like this? You know, who's going to listen to me the next time? Their time is already so limited. Basically, a lot of this gets down to time and effectiveness. A lot of people think that giving foreign assistance shouldn't be in the job description of the U.S. Congress (laugh), let alone micro-managing in a way or suggesting how nations should change their national consciousness to avoid violent behavior. (laughter)"

Roach's second response on reading two paragraphs of the IPPME summary dealt with its relevance to the job of the federal government.

"Initially I can see that [the study is] talking about how changing the psychological aspect of an entire society or a large part of it through this particular practice of Transcendental Meditation could reduce violence. Since I already knew in our conversation that you're concerned about the Middle East peace process, I'm just like, 'you've got to be kidding me.' I mean Representative White is a serious thinker on this issue and although some anthropologists might be concerned or some sociologists might be interested in the fact that this might be true, it's not the job of the U.S. Federal government to have anything to do with Transcendental Meditation in the Middle East. So, initially, it's out of my job description.

In Roach's view, IPPME was "sort of missing its audience. It should be going to psychiatrists in Israel or to social organizers in Israel, not to members of Congresses' staff." She went on to explain that the information that she takes into account must relate to an idea or project in front of her

"I love school and I love ...sociology and things like that but ...I don't have the luxury of having the time...to indulge in looking at research like this."

She the research information would need a different format:

"It's not raw data but it's a fairly scientific study kind of thing. Even if there's a cover sheet or a headline that tells you where you're trying to go with this, what direction are you trying to lead me, to direct my thinking at least. Even the way we communicate with the Congressman, it's, give me one thesis and very few supporting facts that are going to make me want to read it -- any kind of bullets. It's just like reading *USA Today*. It's like (chuckle) give me something easy to understand, right up front and then I'll decide whether to continue with it if it happened to be relevant at that day."

For Roach, the proper presentation of scientific data about a new idea like IPPME would either be at a luncheon or as a backup for ideas already under consideration. She said she hardly considered IPPME long enough to give it a truth test and was not in the practice of examining social science.

Because Roach's job entailed endless sorting through of material that is irrelevant, she took particular exception to the scientific format in which it was presented. Her job, she said, was like "working in a fire station. You know, like, 'Emergency!' and you fix what's broken immediately."

After describing the enormous overload of information that she must deal with, Roach added that after reading the IPPME piece,

"I get angry when I read this kind of stuff because ...it's a waste of my time to have to sort through all this stuff. ...I hate going through my box every day because there's just so much crap in there! Not that this research is useless or anything but it's just so irrelevant to what I need to do. ....It just needs to get here in a different vehicle if it's going to be important to somebody."

Foreign policy aide **Alan Glaser's** work of one year with Senator David Shiff, also a prominent member of a U.S. foreign policy sub-committee, depended on "the big foreign policy issues of the day." Senator Shiff had a long history in foreign affairs, had written extensively, and had also developed principles which guided his aides. Senator Shiff didn't rely on Glaser for information to the degree White did. Senator Shiff's legislative aide Glaser, said,

"My role with him is to know what issues are important to him and to know what his position is on those and what he wants to achieve on those issues; to do all that I possibly can to keep him apprised when necessary and to get him involved."

Glaser needed to be "fully briefed" on issues, in order to "be able to confirm any facts the Senator might use in a speech or letter, which he'll often ask for." Because he represented the Senator, he didn't consider his personal views on issues as important.

"It's figuring out what angle *he's* going to take and then looking for things that will support that angle; looking for things that may oppose that angle, and making him aware of it. So, it's really focused on him, his approach."

Glaser often worked without guidance from Shiff, but anticipated his reactions based on the Senator's past choices.

Glaser offered an analysis of IPPME consistent with his description of his work and Shiff's past priorities.

"Skimming it, nothing jumps out at me as being something that really fits the Senator... Has he ever done anything on this or a related issue in the past? To my knowledge, he hasn't done anything on it ... If he had done anything on it, then not much else might change either, but because he hadn't, it's probably not something that he would be interested in doing now. So it's not something that I would bring to his attention."

He would bring Senator Shiff only "the essentials."

"I have to boil it down to... well, either it's a big issue, it's a national issue, that he has to deal with or it's something that he's taken a strong interest in the past and would want to deal with. So I look at this and it doesn't meet either of those and so, it's probably not something that we would be active on."

Glaser judged that the ideas involved were not admissible to the universe of possibilities for consideration. They didn't require a truth test.



Glaser described another utility test: "There's not much that translates into legislation on something like this. So, it's likely that if the Senator is not going to make an issue out of this, he's not going to have to form a position on this because it's not going to be an issue that comes before the Senate." In explaining why legislation would be unlikely, Glaser reflected what he had earlier described as Senator Shiff's concerns with legal precedent:

"Well, I think there's constitutional questions of is this a religion? Some people may argue one way or another about it. And so there's a question of well, what would you do with this, if in fact this were true, that these groups could diminish aggressive behavior, then do you have a Defense Department program or create these kind of all over the world? If that's what someone who was advocating this would want, I don't know... If the U.S. government was funding these would there be sort of a state sponsored religious activity."

Later in his interview, with some prompting, Glaser said he wondered "is this statistically significant? Is there a large enough group and control group that would validate these findings?" Though this hardly holds up as evidence of examination of the scientific results or a testing of scientific quality, it indicates that Glaser was not averse to social science.

If the research were to be considered more relevant -- if for example Glaser's Senator asked for his advice under the only conditions that he could imagine it coming before him professionally, i.e. "if a member offered an amendment to a bill that funded research on this or something like that" requiring the Senator to make a decision -- then "I'd probably say that research on something like this can't hurt and we might learn something. It might be something that we could support." He would, however, "read it much more carefully than I've done now. I would talk to people who might have a different opinion and see what their reaction is so that he knows sort of what the other side is saying about it." Though he had no idea who that might be, "I would want to get in touch with them to see what they're saying... just like a journalist who writes a story wants to put in balancing quotes." Under such conditions the church/state fears would have to be allayed. The Senator's decision, once it became a "legislative thing," would hinge on who had introduced it, how much they were pushing it, and what they wanted to do."

Neither Roach nor Glaser expressed aversion to the IPPME concepts *per se*. They might take them into account in the event they became more widely accepted.

#### ***Assessment by Committee Aides***

Thomas, who had worked in Congress for 15 years, contrasted the responsibilities of Committee staff with Congressional or personal staff.

"Congressional offices are the eye of the hurricane. It's constantly, there's ten people in the office. The television is going. People are coming in the office, coming, going. The Congressman's coming and going. But they're dealing with a limited amount of information."

On the committee, instead of focusing on 15 different issues,

"you're dealing with one area or one region and you're delving into it much more deeply than they do in a personal office. They're really getting down to the nitty gritty, the facts, the figures, and you need to disseminate that to the subcommittee's members, to the committee's members, to your member, to other staff people."

Committee staff create information instead of dealing with "already existing information, turning the raw data into a document just like lobbyists do, to be able to assess information more equally and more critically." Thomas implied that in their position Committee Staff have more discretion and, based on their experience and focus, were in a better position to judge the messenger as well as the information, something that all of the staffers considered critical. Committee staffer Banks explained that his committee is "not a major legislative body" but that members "introduce resolutions that might deal with the Middle East, usually non-binding sorts of things" which he will help prepare. On foreign aid, "which is the one area where our committee does exert more influence than others, we do extensive oversight of the foreign aid process."

Like the other Congressional respondents, Thomas reacted to IPPME politically, applying primary and instant utility tests. She applied a truth test which dismissed the scientific data and presentation. She was immediately put off by the presentation, calling the layout "boring."

"I don't think that very many people would give even five seconds to the article... we try to make decisions about whether or not something is worth reading... One doesn't have the luxury of reading everything that comes across one's desk and neither do you want to. So you have to become very discriminating."

She showed me the kind of presentation she found valuable, pointing to the *Jerusalem Report*, and which has a *Newsweek*-type format. She referred to IPPME's format when discussing ever considering this kind of research in the future.

"Not in this form, no. It's still much too technical, the whole thing can probably be boiled down to one or two paragraphs. And I don't even know that I would need to save this because what I've read is the kind of information that I can read and remember."

Thomas reported that she reads perhaps more than other staff members, though she finds it "very hard to keep up with all of it. You can sit and spend an hour just reading one day's worth of radio broadcasts and I have piles. I take piles of reading home with me every weekend and I try to plow through different sources." She described picking up signals and clues about the Palestinian/Israeli peace process in more obscure materials that other people had missed.

She pointed out, though, that she had no time to ponder more than "certain criteria."

"I've got work to do. Reading is only one part of what I do. I have to produce reports and analyses, I have to go to meetings, I have to follow hearings, I have to answer questions, I have to draft questions. Now all those are based upon information that I collect, but I can't spend forever deciding am I going to keep something or am I going to throw it away."

Only in the case where a constituent would send the IPPME study or related studies to her would she consider it more than to take note of it mentally before tossing it. In that case she would

send it to "our brown rice, and vegetarian type person" who meditates. "And then, I would wait until he came back to me and we'd see how we could satisfy the person and to what degree we would need to, or not."

"But without any provocation, something like this just showing up on my desk, like many newsletters, without a cover sheet or without telling me why I need to know this. It's somebody else just making work, it's somebody else passing paper along, basically to justify their own existence."

Like Roach, Thomas considered IPPME to be outside of her jurisdiction and that of the Congress.

"I have to know what's within my jurisdiction and what isn't. And what's within the jurisdiction perhaps of what is likely funding. Now if somebody were to come and bring this to me and I would look at it for two or three seconds, I would say, well if you are really interested in trying funding for this, go to some foundations. But you're not going to get the federal government funding for something like this. It's too ephemeral."

In her judgment the TM-Sidhi program described in the research "probably is in the same category as people praying for peace," which the government will not fund and which she does not accept.

"you're talking about one region of the world where more people pray every day than probably anywhere else, and we certainly haven't had peace. So, if something is within a category of a mental exercise, a conjugal mental exercise, it is akin to praying. The federal government doesn't fund praying for peace."

No amount of trying will make it fundable or relevant to Congress, according to Thomas.

"It's very similar to somebody perhaps trying to get Medicare funding for people to meditate. It's not on the spectrum. It's just not there. If you want to go out and meditate, if you want to help people feel better by doing it, go ahead and do it but don't think that in the final analysis, the government is going to fund something like this."

Thomas gave an example of an issue that was "of interest," within "the spectrum," which was particularly important to the Jewish constituents in her district and to her Congressman. In a request concerning communication with their relatives in the Middle East, she saw an issue that she began to "keep an eye on."

"We were contacted many years ago by some constituents who wanted to get in touch with a Jewish community in a Middle Eastern country and they wanted to know whether or not the Congressman could help them contact these people. And I realized that it represented a much larger issue concerning that community that everyone thought didn't exist any more, because the Jewish community there was essentially evacuated in the 1950s. No one thought that there were still Jews there, but there were. There were about a thousand of them. So that one thing then put that country on my horizon. And so whenever things started to come in, I would keep my eye out for things on that country and over time we became more and more involved on the issue. So I always have these antennae out."

In the case of IPPME, Thomas asked herself, "Do I need to know this? Do I want to know this? And is it something that my boss needs to know or wants to know? Is it something that somebody in our congressional district is going to call upon us to know about?" She concluded, "The very most I could see something like this even remotely involving us, is when we get groups who are running symposia or conferences and they invite the Congressman to participate and he can't make it and we send a message and we say, 'We're sorry we can't make it, we hope you have a very successful gathering, best wishes.'"

In decisively rejecting IPPME, she also considered whether it made sense to her -- the first half of a truth test.

"First thing I thought it was kind of nutty, the fact that people are going to try psychologically to solve a problem as intractable as this one. If wishing made it so, we would do it a lot more often, but wishing doesn't make it so."

She didn't consider scientific quality. The scientific nature of the information was as off-putting as the nature of the thesis explored. She unequivocally stated that she did not need to look at the technical side. She didn't need to know statistics. If she ever did she could send it to a technical expert.

"It gets back to the political. Which is ultimately what I have to deal with, which is, what are they proposing? And I don't need to understand how they did what they did. The bottom line is that I understand what they were trying to do, which is meditate for peace."

If her horizon changed, then scientific quality might be important, but scientific quality wouldn't count if she had already rejected the study. Furthermore, she and her Congressman feel that the Federal Government should not intervene in such matters. The fact that six years had passed since the research had been published, without "people espousing this view left and right," also reduced its credibility.

She didn't consider the IPPME study to be practical or the kind of concrete support needed to convince Representatives.

"There could be somebody here saying take this to the United Nations as a way to solve every single problem we've got on earth. But we deal with the practical. We deal in concrete numbers, money. I mean, it's almost impossible up here for people to sit down and talk about peaceful coexistence unless somebody puts money to it, like a conference and everyone goes and has the warm fuzzies and goes home. Unless they are told, you come up with something concrete to prove that spending this money was a good idea. That's how this place is geared."

Of Congressional respondents, Thomas was most adamantly unlikely to consider the Maharishi Effect research in the future for political reasons. Her adamance appeared related to her annoyance at the IPPME concept, which she called a "conjugal mental exercise."

In contrast **Brian Banks** who, with less than one year as a committee staffer, explained that his education gave him more exposure to social science; that he was more inclined to read something not related to what he was doing; and that he "might have more sympathy for touchy-feely things than other people do." His parents had practiced TM when he was young, though he had no direct experience of it.

Banks explained that the research was "an intriguing hypothesis, but concluded that the study was "very unconventional. I'm not saying everything unconventional is not acceptable. Sometimes we really need some creative new kinds of thinking but sometimes things are unconventional, just they don't make sense or they don't strike you, they're unrealistic. This is one of those cases."

Banks explained that he didn't think the research was "particularly useful." He meant that if he considered it he wouldn't be taken seriously, politically.

"I'm looking for things that can enhance the policy debate, that can inform policy decisions. Now if I were going to go to the chairman and say, or if I were going to tell him he should say, what I really think needs to happen to improve the Middle East peace process, is to have five hundred people do Transcendental Meditation on a daily basis for the next three months and it will improve the entire communal environment, I don't think I would be taken particularly seriously. I know he wouldn't be taken particularly seriously. And I don't think it's useful to policy. You have to look for actions that our government can take that other governments can take that promote whatever your objectives are -- in this case, obviously a lessening of tensions. But I'm not sure a U.S. promoted or a U.S. urging of Israelis and Palestinians to promote Transcendental Meditation in the community is going to be taken very seriously."

He also questioned the source, the fact that three of the IPPME authors were from Maharishi International University, which suggested to him that "perhaps they have a certain, as I have my own sort of pre-conceived framework that I'm working in, they probably have theirs as well. And that may be putting an excessive or putting a lot of emphasis on the effects of Transcendental Meditation and maybe looking for ways to broaden its uses."

While the study hit a political nerve, Banks said that he was "not willing to rule out the fact that maybe the IPPME experiment may have something to do with [reducing tension]." His comments indicate that he considered the scientific argument more carefully and thoroughly, though he was very skeptical about the causal claims that were made. Banks engaged in testing IPPME's truth and trustworthiness.

"This is harder to understand because it's the behavior of individuals doing an act that is a self-contained individual act. And that's going to have a wider effect on what is it that they said -- one percent of, or even one percent of one percent of the community can affect the whole environment. It's not consistent with what most of us know from our own experience or at least it's sufficiently different and sufficiently hard to explain and hard to understand that there is a lot of skepticism about it. And I'm sure that my skepticism reflects and is probably less than the skepticism of a lot of other people."

The first question that Banks asked had also occurred to White, to committee staffer Joseph Rumar (described below), to scholarly reviewers Lawrence Moore and George Fisher as well as to several journalists and diplomats: "what are the other possible explanations for the results that they (IPPME authors) are citing?" He didn't dispute the "causal link between their experiment and these results."

"But I thought in my own mind, what would some of the other possible explanations be. I just know from other experiences that there are ebbs and flows in conflicts and there are times of higher intensity and lower intensity and there are various reasons why that happens."

Banks would consider research in the future if "it looked from a preliminary viewing or skimming like it might amplify something I read earlier or make me understand it better or enlarge on the points or some way. ... I might read through it again, I might even read through it more carefully [unless] it looked like more or less a rehashing of something I had already read and formed an opinion." He said, if he were ever in the position of one of his bosses, he probably would not read the study, because he assumes he would not have the time.

Committee staffer **Joseph Rumar**, who had worked for his Senator for six and a half years, would also consider Maharishi Effect research in the future, even though he too came to the political decision that it was too challenging for his Senator to consider seriously and that it was not implementable now. He explained that science makes an argument difficult to refute, "but, on the negative side, it also makes it harder to use."

"Senators aren't social scientists; most of them, at least that I'm aware of. In the context of foreign policy and the Middle East peace implications for how that technique would be used in the future process particularly, I can think of very few ways in which they could stand up on the floor of the Senate or in a meeting, and use that kind of data."

Rumar described the kind of data Senators tend to use, which is "oriented towards diplomacy or biographical, historical type articles, and much less this kind of, I guess this falls under the rubric of social science." He said, "I can't think of one Senator beyond my own who may have more than a passing interest in this kind of study."

Research that might interest members deals with "their counterparts in other countries; you know, it's more of a personal thing; it's not generally targeted towards a grassroots public type of thing; it would be much more focused, say, on the leadership of Israel or the leadership of the Palestinians, or the Lebanon's leaders." Rumar also explained that there is a mismatch between Senators' interest in public opinion -- dealing with large segments of the population -- and the IPPME study which is

"sort of a movement type and very small. You know, the numbers that they talked about in the study were in the one hundreds or two hundreds or three hundreds; it's not a large segment of the population and it's not the kind of thing that would generally grab their

attention. You know, if there were all of a sudden, public opinion polls coming out of Israel that suggest that 60% of the population were opposed to the peace agreement, that's the kind of grassroots public opinion movement that they would be interested in, the vast majority of the members of the Senate."

The theory also intrigued Rumar.

"That there actually is this phenomenon in which a bunch of people getting together can actually improve the quality of life of a population as a whole; that's interesting. I don't want to make a huge deal of it; I mean I wouldn't go home and stay awake nights thinking about it, but it's intriguing."

He said, "My initial reaction when I even read the title was that, well this is a little different; let me read it with a skeptical eye." He later said that "Maharishi Technology of the Unified Field; I have no idea what that means. Am I missing something?"

In describing the process he went through in reading IPPME he said he was looking for the bottom line:

"I'm going to have to digest it and there was, I recall one line that I would highlight or underline something that suggested that when participation in meditation went up that these factors, the conflict in Lebanon, automobile accidents in Jerusalem went down and that's the bottom line to me. Then looking at that bottom line, I'd try to make the judgment is this something of immediate value; is it something I might be able to use at some future date? Then I would either do something with it or I'd file it or I'd throw it away."

Rumar said he would also try to debunk the study, especially because, "Even if it's true, what can be done with this?"

"Can you make a proposal to your Senator to write to Yitzhak Rabin and Shimon Peres and tell them, 'we've solved your problems; all you have to do is get everyone in your country to meditate.' I'm using a little hyperbole here, of course, but I didn't see, even if it's true, how you could implement it."

Rumar described the single largest factor as "the giggle factor," i.e., members' and staffers' reactions to something different or outside the main stream. He explained that within the "very closed world" of senate staff and

"inside the Beltway, one of the things that you learn is that the interaction among people is not all that different from high school. And if you recall how nice people were to each other in high school when somebody made an effort to stand up and say something different or outside of the mainstream, you get much the same reaction here. And so you have to be cautious."

He described what about the IPPME study brings on the giggle factor, namely the language used, unfamiliar concepts, and images evoked by such language:

"It talks about Transcendental Meditation; not many people in the Senate that I'm aware of, practice TM or make a habit, at least, if they do of talking about it and suggesting that it's a way to promote world peace. You know, words like 'Maharishi,' never pop up. At least since I've been here, that's the first time I've used that word. So, it's that kind of thing. And maybe

it's just a lack of understanding or ignorance or even some sort of prejudice involved here, but it's something I have to take into account if I want to be a good staffer."

Based on images that come to mind, Rumar was not comfortable with the thought of his Senator speaking to a "very broad audience" about IPPME. "I mean, again, to be brutally honest, you picture somebody in an airport handing you a rose and saying, read this study."

He explained that his judgment related to press perception, which he described as a new, increasingly prominent factor in assessing how to use information. "And beyond that there's not a heck of a lot you can do with a piece of information like that; in terms of his responsibilities as a Senator, the only type of outlet for that type of information would maybe be a statement for the record, you know, calling attention of the Senate to this particular study. In this case, I wouldn't recommend that he do it... I think that would detract a little bit from his reputation."

Although he was initially inclined to think that IPPME might be "bunk," Rumar's second reaction was that it was a little bit interesting because "in looking at the graph ... this is all so scientific and maybe it is still bunk, I don't know. Maybe lawyers are better trained at looking at these kinds of things or social scientists but I don't have that kind of background."

He decided that the only practical use of such information was perhaps to send it to a member who he thinks would have *personal* interest; otherwise he might throw it away. On the other hand he referred to the fact that reading IPPME increased his own data bank and that he would recall it on an appropriate occasion, which he imagined might be an off-the-record meeting *if* the topic ever arose.

Rumar said he could not debunk IPPME because "I think that with the cautious way that they express their conclusions, you really can't. I come back to the word *suggest* [in the IPPME summary]; well, who can argue with that?" He wondered however whether the study took into account that there might have been some shuttle diplomacy going on or there may have been some other diplomatic or military developments."

#### ***D. Influential Non-Governmental Decision Makers***

This group includes lobbyists, human rights and peace activists, and scholars from think tanks and university institutes dealing with the Palestinian/Israeli conflict who were described by people in Congress and in diplomatic positions as influential in their writing or their relationships. These respondents approach the conflict from different perspectives, attend to different institutions within the policy world, and use information differently. For example, human rights organization research director Peter Neumann and his Israeli counterpart, attorney, and human rights organizer Leon Kaplan monitor, chronicle and attempt to bring political and practical redress to human rights abuses. In so doing they are keenly interested in the accuracy of the information that comes to



them. Neumann, who has a Masters degree in International Affairs, directs research activities and advocacy work on behalf of four countries including the Israeli occupied territories. He said,

"We have lots of information coming in all the time. It's a place where there is a relative free flow of information, so we get stacks of human rights reports and documents relating to political developments. It's very easy to reach people on the phone; they are comfortable speaking on the phone for the most part, so our job involves sifting through that information, deciding what our priorities should be given our limited resources, and then deciding how we are going to go about checking that information and sifting out that which we regard as not reliable."

Neumann, who is involved with the press, with Congress, with organizations on the ground in the various countries and many interested associations and organizations, said that some information demands very high standards, "if we are going to write about what happened in an open court." In other cases, where Neumann and his colleagues are denied access to first-hand verification but have enough interviews, they make the decision to trust the information.

The scholars with whom I spoke, identified as influential in their writing and analysis, were also conscious of having at one time or another expressed views that resonated with the policy community so that they were sought out. Writer/analyst Adam Kern, who described himself as contrarian, said he gained notoriety for understanding some aspects of the end of the cold war in advance. Presidential advisor Donald Cox attracted attention in the 1970s and 1980s when he disagreed with U.S. foreign policy. Think tank scholar Dennis Crawford noted that he had a special niche in the Middle East policy community related to his ability to monitor and use information, although he said he wrote for public consumption. Crawford's acumen with the Middle East was mentioned independently by six other respondents. He said,

"My comparative strength and comparative advantage has been that I have a pretty good understanding of how American policy-making toward the Middle East takes place, having worked in government, and hav[ing] a pretty good academic understanding of what happens in the Middle East. So I work at sort of the intersection of two fields. One is trying to understand developments in the region of the Middle East and relating that to American foreign policy and what we should be doing, what we are doing, to pursue our foreign policy interests. So I see myself as a foreign policy analyst with special expertise on the Middle East."

Policy center director Barbara Roth had gained credibility in Washington on other issues. She was noted for her ability to get involved with "the principals on both sides and talking to people both at the elite level and at the grassroots level." She said that she was "interested not only in what goes on at the top level of negotiations but also how day-to-day life both as an impact on what political leaders can do and have political leaders' decisions or non-decisions can impact the grassroots. So, I get access to a lot of kind of beyond-the-headlines information because I have access to almost all the countries."

The lobbyists, executive director Michael Krachon and senior lobbyist Karen Golden, worked for organizations that represent large populations of U.S. citizens of Middle Eastern heritage. Golden, who had worked for four years as a lobbyist, had served as a legislative assistant for two years in the Senate and three years in the House. She was in constant contact with members and staff in a hundred House and Senate offices and responsible for relations with major foreign policy committees. Executive director Krachon, a native of the Middle East who had completed doctoral course work in Middle Eastern studies from an American university had also been in his position for four years. His lobbying group was an umbrella for over a dozen politically active organizations.

Both lobbyists described their jobs as limited legally to representing U.S. interests and not the countries and regions with which they are associated. They were also limited organizationally to carrying out specific agendas dictated by their boards of directors. Krachon explained that

"information is ...where the real political game is at. ...In doing lobbying, which this association does on behalf of its own constituency, information is the key ingredient. ...Your success can be geared basically in two terms, accessibility and the extent to which you become a credible source of information for the people you're trying to influence, the decision makers."

Golden concurred. "So much of my job is getting information and sharing it on the Hill; that's the whole lobbying process." Krachon noted that his organization was one of "thousands and thousands of people trying ... to [get] key decision makers involved in issues of importance to them, to try to either level the playing field or [to give to the decision makers what is necessary to reach an] appropriate decision." He explained that it is the

"quality of the accessibility you have that determines what gets done with your information. Information alone is not enough. There are a lot of people who have information. Everybody from a perspective let's say of a neutral person in office, being bombarded by all this influx of information, their attitude is everybody's got a point but how do you make your information weigh a bit more than your counterpart or the average person sending the information?"

### ***Lobbyists' Assessment of International Peace Project in the Middle East***

Both lobbyists viewed the IPPME research as an unusual piece of information that they would be unlikely to examine again in their work, though **Karen Golden** was interested because she knew of TM. Her first reaction illustrated the time limits of her job and the way she dealt with information overload while focused on the minute to minute monitoring of "on the ground" events in the Middle East.

"One scenario that I could see happening, usually if I have ten free minutes, I'll pull a stack out of my in box ...ruffle through. I just know that I would see this and see TM and yell out, all the lobbyists are in the same room, it's a big bull pen, 'Oh, has anyone seen this thing on TM?'"

She would do so, "just because it's different. Ninety percent of the stuff in our in boxes is what happened in the last four hours on the U.S.-Syria track or what happened with the PLO implementation of the agreement in Gaza and all of us are still caught up in that technical day-to-day, moment-by-moment kind of information, and this is different."

Golden did not grapple with the meaning of the IPPME study, but said,

"I think I believed it, I just felt it had credibility because I saw all these footnotes and I looked at this just very briefly and I just saw a lot of sources and things like that. I didn't read, I only read a page or so of this [summary], I found...this a little more interesting [longer article]."

She explained that the IPPME study was "90 percent meditation and 10 percent Middle East" and not pertinent to her organization's agenda, or to the Congressmen whom she is charged with influencing.

"Nothing in here, I think, would move our agenda ... The agenda is, again, the relationship [between the U.S. and .....] and the hot issues in the last year or so would be foreign aid and the peace process and all the attending issues. What is it about this material is that, there's nothing in here I think has an impact on any of our agenda items. ...I can't see enough of a hook where a member of Congress would read this and would decide to vote for foreign aid or against foreign aid because of this. I couldn't see a member of Congress reading this and deciding to support the peace process or make noise against the peace process because of this. If anything, I would view this more as a think tank type piece or an academic piece, it's something, that's interesting, it's almost like a newsy of the moment, almost like a conversation tidbit."

Golden said that with the constant bombardment of information, legislative aides are barely able keep up with three newspapers or five newspapers a day. They are looking for information that helps develop the active issues. Unless information was in some way connected to a member's focused interest:

"In their heads a bell would go off that this has nothing to do with anything I'm working on or any issue that I'm developing or cultivating or need to know about. Next!"

"In the Congressional office, this piece of paper [IPPME summary] ...would go to one of the legislative assistants. Because this really crosses two areas, one being meditation and one being the Middle East conflict resolution, it could go to the foreign policy legislative assistant or it could go to the person who would handle domestic issues and social issues. I don't think this would ever get to the desk of the member, OK. ...Let's just say, for this discussion that this were ON the desk of the member, unless the member had a particular interest in meditation, I doubt he or she would read it.... What happens is, the mail comes into each office and they probably get a few thousand pieces of mail a day. ...The person sorting the mail would, ...in a reflexive way, ...just look at what comes in, what's the topic, which LA gets it. There's rarely ever any piece of literature, article or anything like that would go directly to a member. In almost ninety-eight percent of the time, it will get filtered through the legislative assistant. The legislative assistant will see this and ...if they had an interest in TM they might read ...two or three paragraphs and toss it. ...Having been in that situation, that's just how I would deal with information coming in, because unless it was

directly related to my job, my issues, a project I was working on with the member, any information that's needed to help us evaluate an upcoming vote and none of those would apply to this piece of information-- I would read a couple of paragraphs and toss it."

Although Golden believed the IPPME study, she didn't take it seriously because it wasn't relevant. It was so far from relevant that she had difficulty imagining taking it into account for any reason related to her work, though she could think of potentially interested people to whom she might send it. Given many opportunities via indirect prompts to speak about assessment of scientific quality, she only mentioned that it might be written with more flavor and the credibility suggested by footnotes.

**Michael Krachon** did grapple with the summary. He didn't "see the relationship between personal stress and national behavior." He thought the study aimed at getting "Israeli leaders and Lebanese leaders...to sit down and meditate and reduce their stress and tension." To him the study was an academic oddity rather than a practical reality and he judged the scientific aspect as an attempt to kind of depict certain notions in scientific terms, but it's not convincing." He thought about his own experiences with war.

"There's no doubt in my mind that stress has something to do with conflict. That's a primitive notion. It's a fact. But how do you deal with that tension? How do you reduce that tension? I'm not sure whether reducing the tension of the individual-- I'm not sure-- I've seen people wage war when they are in a very tense international situation or totally at peace with themselves and extremely calm on the personal level. So, I don't see the relationship between personal stress and, if you will, national behavior or collective nation state behavior. I cannot connect the two automatically - sometimes they are, sometimes they aren't and reducing one doesn't necessarily reduce the other.

He said that he did not understand or accept the study's statistical argument because he couldn't imagine how relaxation could be multiplied.

"Besides, I found the whole notion of the methodology even of measuring, quantifying, I did not understand it at all. I found it very simplistic. In terms of saying, no, if 100 people in Jerusalem are practicing stress reducing relaxation exercises that multiplies somehow and affects thousands more, how? And then how do you quantify that and trying to link reduction and, I realize I don't see where this data comes from in terms of like a drop in car accidents. There are many other factors, assume that that's the case, we got, let's say, Israeli leaders and Lebanese leaders, what have you, Jordanian leaders to sit down and meditate and reduce their stress and their tension and let's assume that when we started there was a conflict going on. These conflicts by their very nature always last one or two days, they always have 10 or 15 people killed and the next day only half of that and then the third day so, I'm not sure whether it's the relaxation that is doing that. There are other factors that are not accounted for."

As he reasoned further his conception fit with the causality guiding his work: getting to know and influencing leaders both friendly and hostile. He assumed that whatever the IPPME study described it would involve leaders who of necessity control conflict and its outcome and he wondered how two factions in Yemen would relax " in the middle of a conflict." When I explained

that the IPPME authors were suggesting "not that [leaders or people in the Middle East] do this, but that a group of people, volunteers, who do this Transcendental Meditation and ... then they measure what happens," Krachon said, "That's what I didn't understand. Any group of people? Let's say to relax Washington, you just pick 100 people off the street and get them to relax and somehow through these hundred [people, measurable changes occur]." He tried to imagine who these people might be in Jerusalem.

"Who are these people that actually did relax in Jerusalem and who are the others who relaxed in Lebanon to see whether they really-- I'd like to see that. I doubt it very much that that ever happened, I don't think so. [laughs] I just can't see it. I know these people. I can't see Israelis and Lebanese relaxing. In order with that understanding, in order to influence decision making... even participating in an exercise...If someone walks into Jerusalem or Beirut looking for research subjects, they won't even take the time to talk to the person advocating it [because], practically, it's a laughable idea from their perspective."

When he understood that volunteer meditators were brought in to meditate, he immediately concluded, "Now we're in the realm of the spiritual, I guess. That's even more complicated." This was partly so because what he imagined, still holding onto the idea that leaders would need to be changed, was a kind of mental focus by a group on an individual or policy through what he termed "remote control."

"Spiritually I have no problem with that. Practically, rationally I do [because] they are two different universes that overlap but do not necessarily, are not in total harmony. I might sit down and pray to knock some sense into the President's mind or meditate to affect a certain reality; you could use it, I think, equally I guess. If the President is embarking on a certain decision and I want some power somewhere to somehow influence his decision, people do that. OK, I have no problem with that. But as a practice if a hundred of us around town could do that today, I've never seen any scientific evidence that will prove that."

Referring to his own faith, Krachon said he understood the concept of prayer for a specific outcome, but he had also observed the tendency of those involved to rationalize if their prayers were not answered:

"I have my own spiritual life and spiritual whatever faith, system of belief and that is a product of my both 'cultural background' and upbringing but it doesn't necessarily always fit in harmony in terms of influencing policy of decision makers. If you believe in prayer or if you believe in meditation to try to influence a decision by somebody else. You could do that if it's part of your system of belief, but then you have all kinds of rationalization when it doesn't work. It's the same thing when we pray to get out of a certain difficulty or something, illness. And it's not God's will or it didn't work for some reason or another, they didn't do it right or maybe not enough of us did it. ...It's hard to reconcile the two, the spiritual and ...the practical because they are...two realms. Unless of course you believe that one is superior to the other and controls it somehow for the better in the long run. That's what belief is all about. It's like gambling."

Krachon reasoned from his cultural and professional experience. What bothered him was the concept of mind over matter and action at a distance. The IPPME concepts were so challenging

that he didn't have a way to imagine how they might work, except in the unpredictable ways of religion.

Krachon considered leaders to be the most important lever in the outcome of peace or conflict. He dealt with "people of a certain mind set, so to be effective with them, I have to understand where they come from, how they operate, how they think and how they make decisions, so I can plug into that process and influence it somehow." In that context he considered the IPPME study "a laughable idea ... not just [in] the Middle East, [but] even in town here." He said that people on his board would think the idea impractical and that most people in politics would "view it as just purely, nonscientific, impractical, naive, academic notion of solving [conflicts]." The problem, said Krachon, was that they are overachievers, busy, overworked, and tired, so suggesting collective relaxation as a new tool might attract them personally, but not professionally:

"You'd have a problem saying 'Hey guys let's do it collectively before we deal with disarmament and this, or before we deal with the problem of Japan, why don't you send me the team over here that's relaxed ...before they go embark on negotiations. Might be an interesting idea to see if a team would perform better in negotiations if they're all relaxed. But, it's that addiction to practical considerations that seems to be the problem."

Krachon had earlier mentioned that he had a "natural curiosity for new ideas," but that he had "seen a lot of stuff over the years, ...especially in Middle Eastern studies, [that] by the time the ink dries on some things, it's obsolete" and he said that most of the studies cited by the IPPME authors "are obsolete anyway, at least a couple of them are, some of the names, their work is past tense, gone." He said, however, that it was too late in his career to look at empirical work, and explained that people like him "don't have the time for "that kind of stuff," meaning both the IPPME study and studies in general:

"When you're halfway or beyond in your career, it's not the time to go back and learn the basics. But...there are new ideas that work...and the only aspect of Middle East politics that these things come up in is this notion of dialoguing... but people who are in mid-career - their consideration is more practical than academic or conceptual [that is] resolving problems and overcoming difficulties and balancing accounts ... Conflict resolution rather than preparing for dealing with conflicts."

"It's almost like dealing with the normal process of growth for human beings. There are certain ages or stages in life where you have better chance ...to influence a child's concept of things. ...And at one point it becomes too late. In a way, careers ...have their own age, their own chronology, whatever, so in seeking to influence and to deal with it you also have to also plug in at the right time in ...introducing new concepts and techniques. ...I think it might be too late to teach a 21-year-old how to eat."

### ***Middle East Scholars***

All three scholars asserted that they would not take IPPME into account in their future work. None believed the proposition. **Dennis Crawford** wrote on the bottom of the summary that

he read, "I don't buy it because I think the underlying causal theory is nonsense. Therefore, I don't accept that the quantitative analysis proves what it purports to prove. Also never heard of any of authors..."

Crawford was angry, assuming that I was advocating for the piece. He rejected the study more vehemently than any other respondent. He felt strongly enough about the study to assert:

"it almost made me think I ought to write to them (*JCR* editors) and say do you realize that you are jeopardizing your reputation by publishing things like this?"

"I didn't believe in the fundamental analytical assumption for a very simple reason: I don't believe in the kind of underlying causal assumptions that are built into the article and so I basically, if I weren't trying to be polite, I would have tossed it into the trash can."

"Imagin[ing] that people participating in this process, in fact become more peaceful or more relaxed and deal with each other in a more decent way, I would accept that that's a possibility. No problem with that. It's the notion that their doing this creates some kind of affect that influences behavior of other people, that's where the causal weight breaks down for me so I think that all the correlations reported here are spurious."

Like Krachon, he was not prepared to reexamine his assumptions in light of something based on "a kind of theology ...or mysticism." Thinking about what might influence his thinking about it in the future, he said,

"I find it hard to believe that it would overcome my initial bias against it. ...I can't imagine ways around this, but maybe ...a series of studies that are well documented, that go into the explanatory variable that begin to be taken seriously by people I respect, I might re-consider and take another look. But I find it very implausible."

Crawford said he had absorbed "what I think is useful to me at this point" from theoretical work, and that as a political scientist who has paid his dues, though he read journals, he was more interested in his own empirical work. Commenting further on his views about social science, Crawford said that without an explanation of the causal mechanism involved, the IPPME correlations were spurious:

"You can get correlations between sunspots and incidences of crises in the world and there, correlation doesn't prove anything. It's the causal connection, requires a kind of logical explanation of how you get from one thing to the next and since I don't see that here, I just assume it's a spurious, there's some other factor that explains this, assuming that the data is accurate. That's what I assume and I'm not prepared to take more time to figure it out. I'm not going to go back and re-calculate and see if they've done the mathematical calculations correctly, I assume that somebody, if this is serious at all, has done that, but I'm not prepared to do it."

**Adam Kern**, who referred to Crawford's favorable reactions to his own work, was not angered by IPPME, but also considered it irrelevant. He had encountered research related to IPPME at a conference and some of his "best friends" were "into this stuff" which resulted in his being "curious about it," but not interested in spending time "reading some book or literature." To

Kern, "the marriage between Middle East politics and this stuff" was "really too much for me" because he didn't see "any connection between them."

Kern reacted to IPPME as part of a wider body of "political social psychology" which he on some occasions found to be "totally irrelevant," and on others as he considered his encounters with it to be OK, as long as no-one took it too seriously. He referred to IPPME as an extreme example of social psychology -- an experiment thought up by people who tended to reside in their own ivory towers. He offered the example of social psychologists analyzing Saddam Hussein, which he found ridiculous.

"Certainly during the Gulf War, all these people who were trying to analyze Saddam Hussein, you know -- come on, give me a break. So, as I said, those people couldn't even-- probably as patients -- and I'm not sure that they succeeded in terms of curing them from some inhibitions they had, so they're going to tell me, sitting here in Washington what's the problem with Saddam Hussein -- you know his mother was abusive and that's why he invaded Kuwait. Right?"

Kern acknowledged that his judgments were akin to picking a spouse and not necessarily rational, perhaps even wrong or mistaken, involving all kind of elements, including prejudices.

"Sometimes it reduces to bigotry. I mean, people say, 'We don't like Blacks,' and 'we would rather deal with certain other groups' and so on. But that's, I mean again it's irrational, which is another point. We just don't have the time so we develop all kinds of ways of looking at people. And then my experience with this type of people that I met --- and social, kind of political social psychology. And I don't know, I found totally irrelevant... I mean, so you develop a certain instinct when you see this type of people and after a while you are not even interested in wasting your time listening."

Kern found IPPME boring, explaining that he would "enjoy more actually reading some pop-sociology in say *Vanity Fair* by someone profiling Clinton with some gossip and you know stuff like that, which I don't take as a science but at least it's fun reading." Kern had a larger quarrel with the type of experimental effort described in IPPME. He felt that economics, for example, might give certain trends, despite the fact that "two geniuses with Nobel prizes in economics [were likely to] still come with totally different analyses and associations." But Kern preferred to look at foreign policy as an art rather than a science and preferred "to never present [his ideas] as certainties." This was partly because he felt that people "just muddle through," as individuals and certainly on the larger social scale. He expressed suspicion of people managing the economy who could not balance their checkbook.

"We don't know what will happen tomorrow morning for sure. Someone cancels your class and so on. To think you can manage social reality which involves millions and millions of people by all these simple words of the tongue. That, I just don't buy it, so. And I know how things are done in the real world in terms of decision making and so on. And sometimes it's someone had a fight with his wife, sometimes [has] more effect on whether to make a decision that evening whether to send troops to Bosnia, then some war games and experiments or statistics."



Kern expressed distrust of the broader category of social research that

"tend[s] to treat people like they are robots, in the sense that if you do this, this will happen. That's the idea behind social science -- very much grounded in this notion that if you do x, y is going to happen. And I don't think that, again, [that's the way] things work-- certainly when it comes to politics.... People tend to make more rational decisions -- cost-effective when it comes to economics, if you buy a car. But when you elect a president I think the comparison -- the analogy would be more like choosing a spouse which is not necessarily always a rational decision as such."

Kern concluded that IPPME didn't "fit at all into the type of material I would think of someone who is making policy." While he thought people in universities or academia or think tanks might be more interested, he said that governmental decision makers and people in Congress were much more "practical and short term -- and they're always interested in things that are going to help them to just carry out certain policy goals from one day to another -- in terms of agenda or the agenda of their bosses and anything that doesn't fit into that again I think would probably be rejected or would be assigned to an assistant or researcher who would be asked to summarize it or something -- in two paragraphs." Although he said there might be exceptions, he said that people in government "most of the time" attended to the short term -- "practical, policy-oriented type of information. And nothing long term-- you know, big picture, comprehensive."

About IPPME, **Donald Cox** reached a similar conclusion as Crawford and Kern. He asserted immediately and forcefully, "this is nonsense." However, he followed up by considering it from different angles, sifting through the layers of decisions he would bring to the study. Associating it with his wife's yoga and meditation, his first reaction was that it did not make sense. He couldn't imagine how one would practically get people "to do yoga." He thought about leaders and decided they wouldn't be predisposed to meditate. "There's a reason they're all leaders. They have a certain approach to life and they're not a group to be exposed to yoga."

As he considered the concept, the fact that seemed to stun him was that "the numbers are so small." Thinking about his wife's yoga, he said that although ten years previously he would not consider such an idea, he might "if they showed me that they could get, not 152 but 152,000 and that they could show that that had a major impact on the society." In addition, if he could feel "that it's doable and that it could have an effect on the peace process," he would consider it. But he quickly added, "I'd need more figures; I'd need more data; I'm not convinced from what I've seen."

During the interview he thought about how others might react and in that context he identified himself and others in the Middle East policy network, in the middle of a continuum between international relations "do-gooders" and those who are more Hobbesian, who think that human nature is essentially evil. The do-gooders, according to Cox, "think that human nature is essentially good," and that we have "evil in the world ... because people have gone astray and they

are ultimately malleable, flexible and ... we can develop techniques to make people ... pursue their essential goodness, be as good as they really are." He said his colleagues would categorize the IPPME study as one of the "sundry theories, ... some of them a little kooky, some of them impractical and they're certainly not in the mainstream of the real world of the political science as I know it."

Cox confirmed his rejection of the study when he observed, "India isn't exactly a peaceful place. That's another point -- just occurs to me now. So that if they're so very effective, why don't we have peace in South Asia? Very violent society." He said, "So, now that I did think about this, actually I'm reinforcing in my skepticism: ... I think perhaps I was too kind earlier."

As he thought about his role in sharing knowledge with others in the Middle East policy community, he became more convinced that, "its way out, impractical, unconvincing, and not a little foolish." He would not share such information or others would laugh at him. Cox reversed himself again at the end of the interview, reflecting that, "I'm being perhaps too strong. Maybe I'm going off in the other direction." He reiterated his discomfort with the numbers, finding them "insufficient" and "unconvincing." He said, "I just assumed that they had, in a sense, prejudged it because they wanted it to come out this way and that there is some data there that disproves them and it doesn't make any sense to me." Then he decided he was ready to be convinced, but that it would be very tough because he didn't think it would work.

"But what I tried to do, as often as possible, [was] to keep as open a mind as possible, not to shut matters off. I told you the story of the PLO-Israel connection, of when everyone thought the PLO didn't want to deal with Israel, ...Israel didn't want to deal with the PLO also; even more to the point, couldn't. I tried to keep an open mind, and ...eventually in that case, [I] did become convinced that what sounded foolish was not foolish-- was actually correct. So, I would try to keep an open mind but I would start...with eyebrows more than a little raised."

### ***Human Rights Watchers***

Neither human rights professional had come to a fixed point of view about IPPME, but each continued to think and talk about it. It was likely that attorney Kaplan might consider IPPME-related research in the future, though research director Neumann would not.

**Leon Kaplan** had practiced TM and had given it up because he said he "didn't find that it delivered what was promised." He said that he had "nothing against it. I think that it certainly didn't do anybody any harm." He called himself a realist, even though he worked for a human rights organization. He said the people with whom he worked tended to be closet idealists.<sup>338</sup>

Kaplan questioned the trustworthiness of IPPME in light of his observations about the human condition.

"Social life as very complex with tremendous forces of life from various directions coming out of the psychology of people, coming out of the social psychology of groups, coming out of economic factors. I see a lot of evil. I see a lot of tendencies of repression. I see a lot of tension amongst people. I see people respond and predominantly behave according to negative factors that burden their lives."

Given what he had observed, he was afraid IPPME sounded "too good to be true."

"I rarely find that all of such issues can be like overturned and like annulled with a very easy, quick method or a kind of almost magical. The way it is presented to me in this summary is that it's virtually magical. It's like people just get together once or twice a day and they do something very easy, very costless, and very effortless in twenty minutes. And out there magical things happen. Or I shouldn't use the word magical, but out there beautiful things happen. So my viewpoint is one of much more of a realist. This will not happen. You've got to change people's attitudes through long, arduous efforts in education and in explanation of methods of building discourse, toleration. You've got to do lots of things out there. You've got to make sure that they're not deprived of minimal respect and of social, and of economic sustenance. There's a lot of hard, real things to be done out there. I find it too good to believe that through the square root of one percent of people doing something really effects in very large, very strong influence. Tens of percent that were knocked off of life's tragedies."

On the other hand, Kaplan, was not able to totally discount IPPME, partly because he was also "aware that the ignorance has stopped lots of good things in the world."

On the other side of the truth test, he indicated that he had background in regression analysis, that he had considered the research information from that perspective, and that it was the data and their significance that led him to consider IPPME further. He was unique among the respondents in commenting on the unusual level of statistical correlations.

"A 70% change is too great. One doesn't even have such changes in social science. ...To effect such phenomena by 50, 70%, 30% I don't think that normally there's any one factor -- if you'd run a correlation, a regression on what are the factors that cause deaths in the war or car accidents, I don't think that any one factor goes up to 70% because there are just so many factors and the correlations are innumerable. So again, that's why I'm saying to me this is overwhelming, and overwhelming increases skepticism."

Kaplan also expressed skepticism about the way data was collected and interpreted.

"If it's incidents from the news, how do we understand it? For example, there's a lot to do with what the other side is doing to you. Now maybe the effect of this consciousness, maybe when you cross the boarder, maybe both sides kind of calm down. I don't know. In car accidents there is a lot to do with weather conditions, with the width of the roads, with the quality of the cars. There's just so much out there."

Kaplan compared the IPPME with the "fantastic" religious promises like that of afterlife, about which he, as a secular Israeli, was very skeptical. He allowed room for perhaps not knowing all there is to know. "Maybe it's because I'm limited in my knowledge of not just this field, but our own knowledge of Western society -- what we think, is limited." He concluded that unlike religion,

perhaps "this thing can be made acceptable to people like myself. I don't think there is a way I can be proved an afterlife. There is a way this can be proven. And if it is true, I'll be very happy."

Although he had read only the summary, Kaplan said that in a policy role he would seek extensive validation of the data from someone he trusted. He pointed out that "the higher the price, the higher the effort, the higher the chance of it failing and backfiring, the more reluctant" he would be to apply it. He said that he would be less skeptical reading about IPPME in *JCR*, though he was not familiar with it, than he would be reading about it in the newspaper. Because he wasn't familiar with *JCR* or the field he questioned the credentials of the authors.

**Peter Neumann** decided that, in addition to being "not *directly* relevant" to his work, IPPME didn't fit with his view of things. Neumann pondered the description of causality that the study provided, reasoned about its implications, and decided he was uncomfortable with them. Neumann concluded, "there must be something wrong with it" and found a seeming contradiction in the scientific rhetoric. He noted what could be taken as a mistake in Orme-Johnson et al.'s square root of one percent formula notation and decided that Orme-Johnson et al. appeared to be careless.

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But the real test for Neumann was what other people would think. As a human rights researcher he was constantly checking the reliability of facts with many sources. He engaged in a little of this with IPPME, asking a source between interviews about "Maharishi University," and receiving the incorrect assessment that it was "a very well-endowed university and that people from the this university did not come to the source's American Philosophical Association Conferences, leaving Neumann with questions about whether it was a serious academic institution. He said he got the impression that "it wasn't terribly -- that the fact that this is the identification tag of an author wouldn't be a *positive* factor in deciding whether I would spend more time on it. ...If somebody were from Harvard I might think maybe I should try to read this more carefully."

He said later in the interview he wanted someone else "who's more knowledgeable" to tell him what was wrong with it: "how they chose the variables, that they were careless or misleading or the data is fabricated or something like that." He explained that he didn't also ask himself, "what if this were true?" He explained that "in a normal working day" he wouldn't take the time to examine IPPME that closely because he would have to study it closely and he didn't feel he had the statistical background to critique it seriously.

Neumann explained his skepticism

"My whole notion, understanding of causality and the world and if this is true, then my whole way of seeing the world has to be thrown out and I have to start over again. Because basically these people can, by physically doing nothing beyond their own bodies, affect the outcome of events many miles away."

Later he explained how he saw the implications of IPPME in relation to his work:

"[It] would undermine the work that most of us do because we would have to stop doing what we do and try to set up these experiments. Get 200 people in Indonesia and China and Taiwan and everywhere else to meditate so that we would have a more positive impact on human rights conditions and the well being of people. And we would by all the other methods that we currently use. ... And I would be negligent, I think, to just keep on working on what I was doing if this is the way the world works. I certainly would have to think about whether I wanted to work here any more if I believed that this is the solution to some of the problems we spend our day, our time working on."

Looking at IPPME as he would if it had come across his desk, Neumann explained that

"it just doesn't *sound* like my notion of the way things work in the world and then I look at who is making these allegations and if it's somebody, some organization that I have no respect for or perhaps I never heard of, I just put it on the side. I don't spend any more time on it. And then if I get the report in the newspaper from a credible organization, I may retrieve it. I also may, if I'm talking to somebody say, hey I got this funny report on poisoning the water, did you hear anything about this, has it been in the Israeli press? What do you know about it?"

For him academic journals were not necessarily credible and publication in *JCR* would not help him to overcome his initial skepticism. He said, "you can get stuff into those journals that is garbage." An article in *Science* or the *Economist* would impress him.

"Everyone has limited amount of time and makes decisions about how to do shortcuts ... If my instinct is not to spend time on this but I will if the *Economist* says this is a path-breaking study ... then I will divert time from other things to this."

He said,

"I obviously have a vested interest in my way of seeing things and I'm not inclined to change it on the basis of an article like this.... I'm not going to take time out from doing things that I feel I have to do to examine whether this is a better way of seeing the world."

Like Kaplan, **Barbara Roth** applied a truth test in that she examined the IPPME ideas in the context of the scientific argument and that she thought about the quality of the science. She read the summary and the longer article carefully. She explained that she did so where she thought her colleagues would not because she had more time. Time constraints sometimes prevented she and her colleagues from meeting even to discuss issues of immediate concern, largely because they had to assign a still higher priority to financial survival, since their organizations are dependent scarce donations. Some represent very large numbers of people in the United States and in other countries, and they work together, but time is always short. She said that if she had received the study last year, she would have been very pressed to take time to read it.

She made the decision that she would file it for future consideration principally because:

"They admitted modestly in their conclusions, that they didn't know everything they needed to know and this was worth studying. Had they come across with 'this is the approach,' I would have probably thrown it away."

Barbara Roth struggled with the statistics and determined that they were beyond her. She said that she assumed that the journal editors were careful in their review or they would not have published it. To her, future use of the IPPME research would depend upon further scientific evaluation. But she feared a possible drawback in resolving tensions through the Transcendental Meditation technique. In her estimation, conflict is most readily resolved when there is enough pain among the parties, and the time is right. She has observed that a skillful negotiator will identify the point of pain to go in and resolve a conflict. By reducing tension and pain, she feared TM might render the parties less inclined to negotiate.

Despite her doubts about TM's utility in the Israel-Palestine conflict, she was ready to consider the IPPME research further. She said she wouldn't consider ideas that she considered silly, but aside from those "I just felt that people who were trying to do what I'm trying to do can't afford to say 'I'm not going to read it or go further.'"

Roth, who described her early choices between a career as a concert cellist and her current work, commented that compared to scientists she thought creative people were less averse to risk:

"But we take calculated risks; we won't take up an issue just because it's the right thing to do...There's a willingness to take risks and explore the unknown amongst creative people. I think creative people don't precisely know where they're going to come out in the end ...they kind of trust their own instincts, which sometimes makes it very hard for people to cope with us."

Twenty years' experience in resolving conflicts in different countries had cultivated Barbara Roth's unusual ability to listen to competing viewpoints. She approached the IPPME research information by first sorting out and addressing the difficulties different parties might have with it. She considered the Israelis and especially Israeli leaders' discomfort with foreign ideas, especially mystical traditions that might overturn Judaism, and then the ways in which it could be explained to them. She had no prior knowledge of Transcendental Meditation but from advertisements she'd seen there she knew that there were people in Israel who did. She considered it equally from the Palestinian and Arab points of view. And she was impressed that the IPPME researchers had been able to gain the cooperation of these parties in order to conduct the research.

#### ***E. Government Officials with Diplomatic Responsibilities***

Respondents in this group were four U.S. diplomats who have worked over a period of time in concert with each other and each other's organizations to conduct U.S. foreign policy in the Middle East. Each of the four is a senior policy maker with a distinguished record. Two have focused principally on the Middle East and the other two have held positions in Europe and Asia as well. Each has a Ph.D. Three have been college or university professors and one, a University administrator.

One of the four, **Craig Sampson**, who had worked in various management capacities related to the region for roughly 20 years, said his job is "to frame choices ... to take information and to put it in policy terms." He provided a picture of the reasoning that he and his colleagues and bosses would apply to a piece like IPPME. He said that they would have no experience with TM and the research hypothesis and not enough information to understand what it really was. Certain images would come to mind, like Hare Krishnas at the airport. But the bottom line consideration would be, 'is it real?' He described the diplomats running around plugging the latest hole in the dike of the peace process. Of necessity they were oblivious to anything that did not pertain to chosen modes of dealing with the priorities currently arising.

***Information use given history, policy constraints, and operating philosophies***

The interviews involved an explanation of the historical context, related policy constraints and the day to day reality of structuring diplomatic approaches, which greatly influence the way in which information is filtered. Respondents were concerned to varying degrees with the tension between policy constraints and their ability to interpret shifting conditions through a wide enough lens.

**Walter Paris**, a strategist and negotiator, and according to one journalist, "as much of an expert in the area as there is," said that in his Middle East role he was concerned with how to approach a problem and how to carry it out, always testing underlying assumptions. In the Middle East policy context, which has been greatly influenced by the dissolution of the Soviet Union and the Persian Gulf War, he explained that successful breakthroughs in negotiations have occurred because of conceptual shifts away from old formulas. He noted that correct assumptions are not always based on "the world as we know it" and he described a policy situation where others judged a particular policy and objective as impossible, whereas he viewed them as necessary. "Sometimes what you feel you have to achieve can form your assumptions. But then I made a series of derivative assumptions about what it would take to be able to achieve that."

He described his conceptual departure from past assumptions that he considered "a prescription for stalemate." There had been "a basic argument on the part of some that ... the U.S. needs ... to produce a blueprint and put it on the table and that's the only way you can make headway. That was an assumption [which] reflected understanding of the area and how you should proceed."

"I've always operated under a different assumption. I said that will not work. ... We have to bring the parties around to the point where they are genuinely negotiating with each other. We'll have a role to play in that negotiation but it has to...come from them. It cannot look like it comes from us. My analysis at the time was that the Intifada created an opportunity but not across the board. It created an opportunity for the Israelis and Palestinians because there were pressures on each to begin to deal with each other. That was not an assessment that was shared by many. But in fact that's what guided us ..."

**Maxwell Hall**, an author and senior diplomat, explained that the process takes place within "very tight parameters," dictated largely by past policy of a super-power.

"Some are political, some are substantive, some are analytical. Usually the propositions you're testing, you see if this were an ideal world, you would consider all the information and you would formulate a policy based objectively on what you would need to meet the requirements for both sides. But it's not an ideal world and it doesn't work that way. So, a lot of what we do is drawn from the past. We're very much creatures of the past and traditional American policy which is set for us. Sometimes we can shape it or shift it in response to change in circumstances. But this is what is very difficult for people outside the government to appreciate: That it's an enormous responsibility and things move very slowly here. Because you're speaking on behalf of a government, on behalf of a super-power which has a set position on any number of issues."

He described the difficulties that arise because of the government's set positions.

"Very often the policy becomes an obstacle to achieving the objective. That's not to say that the policy is wrong but it makes it extremely difficult and ...you've got to act within the limits of the policy. Within the limits of the whole range of American statements and actions about any particular issue. Sometimes the information, often the information conflicts with that. You've got to figure out, what are you going to do with that? What are you going to do with the fact that the Palestinians say they want an independent Palestinian state and American policy says we don't support a Palestinian state? What are you going to do with the fact that the Israelis, that Mr. Rabin is under serious political constraints or Arafat has a major domestic opposition within the PLO? All of it has been factored into it, and this is where it becomes very difficult."

Such an environment influences information use. "People are under pressure here to make decisions. They suffer from time constraints and space constraints in terms of what it is they write." They are affected by inertia typical of large organizations, personalities of senior officials, and the reality of events over which they have no control, the political realities of the moment.

Paris had also indicated that he didn't have time to read:

"I think one of the problems of the government is that I can't tell you that I get enough of [the information he really needs] ... and I don't mean to suggest that I'm faultless in it. I mean, my day will get taken up. Some days I'll have nonstop meetings."

On the other hand, he might choose to read something that is not immediately relevant because

"it's something that I know is an important thing for me to read in terms of the stockpile of vital information ... The reality is that there is an issue of processing information there's also an issue of ... hav[ing] enough to draw on both to validate or invalidate your assumptions so that you have enough to draw on to sort of make judgments about whether or not the overall approach that you're doing or that you're engaged in makes sense. And there also may be a lag time. You know, sometimes you'll get a piece of information that I'll interpret one way, based on everything else I know, but it'll stick with me for a while and maybe, you know, somewhere down the road, as I'm going through the process of rethinking, I'll think back on that thing and I'll say, you know, I misinterpreted that...."



Hall said that numerous factors "constrain debate and decision making and, and make it much less effective than it could be." Given "a certain amount of inertia" information that "doesn't tend to support the existing view, sometimes is...discarded or not paid close enough attention to." History, for example, does not get "enough exposure ... in part because people are under pressure to make decisions." Although "a loving critic of the system," he concluded that "you're not getting all the time, decisions taken with the kind of perspective that they need to be taken."

According to Hall, the "most difficult and challenging piece of information," and one that government does not handle well, is information that comes from outside the system, i.e. from non-governmental people talking to principals in the Middle East or from academia. The challenge is to integrate such information with information from inside the system, "and by that I mean Embassy reporting, reporting from intelligence agencies, newspaper reporting, any number of other stuff that we collect or we have access to from the information that is outside the system."

Referring especially to people reporting conversations with principals, Hall said, "You've got to evaluate whether or not what this person has to say is *real*." In this situation government "becomes very insular, very protective. And I think we need to be better attuned to two things... analytical frameworks which academics project and information of real-time emissaries."

The question is "what to do?" He described moments over the last five or six years "when we had information that appeared to be at odds with instinct and information that appeared to be at odds with other information. And the question during those moments was what to do. And I'm not talking about decisions which, you know, for example, Iraq invades Kuwait. ...These are not Pearl Harbor-like decisions. These are more decisions related to diplomacy. What we know about any given situation that we're learning from. "

Within a debate, Hall talked about views making sense, arguments being compelling and indicated that one knows when an idea makes sense or is "worth listening to" or when it "seems a little off and sometimes a lot off." When I asked how he knew he said, "Well, I mean, I think its in part because we have a sense of what has worked in the past." He thought about his answer and reflected that "This is a very dangerous standard, because what works in the past...doesn't always work in the future." But he said that given the conditions -- the pressures of the small group debate at least allows a winnowing of ideas among members of the team.

"Many times in meetings I've taken off after an idea that someone else has raised, only to be contradicted by someone else or two or three other people. And by the end, I have walked back my own view and or merged it with someone else's."

Such debate should, Hall suggested, "under difficult circumstances ...produce the wisest possible response." While "there are million reasons why it doesn't. The system breaks down. There's a real chance to do it here."

The team debate described here offers the possibility of wiser responses, but also encourages conformity to tacit understandings among the group. They could all make the same mistake as when they "missed the boat" regarding an alternate negotiating channel.

"Because you have guys, ...primarily guys, who have been involved in the business of negotiations now for ten years...They've seen a lot happen, in terms of American policy toward the Arab-Israeli conflict. For ten years, their analysis is shaped in a certain way...even though they try to maintain an open mind and they try to be flexible. An [instance like this] comes along which is completely inconsistent with what they know analytically to be possible..."

These diplomats were working, as Sampson explained, with the benefit of unusually collegial relationships.

"We've worked together for many, many years... [and] you form very close relationships and...it's hard to describe if you haven't lived it. ...You start at level eight when you have a conversation rather than starting at level one...I think...that's unique to the Middle East peace process...I don't think there's anything else...in this administration in terms of foreign policy that has that same kind of continuity of relationships and players."

**Mitchell Tasman**, who oversaw relations with many countries and managed hundreds of people was considered an effective and pragmatic negotiator. He described the historical constraints involved, demonstrating how real the weight of convention and precedent was for him. He said that because the basis of Middle East policy has "been almost unchanged since 1967," modifications to the policy tend to fall "within certain parameters. It...rarely goes outside of them." Conceptually policy-making for him involves dealing with an "inherited ... mass of life that has gone on before me and will go on after me." The basic question in policy making was, "Are there parts of it which need to change? And if so are they big enough that ... [a senior official] has to change them or the President or are they small enough that I can do it or are they somewhere in between?"

"There are relatively few moments in my career where you...walk into the office on a Monday morning and say , 'we're going to develop policy.' it just doesn't happen. It's a process. You left something on Friday; you had some thoughts on the weekend; something happened out in the region; something happened in Congress; something you didn't know occurred and then Monday there's revisions and changes and you start to do up a strategy and the strategy will uncover the fact that if you follow the line you had been following it may not work to solve the way the problem exists today. And that's 95% of the time, that's the policy. It's modifications, adjustments, amendments, sometimes even initiatives but not grand initiatives."

Whether diplomatic respondents found themselves more concerned with establishing the broad outlines of negotiation policy in response to changing world conditions or whether, because of their particular roles and responsibilities, they were more aware of policy constraints, all held tacitly to a view that is challenged by the IPPME study. For them peace is achieved in such a conflict *primarily* by consultation with elites in Middle East governments.

As Hall put it,

"Forget TM. You've got Rabin and Assad. They have certain needs and requirements. This isn't a question necessarily of affecting mass -- I mean popular attitudes -- although part of that negotiation is. But it won't be effected this way. It'll be affected by what these two guys do and what they say. And so our objective is to change that. This [the IPPME study] is not relevant to that. TM is not relevant to, or any of the conclusions reached in here are not relevant to affecting the attitudes of two men."

Negotiation in this situation is described as a matter of effecting the will of the principals; of selling a product to them; and selectively using information to do so. Hall characterized himself as "a believer" rather than "objective analyst."

"I am not dealing with probabilities only. I'm dealing with possibilities. It's not good enough to know how Arafat will respond in situation x. I then have to take that and try to make it different -- create a different set of circumstances to elicit a different response and that requires a different use of information. It requires in some respects a much more selective use of information, because you draw not only on information in analyzing. You draw on this elusive factor of, I would call it will -- either his will or our will or Israel's will to go beyond where the analysis will take you. And that's quite different. So, I think and I point out to people often...that there's a certain measure of non-objectivity in this...little situation. I'm not an objective analyst. I'm a believer... I'll say that quite openly and honestly. ... I'm not an ideologue -- but I'm a believer and I will use information to try to make something happen..."

Hall looked at his job as selling and seeking information that would help him in making compelling arguments.

" So I will take from whatever source I can interesting statements, insights, pieces of analysis to use. You see, I'm in the business of sales, that's what it comes down to. The information I get, I want to use because ... my job is to persuade people to do things that they're reluctant to do. So, I'm interested in looking for the most effective and compelling way to present a set of arguments. So I'm looking constantly for those analytical insights that will allow me to read what it is the other guy is doing and then find a way of packaging it to convince him or her to do it."

Information is particularly valuable, said Hall, "if I know what somebody is trying to do before I sit down with them. That's truly useful information." He hastened to add that he did not consider this manipulation. "What I need to do is to try to convince, try to find the best way to make someone do something that is in their own self interest and to recognize that it can be done in a way that meets the other person's self interest. That's basically what a negotiation is -- it's a win-win situation."

On the other hand, Tasman gave a detailed description of the information use that is cultivated in the diplomatic community. People are promoted or demoted on the basis of how they select information for attention from an enormous overflow. If they understand what is real in the context of the *use* that top diplomats, like my respondents, would put it to, they advance in the bureaucracies. Only a certain sensibility survives. The sensibility is pragmatic and defensive, involving monitoring information for what's "going to come back to bite you."

Each of the respondents mentioned the importance of intuition in their work and in selecting information. Paris said,

"It's intuitive. I mean I can look at something and I'll make a judgment pretty quickly. Is this useful or not? Have I learned something or not? What do I make of this? Sometimes, some of the more interesting things that appear are analyses that are written in the newspaper. ...It will...give you a different slice that you're not getting, that you won't get [otherwise]... But sometimes what appears in the press, ...reflects a different slice of people who are nonetheless important. Or, what sometimes appears in the press is a reflection of what is consciously being said and then you have to sort of evaluate why is this being said? How does it fit with what else I know? How does it reflect political pressures? How does it respond to a set of political needs? ...The array of information comes in a variety of different ways. And, I think it's more intuition than anything else that sort of tells me, you know, what is it that I'm going to pay attention to; what is it I'm going to put special emphasis on? What is it that is going to be forming or have special weight in my own judgment? "

Hall described a situation where information was received about an alternative negotiating channel. The information was deemed interesting but probably not productive, but turned out to be stunningly productive. He ruminated about the conditions that existed at the time and how he and his colleagues were thinking. He discussed the role of his team's political analysis, their assumptions about what might or might not be possible, and his assessment of their overestimation of their own role. He also spoke to how one responds to such information:

"There's also nothing you can do about it. In other words, you have to keep that in mind too. You get a piece of information that's counter-intuitive. It's interesting. What are you going to do with it?"

If accumulated experience and intuition has been guiding you one way, how can you suddenly launch in another direction? "In a way, we can't be held responsible for not knowing more about [such an instance] because we chose with a piece of information, here it was, we were involved in our own efforts, so we set it aside."

Hall and Paris independently pointed out that, as Hall put it, "Information in the public domain will mean one thing to someone who is in possession of ten percent of additional information and maybe it will mean much more to someone else who is in possession of forty percent of the information." "A relevant piece of information" may be "factored in to a decision that is actually taken." Other information "informs my general view of the Middle Eastern, of the Middle Eastern world and all of this just basically comes in and builds and builds and builds and I try to recall as much as I can, to let it inform in a positive way, my work."

"I will not put that much stock in any single piece of information because for me it is a puzzle that needs to be assembled. Some of the pieces are small, smaller than others, some are very large but it needs, it's part of this picture and if it doesn't jell with my own conceptual framework, I tend to look very skeptically at it from the outset. "

Hall pointed out that information in this context is more than just the bits and pieces that come from reading the *New York Times* or an embassy cable from Beirut or an interesting report from the CIA, or talking to a Lebanese politician and a Lebanese journalist. In the broader sense information has to be seen relative to larger political, organizational, circumstantial "inputs" which determine "how information is received -- how it's filtered."

"The world of information-- it's so large. There are so many pieces. It's hard to categorize it. A lot of it is instinctive. ...A lot of it I reject because it just doesn't sit right which is inevitable because you ...cannot assimilate every piece of information -- some of it you need to reject really to move on. **Rejecting, meaning?** Rejecting, meaning it's not important or it's interesting but it's not useful."

### ***Diplomatic Response to the IPPME Study -- Likelihood of Future Consideration***

There is no place for information like IPPME, on grounds even more fundamental than lack of experience and negative associations that each respondent mentioned. It is not the kind of information that they seek out, especially in the sense of furthering the agenda of selling U.S. diplomatic product to Middle East principals. These respondents described the cultivation and use of information that tends to preclude consideration of research information that is not *intelligence*.

Two of the four diplomats (the most senior Paris and the most junior Sampson) differed from their counterparts in that they examined the study within the scientific rubric. They asked questions about the study taking into account how it was designed partly because they had training at the doctoral level in social scientific analysis.

Even though Paris and Sampson examined the science more carefully, all four concluded they were not likely to use the research in their work, because it flunked the reality test. Sampson indicated that he cultivates an open-minded approach and if given the proper evidence would be very likely to consider it in the future if -- and an unlikely 'if' -- there were more openness in the policy community and the society at large. Paris would consider it only if it took into account "essential variables" that he felt were not explained. Hall and Tasman fairly adamantly rejected any further consideration.

### **Truth Testing and Utility Testing**

The primary question that these diplomats posed to IPPME was, 'is it real?,' and therefore useful. Part of the answer was determined by lack of knowledge and explanation and 'images that come to mind.' Scientific evaluation would not tend to occur unless the study was judged "realistic" and even then, the technical analysis and understanding of the language of time series would be subject to time constraints.

Sampson described how and why he thought his peers and bosses might respond to the IPPME study. He articulated themes that show up very clearly in interviews with the other diplomats. He said that for his bosses and peers it would be "a stretch." His first response in the

second interview was, "The communication process is the point number one-- is very important. If everyone is going from the same perspective, from the same wave length, you are light years ahead of yourself", something which he notes is not necessarily done among busy decision makers.

He talked about the inability of policy makers to read academics' work and the need for academics to do a study in a relevant and accurate way. He said that TM is unknown and not described sufficiently in the summary. Certain images pop to mind when words like Transcendental Meditation are used i.e., stereotypical images like "people begging in airports." And the decision maker has no time to investigate or "make the stretch." Communication, which he says is of primary importance, in this case breaks down.

"Number one, it doesn't say anything about...Transcendental Meditation and TM-Sidhi. ...But obviously you have to know something about what this is because this is saying that this technique, that this intervention of this variable, can have some very positive impact on people. The best that you can hope from this is you read it, you say, 'Wow, yeah, I've got to go check into what TM-Sidhi and TM is. And maybe get some TM programs in the department or something like that.' That's what your intended effect of this, I think, would be because it doesn't tell you anywhere in here what it is. That's another stretch.... You want to tell people right up front...what's going on here [so] that they're not going to have to say, OK, full stop, I'm going to go check this stuff out. Give them a little bit. Tell them a little bit of what's involved. As soon as you say Transcendental Meditation, you think of...Hare Krishna and all that kind of stuff and...most people would just turn off with that right there. You think of people begging in airports and...all that kind of garbage."

Sampson explained the crucial reaction of his bosses and peers, people like Paris, Hall, and Tasman, referring as they each did in their interviews to time:

"This gets to some very basic questions ...how do you get ahead in the academic community? How do you get ahead in the policy community? You get ahead by appealing to your peers and superiors who, if you're in the academic side, are not the policy folks, and if you're in the policy side, are sure as hell not the academic folks..."

How his colleagues and superiors might respond to IPPME was an immediate and ongoing concern for Sampson. Later he said,

"You're not going to get [Secretary of State] Warren Christopher or others to read this. What they want to do is they want to take like Graham Allison's stuff on the Cuban Missile Crisis... That made the transition from the academic community to the policy community and there's a lot of stuff...in there that is hard core academia but he made that transition in a way that got the policy community paying attention."

Returning to our interview from several higher level interruptions, he said he reflected on

"Transcendental Meditation' while he thought about his bosses and peers.

"Anyone who's a policy maker...who he or she would like to get to influence, doesn't have time to make the stretch... doesn't begin to have time and if it's not presented there, very, very clearly, it's not going to happen. ... And this...brings us to full circle. This reinforces the split between the policy and the academic communities....I'm kind of thinking, Transcendental Meditation, and [meanwhile] these guys are running around 24 hours a day trying to plug the latest hole that has sprung up in the dike of the peace agreement. You're

missing. The two are talking past each other. Who's right, who's wrong? That's kind of irrelevant."

Later he said that the IPPME research information was "a good example of what I'm trying to talk about," referring to academics writing for themselves.

"The academic community, the criticism is that they live in a world without...real world constraints and...the imperfections of the real world and...constraints...of time, of money and the other things that are...our biggest constraints on a day to day basis...So when they propose solutions, you don't disagree, you just say, 'that's not real.'"

He pointed out that while a policy piece would be ridiculed by academics as not "rigorous enough," that 99% of his colleagues "aren't going to remember what standard deviation is, much less what is significant."

### **Whether and how Sampson determined if the study was trustworthy**

Sampson did not have time to read more than the summary. However, he appraised the trustworthiness of the IPPME study. Like a doctoral student or researcher among peers in a highly empirical/quantitative political science program, like the one he attended for his Ph.D., he attempted to make more sense of the study as we talked. He started asking me questions about the study: 'Is this what's meant?' Thinking about his experience in Lebanon at the time, he probed the study and its design.

He examined the variables first. He asked himself 'what are they studying?' He observed that Orme-Johnson et al. were not studying the period of greatest intensity of the war as he remembered it from being there at the time. He therefore judged their findings of a drop in war intensity to be spurious. He decided that what they studied was not what they thought they were studying.

He had not examined the analysis more specifically. He asked, 'What should have been happening if they studied this?' And then he discussed causality: 'What caused this was hostilities, not the other way around.' In other words, he said hostilities cause war deaths and war intensity. He didn't understand the causal mechanism -- how it could be true that people meditating could cause the changes described.

Then he asked if there is a control group: a question that would have had to be answered by a much deeper reading of the study. In an awkward attempt to answer without interjecting my views, I answered, 'There wasn't a control group. They used time series analysis.'<sup>340</sup> He then concluded,

"...You know the old placebo [effect]. ...I would wonder what would have happened if there had been a control group that didn't have any TM-Sidhi intervention and whether that would have -- whether that group would have had similar findings as the TM Sidhi group. I don't know. But for Israel and Lebanon -- for Israel and Jerusalem, I would...posit that. I don't quite understand what the causal connection is between this and Lebanon, the war in Lebanon and TM-Sidhi intervention and the variables that they are measuring here."

He questioned war intensity as an objective variable.

"War deaths of all factions. ...That's a function of the war, that's not a function of TM-Sidhi intervention and are derived from news content analysis. News content analysis measuring war intensity and war deaths, makes an assumption: ...It's not measuring objective criteria, what's happening."

Referring again to what he thought was an inappropriate time period for study, he said, "I don't have the time to look into why they didn't get it right..."

In deciding whether the study was worth considering, whether it was trustworthy, Sampson did not test it against his experience independently of his assessment of science. As a trained quantitative analyst, he thought scientifically. The experiment was most pertinent to him relative to two questions: *Given what I remember to have been happening at the time*, are they testing what they say they are testing? What is the logic of what they're doing?

Largely because he has so little time, which he mentioned on six occasions, Sampson missed the fact that the comparison made in the study was participation vs. non-participation days interspersed throughout the study period. He thought that a period of hostility -- a before period-- was being compared with an intervention period and concluded the authors "don't have a clue about the period of time in which they were studying."

"There are so many different options out there...so many different methodologies for being a better decision maker out there, I think I would just throw this one-- just discard it because...I would figure that if these folks don't even know what the history, the time period that they're studying well enough, if they haven't bothered to do their homework sufficiently to know that this was not a high level of hostilities and the level of hostilities was really in the six weeks preceding this period, or two months preceding this period, then I think I would kind of dismiss it." <sup>341</sup>

Validation was not only scientific. While acknowledging that he didn't usually look at footnotes, he explained that the footnote on the IPPME summary referring to 38 studies, some of which were published in the *Journal of Crime and Justice*, *Journal of Mind and Behavior*, *Social Indicators Research*, "doesn't say a hell of a lot." He continued,

"I would like to see some kind of validation of this thing. Has this thing worked before in other circumstances? ... Now, if I saw something like [he names a Harvard professor whom he had met with interest in experimental approaches to Middle East problems] for instance and I saw that he had gone to South Africa ... and I just read a *Newsweek* article about him - - I'm making this all up but I wouldn't be surprised-- that's validation to me; that...it's widely recognized that this had been used."

Aspects of his experience also influenced Sampson's response to this study that he seemed to associate with it. At the end of Part I, he referred to his participation in a workshop with the same Harvard professor mentioned above. Sampson was fascinated with aspects of his experience.

He also compared the IPPME presentation with the difficulties that a former colleague had trying to interest others in EST. He described with some discomfort attending a meeting with his wife to which his colleague had invited them. He then explained that the colleague,



"continued to go around with this big smile on his face ... as ineffective before as he was after he got ESTed. The point is that no one else had a clue of what this was all about. It didn't translate to the audience with whom he was interacting and so, while it might have made him feel good about life, and that's nice, it didn't at all contribute to, his ability to influence others, to interact with others, to interface with others, in any way that affected policy at all."

He talked about his wife's familiarity as a social worker with TM and his general reaction to it:

"Well, not knowing much either about Transcendental Meditation or TM-Sidhi program other than what I've picked from my wife who's a social worker, it sounded interesting. It sounded like the kind of thing that particularly with so many Type A's around [this business] it would be something useful."

He mentioned [four times] the need to be open,

"I just think there are a lot of different ways that we can do our jobs better and the day that we stop searching for those ways, the day that we close ourselves off to remaining open to those kinds of ways, is the day that we better hang it up."

And he mentioned that he is an upbeat sort of person. He appeared to live happily in many worlds. One of those worlds was science, which has influenced some of the approaches he takes.

Although Sampson said he would discard IPPME based on his judgment that it lacked proper controls, he acknowledged that he didn't understand or know enough to completely close the door on this or related studies. He might read them again on a weekend. He was interested in empirical work. He mentioned that he didn't go searching "for stuff. I get a lot more stuff than I can possibly read. And I make decisions. Do I want to read this, do I not want to read this? If someone put this in my box, yeah, I'd take it home and read it on the weekend." He explained that he, probably more than his colleagues, would read it "because of the background that I come from, this would be interesting. Ph.D. out of one of the most empirical programs in the country ... I'd be interested in something like this. I'm not sure that people who came from [less empirical] backgrounds...would find this particularly interesting." He also said, "I enjoy this kind of stuff. I'd pick it up and read it because, it's interesting." But he quickly added, "If you had given me 500 pages, I wouldn't have read it."

The IPPME study did not challenge Sampson to the degree that it did others. He didn't react emotionally. It didn't seem to challenge his world view unduly. Even if he looked at the science and wondered if it was the wrong kind of science or approach -- e.g. noting what he thought was the wrong period for study -- it didn't cause him to dismiss it summarily. While he hadn't had the time and/or motivation to look into the study deeply, he exhibited the kind of mind that considers possibilities and options, that would even consider looking at something like this again, even though

he felt it didn't pass those tests. This may be the same kind of even-handedness that he espoused regarding his work in the first part of the interview -- being honest, considering options and then choosing and justifying one.

Hall responded to IPPME in words almost identical to Sampson's predictions for his colleagues/bosses. He summed up his response to the scientific nature of the information with the phrase, "not impressed."

"Look, I took enough psychology courses and I'm familiar enough with the terms and environment of psychology to know about validation of results. ...I'm not saying they're wrong, but ...I'm not impressed by the fact that they've run studies and done statistical analyses. But let me...keep the focus where I think it should be. In terms of its ... relationship to me and my work, in terms of its relevance, it's not relevant. Not, it's wrong or it's right. It's just not relevant."

Hall's response was highly emotional. Within Part II of the interview he stated 28 times that he found it irrelevant, three times within the first three minutes of the interview. He said he didn't understand why I was bringing it to him. To him IPPME was an inappropriate piece of information to test his use of information. He mentioned this 10 times.

He said that IPPME was not a discrete piece of information, but an approach that could not be integrated, and an academic article which should not be in front of him because it did not belong in the real world. Twice he referred to words like "Maharishi" and "Transcendental Meditation," saying that if he had seen them associated with "Arab/Israeli conflict," he would have tossed the piece, which he then literally did. The second time he mentioned Maharishi he did so with disdain, referring to Maharishi's statement regarding the relationship of stress and "large scale violence, war and civil uprising." Hall said, "I mean you don't need the Maharishi Mahesh Yogi to tell you-- you know that's--" He stopped himself as he spoke. He looked up at me and said, "I had a hard time relating to this, OK?"

He considered the ideas/approaches described to be "not of this world" and outside of the world of the people who are his focus -- the leaders of the Arab/Israeli world. The approach "could not be integrated into the work that we do." Of reading the words "TM" and "Maharishi" in relation to conflict resolution, he said, "That it is wrong would be an understatement of my relationship to this." On the other hand he did not want to discuss its legitimacy: He later reasoned that it might be relevant in another setting, but not in his work. "So what I don't want to do is to trample on the legitimacy of -- I mean to just make this a question of, 'is this a legitimate piece of information or not?' It just doesn't fit into the world in which I function."

Hall read the *entire study* twice and was most interested in whether it had any utility, relevance, or feasibility, and whether it would be relevant to the people with whom he deals. Scientific merit was not important in his judgment. When I mentioned the efforts of the IPPME authors to "put it in some kind of objective framework" that was "published in a journal," Hall said,

"I'm trying to figure out how to use anything in there. I have a utilitarian relationship with information ... Hopefully it is not an ideological relationship. It's utilitarian. How is this useful? How is this going to be useful to me? Why am I bothering reading this? What is this? ...This applies to an op/ed piece in the *New York Times*, an academic article, a report from the CIA, an embassy report, a press report, a conversation. What is new here? Number one. And how can I use this?"

He called the IPPME study "an odd piece of information because I cannot relate to it, at all.

"I'm telling you, I used the John Mack<sup>342</sup> image because that's the one that sprang into my brain. I ...cannot relate to this as a ...tool...What does it have to do with anything -- that I relate to in my work?...As a consequence, most of my brain at that point shut down, even though I continued to read this. I don't get it, frankly. It's not a ...discrete piece of information. And that's part of the problem. This is an approach. ...As an approach I reject it as basically irrelevant to the kind of work that I do. It would be great to get these guys in a room and go through some techniques of stress reduction and hope maybe that would have a salutary influence on their view of the conflict. But that's not of this world ... whereas a piece from *Pravda* would be 'real'."

I asked him what 'real' means.

"'Real' means that it's either involved -- flows from -- it involves people with whom I deal, the Russians and the Syrians. That's number one. This involves nobody with whom I deal. The issue is how this relates to what I do -- see, that's really the problem...with this piece. There's a lot of stuff on conflict resolution that you could have brought me that is academic and theoretical -- highly speculative in nature that I might have reacted to in a far less strident manner. But ...this piece, is just -- it has no relationship -- that's the problem with it..."

Hall was clearly aggravated by my bringing this information to him in the context of our conversations.<sup>343</sup> He was also irritated by the information to the point of reading the entire study twice in exasperation and then having what he described as a "shut down."<sup>344</sup> When he became most adamant at the end of the interview after repeating many times that it was not relevant, not an appropriate piece of information to test his use of information, and not a discrete piece of information, he explained that he was involved in very serious and demanding work, involving other causal factors, primarily the dispositions of the principals, with whom he "deals."

"We're trying to get the Israelis and the Syrians to agree on a process -- to get the Syrian/Israeli negotiations to a point where they could actually lead to an agreement."

Taking up his disagreement with the causal claims of IPPME, Hall said,

"Look, the assumption that stress exacerbates violence and tends to prolong conflicts is ..undeniable. My only point is, we go about trying to reduce stress in ...other ways. By eliminating ...social and economic dislocation, by meeting political requirements, by trying to defuse confrontation, literally by physically separating people. You take Gaza. I don't care how many courses in TM would have been used on the people of Gaza. What has helped in Gaza is that life in Gaza is now more normal than ever. There are no curfews. There are no strikes. People go to the beach. They stay out at night. They don't have Israelis and Palestinians in ...a perpetual state of confrontation. That's what's reduced stress in Gaza.

And what would reduce it even more would be getting at the sources of economic discontent, create infrastructure, cleaning up the place, creating jobs. Now if they want to run TM courses in addition to that, that's fine. But ...I can understand, appreciate the other sources of stress reduction, quote, unquote. That's all I'm saying and that's why this descends as if it was from another planet."

Hall mentioned several times how IPPME does not fit into his world and into the world of his clients. This is not just a world either. These are clients whom he persuades "to do things that they're reluctant to do." Hall's clients are probably some of the more demanding clients in the world -- principals in one of the most recalcitrant conflicts in history. He declared that they couldn't relate to the IPPME study even, "assuming they have the time. And time is very important to them."

"It doesn't fit into my universe, ...You know why?...It's not just me. It's because the world in which I function defines a certain, common set of elements. ...While Arabs and Israelis disagree on a million different things, there is a certain set of values that unify them -- that unify all of the people who do business in this conflict. And Transcendental Meditation is not one of them. You could shop this around to Christopher; you could shop it around to Arafat; you could shop it around to Rabin -- they would all have the same answer..."

Hall explained that based on his experiences of Arabs and Israelis he judged that "they're not open to these sorts of approaches." He contrasted their potential receptivity to IPPME with their receptivity to track two diplomacy efforts that have gained credibility over the years.

"In the world of conventional diplomacy and interrelationship between states and power and conflict resolutions and war and peace agreements and peace treaties and even affecting the quality of people's lives on a national level, TM as an instrument and tool on a government-to-government basis is not something ...that we can relate to. Track two diplomacy by itself has taken years to gain any kind of credibility or respectability. ...What I mean by that essentially is small group interaction among private citizens. Now I was very skeptical about this in the beginning. XXXXX taught me a great deal about track two diplomacy. I learned a great deal from him.... That kind of unconventional approach to conflict resolution, into affecting the attitudes in a positive way -- that I can understand. That's real because it relates ...changing people's views of one another -- not just stress reduction. And that's a universe - - that's a fairly common universe. Rabin can relate to that. Arafat could relate to that. Christopher could relate to that. This however, I would venture to say without too much fear of being wrong, would not be one of the variables that anybody who deals with this conflict could relate to as a serious approach to conflict resolution."

Indicating his concern with other diplomats as well as principals, he asked me what a colleague might have said about the study.

Part of the irrelevancy of this piece for Hall was that it was unfeasible.

"It cannot be integrated -- you can't integrate an entire approach such as this into the kind of work that we do. If you brought me something that said, well, if you work through -- if you work through ...maybe it's the unconventional nature of the piece that -- the unconventional nature of the approach to my work that basically made me think, 'I can't relate to this.' Because it doesn't deal with the conventional aspects of the world in which I live."

Twice he protested that he was not biased or rigid, rather it just doesn't fit:

"This is not my bias against TM. It's my sense that, as an instrument, as a tool to affecting attitudes in this conflict, it's not relevant."

Tasman's response to the IPPME study was remarkably close to Hall's and to what Sampson had predicted. He didn't have deep or direct knowledge of the elements of the study; but images and associations came to mind. He judged it as irrelevant, but did not assess -- and did not feel qualified to assess -- the scientific quality of the study.

Tasman didn't think he would take the research into account in the future. He said,

"And I can also tell you, even if [an author whom he respects] told me to read this, I wouldn't have read it. Because again, I'm reading the title of this piece. It doesn't make any sense to me that it's relevant to what I do in the Middle East ... The second line [of the title] was something that's totally out of my -a system of thinking... I'm just telling you honestly, this is something that I would have not bothered spending even a minute on, with a glance at the first page."

At other points in the interview he said that "maybe I'll start paying attention" if the ideas were better known, disseminated, and if the science proved itself;" if people were involved besides those associated with Maharishi International University; and the study involved opinion and political leaders instead of "people who are already adherents to the philosophy." "It can't be an investment of time right now.... There's intellectual triage you have to go through... You just have to decide what it is you're going to read and not read." He mentioned triage in reaction to the study when he first received it and several times during the interview.

Tasman did not examine the truthfulness of the study. He encountered the title, judged he didn't know enough to get into charts/graphs in the longer article, and determined, "I don't know enough about it to believe in it and therefore to me it's not relevant."

"Somebody did this kind of research and published these kinds of results and believes that this had an effect on social-wide behavior in a number of areas. That may be fine for people who believe in ...the theory and practice ...what the Maharishi was saying. I don't. And therefore it didn't say anything to me. I was in the Middle East at the time that this study was undertaken and could not discern any impact it had on traffic accidents and the war in Lebanon. But that's fine. There's a lot of research going on things that I don't understand."

A revealing aspect of Tasman's consideration of the IPPME study was his lack of engagement in the actual language, ideas, and results of the study. He declared several times that he didn't have much knowledge, didn't understand, and may not have known enough. He had only "three pages of expertise." With two exceptions, he did not mention meditation or details of the experiment. He mentioned the expected results of the experiment once and he expressed doubts about the sample. He asked what involving people who were already adherents to Maharishi's philosophy would do. He suggested what he thought would be a better experiment, i.e. one involving decision makers.

Tasman either did not understand and/or did not accept the causal explanation being explored by Orme-Johnson et al.. He appeared to have so thoroughly rejected this study -- even on reading the title -- that he did not intellectually engage with its details. He explained that he is not a

quantitative analyst. Tasman acknowledged that some of his responses might not be scientific or right. His principal objection to the study concerned his questions about the causal claims made. He assumed that changes in Middle East conditions occur due to behavior of political elites and he asked how people at a distance who are Maharishi adherents could possibly influence decision makers. His comments indicated that he also tended not to trust the Maharishi people.

"Did they take people who were involved in decision making processes or did they take people as was suggested who were already involved in the Maharishi's activities? If they take people who are involved in the Maharishi's activities it's almost like a sect going off into the Judean hills. ...What kind of impact is that going to have on what decision makers in Jerusalem do? If they were taking decision makers in Jerusalem, then I'd better pay attention... So if they said right up here in the front of the article, 'we took 62 of the top 110 Israeli decision makers. And this is how we define top decision makers; rank of under-secretary and above, and the defense, foreign affairs...', and they gave you the criteria and you were able to get to 62 of these people. And over a period of x number of weeks you gave them Transcendental whatever it is -- the Maharishi's business, and in that period we found that x, y, and z happened, then I better pay attention. I don't know who the sample is here. There is the suggestion that they took people who are already adherents to the philosophy. And I don't -- From my three pages of expertise now, which is all I have, I say 'whoa, this is not for me.'...There's nothing here. ...This is my analytical part coming up. ...I had trouble getting past the title. But once I read this three or four pages, there was nothing there to indicate that they had done anything more than talk to people who already believe in the Maharishi's unified field theory or whatever it's called. And what does that do?"

Tasman stated several times that he found the results to be counter-intuitive. "I would not intuit, and I keep using that word 'cause I don't have enough scientific background in this to tell you otherwise. I don't intuit that this has a lot of relevance to what I do or to what the people in the Middle East do."

"They took a couple of hundred people over a couple of months ... And they claim through the use of their methodology to have led to a society-wide reduction in four or five areas: traffic accidents, stock market fluctuation. ...It's counter-intuitive. ...I'm being very unscientific with you, because if I were quantitative in my analysis, I would probably say to you things like the sample wasn't good enough. ...I don't do that, because I'm not a quantitative analyst. It's just -- It's not intuitively a sound way."

His intuition about the IPPME study was uninformed, except for three pages and images that came to mind, specifically i.e. the Anti Vietnam war movement, John Lennon and Yoko Ono, "and all the rest."

"I'm 45 years old. I grew up when this stuff was hanging around the anti-war movement. I never joined this way of thinking about whether or not I agreed with Viet Nam. ...I like music that may refer to this, but ...it doesn't do anything for me. That may also account for my [lack of] interest in reading about it. ...If you ask me would I read it relative to the Middle East process, you get not only whatever inherent biases I may have for or against it. You get also -- I don't think it has any relevance to the Middle East.."

Tasman's reliance on his impressions bore out an earlier description of the importance of first-hand knowledge of a person as a criterion for taking writing into account. Tasman had also

described his assessments of subordinates' judgment influencing whether he took information and the decisions they offered seriously.

Like Hall, but in lesser evidence, Tasman was angered by having to take the time to read the IPPME study.

"If you were working in the psychology department -- which you're not, I gather -- I would have said to you -- and I don't think it's relevant here--I would have said to you that I even was angry when I first read it, because 'why -- why is this person wasting my time, when I told her I don't have a lot of time to do this.' ... I considered this a real waste of time."

He mentioned the importance of not wasting time nine times in the interview.

Tasman, who is widely considered a pragmatist, said that he would take the IPPME information seriously if he judged it to be a practical tool -- meaning a way to influence the elites with whom he deals.

"So if these folks came up with a way of saying to me that we talked to opinion leaders, political leaders and we found a way to calm them down, I'm going to pay attention. Because I may be able to use some of those lessons as a tool. And that's all I'm looking for here -- tools in what I do."

Useful to Tasman is an aid in influencing the thinking of those in the elite whom he needs to influence. Hall, whose philosophy was very similar, denied that this is manipulation, but Tasman acknowledged it as such -- and explained that it is just part of his job. Referring to the Maharishi premises as "Maharishi du-da," he explained that if such premises were found to be useful in influencing a principal with whom he were dealing, he might look at it, just as he considers such information as a principal's favorite color when selecting what tie to wear when he prepares to negotiate.

"If somebody says to me that Yitzhak Rabin loves green, I'm going to wear a green tie when I see him. Now, I'm -- Am I trying to manipulate his thinking? Of course I am. OK, will I wear all green, probably not -- I would look silly. If they tell me he hates red -- I'm not going to wear red."

This view of what is useful relates to how Tasman sees his job. Contrasting his approach with IPPME's he said that he solved problems more realistically. He answered, "I take the *reality* of political events. I take the *reality* of human interaction and I try to figure out a way to get people to talk to each other, when they haven't been talking."

It may have been pragmatic reasoning that prompted Tasman to say that he would give consideration to a paper on Sufi mysticism, which would be indigenous to the Middle East, while he wouldn't do so for IPPME.

"Maybe somebody found something -- maybe there's a key that has unlocked the chamber in people's minds out there. And I probably would have read the first page without saying to you a couple of months ago I'm not going to read this. Is that scientific? No. Is that right? No."

Further, he said that he could list "25 ways in which non-traditional conflict resolution things could impact on the Middle East process that would have more relevance to me as a policy person than this." He mentioned "the role of religion," "societal factors," Jewish and Arab.

"And I could -- we could go through a whole range of ideas which, if you asked me do I apply those on a day to day basis scientifically, I'd say to you, no. I may intuit them, but I don't apply them. But as courses of study -- as to the influence that these things may have on the perpetuation, intensification of conflict or its resolution, I'd say those are real -- because those are things that I have experienced or have seen or believe that people in the Middle East see."

Paris would probably agree with this depiction of what is real, and he agreed with the other diplomats in judging the IPPME study to be irrelevant and not reflective of "reality." However he expressed his disagreement with IPPME author's assumptions about "conflict between states or between ethnic groups." He judged their assumptions to be that such conflict "is a function of the lack of... this kind of coherence as opposed to the fact that there may be "many other factors, like history." He felt conflict, its resolution, and "the ethos of a group or of a state" have other causes or explanations. Paris understood the causal claims made, expressed them in terms that would be recognizable to Orme-Johnson et al., and disagreed with them.

Like the other diplomats, Paris said that he depends for the substance of his work on his knowledge base, his accrued insight into how the principals are in fact relating, what they are saying and meaning, etc. His intuitive ability to make judgment calls flowed from such understandings. He described research as interesting to him to the extent that it gave him a grasp of behavior in the regions of concern.

He said that he determined within the first few minutes of reading the IPPME study that it was not meaningful.

"Well, let me put it this way... Had you not asked me to read it, that's something I would have taken a very quick look at and within the first paragraph, I would have been dubious about it. Now, given the pressure on each day, that's probably the kind of thing I would have put down after that point. I also didn't find it interesting."

He stated, "There's nothing wrong with the scientific process" but that it gave a "false sense of security."

"I believe in the process-- in the process of establishing propositions and then testing them. Obviously there are statistical ways to test propositions but an article like that is appropriate in terms of the scientific process. Where it falls short, as I said, it's not taking into account factors and yet it's producing results and claiming something on the basis of those results..."

He took the science into account, but rejected the evidence because he didn't believe it.

Like other respondents, he argued emphatically three or four times, that IPPME did not take into account other factors" that might influence conflict on a daily basis in Lebanon [which] could be totally extraneous to what is suggested in the article." He concluded that he could not "draw



many lessons from it that would have much application." He could not predict whether he would take such research into account in the future, unless it took better "understanding of the texture in which the ... Middle Eastern reality unfolds."

The way he approached this discussion suggested that he didn't trust the experiment or the researchers. For Paris the IPPME measure of "purely deaths," for example, did not explain enough. He needed a picture of intervening steps -- almost a hands-on verification of behavioral change. Paris described the kinds of factors that were not taken into account, the kinds of questions a study like IPPME would have to answer to satisfy him, and the kinds of studies and statistical analysis he would find interesting.

Paris offered counter-explanations for IPPME. While he thought "issues like crime and so forth" might be affected, he questioned the impact on conflict. He listed the factors that he felt were missing three times. On the third occasion he said he would need to:

- See "much more systematic over a much longer period of time";
- Know "the size consistently of the groups" and .. "that the groups themselves were diverse ethnically";
- See "a correlation between the relationship between those groups, those specific groups that were going through this and their behaviors, especially in context of ... who it is they are in competition and conflict with.  
Did it change their behavior?  
Were there a reduced number of incidents?"

Paris asserted that

"Any scientific process, if it's based on hypotheses and how you test those hypotheses and making a judgment on what the essential variables are, cannot exclude variables that are central or that could affect the results. And ...this does... in my view."

He would need explanation or controls about such alternative explanations as historic competition for territory and periodic lulls in fighting, which he said are normal because people may, for example, need to rearm.

To be of interest to Paris, research would need to provide detail that would help him to discriminate and make judgments in his work. Such research would be derived from a more familiar causal model that, for example, would incorporate "more of an effort to look at the factors within these conflicts like ...the rate of taking casualties, the relationship of that to arms supply ..." He said he would be interested in a study about "what works in negotiations."

"What are some of the underlying consistent principles or elements that you can see working across negotiations? Taking into account ...that...every negotiation is unique and...every effort to create generalizations is going to exclude certain particular elements. ...That's part of the process of trying to generate and discern patterns and generalizations. You can't account for everything but ...you may be able to discern certain things that will be common in varying ways in different situations. ...If you presented me a study ...that compared a series of different kinds of negotiations and what made them succeed, and what

seemed to be common, and where were problems created? I would be interested in reading that because that might produce something I hadn't thought of."

Because he lives in a world where it is relevant, he would be interested in a study about coercion: "Studies that again would look at where coercion works and where it doesn't; what has to exist to have it work. ...I would be interested in that."

Paris indicated indirectly that he might not have the time or expertise to evaluate a piece of information like IPPME. He said that full-time analysts in the intelligence community "might be much more open to looking at a far more diverse set of information because that's their job." At this point his job is "to try to change the world as we know it in the area where I have the responsibility."

"The more general the responsibilities, the wider the array ...of points of information that will be relevant; and in all likelihood, the less time he'll also have or she'll also have to be surveying all the points of the information. So you have a tension between what is a wider field of relevant information, but less of an ability to have the time to actually survey it and then read and absorb it."

Like Tasman, Paris also said he did not find the IPPME study *interesting*. He wasn't inclined to read it as he might another theoretical piece. With a piece of information like a newspaper article he might be expanding his "knowledge base." Another theoretical piece -- even one that was not directly related to his work or one that was also statistically based -- might be of interest. With an advocacy piece he might be reading because "(a) I may want to read it because I figure ...I'm going to learn something (b) ... It's an argument that I at least should be aware of:...does it make sense or not? (c) How does it affect what I already believe?" Regarding the IPPME piece, he said,

"I would not have read on. Is there something else that would be theoretical I'd look at the first paragraph and say, yeah, this is worth reading. ... Its immediate relevance may be distant but there's something interesting here that I want to think about. That it forces me to think about something that I haven't thought about. And it's not that it requires that I have to see the explicit assumptions. This, I would require to see it because I think that it is so narrow and it so distorts the reality that I need to know much more about how they, why they chose to pursue this. **And narrow in the sense?** Narrow, in the sense, as I said, they are trying to, it is excluding a whole set of factors that are highly relevant to these kinds of results."

Paris explained that looking at a news piece about Arab and Israelis he does not need to test assumptions necessarily. The IPPME study, however, raises too many questions. Paris called the approach "pseudoscientific":

"Look, this is a piece that also seems a-historical. You take it out of a context and you try to create what I would try to describe as a pseudoscientific approach to try to deal with the issue of conflict resolution. Pseudoscientific in the sense that its, you know, the proposition. I only read the summary...To be fair to them I would need to read the whole article to ...understand ...what led them to produce this proposition, this hypothesis. ...What was the

derivation? What was the genesis? Why did they come to it? Now, maybe in the longer article, they deal with that. But at least in my mind it leaves too many questions."

The final several minutes of the interview involved Paris' explanation of the above characterizations, i.e., pseudoscientific, narrow, distorting reality, leaving too many questions. And then he said, "Yeah, it gives you a false sense of security. It leads you to think that maybe this is what could actually reduce conflict in Lebanon..." If one accepted the science one would have to accept such an interpretation which to him was not consistent with reality.

At the end of the interview he suggested factors alternative to IPPME that might reduce conflict. In doing so and in tacking on a possible correlation involving Transcendental Meditation that he would find acceptable, he revealed something about the nature of his own causal assumptions and those he would consider relevant:

"It leads you to think that maybe this is what could actually reduce conflict in Lebanon...I would be quite prepared to suggest that would not reduce conflict in Lebanon. ...Maybe having Syrian forces in or out would affect the level of conflict in Lebanon. Maybe disarming some of the militia would affect conflict in Lebanon. Now, you could argue if we had wide circles of people engaging in Transcendental Meditation, then maybe you'd increase the chances that you could disarm the militia and that I might be prepared to think about."<sup>345</sup>

### ***Summary***

What drove information use for these respondents was the disciplined daily funneling by many layers of people of an enormous stream of information -- namely intelligence-- toward officials like these respondents who are normally already apprised of most of what is happening. They used information to persuade, manipulate, or even coerce elites within Middle Eastern governments to *buy* U.S. propositions.

When confronted with IPPME the respondents would instantly raise the question pertinent to all such information, "Is it real?," meaning pertinent to their task in the region. 'Right or wrong?' would not be considered. All of the respondents considered whether the IPPME research information was 'real,' with reference to their often frantic, "24-hour" level of activity. Tasman, for example, as a diplomat among my respondents with perhaps the longest tenure in a bureaucracy, coolly described the necessity of triage and explained that he "would have not bothered spending even a minute on [IPPME], with a glance at the first page." He did not engage in the details of the IPPME study because from the outset he didn't believe it. He would only do so if it served a purpose within the framework of his persuasive efforts.

Hall's reaction, which he called a "shut-down," indicates something about another test alluded to by Sampson, which others were wary about naming. Hall in his exasperation mentioned several times that IPPME was "not of this world." In comparing it with John Mack's exploration of extra-terrestrial visitations to earth he meant 'unreal' in an even broader cultural sense than the

professional unreal of data that might be mined by a diplomatic underling and rejected. *Unreal* in this sense is unacceptable in the larger culture -- the Hare Krishna in airports variety -- something almost offensive, foolish, foreign, abhorrent, *spiritual*. In the judgment of diplomats who themselves are known to have serious religious commitments, though they didn't mention them, IPPME was not deemed compatible with the Middle East traditions.

IPPME was all the more abhorrent to Hall because of its contrast to the serious, prestigious, important and successful work from which he derived personal and professional identity. He would be much more comfortable discussing the components of that success instead of something that he considered as far from relevant as he could imagine.

Real has another dimension: the place assigned by journalists and others to ideas within the culture. Sampson said that he needed validation that IPPME was *real* within the universe of viable ideas in the culture as described by say, *Newsweek*.

Hall pointed out that consideration of IPPME as not *feasible*, repeating many times that the IPPME study was not a discrete piece of information. It was an approach that had to be adopted whole and couldn't be integrated. Part of his experience of shut down stemmed from the fact that it was also *too challenging* -- something that he considered outside of "the conventional aspects of the world in which I live." At one point Hall said that he had no time to consider whether the IPPME findings might have relevance for negotiation climates because he was too busy communicating within those negotiations. As he pointed out in relation to a counter-intuitive idea that later bore fruit, acting on such ideas can't be expected because "we were involved in our own efforts."

Concern about what others might think of openness or acceptance to such ideas in an environment of intense collegial relations between diplomatic officials and intense personal relations with leaders of other countries was an underlying concern expressed by Sampson and Hall. They had to be concerned about whether the ideas would be 'real' to colleagues and bosses who are their final judges on a daily basis.

Two of the four respondents, Paris and Sampson, did apply truth tests that involved engagement in the scientific arguments and weighing of scientific quality, although neither man -- in the context of work -- would have done so outside of the interview context. Paris found no problem with the scientific quality, but like the social scientists who reviewed it, determined that something must be wrong and conjectured about what it might be. His concern with scientific quality involved the question, is it real? He said IPPME didn't reflect the "texture in which the ... Middle Eastern reality unfolds," requiring behavioral data -- intelligence, basically -- to verify the results. Paris rejected the study because it "distorts the reality," and therefore the science must be flawed. He assumed it might have a pseudoscientific origin.

Sampson's questions also reflected a concern for whether the study was grounded in reality. He initially dismissed the study when he found it was not conducted at the time of most intense conflict. He continued to question the study while we talked. Both men tangled with the different causality described by the study and neither had grounds for understanding or accepting it. Both doubted the competence of the IPPME researchers relative to knowledge of the Middle East.

In fact the study technically was not flawed in the ways that it appeared to be to these respondents. Because the study contrasted participation and non-participation days, it was not so critical that it be conducted at the precise time of greatest conflict, for example. As much as Paris tried to and did understand the IPPME propositions, he couldn't accept them enough to consider that it didn't matter what the ethnic makeup of the meditating group was -- from the stand point of the experiment; that the effect expected would radiate regardless of ethnic makeup to any groups in conflict within the affected populations. Tasman's concern about Maharishi adherents participating in the study overlooked the fact that the behavior being measured had nothing to do with them, but with people independent and at a distance from them. The intervening variables that Paris needed to have verified were also technically accounted for.

As far as communication, however, the diplomats were asking for verification in the language in which they operate, referring to the details of conflict as they normally confront them. As Tasman made clear, science is not a language that communicates readily to he and his colleagues. Scientific rhetoric demands careful attention and the more specialized rhetoric associated with time series analysis and with the TM techniques requires more than these diplomats had time *or inclination* to give. This is where the associations and images formed without much knowledge about Maharishi or Transcendental Meditation are perhaps crucial. Tasman explained that to engage in the study he needed to believe it, which he did not. Even Paris, who followed the argument carefully, would have had to credit the idea to explore the implications of the findings more fully.

But Paris found the assumptions, the approach to conflict resolution, and scientific approach all disturbing. Orme-Johnson et al. signaled that they did not approach conflict resolution using the language of negotiation, coercion, and intricacies of war-making in the region. They did not produce the kind of correlations that are pertinent in Paris' judgment, i.e. correlations that indicate under what conditions one might have confidence in manipulable variables in the context of social control. Orme-Johnson et al. are beating another sort of drum. They are saying social change might occur with the introduction of a new element unrelated to understanding the details of the conflict or the region.

## **VI. *Comparison of Respondent Groups***

Each respondent group, having distinct functions, objectives, and methods, might be expected to consider the trustworthiness and usefulness of research quite differently. Yet in important ways their assessments were similar. Most respondents had misgivings, but scholarly reviewers, for example, gave them more weight, citing historical and philosophical precedents. While asserting scientific authority, they were not more scientific than other respondents in their approach, in the sense that they did not seriously consider whether they might be wrong. With the exception of two reviewers, they did not ask a question inherent in the truth test, 'Even though I don't believe these premises or outcomes, might they be true?'

Scholarly reviewers shared in the pattern observed among all groups: The premises and conclusions of IPPME seemed highly implausible. The research challenged fundamental assumptions about causality. Although, perhaps due to scholarly reviewers' preparation and approaches to IPPME as reviewers, the study elicited a more fundamental level of dissonance.

Respondents were also concerned with the consensus in their profession and in society. They gave primary importance to the usefulness of the information in relation to that consensus. Each group described a distinctive assessment pattern relative to their concerns with what might be useful. Scholarly reviewers, most of whom were concerned that IPPME was not legitimate and posed a threat to their paradigms and professional status, first observed 'Something must be wrong' and then sought alternative explanations of the data. Reporters questioned whether IPPME could be admitted to the universe of possible stories or, more typically as reporter Nielsen described, whether it was one of many bad ideas they should keep out of the paper. Most reporters were concerned with whether IPPME was credible and, more importantly, relevant to the foreign policy debate. Members of Congress and congressional staff calculated IPPME's political feasibility: Were such ideas within their jurisdiction? Would they be of interest to constituents and colleagues? Lobbyists and human rights activists asked whether IPPME was relevant to their mandates, and especially what the reaction of Congress and the media would be. Diplomats raised the question, "Is it real?" - meaning pertinent to their task; whether IPPME was right or wrong was at best a secondary concern.

Respondents across the groups echoed the diplomats' concerns that IPPME did not fit their policy agendas, strategies and need for information. This was true even for scholars and scholarly reviewers whose personal scholarship was not all focused on conflict resolution in the Middle East. Scholarly reviewers were concerned as virtual gatekeepers. Some were concerned, albeit implicitly, about potential negative opinions of funding sources, such as the intelligence community.

The groups varied in their examination of scientific quality. All scholarly reviewers to varying degrees examined scientific quality. Reporters tended not to examine it at all. The groups

were similar, however, in that only a few respondents within each group incorporated such examination into their assessment. Within each group over half rendered decisions about the truth of IPPME on the basis of authority, expertise, experience, and utility -- not as a result of (and often in spite of) examining the scientific argument and the data.

Respondents in all groups assessed the IPPME research information as they did partly because it was unlike the information that they typically encountered or considered useful. Useful information for them fit specific purposes: People in the diplomatic service in particular are promoted or demoted on the basis of their information- or intelligence-filtering skills. The information is *used* in a foreign policy process that is narrowly defined, requiring its participants to master knowledge of complex regional relationships as well as several languages and a continuous flow of events. And that is based on legal precedent and on tacit assumptions regarding how and why conflict occurs, how to change it, and even about human nature itself.

What have evolved are boundaries to "the game," meaning the policy debate, the tacit story line, and the "radar screen" to which the media then attends. Once a piece of research information is judged to fall outside the debate, "it sort of drops off a cliff and it doesn't matter" (reporter Berman).

IPPME was not just rejected by the majority of respondents in every group as outside current scientific paradigms, newspaper story lines, Congressional jurisdictions, lobbying agendas, and foreign policy debates. It was decisively located in a non-compatible area, which a little over half of the respondents vaguely and negatively associated with spiritual and other practices which they described as unreliable or reportedly unreliable: "divine intervention," gurus as "con men," EST, ESP and other phenomena, all depending on the particular belief (or as some interpreted it, gullibility) of the participants. The most common stereotype -- repeated in a range of derogatory tones by one member of each group -- was the image of Hare Krishna devotees in U.S. airports selling flowers.

14 of the 35 respondents did not mention derogatory images. All but two of the 14 -- reporters Berman and Gaines -- already had some direct knowledge of Transcendental Meditation or other meditation practices. Gaines subsequently learned to meditate, but not at the time she wrote her first article on the subject. Both explained that it was not their job to judge the truth of IPPME. Most respondents had insufficient information to distinguish IPPME from such negative images. Having neither the time nor the expertise to study IPPME more deeply, they would require a much more succinct, policy-oriented presentation than the summaries or article they reviewed for this dissertation. For many, decisions not to take time to consider IPPME were more emotional than rational, involving a deeper commitment than some respondents were prepared to make, i.e., examining their own assumptions in the light of different ones. Respondents across all of the

groups said that they routinely relied on other filtering mechanisms to screen such research for them. They would require reassurance from such sources such as the *New York Times*, the *Economist*, *Newsweek*, or *Foreign Affairs*.



## ***VII. Comparison of Respondents Across Categories***

In the previous chapter, I explained that in examining IPPME the majority of each respondent group gave precedence to questions of relevance, feasibility, and challenge to the status quo. In this chapter, comparing respondents across the groups will give more perspective about a secondary concern: the trustworthiness of the study. Using the 35 respondents' assessment of scientific quality as a primary means of distinguishing between them, I identified three groups (see Chart I)

<b>Group 1</b>	12 respondents who gave minimal or no assessment of the scientific argument.
<b>Group 2</b>	15 respondents who assessed scientific quality at different levels of thoroughness, but who did not tend to take this assessment into account in their final judgment.
<b>Group 3</b>	8 respondents who said scientific assessment was decisive in their judgment.

**Group 1:**

Four of the 12 who assessed IPPME's scientific argument minimally or not at all expressed skepticism about scientific solutions to social problems in general. Three of them were reporters (MacBain, Taft, and Klein) and the fourth was a governmental consultant, who also worked as a reporter (Kern). They said,

"There is no track record of scientific solutions to social problems" (MacBain).

"I don't think you can put people in a box and conduct experiments with them and come up with any -- I just think there are too many variables. And they mention here accounting for variables and stuff like that" (Taft).

"It looks like if you have enough money and if you are willing to waste your energy on the connection between x and y, whatever they are, you'll find something. But again, it's quite bizarre" (Klein).

" I don't believe really in social science and even political science. So, I think human behavior is not a science and it's more like art. Even economics which is supposed to be like the most scientific of all the social sciences, I think, is probably -- now you see there two geniuses with Nobel prizes in economics, and they still come with totally different analyses and associations. So clearly there is nothing certain about it. It's -- I think it's intellectually challenging and it helps you to organize your thinking, and but to call it a science." (Kern).

**CHART 1 -- Aspects of Observed Assessments, Organized According to Respondents' Consideration of Scientific Quality**

Total n=35	Group 1 n=12	Group 2 n=15	Group 3 n=8
	<p><b>5 Reporters</b> MacBain, Taft, Klein, Berman, Nicosia</p> <p><b>2 Congressional Staffers</b> Thomas, Roach</p> <p><b>2 Lobbyists</b> Krachon, Golden</p> <p><b>1 Consultant/reporter</b> Kern</p> <p><b>2 Diplomats</b> Hall, Tasman</p>	<p><b>4 Scholarly reviewers</b> Fisher, Gurney, Walsh, Lambert</p> <p><b>4 Reporters</b> Nielsen, Nolan, Rohan, James</p> <p><b>3 Congressman / Staff</b> Rep. White, Rumar, Glaser</p> <p><b>2 Analysts/ Advisors</b> Crawford, Cox</p> <p><b>1 Human rights research director</b> Neumann</p> <p><b>1 Diplomat</b> Paris</p>	<p><b>2 Scholarly reviewers</b> Moore, Harrington</p> <p><b>1 Reporter</b> Gaines</p> <p><b>1 Senator and 1 Staff</b> Sen. Marcheselli, Banks</p> <p><b>1 Human rights attorney</b> Kaplan</p> <p><b>1 Center Director</b> Roth</p> <p><b>1 Diplomat</b> Sampson</p>
A. Thoroughness of Assessment of Quality of Science	Minimal or no assessment.	Mixed -- Evaluation to a point by respondents with expertise; as fully as possible by some without expertise; and minimally by others.	3 levels -- thoroughly by respondents with expertise to as fully as possible within time allowed.
B. Attitudes about Science	Skeptical about science; judged it irrelevant.	Valued science, but not this experiment. "Resisted the data."	Valued science, took it into account in assessment/decisions.
C. Level of Engagement -- crediting of the research	Didn't credit -- did not know enough.	Engaged, but also experienced cognitive dissonance.	High engagement.
D. Weight given to Utility -- Kind of Utility Test	Emphasis on lack of relevance/expediency.	Concern with challenge to the status quo.	Interest in feasibility, challenge. Judged relevant because of outcomes.
E. What Involved in Trustworthiness Assessment	Assessment based on experience and authority.	Assessed in relation to consensual belief. Concern with legitimacy.	A lot of caution, but science helpful in continuing to think about ideas.

Kern called IPPME "political psychology," which he said came from "people who sit in their ivory towers [who] are not in the real world." He called it "marginal" and "irrelevant."

Five respondents (Congressional staffers Roach and Thomas, diplomats Hall and Tasman, lobbying organization executive Krachon) found the scientific presentation irrelevant to their jobs. They didn't value the format (Roach and Thomas); felt one would have to believe the research in order to consider it seriously (Tasman); expressed skepticism about statistical analysis (Hall, Krachon); and described being too old or too far advanced career-wise to put aside practical concerns for an academic or conceptual presentation (Krachon).

The four respondents who thought science could not be applied to social problems and the five who felt scientific presentations were irrelevant to their work also associated IPPME with negative impressions of gurus, cults, and unreliable spiritual practices. All dismissed the premises and outcomes of the IPPME research. Two respondents who did not assess the science expressed no negative opinions about science or IPPME: reporter Berman and lobbyist Golden. They rejected IPPME because it was irrelevant to the debate (Berman) or to organizational agendas (Golden, who was the only respondent who said she believed the study.) Reporter Nicosia's judgment of IPPME as untrustworthy involved no mention of the scientific argument.

The most important question to Group 1 respondents involved expediency: Is this information I need or someone else needs? Would it be too risky to consider? Relevance was the concern of ten of the 12 respondents. Four of the ten explicitly stated that they were not interested in whether IPPME was right, wrong, or true: diplomat Hall, Congressional staffer Roach, reporter Berman, and lobbyist Golden. For them, almost the *only* consideration was that IPPME was irrelevant either to high diplomacy, to Congressional funding, or to "the debate" which reporters cover.

Relevance involved another decision, namely, whether to credit the research enough to consider it further. Several respondents referred to the limited amount of time they had to make decisions and expressed anger at having to take *any* time to consider scientific research -- much less an unorthodox piece like IPPME.

## **Group 2**

A second category of respondents examined the scientific quality of the IPPME research information at different levels of thoroughness and decided that they would "resist the data," in the words of presidential advisor Cox. They were so inclined because they

- determined at the outset that 'something must be wrong,' and observed that IPPME did not *fit* in the world as they knew it. The evaluation of scientific quality was focused on and eventually limited to a search for indications of what might be wrong: scholarly reviewers Fisher and Gurney.
- rejected IPPME's assumptions and implications: scholarly reviewer Walsh, diplomat Paris, presidential advisor Cox (initially), and think tank scholar Crawford.
- could not accept IPPME's metaphysical approach: scholarly reviewer Walsh and reporters from pace-setting newspapers Nielsen, Nolan, Crawford, and Neumann.
- found, based on a more superficial examination, what they considered to be flaws in the research quality, justifying their decisions to not consider the research seriously: human rights research director Neumann, reporter James, and Representative White.
- found, also after a superficial evaluation, that IPPME was too challenging and/or infeasible for colleagues or bosses: scholarly reviewer Lambert, reporter Rohan, and Congressional committee staffers Glaser and Rumar.

The last four were slightly more likely to take IPPME-related research into account later. Diplomat Paris was also among Group 2 respondents who were more likely to take the research into account than the remaining ten respondents, although he found it uninteresting.

The first and second groups of respondents pursued their assessment of scientific quality within institutional and strategy constraints. The second group's decisions show the consequences of juxtaposing scientific corroboration with a set of ideas that violate their common experience or philosophical assumptions. Even reviewers with high-level expertise experienced cognitive dissonance and decided something "must be wrong." Like other respondents in Groups 1 and 2, they found it difficult to step into the circle of IPPME assumptions and measurements; in order to do this they would have had to step out of their own comfort zone.

### **Group 3**

Eight of the 35 respondents found scientific quality decisive in their assessments of IPPME and in their decisions to give further consideration to related research. Among the eight were respondents whose

- more thorough reviews of the research were central to their decisions to publish it: scholarly reviewers Moore and Harrington.
- less thorough reviews were not totally convincing to them, but were decisive for further consideration: reporter Gaines, policy center director Roth, and human rights attorney Kaplan.
- evaluations took into account IPPME's scientific quality even though their institutional commitments would dictate that they not incorporate its results: Congressional committee staffer Banks, diplomat Sampson, and Senator Marcheselli.

The entire third group examined scientific quality largely independent of institutional and societal constraints. Their view of IPPME's trustworthiness was therefore more like what Weiss found: primarily based on how the research conformed with their expectations, independently of what might be valued by their agencies. Their judgments of IPPME's high quality tended to offset its seeming implausibility.

The context for examining of trustworthiness for this group tended to be cautious intellectual scrutiny with much less cognitive dissonance than Group 2 respondents. Group 3 respondents did not embrace IPPME, but did acknowledge the scientific inquiry and were curious about the scientific results. Their concern with utility tended to focus on questions about its feasibility, given what they knew of the Middle East and the world. At the same time they recognized that IPPME would challenge conventional wisdom. The scholarly reviewers in Group 3 were also more committed to following the scientific results where they led, though they eventually succumbed to pressure and doubts raised by IPPME critics.

### ***Respondents' Concerns About Scientific Quality***

An analysis of respondents' scrutiny of IPPME's scientific quality indicated a common set of concerns, but different ways of posing them. The most frequently-mentioned concerns were addressed to the counter-intuitive nature of the IPPME results and the fact that, as Harrington pointed out, the research was at an early stage; it had not yet identified in detail the mechanisms involved.

Respondents who took their scientific evaluations into account had the same concerns as those who were inclined to resist the data, but they were willing to suspend judgment or await further evidence. For example, seven respondents didn't accept the IPPME premises or assumptions. They considered them to be "unobservable" (Walsh), religious (Fisher, Crawford), "touchy-feely" (Banks), "do-gooder" political psychology (Cox, Kern), or not reflective of the Middle East or of the models and arcane knowledge of specialists (Paris). While Fisher would end deliberation of IPPME, Moore would and did cautiously consider further data. Banks, like Paris, found the study "unrealistic," but was "not willing to rule out the fact that maybe their experiment may have something to do with it."

Paris, James, Nolan and Taft needed hands-on verification, or they could not imagine verifying the results. Likewise, Marcheselli said he had difficulty imagining a riot scene being calmed down by meditators. But he, Roth and Kaplan would suspend disbelief, and would consider additional demonstrations or evidence of the IPPME premises. Krachon, who was born in the Middle East, and Nicosia, who spent 17 years there as a reporter, could not imagine Israelis and Palestinians either meditating or changing their behavior as a result. Roth, who also had

considerable experience in the region, could see the benefit of reducing stress, whether or not the study had the described result on conflict at a distance. She was willing to consider further studies, though cautiously.

Six respondents questioned how such a small number of meditators could affect such a large population at a distance. Congressional staffer Rumar commented that just because the numbers of meditators doing the intervening were so small, Congressmen would not be interested in IPPME. Presidential advisor Cox said he would be more convinced, "if they showed me that they could get not 152, but 152,000." Taft (who said he was skeptical about statistics generally), based on what he called reportorial instinct, mistrusted the numbers. Kaplan called the findings "tremendous leaps," and "too good to be true," but he compared their scientific basis with religious claims. He said that he might be convinced, however, because the statistics were "provable." The 1% effect was also counter-intuitive to reporter Gaines, but she wasn't interested in whether it was true or not-- just in how one might think about it. She sought out explanations of the mechanism involved from physicist John Hagelin, which allowed her to think about the concept from a different perspective.

Fourteen respondents wondered about how they could have confidence in the findings and expressed their reservations about controls. Given the difficulty of controlling for innumerable variables in a macro-level experiment, these questions were central to the study and its review. Respondents varied in their expertise, thoroughness, and in their comfort with results.

The scholarly reviewers responded three ways. Fisher and Gurney charged in their critiques that Orme-Johnson et al. had not sufficiently controlled their experiment, though neither had thoroughly examined the Box Jenkins analysis used by Orme-Johnson et al. to test the validity of their findings. Walsh said that IPPME met normal standards, but should not be published. Moore, after twice examining the IPPME data, cautiously wrote, "I do not imply that the author(s)' claim is correct but rather that the authors' claim and the findings are, at this point, consistent."

In considering future research, however, Moore would require a controlled experiment because "new evidence of an equivalent nature doesn't mean anything." Moore said that IPPME raised the question of "how to attribute causation more carefully." He explained that although a hallmark of the sciences is doing research that can be replicated, "one of the hallmarks of the social sciences is that if it's replication, it's not contributing to the knowledge and you don't publish it."<sup>346</sup>

Fisher, Harrington, and Moore asked for more certainty from future Maharishi Effect research than they would of other research. Other social scientists might argue that such certainty cannot be delivered by statistics, which is the science of probability and not of certainty. Moore described his peer review as a craft requiring "surprisingly easy" judgment calls, involving levels of comfort and experience with the ways that data tends to behave. His difficulty in posing alternative

explanations for the variables not associated with the war in Lebanon -- crime, auto accidents, fires, stock market, and national mood -- contributed to the cognitive dissonance he felt, even six years after reviewing IPPME. While still bothered by the "little stuff," he was convinced there was an alternative explanation for the war data and had not really considered Orme-Johnson et al.'s responses to the alternative explanations proposed in early written critiques. His judgment call requiring randomized design of future IPPME studies reflected his discomfort (and informal urgings from colleagues) more than scientific necessity. Moore understood that randomized design could be so prohibitively expensive as to preclude further experimentation and publication.

The respondents without statistical expertise brought up concern with controls and expressed varying degrees of comfort with the ideas. Reporter James, who felt uneasy about IPPME, asserted, "They can't control for other factors, so it's not a testable theory." Her rationale for not examining the research further was similar to Taft's intuitive feeling that the figures were "cooked." Reporter Rohan and Congressional staffer Glaser, who were highly skeptical and ambivalent, mentioned the need to check controls if they were to examine IPPME further. Staffer Rumar felt that the research stood up to his efforts to "debunk" it, though he wondered if Orme-Johnson et al. had taken into account efforts like "shuttle diplomacy or diplomatic or military developments." In justifying his decision not to take time to learn more about IPPME, Representative White referred to his knowledge of statistics and the possibility of "literally thousands of other explanations." Reporter Nolan, who gave much more considered thought to the study, doubted "the sort of spill-over effect of this claim." He said his difficulty was with the metaphysics, but as a result he also doubted the measurements. He understood that controls, in the way they are normally designed, would be hypothesized to function differently because of the causal claims. Sampson, who had little time but more expertise than others, asked whether controls were in place in the study and considered the answer decisive. On the other hand, the fact that Marcheselli, Banks, and Roth were genuinely interested in the outcomes may help to explain why their response to questions about controls involved seeking further research.

Respondents without advanced statistical training didn't understand that participation days (when numbers of meditators had reached a threshold) were alternated with non-participation days (when numbers of meditators went below a predicted threshold), effectively providing a control. Working on a highly technical level, Gurney also missed this point. He didn't notice that Orme-Johnson et al. achieved comparable results with transfer function, cross-correlation analyses, and a separate impact assessment analysis using four binary variables -- one for each level and pattern of meditator participation. Like respondents without advanced statistical training, Gurney missed the implications because he misunderstood the details of the study, although they were explained in IPPME and in a response to a critique of it that appeared two years later. Gurney's editors assumed

that he was right. These assumptions gained momentum because the review involved an emotionally motivated effort to end debate.

Five respondents asked a related and equally pivotal question: 'Are Orme-Johnson et al. measuring what they say they are measuring?' Fisher criticized Orme-Johnson et al.'s specification of the affected population and therefore the size of the meditating group as not inclusive of nearby cities in Jordan and Syria. Fisher rejected Orme-Johnson et al.'s response that they had always considered the influence of the Maharishi Effect relative to political and social boundaries. Similarly Fisher and journal editors rebuffed the Davies and Alexander paper that provided multiple examples of interventions like IPPME where no dispute about political and social boundaries vs. geographic boundaries would arise. Fisher contended that Orme-Johnson et al. did not understand the Middle East, but his real argument was with their meta-behavioral level of analysis and their causal attribution.

Paris' concerns reflected his focus on the details of the Middle East conflict. He would want a closer examination of "the factors within these conflicts like the rate of taking casualties, the relationship of that to arms supply ... an objective set of things that might be worth looking at to see if there's some kind of correlation among all these factors and behaviors." Paris' deeper concern was with IPPME's description of causality. Given the way causality is commonly understood, cessation of conflict brought about without focus on the conflict's behavioral realities was beyond comprehension and the research was not studying what it set out to study.

For a number of respondents, the fact that the study was conducted in 1983 instead of at the height of the conflict in 1982 also meant it was not grounded in reality. Diplomat Sampson raised this point, overlooking again that at whatever level the conflict was occurring, comparing participation days interspersed with non-participation days would provide a meaningful test of the predicted impact of the group. One of the difficulties for journalists was that they normally use isolated anecdotal descriptions to illustrate events, and trends such as those IPPME purported to study are not always apparent on a day-to-day basis. Reporter Taft said that he had more faith in his ability to "wander around and talk to people." He said such research would be "tough to prove journalistically." His difficulty in imagining the Maharishi Effect influencing difficult cases was a factor in his decisions to dismiss IPPME.



### *Likelihood of future consideration*

Many of the questions posed about IPPME's quality arose from respondents' efforts to grapple with its unfamiliar description of causality. Both their comfort levels and their estimations of quality influenced whether they would consider such research in the future. In order to reflect respondents' decisions about further consideration, I split each of the groups into two. Whether respondents took scientific quality into account (Chart I) did not correspond perfectly with their likelihood of taking further research into account, because some respondents used other criteria (Fisher, Gurney, and Walsh). Some changed their minds (Harrington, Moore). One behaved like respondents who would ignore their assessment of quality, but then decided he would not close his mind (Cox). Two would examine research thoroughly and take it into account, but would not be disposed to consider it further (Banks, Sampson).

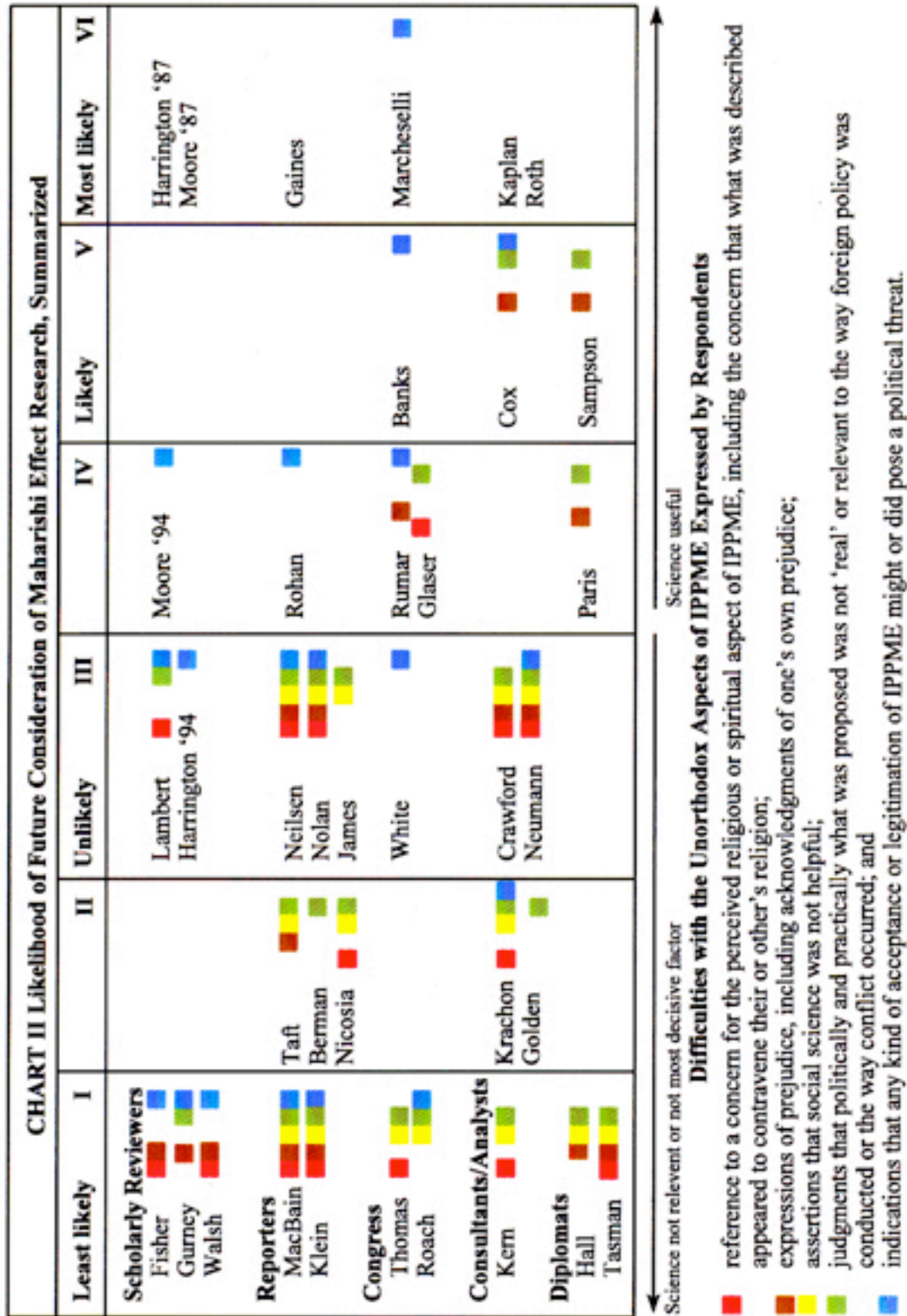
Chart I-A catalogues persistent factors weighed by respondents.<sup>5</sup> The chart shows respondents ranged from least likely to most likely to give future consideration to the IPPME research. Using this chart, I compared each respondent's unique weighing of the IPPME research. Respondents on the left of the chart not only questioned the relevance of science, but also posed many other questions. To examine the extent of those questions further, I created Chart II (next page) using letters to represent their most frequently mentioned difficulties. The chart is not a summary of qualitative data (which tends to defy such graphic summarization), but a means for comparing 35 respondents.

Clearly most of the respondents who gave less weight to IPPME's scientific quality were concerned with many assumptions, values, fears, and even taboos. Respondents who gave more weight to scientific quality had some of the same concerns, but their concerns were not so pronounced or compounded.

If, as Weiss found, counterintuitive research was rejected as "suspect on intellectual grounds,"<sup>347</sup> why did respondents who didn't believe IPPME find it trustworthy enough to suspend judgment so that they would be more likely to consider future IPPME-related research (columns V and VI)? Why didn't they reject it on both intellectual and political

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<sup>5</sup> Because Chart I-A is so encyclopedic it is included in Appendix E. Chart II, described below, is a simpler version included in the text.



grounds as respondents in column I did? I examined whether they had more prior knowledge of TM and the concepts presented in IPPME than other respondents in Chart II-A (Appendix E). The answer is not unequivocally no, but neither is it yes. Not including the scholarly reviewers, seven of the thirteen respondents, (excluding Harrington) had some prior knowledge of TM or meditation. Nine of 22 respondents (again excluding Harrington from being counted twice) had some prior knowledge, meditated, or knew someone who meditated, or had some prior professional contact with the material. Five respondents in columns IV-VI had more direct knowledge and Gaines, who initially did not, started meditating to prepare for a story. Including Gaines, three had practiced meditation and three had relatives who meditated (not necessarily using TM). Of those with knowledge of TM in columns I-III, one practiced TM and two knew someone who did. But three had examined Maharishi Effect research before, whereas only Gaines in Column VI had done so.

More first-hand experience may have accounted for less prejudice, less tendency to question IPPME as religious or antithetical to a given religion, and less difficulty in imagining the possibilities examined in IPPME. No one in the least-likely category and only one in the unlikely column described having direct experience of meditation. However, the concepts studied in IPPME were just as counter-intuitive to meditators as to other respondents. Of the three respondents who had first-hand experience and were also less likely to consider the research further, two were less vehement in expressing prejudice. Nicosia, the sole meditator among those 23 respondents, did not refer to negative images, but was emphatic in his judgments of Orme-Johnson et al.

In Chart II, the nine respondents in the likely and most likely columns may have been more open-minded and interested intellectually in IPPME. Moore, for example, differed from some of the other scholarly reviewers in that he was generally more interested in intellectual puzzles than substantive applications. He expressed less interest in competing for funds and position in academia than Fisher and Harrington.

Moore was willing to review questions like those posed statistically by IPPME, even though he didn't believe the outcomes. His interest in philosophy and physics as well as his background in Box-Jenkins time series analysis gave him a context for considering the ideas. As he pointed out, however, thinking and experiencing are not the same. IPPME contravened his common sense and professional experience as much in 1994 as it had in 1987. But he continued to expect that we would "know more about the way the world works" and did not count out the possibilities described.

All nine of the respondents in the likely and most-likely columns expressed open-mindedness and/or the importance of being open-minded. With the exception of Cox, they didn't come to the immediate conclusion that IPPME couldn't be true, although they didn't believe it. They were able to step back and consider the ideas and then to wait for more evidence. They expressed

curiosity and reservations, but did not have a black and white, either/or reaction. They were open to the possibility that they didn't know all there was to know.

In this data women were not necessarily more open-minded than men. Two women in Congress, Thomas and Roach, reacted like their male counterparts in rejecting both the science and IPPME concepts, while Gaines and Roth considered both.

Respondents on the chart's left had difficulties confronting the unorthodox aspects of IPPME. The IPPME study bothered them because it appeared to violate tacit assumptions and practices at the junction of science, religion, and foreign policy -- not merely values, but mega-values, metaphysical or guiding assumptions. IPPME did not fit within the story line, a journalistic image described by Lederman as the "frame into which a journalist can place seemingly random events and give them coherence."<sup>348</sup> More importantly, it did not fit with super story lines, which Friedman described as the constructs which help "us to explain the world to ourselves, to determine the information we will treat as significant," and "whose experiences get interpreted and whose don't."<sup>349</sup>

Part of what bothered respondents is that these kinds of questions were broached by IPPME on multiple levels. Reporter Nielsen probably accurately expressed a common reaction of Middle East policy makers he had covered for 32 years when he referred to the buttons being pressed in his head, activating a "subconscious calculus." He listed his prejudice, the difficulty of exploring what he considered religion scientifically, a psychologist's classification of IPPME as beyond the mainstream, and his bottom-line consideration, "What does this have to do with foreign policy?" For Nielsen and others, the subconscious calculus was not only intellectual but also emotional and visceral.










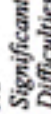
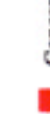
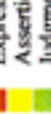


Chart III illustrates how respondents thought about IPPME, according to difficulties with unorthodox aspects of IPPME they described. Except for those on the lowest line, all referred one or more times to the difficulties listed as important in their assessment. Harrington and Moore in 1987, Gaines, Kaplan, and Roth were aware of difficulties posed by IPPME's unorthodox hypotheses, but did not describe these as pertinent. Harrington, Moore, and Gaines actually used IPPME, i.e., published and reported it. Kaplan and Roth might have been in better positions than other respondents for considering its use. Kaplan and Roth each worked on the community-organizing level -- identified by Congressional staffers Roach and Banks, and Senator Marcheselli as a more appropriate level of consideration for IPPME than Congress.

Those who responded with three or more of the difficulties listed on the chart also tended not to consider the scientific quality of IPPME, or, if they did, tended to discount it. Scholarly reviewers Fisher, Walsh and Lambert appraised parts of it, but considered other factors as more important. Among those noting difficulties, twelve felt that politically and practically what was






proposed was not "real" or relevant to the way foreign policy was conducted or the way conflict occurred. Six of those also noted prejudice regarding the perceived religious or spiritual aspects of IPPME. Of those six, five felt that associating with IPPME might bring them negative political consequences: pace-setting reporters MacBain, Nielsen, Nolan, Israeli reporter Klein, and human rights research director Neumann. Three respondents expressed close variants of these multiple difficulties: analyst Crawford, lobbyist Krachon, and diplomat Tasman.

These eight respondents experienced IPPME as irreconcilable with several of their identities at once: cultural, professional, and personal. It was not only that they had negative and strong prior associations regarding gurus or spirituality, but that they didn't think the collective practice of Transcendental Meditation had any place in the Middle East or in their work. MacBain and Nicosia judged IPPME in relation to their extensive contact with the gruesome reality of daily conflict in the region. Israeli reporter Klein forthrightly explained that he was prejudiced and feared political risk. Diplomats Tasman and Hall felt U.S. diplomacy is appropriately focused on selling U.S. solutions to regional leaders. Lobbyist Krachon spoke both of the realities of the conflict and of the mind-set in Washington.

**CHART III Difficulties**

	Scholarly Reviewers	Reporters	Congress	Analysts, Lobbyists	Diplomats
		MacBain, Klein, Neilsen, Nolan		Neumann	
				Crawford	Tasman
				Krachon	
	Fisher, Walsh				
	Lambert				
		Nicosia	Thomas	Kern	
		Taft			Hall
	Gurney			Cox	
			Roach Glaser		
					Sampson, Paris
			Rumar		
		James			
		Berman		Golden	
	Moore 94 Harrington 94	Rohan	Banks, Marcheselli, White		
<i>No Significant Difficulties</i>	Moore 87, Harrington 87	Gaines		Kaplan, Roth	

**Difficulties with the Unorthodox Aspects of IPPME Expressed by Respondents**

-  Concern for the perceived religious or spiritual aspect of IPPME, including the concern that IPPME appeared to contravene their or other's religion;
-  Expressions of prejudice, including acknowledgements of one's own prejudice;
-  Assertions that social science was not helpful;
-  Judgments that politically and practically what was proposed was not 'real' or relevant to the way foreign policy was conducted or the way conflict occurs;
-  Indications that any kind of acceptance or legitimization of IPPME might or did pose a political threat.



Human rights research director Neumann's reflections centered on implications for his job. Respondents on the next tier of the chart also had multiple difficulties in relating IPPME to their work, but with different emphases.

### ***IPPME in the Context of the Middle East, Conflict, Human Nature and 'Religious' Solutions***

Trying to explain all at once how the unorthodoxy of IPPME could not be considered in relation to Middle East conflict, pace-setting reporter MacBain repeatedly asserted that the "whole esoteric, one-step solution to anything" made no sense to "anything as complicated, as bloody, as historically difficult as the Middle East crisis. It's completely crazy." Nicosia, who like MacBain and Klein logged more hours than most reporters in the Middle East, similarly described the conflict as tribal, political and zero-sum, with events on the ground outracing deliberations and with players more interested obliterating their enemies than shaking hands with them. The conflict was not just an experience against which MacBain and Nicosia weighed the research. It was primal, wrenching, and hardly conceivable of solution.<sup>350</sup>

Involved in their assessment was estimation about how much it would take to change a person or to change cultural dynamics. Reporter Taft argued that Orme-Johnson et al. tried to make the jump from "taking time out to sit there quietly and become healthier and probably live longer ... to the fact that they've got a cure here for war, poverty and disease." Referring to differences in "cultural touchstones" in the Middle East, Nicosia pointed out that people there were

"deeply embroiled in a bitter, savage, communal, tribal, family conflict, okay? And in the midst of this conflict, they are grasping at those things which will strengthen them as they try to prevail in this conflict."

The four reporters who found IPPME irreconcilable in so many ways described the myriad crazy ideas that people threw at them for solving the problems of the Middle East. These went into mental bins with steep sides. An idea with spiritual connotations coming from an Indian teacher had a particularly difficult time being recognized as serious; in the bin it went, unlikely to escape. For these reporters, decisions about what might be *useful* to publish involved discrimination about the relative merit or the relative place in the hierarchy of cultural ideas and phenomena of ideas that *seem* religious. People who *seem* religious or cult-like are likely to be subject to suspicion. Truth tests were set aside or not engaged on this basis.

Religion was considered a private matter, with each religion a closed world unto itself. Respondents Tasman, Neumann, and Crawford explained they didn't believe or didn't need to explore alternative spiritual solutions and therefore didn't want to go into what they assumed to be IPPME's world. Interestingly, respondents with more exposure to meditation and TM did not



consider it religious, or part of a closed belief system, but rather as Nicosia noted -- a way to lower blood pressure and sleep well.<sup>351</sup>

Lacking either experiential or intellectual means for differentiating these techniques from religion, some respondents reached for religious comparisons. Krachon explained that such solutions would not appeal to warring people and implied that religious experiences were undependable, involving "all kinds of rationalization when it doesn't work." He said that they were also untestable and that "usually people engaged in a conflict are not necessarily amenable to TM or any other civilized way of handling conflict."

Religion was considered a societal factor by Tasman, to be taken into account intuitively in understanding "what the decision makers think and say." Tasman said he rejected IPPME because it in no way helped him with politically influential groups. He would pay attention to it if it had relevance to a leader with which he dealt, very much in the same way he would take account of the colors that person preferred.

Tasman referred to triage in practically marking off IPPME as illegitimate. Other respondents' demarcations involved science but often excluded science and *were instead* religious, professional, and political. Respondents estimated that IPPME was untrustworthy based on their intuitive grasp of their work, the way the world works, and their unformed -- and in some cases highly negative -- ideas about the concepts and techniques tested. Nielsen, Nolan, Neumann and Crawford, for example, considered IPPME's scientific merit, but gave many other considerations more weight. They tended to draw conclusions quickly when they experienced discomfort. Respondents expressed fears that IPPME might pose a political liability for constituents, colleagues, bosses and others, who might question their association with it (Marcheselli, White, Roach, Neumann, Lambert).

Scholarly reviewers Fisher, Gurney, and Walsh were concerned more ideologically, philosophically, and politically with resources, status and primacy of their analytical approaches. Religion for Fisher and Walsh was depicted broadly as an ideology to be feared or countered. They also used their labeling of IPPME as religious as a rhetorical means to create favorable positions for their own scientific inquiries and to justify knocking off IPPME in competing for resources. Such practices have been common in the history of science, described by Campbell as mechanisms for maintaining "tribal 'continuity'" and identity within groups of social scientists.<sup>352</sup> Laudan suggested that the pursuit of demarcation of science from non-science in polemical battles of this sort is more emotive and more "suited to the rhetoric of politicians ... than to that of empirical researchers."<sup>353</sup>

#### ***Additional Contrast with Respondents More Likely to Consider Future Maharishi Effect Research***

Respondents who were most likely to consider IPPME in the future were not overshadowed by the multiple factors described by the majority of their counterparts who were unlikely or least likely to do so. The more-likely respondents examined scientific quality and took that assessment into account. When they applied their common sense, experience, street wisdom or professional instincts to understanding what was described in the research summary, they did not find that it contradicted basic assumptions to the intense degree or at the level of meaning experienced by scholarly reviewers or by other respondents who expressed the most difficulties.

The more-likely respondents were able to stand back from the propositions and evaluate them despite their skepticism. With the exception of Glaser, who was speaking for his boss, none seriously objected to IPPME on religious grounds or due to negative associations. Several made religious comparisons (Kaplan) or mentioned images that came up (Sampson), but considered IPPME independent of these. Both Roth and Kaplan were aware that Israelis practiced TM without challenging their Judaism.

The more-likely respondents didn't react ideologically. Gaines emphatically stated that she "didn't believe there are any big truths in this world," and "no ultimate criteria." Her aim was to understand how people reached conclusions. Roth was used to juggling the views of disparate players, and in her reaction to IPPME, as if by second nature, considered the barriers people in the Middle East would have to overcome.

IPPME also did not shake these respondents' identity as it did for their counterparts. They expressed skepticism and disbelief, but accepted the study as they would any other. They were not bothered by ideas from other cultures and perspectives, but welcomed them. Roth, Kaplan, and Marcheselli were more interested in the concrete results for the population than in what anyone else would think about it. More-likely respondents generally didn't mind taking risks. Harrington and Moore, for example, exercised academic freedom in risking initial publication of IPPME on the basis of thorough scientific inquiry.

Most likely respondents differed, although not entirely, from their counterparts in that intellectually and politically five of the six were not as much in the mainstream of power politics. At the time of her interview, Roth said she had more time to read and think than usual. Gaines purposely covered stories that other reporters wouldn't consider. Kaplan was studying law, preparing to return to human rights advocacy -- involved with the mainstream, but not of it -- in Israel. Marcheselli was proud of his independent record as a Senator. Though he often voted with his party, he was used to taking positions outside of it. Moore was less competitive about pursuing mainstream substantive issues. These respondents would also be subjected to less heat from

colleagues and superiors for showing open-mindedness to controversial ideas. The exception is Harrington, who probably did feel quite a bit of heat for his review of IPPME.

Not all of the more-likely respondents, however, were functionally independent from their organizations. Respondents Rohan, Rumar, Glaser, and Paris (in column IV) generally engaged more fully with IPPME and would not say that they would dismiss further consideration of it, even though they had institutional ties similar to counterparts who were unlikely or least likely to consider the research further.

Sampson, for example, was responsible for many hundreds of other people and for U.S. relations with a Middle Eastern country. He was like the more-likely respondents in that he considered the scientific argument intellectually without fuss or difficulty, even though he recognized the difficulty that his colleagues and bosses would have with it. Like Gaines, he engaged with the study intellectually without prejudging it.

## **VIII. Conclusions**

My research question is: To what extent do individuals in the Middle East policy community apply "truth tests and utility tests" in determining whether to pay attention to research information in the ways Weiss describes, particularly *when the research involves unorthodox assumptions like those in the IPPME study?*

Because my interviews and analysis were confined to five small groups of respondents in the Middle East policy community, my generalizations are confined to them. My inquiry suggests that in assessing the IPPME research, my respondents made use of what Weiss referred to as "a series of implicit filters."<sup>354</sup> However, confronted with statistical evidence suggesting the viability of a creative solution to Middle East stress, conflict, and violence from another tradition, members of this foreign policy community did not all value social scientific research. Twelve did not examine scientific quality and nine of those said that science could not contribute to social solutions or was irrelevant to their jobs. Fifteen were inclined to resist the data, even though they looked at it. Eight took their own evaluations of scientific quality into account.

I found that the likelihood that respondents would give future consideration to related research was associated with whether they took scientific quality into account, but also with how respondents weighed their values, experience, and expectations against the IPPME findings. My analysis suggests that people who tended to be more likely than others to consider related research in the future were able to evaluate the study independent of prejudice, pre-conceptions, taboos, and conventional professional thinking. They also took science seriously and tended to think scientifically, expressing curiosity, willingness to take calculated risks, and the conviction that they could not afford to "be closed minded" (Roth). They didn't think, as Gaines said, "that an idea could hurt them."

To an extent, whether respondents would consider this research, research in general, and related research was influenced by whether respondents had prior meaningful exposure to the techniques involved, whether they had prior social science training, and whether they worked in the mainstream of the foreign policy network.

### ***How respondents decided whether IPPME research information was trustworthy***

When I divided the respondents into a group of 23 who were less likely to consider the research in the future and a group of 14<sup>6</sup> who were more likely, I found that they inhabited almost distinct worlds. The group of 23 decided how trustworthy IPPME was largely independent of their assessment of scientific quality. The group of 14 approached the premises with caution, but they

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These figures include reviewers Moore and Harrington who are counted twice, once for 1987 and once for their changed position in 1994.

engaged with the scientific analysis at levels of thoroughness commensurate with their skills and roles. Their engagement with the scientific analysis resulted in some raising alternative explanations, some suspending judgment, and those who gave more technical review putting aside their alternative explanations and publishing or reporting the piece.

An important difference between the two groups was the higher levels of emotional intensity expressed by less-likely as compared to more-likely respondents when they confronted the IPPME study. Several respondents were angry about taking the time to look at IPPME, but the anger actually resulted from their judgments that IPPME was unrealistic or irrelevant.

The 14 more-likely respondents examined the trustworthiness of IPPME in ways predicted by Weiss' research. That is, they separated their investigation from the philosophies and practices of their organizations, and from the tacit assumptions that operate in the diplomatic community, on Capitol Hill, within the community of conflict resolution scholars, and among reporters. They were able to maintain some distance from those assumptions and examine the ideas in the light of their personal experience in relation to the quality of scientific discourse. Their distance from operational and other assumptions did not appear to arise because respondents were critical of their organizations, but rather because they were intellectually and morally able to step aside, aided by science.

Those more likely to consider further research were less attached to conventional wisdom in their assessments. In some ways, those more likely to further consider IPPME-related research were more willing to be thrown into the position that Lederman described for reporters who step out of an accepted story line. He wrote that they have to face their own inadequacies and not be afraid to ask what may seem like dumb questions. It didn't trouble them to confront another way of looking at the world, because they were used to confronting different kinds of views, and as Paris put it, being explicit about and challenging their own assumptions. Their deepest assumptions were less challenged because they were used to risking them or used to examining others. Thus they explained that science is more than a tool; it is a way of conceiving problems and how to test them. Being more in the habit of examining scientific ideas, these respondents may have been used to putting their preconceptions aside.

The 23 less-likely respondents tended to intermingle their own estimations of IPPME's trustworthiness with those of their organizations, beats or policy repertoires. They didn't examine scientific quality or dismissed such examinations, giving more weight to the tacit assumptions that guided their work. They tended to give more weight to societal assumptions and tacit agreements about the place of ideas like IPPME in the cultural hierarchy. They felt that practices like Transcendental Meditation were out of place in high diplomacy and the cultural and tribal realities of the Middle East conflict. Most of these less-likely respondents reacted strongly to the research

information, as if it challenged their identity. This translated into questions of political acceptability, which Weiss had expected to find important to her respondents, but which they did not express when they assessed science in a more conventional range.

The respondents most unlikely to consider further IPPME research, however, tended to be concerned about many factors at once, particularly those that challenged their basic paradigms. The paradigms, however, involved such considerations as 'we in the foreign policy community don't take social science seriously;' 'we espouse but don't believe in religious solutions;' or alternatively, 'our authoritative judgment is derived from our religious judgments.' They believed that the way the world works and war occurs is beyond what they took to be naive solution; human nature is not amenable to this kind of change; and peace is created not by changing popular attitudes but through negotiation with elites. These assumptions involved the conventional wisdom of those in power. Challenging them was emotionally disturbing.

A special case within this group are scholarly reviewers Fisher, Gurney, Walsh and Lambert, who certainly considered the scientific quality of IPPME, but who gave more weight to their misgivings about the data's legitimacy. Reviewers Harrington and Moore are counted among the more likely 14 respondents because initially they followed the data and explored it thoroughly, despite continued reservations about the nature of the premises. Later they, too, appeared to be influenced by the implications for their reputations and standing among their colleagues and funding agencies.

### ***How respondents decided on usefulness of IPPME research***

Each respondent group described a distinctive assessment pattern relative to what was a primary concern with utility. Scholarly reviewers observed that 'something must be wrong' and asked what this was, seeking alternative explanations of the data. 'Useful' in the greatest sense meant of enough merit scientifically to publish or of even greater usefulness, to illuminate a question of interest for a scholar's larger work. Reporters attempted to determine whether IPPME could be admitted to the universe of possible stories and were concerned with whether IPPME was credible and relevant to the debate. Congressional respondents calculated IPPME's political feasibility and whether it would be of interest to constituents and colleagues. Lobbyists and human rights activists asked whether IPPME was relevant to their mandates, and especially the reaction of Congress and the media. Diplomats raised the question, "Is it real?" -- meaning pertinent to their task, pertinent to the leaders in the Middle East, whom they were trying to persuade.

An over-riding concern across domains related to the policy agenda as defined by legal and other precedents in the diplomatic community. Rohan observed that some people including journalists had "entered themselves into the sort of power sweepstakes of the community" because "information is power." Consideration of what was useful in this context was constrained by how

those in power defined their information needs. The respondents least likely to consider further Maharishi Effect research were most concerned with this type of utility.

A big concern for respondents less likely to consider IPPME was the legitimacy of the IPPME research. The concern for the reporters and decision makers was 'can we legitimately examine information that has religious overtones or has negative associations attached to it?' As Lambert pointed out, the legitimacy question sometimes hinged on others' willingness to allow examination of religious-sounding ideas different from their own. Inherent in the question of legitimacy and usefulness was concern for the causal explanations examined by IPPME. The thought that action at a distance might be observed on a social level involved even deeper taboos than those associated with the introduction of a scientific *and* a spiritual dimension into the field of foreign policy -- up to now dominated by *realpolitik*.

In aiming to "go after" the IPPME research and researchers, Fisher and Gurney were also primarily concerned with legitimacy. The fact that they each only pursued partial tests of scientific quality illustrated that what they perceived could not be legitimate also could not be true. With their efforts at demarcation, they aimed to protect their own paradigms, status and resources. They also used the question of legitimacy to persuade their colleagues to deny publication or future research and to end deliberation.

***The importance of factors involved in estimations of trustworthiness and usefulness to respondents in deciding whether they would give IPPME or related research further consideration or whether IPPME ideas would substantively contribute to their work***

The 14 respondents more likely to take the IPPME research into account in the future -- and especially the six respondents who were most likely -- found scientific quality more important than IPPME's highly counter-intuitive premises. The six most likely valued the implications of the evidence if they were found to be true.

One of the more important factors for the less-likely respondents was IPPME's unorthodoxy. As MacBain put it, the problem was "it," the IPPME study itself. The fact that IPPME rubbed against taboos and fundamental convictions, that it contravened common sense and reportorial instinct, was critical to most of these respondents. Furthermore, it was not perceived to fit within the mission, practices, and information needs that often severely constrained them.

***In what sense did the unorthodoxy of the IPPME study influence respondents and how did this interact with the other considerations they took into account***

None of the respondents was intellectually or psychologically prepared for the description of reality that the IPPME statistics provided, and none was convinced that they now had a solution to Middle East conflict. Only one respondent declared that she believed the results outright (lobbyist Golden), but she dismissed it as irrelevant to her job. There were, however, striking

differences between those more likely and less likely to take the research into account. Those who were less likely were troubled that the study described causal reality from a different perspective. They felt that it presumed to offer a solution to an intractable problem from what seemed to be equally unacceptable perspectives: religious and scientific. Those who were more likely had similar reservations, but not nearly to the same degree. Their core assumptions or identities were not challenged or questioned. Their engagement with science gave them room to examine other aspects of the study. Those who were more ambivalent raised considerations similar to those of the least-likely respondents. Even the more-likely respondents included people who were shocked by the data (Cox), but who recognized the unorthodoxy without finding it so off-putting.

A real stumbling block for most respondents was finding themselves unable to imagine a mechanism by which IPPME could be realized -- one that jibed with their experience of causality. This was perhaps most dramatically apparent for scholarly reviewers who were in a better position to understand IPPME's statistical fine points. Like many other respondents they sought alternative explanations for the data. One reviewer (Harrington, 1987) was among several respondents who were less troubled with having no explanatory mechanism. Harrington cited precedent in scientific endeavors for mechanisms being explained after initial discoveries. Like Harrington, some respondents who had less trouble with *not* having explanatory schema (Roth) also appeared to be more open to following science where it might lead.

Some respondents described theoretical models or mental pictures related to their past experience that appeared to allow them to consider IPPME as real or possible. Gaines, for example, was used to thinking of causality as problematic. She felt that there were many truths and was fascinated with mind/body models, which helped her to ask questions and build a fairly detailed understanding of what Orme-Johnson et al. found.

For the less and least-likely respondents, inability to imagine mechanisms may have translated into intense perceptions of incommensurability of the IPPME with their experience and models. In many cases, however, they were not concerned with scientific incommensurability, but with professional and cultural assumptions. In many cases the fact that IPPME involved scientific discourse contributed to its perceived lack of fit.



### ***Contribution of These Findings to the Field of Sociology of Knowledge Application***

My dissertation has been concerned with the larger question of how members of a policy community set about determining what is true and best for the larger community. Specifically I have been concerned with the assessment of a piece of research *not* designed according to the stated needs of policy makers, but representative of a creative solution from another tradition.

In observing the ways in which members of the foreign policy network assessed *International Peace Project in the Middle East*, I found that a small subset followed scientific evidence where it led or at least considered it and gave it as much or more weight than their own predilections. Another larger subset gave greater weight to their own values: within the imposing constraints of their jobs and overwhelming amounts of information, they found the study profoundly out of place with the hard realities of conflict and with the political realities of the Middle East as they defined them. In observing the contrast between those who were more likely to consider the IPPME research in the future and those who were not, I found a structure of concerns that was more than a collection of individual filtering mechanisms. This community-wide net of tacit assumptions was highly articulated and actively used. Most respondents identified with them fully, while some few acknowledged these assumptions but easily put them aside when confronting new and unusual scientific findings.

## ***IX. End Notes***

1. Progress in negotiations between Palestinians, Israelis, and Jordanians since 1992, for example, has involved several pivotal changes in policy-makers' assumptions -- a central one being that more could be achieved if the parties negotiated directly with each other. Even having made that change in assumptions, policy makers were surprised when the parties secretly negotiated with each other in Oslo in the summer of 1993 for an historic breakthrough. See Corbin, Jane.
2. George, Alexander, 1994, 172. Weiss, Carol, 1978a, 1978b. Kruzel, Joseph, 180. Rich, Robert, 17.
3. George, Alexander, 1994. Galvin, General John C. (Retired).
4. Azar, Edward, 2.
5. Orme-Johnson, David, et al., 1988.
6. Bill Katz and Linda Sternberg Katz (772) strongly recommend *JCR* for all academic social science collections. They wrote that its extensive indexing indicates its status. A key informant involved in U.S. foreign policy noted that *JCR* is one of four main journals in the field.
7. See Appendix B. Eight research articles, including IPPME, have been published by six social science journals. The five journals besides *JCR* are *Journal of Crime and Justice* (1), *Journal of Mind and Behavior* (2), *Social Science Perspectives Journal* (2), *Psychological Reports* (1), and *Journal of Social Indicators* (1). Another six of the 39 are included in professional association conference proceedings.
8. To honor confidentiality agreements with these reviewers and all other respondents, I will not name them and will not quote or cite their writings.
9. Edwards, David, 1-3.
10. *ibid.*
11. *ibid.*
12. David Orme-Johnson and Charles Alexander, in an unpublished summary, wrote (p.24):  
"It bears keeping in mind that while contemporary social theory views human beings "classically" as ontologically separate individuals, the longest tradition of philosophical thought in the West -- the idealist tradition -- has maintained, at least implicitly, the connection of human beings on the level of consciousness. It is also the case that several of the founding theorists of modern psychology proposed the concept of consciousness as a field through which individuals may be fundamentally connected. Fechner, for example, described a unity or continuity of "general consciousness" underlying the discontinuities of consciousness associated with each individual, accessible in principle simply through lowering the threshold of conscious experience (in James, 1898/1977). James (*ibid.*) suggested that the brain may serve to reflect or transmit, rather than produce consciousness, which in

turn may be conceived as a transcendental, infinite continuity underlying the phenomenal world."

13. *ibid.*, cite Klein, D.B.
14. Telephone conversation, John Davies.
15. Telephone conversation, Orme-Johnson. See comparison of cooperative events, verbal hostilities and hostile acts, Orme-Johnson, Dillbeck, Bousquet and Alexander, 1985, 2540-2541; content analysis of President Reagan's public statements, Gelderloos, 1988, 1989; and indications of negotiation breakthroughs during TM assemblies as well as impact on reduction of hostilities, Davies and Alexander, 1989, 27.
16. Chandler, Kenneth, 1987 (Jan.), 8.
17. Kenneth Chandler compared Aristotle as an exemplar of Western thought with Maharishi, who like Aristotle, has described human transcendence as the foundation of science.  
 "Aristotle's first science is intended as a science of first principles that lie at the unity and foundation of all the sciences; it is a science of *nous*, that state of being just as being, or a state of consciousness which is perfectly simple, unchanging, and eternal. Divine *nous*, Aristotle writes, "*is the absolutely simple and unified consciousness of itself throughout all eternity.*" (*Metaphysics*, 1075 all) This is a state in which God is said to be permanently, but man enjoys, Aristotle says, "in his best moments." (*Metaphysics*, 1072b15); *Nous* is that state of perfectly clear intuition which functions in Aristotle's logic as the source of immediately clear knowledge (*Posterior Analytics*, 100b5-13) and in the psychology (*De Anima* 430a25) and metaphysics as the foundation of the human mind and the highest human self, the principle of the divine in man. It is this state of divine *nous*, which Aristotle designates as man's best and highest part, that links Maharishi's Vedic science with the Western heritage. Unfortunately, it has never or seldom been recognized that Aristotle is writing about something that anyone can experience through a procedure such as Maharishi's TM technique." (unpublished correspondence)
18. For example, see Smith, Adrian, 1983, a Catholic priest. Father Smith also includes essays by Fr. Diarmuid O Murchu, MSC, BA and Fr. Myules O'Reilly, SJ, BA.
19. Chandler, Kenneth, 1987. 8, 9.
20. Alexander, Charles, and Davies, John, et al., 1990, 310-311. Authors cite Maharishi Mahesh Yogi, 1966; Wallace, R. Keith, 1970.
21. *ibid.* Authors cite Badawi, Wallace, Orme-Johnson, and Rouzere; Farrow and Hebert; Dillbeck and Bronson; Gaylord, Orme-Johnson, and Travis.
22. *ibid.* Authors cite as examples Dillbeck, Orme-Johnson, and Wallace; Hernandez; Orme-Johnson and Haynes; Orme-Johnson, Wallace, Dillbeck, Alexander, and Ball.
23. Orme-Johnson, David, 1987.
24. Suurkula, Jaan.

25. Alexander, Charles and David Orme-Johnson, 1983, 3-4.
26. Orme-Johnson, David, and Dillbeck, Michael, 1987, 226.
27. Davies, John, 1988, 36. Orme-Johnson, David, and Dillbeck, Michael, 1987.
28. Maharishi International University is now called Maharishi University of Management. Two of the five IPPME authors, Orme-Johnson and Alexander, are at MUM, though at the time of the study Alexander was at Harvard University.
29. Clark, Gregory, 1985, 54, 55. Clark cites Aristotelian scholar William Grimaldi, p.4., who contends that through the enthymeme Aristotle "bring[s] together the results of the activities of the speculative intellect and those of the practical intellect and make[s] them accessible to all for more responsible everyday living in the polis" (See Grimaldi, 54).
30. Kidder, Rushworth. Cites Bok, Derek.
32. Deitchman, Seymour.
34. Weiss, Carol, 1983a. Rich, Robert. Scheffler, Israel, 1984, 1985. Lindblom, Charles, and David Cohen.
35. Caplan, Nathan, 1980. 164.
36. Aaron, Henry, 1978. 98. Lindblom, Charles, and Cohen, David, 1979. Cohen, David and Janet Weiss.
37. Aaron, Henry, 1978. 10. 165. *fn* 176.
38. Ward, Benjamin, 19.
39. Lindblom, Charles, and Cohen, David, 1979.
40. Phillips, D.C., 30-33.
41. Krumholz, Norman, and Cogger, Janice.
42. Holzner, Burkart, 1978, 15-16.
43. *ibid.*, 13-14.
44. Weiss, Carol, 1980a. 60.
45. Caplan, Nathan, 1980. 164-165.
46. Weiss, Carol, 1982, 34.
47. Weiss, Carol, 1978, 23.
48. Weiss, Carol, 1982b. 20.

49. Weiss used a total of 50 summaries which varied as to manipulability of independent variables, administrative implementability of conclusions/implications, and political acceptability. 1980a. 54-55.
50. Weiss, Carol, 1980a, 1983b. 225-226. See also Caplan, Nathan, 1977, and Knorr, Karin, 1977.
51. Weiss, Carol, 1983. Caplan, Nathan, Morrison, Andrea, and Stambaugh, Russell, 1975. Donnison, David, 1972.
52. Caplan, Nathan, 1980. 175, 184-85.
53. Weiss, Carol, 1980. 382, 1980a.
54. The enlightenment model was first elaborated by Crawford, Elisabeth, and Biderman, Albert, 1969, and Janowitz, Morris, 1970. For a discussion of rational models see Carol Weiss, 1979.
55. Weiss, Carol, 1979. 427.
56. *ibid.*, see also Weiss, Carol, 1983a. 365-367.
57. While the enlightenment model of research use seeks to explain the filtering of social science research concepts and theoretical perspectives into the decision maker's fund of working knowledge, it is not conceived as a panacea. Weiss (1979) explains that endarkenment is also a possible outcome of this process. The unguided process may result in "partial, oversimplified, inadequate, or wrong understanding. Important results may not reach the policy audiences. Divergent results may leave policy makers confused" (p.430).
58. Weiss, Carol, 1980a. 105.; 1980b. 311.
59. *ibid.*, 256.
60. Weiss, Carol, 1981. 198. See also March, James and Herbert Simon, 174; Downs, Anthony, 195-210.
61. *ibid.*, 199-200.
62. Kronick, David, 1321.
63. Harnad, Stevan, 1.
64. Kronick, David, 1321.
65. Ogburn, William. Cited by Denzin, Norman, 42.
66. Relman, Arnold and Marcia Angelli, 829.
67. Lakatos, Imre, 91-196. Cited by Phillips, D. C., 30. See further discussion of this point below, Part III.

68. Campbell, Donald, 1978. Cited by Phillips, D.C., 18.
69. Gray, David, 35.
70. Zelditch, Jr., Morris, 52, 54.
71. Lindsey, Duncan, 1979. 45.
72. Singer, Benjamin.
73. Lubans, Jr., John.
74. *ibid.*
75. *ibid.*
76. Horrobin, David, 1441. Cites Polanyi.
77. Lindsey, Duncan, 1976, 1977, 1978.
78. Atkinson, Malcolm. 148.
79. *ibid.*, 154, 158.
80. Barber, Bernard.
81. Atkinson, Malcolm, 150-151. Yallow, Rosalyn, 60.
82. *ibid.*, 154. Hanson, Norwood, 175.
83. *ibid.*, 152. Koestler, Arthur.
84. Lindblom, Charles, 1990. 192-195. Cites Mulkay, M.J., 1977.
85. Doherty, Michael, et al. Mynatt, Clifford, et al.
86. Lord, Charles, Ross, Lee, and Lepper, Mark.
87. Leahey, Thomas and Richard Harris, 224. Cite Nisbett, Richard and Lee Ross.
88. *ibid.*, 231. Mahoney, Michael J. and Bobby G. DeMonbreun.
89. *ibid.*, 229, 232. Kern, Leslie and Herbert Hinshaw, 131-146.
90. *ibid.*, 232.
91. *ibid.*, 232. Tversky, A. and D. Kahneman.
92. *ibid.*, 232.

93. Mitroff, Ian, 1974a.
94. Leahey, Thomas and Richard Harris, 234. Mitroff, Ian, 1974a, August, 1974.
95. Mitroff, Ian, August, 1974. 589.
96. *ibid.*
97. *op cit.*, 588.
98. *op cit.*, 587, 594. Cites Merton, Robert K., 1949, 607-10 and 612-14.
99. Feynman, Richard, 1985, 311-312.
100. *ibid.*, 316-317.
101. Although as Campbell, 1988, also indicates, the social nature of inquiry had been "insisted upon as far back as 1900 by Charles Sanders Peirce, James Mark Baldwin and John Dewey." 489.
102. Kuhn, Thomas, 4.
103. *op cit.*, 4-5.
104. Campbell, Donald, 1978, 183-185.
105. *ibid.*, 197-198.
106. Kuhn, Thomas, 175.
107. Morris, William, 665.
108. Walter, Maila, 107-108.
109. *op. cit.*, 112.
110. *ibid.*, 194.
111. *op. cit.*, 196.
112. Gieryn, Thomas F., 1983, 792.
113. *ibid.*, 781.
114. *ibid.*, 783.
115. Laudan, Larry, 1983, 125, *fn.* 127.
116. *ibid.*, 119.

117. *ibid.*, 119-120.
118. *ibid.*, 117-118.
119. *ibid.*, 124-125
120. Pinch, Trevor, 14. Cited by Taylor, Charles Alan, 408.
121. *ibid.*, 412.
122. Sullivan, Dale, 285.
123. Clark, Gregory, 37.
124. *ibid.*
125. *ibid.*, 50.
126. *ibid.*, 54-55. Cites Grimaldi, William, 5.
127. *ibid.*, 250-251.
128. See Chaim Perelman.
129. Perelman, Chaim and L. Olbrechts-Tyteca. 55.
130. Clark, Gregory, 185.
131. Clark examined Timothy Dwight's use of epideictic rhetoric in a period of crisis of authority for the Puritan New England leaders. See page 172, and the foot note on page 244.
132. Duffy, Bernard K. 90.
133. *ibid.* and Sullivan, Dale, 285.
134. Sullivan, Dale, 284, *fn.* 302.
135. Caplan, Nathan et al. Weiss, 1983, 228; 1978b, 71.
136. Weiss, Carol H. and Eleanor Singer, 1,2. Cites Gallagher, James J. and Joseph Sanders., 22.
137. Cook, Fay Lomax et al., 32.
138. Kingdon, John W., 62.
139. *ibid.*, 63. See also Sigal, Leon V., 47. Heren, Louis, 142.
140. Campbell, Don, 1991, 114.
141. Rivers, William L., 78.



142. *ibid.*
143. Stocking, S. Holly and Sharon L. Dunwoody, 153.
144. Weiss, Carol, 1983a, 372.
145. Dunwoody Sharon L. and S. Holly and Stocking. McCall, Robert B. and S. Holly Stocking. Dunwoody, Sharon and Byron Scott.
146. Weiss, Carol H. and Eleanor Singer, 14.
147. *ibid.*, 124, 128.
148. *ibid.*, 151.
149. *ibid.*
150. Tomalin, Nicholas. Cited in Goodfield, June, 16.
151. See also McCall, Robert B. and S. Holly Stocking. 1982, 988.
152. *ibid.*, 136, 137.
153. *ibid.*, 988. Ridder, Pamela, 62.
154. Heren, Louis, 132.
155. Redmont, Bernard S., 89.
156. Weiss, Carol H. and Eleanor Singer, 137-139.
157. *ibid.*, 152.
158. *ibid.*, 152-153. Cites Merton, Robert K., 1968.
159. Rosten, Leo C., 240.
160. Weiss, Carol H. and Eleanor Singer 143.
161. *ibid.*, 143. Cites Darnton, Robert, 189-192.
162. *ibid.*, 144-151.
163. *ibid.*, 150.
165. Weiss, Carol H. and Eleanor Singer, 163.
166. Gwertzman, Bernard, 37-38.
167. Weaver, David H. and G. Cleveland Wilhoit, 156, 162, 181.

168. *ibid.*, 184.
169. Ahern, Jr., Thomas J. 251.
170. Paletz, David L. and Robert M. Entman, 215-216.
171. *ibid.*, 219-221.
172. Darnton, Robert, 191.
173. Sigal, Leon V., 49. McCartney, James, 98.
174. Hess, Stephen, 106-107, 49. See also Blumer, Jay G. and Michael Gurevitch.
175. Montalbano, William D., 24.
176. I include Lederman's discussion in detail because it provides rare insight into the missing of a story line. Some may object to Lederman's characterization of reporters as over-simplified as key informant Judith Gaines has done. *New York Times* reporter Joel Greenberg, 1994, has confirmed many aspects of Lederman's analysis, suggesting that Lederman may have been addressing the uniqueness of the situation.
177. Lederman, Jim, 4.
178. *ibid.*, 16.
179. *ibid.*, 17.
180. *ibid.*, 12.
181. *ibid.*, 13.
182. *ibid.*, 14.
183. *ibid.*, 15.
184. *ibid.*, 18.
185. *ibid.*, 27, 123.
186. *ibid.*, 279-280.
187. *ibid.*, 23.
188. See Friedman, Robert I.
189. Friedman, Thomas L., 1993.
190. Friedman, Thomas L. 1989, 166.

191. *ibid.*, 426.
192. *ibid.*, 178.
193. *ibid.*, 428-429.
194. Lederman, Jim, 239-241.
195. *ibid.*, 220.
196. Smith, Michael Joseph, 219. Cites Morgenthau, 1962, 121.
197. *ibid.*
198. *ibid.*, 221-223.
199. George, Alexander. 1993, 108. See Waltz, Kenneth.
200. Blight, James G., 1988. 13.
201. *ibid.*, 25.
202. Blight, James. 17, cited Richard Neustadt and Ernest May's assertion that "it may be easier to get a million dollars of public money than a minute from a president or cabinet officer." Neustadt and May, 1986. 1. He noted that the President receives "one 20-minute briefing per year on the state of all of the sciences." Cites Deutsch, Morton. 1973. 1983.
203. Blight, James, 1987, 18.
204. *ibid.*, 22
205. *ibid.*, 23.
206. Holt, Robert, 1984, 325.
207. Blight, 1987, 24-25.
208. Holt, Robert, 325.
209. Smith, Michael Joseph, 135. Cites Morgenthau, 1946. See also Morgenthau, Hans. 1972.
210. Morgenthau, Hans, 1946, 220.
212. *ibid.*, 269.
213. Henrikson, Alan, 242.
214. *ibid.*, 259.
215. Telephone conversation with Alan K. Henrikson.

216. Gaddis, John Lewis, 6.
217. *ibid.*, 7-8.
218. *ibid.*, 8-9.
219. George, Alexander, 8.
220. *ibid.*, 9.
221. Goodwin, Richard, 130.
222. *ibid.*, 131.
223. Michael Fry (16) cites six points written by May and Neustadt "First, look before you leap; do not over-react; do not lock into hastily constructed assumptions; seek perspective; guard against excessive zeal; frame the problem carefully before looking for solutions; do not rush to act. Second, avoid dependence on fuzzy analogies; look for differences as vigorously as for similarities; get to the really salient questions; what comes to mind is not necessarily what is relevant; do not let analogies cloud vision. Third, know the history of the problem; know how it arose; sort out the facts; distinguish between the known, the unclear and the presumed. Fourth, analyze alternatives carefully; think about the basic presumptions behind alternatives; analyze rather than advocate solutions which one has already decided on prior to the debate; ask about alternative solutions the following questions -- will it work, will it stick, and, if not, turn to what other solution; know the history of one's own society, as well as that of others and thus have a sense of what society, in light of its traditions, values and precedents, will accept. Fifth, do not use stereotypes to 'place' persons or organizations; get a clearer picture of them so as to be able to understand their conduct. Sixth, see choice as part of a historical sequence; connect events over time and see if the connection makes sense; understand the nature of change over time; see time as a stream where what is now has flowed from the past but is now, in certain critical ways, different from the past; get a clear sense of the extent to which the present is and is not different from the past."
224. *ibid.*, 10.
225. May wrote in the preface to his 1973 book (xiii) that if such analyses were well done, "they might help people who must make choices or must estimate the future to free themselves from the analogies, parallels, or trend readings which they might otherwise unthinkingly apply. Even such a limited objective calls for work by analysts sensitive to the variety and complexity of the past, the tentativeness of most historical reconstructions, and the many hazards associated with claiming that one occurrence is 'like' another." See also Neustadt and May, 1986.
226. Fry, Michael, 19.
227. *ibid.*, 13-14.
228. George, Alexander, 1993, 12.
229. *op cit.*, 11-14.

230. Judis, John, 19.
231. *ibid.*, 18.
232. *ibid.*, 19.
233. George, Alexander, 1993, 42-43.
234. *op cit.*, 115.
235. Haass, Richard, 48.
236. *ibid.*, 55-56. Both Haass and George were writing after the fall of the Berlin Wall in 1989, the collapse of the Soviet Union ten months later, and the end of the Cold War by 1991. These events are credited with diminishing the widely-accepted realist domination of policy making. On leaving office in January 1993, former Secretary of State Lawrence Eagleburger said, "We are in the middle of a global revolution, a period of change and instability equaled in modern times only by the aftermath of the French and Russian revolutions." (Vanden Heuvel, Jon, 1993.)
237. *ibid.*, 40.
238. *ibid.*, 116-117.
239. Repertoires refer here to the "limited number of programs that an agency develops and practices" within its "range of effective choice." See Weiss, 1981. 185. See also Hecla, 1978, who explained, "like experienced party politicians of earlier times, policy politicians in the knowledge networks may not agree; but they understand each other's way of looking at the world and arguing about policy choices." 117.
240. Blight, James, 1988, 327.
241. Uliassi, Pio., 1977.
242. Weiss, Carol, 1983a, 372.
243. Weiss, Carol, 1987, 1989. Between 1983 and 1987, Weiss (1989, 429) interviewed 83 Congressional committee staff, officials from Congressional support agencies and offices of Congressional relations in the executive departments, and lobbyists. The 51 staff interviewed were "about evenly divided between the House and Senate." She explained that four Congressional support agencies had grown since the 1970's: the Congressional Research Service, the Congressional Budget Office, the General Accounting Office, and the Office of Technology Assessment.
244. *ibid.*, 419.
245. *ibid.*, 420.
246. Dreyfus, Daniel, 100.

- 247. *ibid.*
- 248. *ibid.*, 101-102.
- 249. *ibid.*, 103.
- 250. *ibid.*
- 251. *ibid.*
- 252. Weiss, Carol, 1989, 412-413.
- 253. Hecllo, Hugh, 88.
- 254. *ibid.*, 94-95.
- 255. *ibid.*, 99.
- 256. *ibid.* 103.
- 257. *ibid.*, 104
- 258. *ibid.*, 121.
- 259. Phone conversation with Hugh Hecllo.
- 260. Laumann, Edward and David Knoke, 375.
- 261. Weiss, Carol, 1989, 421.
- 262. *ibid.*, 421.
- 263. Weiss, Carol, 1987, 104.
- 264. Rosenbaum, David E., D1.
- 265. *ibid.*, D3.
- 266. Weiss, Carol, 1989. 413. Cites U. S. House of Representatives, 1977.
- 267. *ibid.*, 414.
- 268. *ibid.*
- 269. *ibid.*, 416.
- 270. *ibid.*
- 271. Weiss, Carol, 1989, 422.

272. *ibid.*, 424.
273. *ibid.*, 427.
274. Weiss, Carol, 1980b.
275. Weiss, Carol, 1977a, 80.
276. *ibid.*, 427.
278. Weiss, Carol, 1980. Cohen, David K. and Michael S. Garet, 1975.
279. Heclo, Hugh, 1978, 88. Heclo asserts that this is even more evident in 1992 than in 1978. Phone conversation. See also Laumann, Edward, and Knoke, David, 1987a, 44, 375. Cites McFarland, 1976, Salisbury, 1984.
280. Weiss, Carol, 1989, 421-422. Heclo, Hugh, 1978, 99-104. Kingdon, John, 1984.
281. Patton, Michael, 1980, 102-103.
282. Laumann, Edward, 1987, 95.
283. Miles, Matthew and A. Michael Huberman, 1984, 151.
284. Hess, Stephen, 1981, 25-26.
285. Three of those selected are almost universally named as Washington, D.C. pace setters by both academics and policy makers. The fourth was named with the other three as a pace setter by everyone but Hess below, who articulated the criteria for "inner ring" newspapers as stated in the text. I considered newspapers according to circulation levels as described in a summary provided by the Newspaper Association of America (see Fitzgerald, Mark). I also referred to *Media Guide*; to Weiss, Carol, 1974a; and to Hess, Stephen, 1981, p. 24-27.
286. Hess, Stephen, see citation above.
287. In consideration of the respondents' time constraints and in line with the summaries used by Weiss in her study, an effort was made to limit it to two pages. The graph included required an additional half page.
288. As lobbyist/consultant Basil Henderson (who served as an informant) noted, the standard presentation for personal staff in Congress, for example, is "no more than two pages, bulleted and predigested." He noted that because people in Congress tend to look at what they receive in terms of advocacy they have problems with considering "science qua science."
289. Dexter, Lewis, 1970, 19. Briggs, Charles, 1986, 97,94.
290. *Newsweek*, 6/1/92
291. Shelby, Barry, 26-27.

292. I interviewed one diplomat days before Israeli settler Baruch Goldstein shot 29 Muslims at prayer in the Hebron mosque (2/25/94). Edward Said described the slaughter as the inevitable result of monotheistic (Christian, Jewish, and Islamic) religious passions. His article appeared within the same month that Israel and the Palestine Liberation Organization signed a detailed agreement for implementing the first stage of autonomy in the Gaza Strip and the West Bank town of Jericho (5/94). 2/5/95 Thomas L. Friedman described the devastation felt by Israelis when a suicide bomber killed 19 Israelis (1/22/95) which Friedman said "snapped something in the Israeli psyche." 2/6/95 Andrew Bilski reported that in the 16 months since the signing on the White House lawn, "at least 112 Israelis and 195 Palestinians have died in continuing bloodshed."
293. Exceptions to this were reporters. Key informant Judith Gaines told me that reporters would consider me more credible if I did not initially offer confidentiality. Two did, however, explicitly request it.
294. Reporter Judith Gaines said that on principal she felt that one should stand by their statements; she wanted her correct name used.
295. Exceptions to this were reporters Gaines, Rohan and presidential advisor Cox and all of the scholarly reviewers. Gaines had already referred to the research in the course of her reporting and did not need to read it before conducting the second part of the interview. Rohan in an early preliminary interview looked at the longer article while I waited. Cox's schedule would not allow another interview on the next day, so I stepped out while he took the time to read it and I interviewed him when I returned. All of the scholarly reviewers, except for Walsh -- whose interview was conducted on the telephone -- involved a longer session including both part I and II. I sent Walsh a copy of the summary and article to refresh his memory before the interview.
296. I am especially grateful to Dr. Suzanne DeMonchaux for sharing her knowledge of interview techniques with me.
297. Coding was completed for scholarly reviewers and officials with diplomatic responsibility. The final three groups were not coded, but were analyzed using the large wall charts.
298. Miles, Matthew and A. Michael Huberman, 1984, 69.
299. Schatzman, Leonard, and Strauss, Anselm, 1973, 118-199.
300. I also used data displays to chart prior exposure of the respondents to related research and to track the elements of scientific quality that were of interest to respondents.
301. She happened to select a scholarly reviewer who had written extensively, Fisher, as one of the focuses of her audit.
302. Patton, Michael, 1980, who cites Cronbach, Lee, 280. Maxwell, Joseph, 1992, 294.
303. As summarized by David Orme-Johnson (1991) "This study replicated ... [findings of the IPPME study] by examining the seven occasions in which groups practicing the TM-Sidhi program have been sufficiently large and close to Lebanon for a predicted impact on the war (based on the square root of one percent formula). Three of these groups (with up to 8,000



participants) collected in the United States, and one each was in Israel, Lebanon, Yugoslavia, and the Netherlands. This study improved on the earlier study by examining more information relevant to conflict in Lebanon, using more sensitive conflict scales and data from a greater number of sources. Event data were scored by an independent, expert Lebanese coder who was blind to the hypotheses being tested."

304. If there was a letter it had already been discarded, so I could not examine it.
305. Davies replied: "I fully share your concern over the dangers of selectivity in social science research. It was for this reason that, having seen the apparent positive impact of the assembly held in Jerusalem (as reported in our earlier paper in *JCR*) I felt it was essential to undertake an evaluation of the entire sequence of assemblies of sufficient size and proximity for an impact to be predicted in Lebanon using the standard proposed formula. Only one assembly was excluded, because, as noted in footnote 1 to the paper, it was too far [in] advance of the others (by four and a half years) for its inclusion in a single time-series analysis to be justified, given the expense of generating detailed daily data using independent coders... [and had been] already evaluated in another paper... As a further control against selectivity, prior to every major assembly, the predicted impact of reduced violence, reduced tension and improved coherence, as reflected in indices of quality of life and progress toward peaceful resolution of conflict, was announced to the press, and in some cases even to independent review boards... the only exception... [being the] Lebanon assembly, which was quite small, and necessarily low-key because of the very real dangers in bringing together participants from the Christian, Druse, and Moslem communities... All assemblies outside Lebanon big enough for a predicted influence there are thus a matter of public record. In addition, the specific measures for the present study were defined in advance of any data collection or analysis, as part of my doctoral research proposal. No measures were tried and dropped."
306. I was not able to interview the other reviewers because the *Journal* routinely discarded correspondence with reviewers after a number of years and no longer had records to indicate who they were. As mentioned above, pseudonyms are used here to respect confidentiality agreements.
307. I will quote from Fisher's and Moore's unpublished essays with their permission.
308. Provided by David Orme-Johnson.
309. It is important to note that Fisher's interpretation of how 300 years of social behavior are viewed within the Western world overlooks the details of scientific history and competing theories, which were and are legitimately scientific. See Kearney, Hugh, p. 17-48. There have been many scientific efforts over this period, which are still continuing, to examine collective consciousness.
310. Fisher explained that "As a statistician I see screwy Box Jenkins things all the time, well not all the time, it's not as bad now as it used to be. People have tended to stop using it for exactly that reason." In contrast, Moore, an expert in Box Jenkins methods, who said that when he reviewed IPPME in 1987 'perhaps 50 people were expert enough in Box Jenkins to review it, but that at the time of the interview in 1994 the number was probably 3-400.'

311. See Cook, Thomas, and Campbell, Donald, 1979, 234-237. Orme-Johnson et al. pointed out that their results were consistently significant using several different analysis techniques -- impact assessment analysis, cross-correlation and transfer function analysis, and the analysis of the thirteen-day experiment within their experiment. They presented several additional reanalyses to address the criticisms based on their use of Box-Jenkins time-series methods, with particular regard for the "Lebanon war index, which combines both conflict variables." The authors explained *how* and *why* Box Jenkins techniques were applied and that they followed the school of thought that cautions against 'spike hunting.' They described presenting the simplest adequate models using an objective criterion for selecting most adequate model (Akaike's information criterion) to avoid risk of biased model selection.
312. Paper submitted to *JCR*, provided by an editor with the proviso that it would not be cited or quoted directly.
313. See Figure 3 in the IPPME Summary, Appendix B.
314. This critique, the review by Fisher with his allegation of manipulation, and the response by the IPPME authors all occurred within peer review and therefore removed from public scrutiny. Nevertheless, it represented a rhetorical battle which affected scholars, who for example, reviewed the critique, but not the authors' response. No forum was available for the authors to answer Fisher's charges.
315. Larimore, Wallace E.
317. McMullin, Ernan, 5; Kuhn, Thomas, 94.
318. See for example, Wheeler, John and Wojciech Hubert Zurek.
319. The fact that this was always the case with Orme-Johnson et al. and not a rationalization is born out by their research proposal (Alexander and Orme-Johnson), which stated in a footnote on page 16:

"We recognize, however, that this number reflects only an estimate based on prior research. Given the great collective tension in this part of the world and the irregular distribution of the population of Israel, it may be that a somewhat larger number is required. If so, the planned systematic increases in the number of participants involved may allow us to determine what the threshold might be. Statistical procedures are available for testing for such threshold effects; but if no such threshold is apparent with 460 participants (twice the predicted critical number) for at least one of the three broad conceptual factors or a major factor empirically defined..., the experimental hypothesis should not be accepted."
320. Telephone conversation with John Davies.
321. This particular way of expressing what appears to bother Fisher, was articulated by a bio-chemist and doctoral candidate in Philosophy of Science in a preliminary interview. He had no trouble with the science involved in IPPME, but he asked whether the term "pure consciousness" ever surfaced in conflict resolution research and what the reviewers thought when they read this. He thought that "Of course most people who are somewhat intelligent have heard of TM," but pure consciousness would not be a familiar term.

"Remember our reality has been constructed for us through our education. That's so very important. That's why [Orme-Johnson et al. and the reviewers] are so out there. [Orme-Johnson et al.] have been educated one way and these people have been educated another. And [Orme-Johnson et al.] are using terms that are present within the environment, but ...in just such vastly different theoretical ways -- with such very different world views. And how do you bridge that? This goes back to where we are in the philosophy of science -- what are the bridge principles between these terms that [are used]?"

Later, in talking about the units of measurement, he said,

"[Orme-Johnson et al.] are describing something as pure consciousness. OK, but [they] didn't say that -- they're talking people, individuals. That's their unit. But how much pure consciousness is that? Is that a force? What are they talking about? You see? I mean it's a metaphysical term. It's not a quantifiable term. You say x numbers of people, but I mean how much pure consciousness is that?"

- 322. For a striking historical example of this phenomenon see Kearney, Hugh, 163-171.
- 323. Phone conversation, Charles Alexander.
- 324. Telephone conversation, John Davies.
- 325. Descartes wrote, for example, "of an experience that he had as a young man of 'penetrating to the very heart of the kingdom of knowledge' and there comprehending all the sciences, not in sequence, but 'all at once.'" See Chandler, Kenneth, 9.
- 326. See Smith, Huston, 41-42.
- 327. See Appendix G for Dr. Hagelin's biographical sketch and publication list.
- 328. A legal suit brought against the *Journal of the American Medical Association* on behalf of the organizations involved was settled confidentially. No further publication of this sort has appeared in *JAMA*.
- 329. Holzner, Burkart, 13-14.
- 330. Before conducting these interviews, a political scientist informant (with no ties to Orme-Johnson et al.), suggested that this community of social scientists did "not want to hear what Orme-Johnson et al. had to say, no matter how it was wrapped." This was true, he said, "despite [Charles Alexander's and the other authors'] credentials and even though [Alexander and the other scientists] have given them back their methodology in spades.' In his opinion the U.S. was 'moving to the right,' which he thought might scare people who might ideologically be more sympathetic to the IPPME research.

He observed that in such an environment the issue was political and related to limited resources. It was not an issue of 'disagreement with the IPPME study and the studies that preceded and follow it, but really a struggle for identity.' My confidant said, "They won't tell you what they really do... Journals and funding are tools for war: a group of academics like Orme-Johnson et al. could also capture a journal and funding and dominate the field." He said that some social scientists were afraid of losing their hard-won elite status. He

mentioned that Fisher and his colleagues are well known for their work with the intelligence and defense industry, which are not necessarily interested in peace.

My analysis suggests a more complex and nuanced pattern of consideration which respondents did discuss. The political science informant accurately pointed to a highly competitive academic context that appears to have influenced referee decisions.

I learned in the course of my research that Fisher has at times been in a competitive relationship with IPPME author John Davies in the pursuit of research funding and, according to Davies, has "become a persistently negative and strident critic" in reviewing papers, grant applications etc., which are in no way related to IPPME, and has explicitly avoided direct communication with him. It would appear that Fisher's behavior is consistent with an attempt to limit Davies' ability to continue to participate as a legitimate player in the field.

- 331. Reported by political science professor.
- 332. As described by John Davies.
- 333. Mitroff, Ian, 1974b, 589.
- 334. Rohan was among the first preliminary respondents. He did not read a summary, but looked through the larger article as I waited. He also read printed comments by an editor and reviewer, which increased his skepticism.
- 335. See Maugh II, Thomas, A2.
- 336. George Fisher also was aware of the assembly because he follows the research closely.
- 337. Castaneda, Ruben and Cindy Llose.
- 338. Since Kaplan was on leave from his human rights organization, he was not reflecting in the context of an organization that might find a lot of discomfort with his views. However, the interview may be fairly representative of a position he might take because as a person that was reportedly effective in his work, he was in a position to speak from experience.
- 339. Neumann observed that the square root of 1% is 10% (10% times 10% = 1%). The IPPME authors probably should state the formula as "the square root of one percent *of the population.*"
- 340. I answered this question in an attempt indicate the direction that he might explore. This attempt misfired, because it misinformed him. If I had answered correctly I would have had to say, 'I believe there is a control within the time series analysis.' I knew about the control -- because it had been a big factor in writing the summary, but I felt bound to not interject anything about the study. Inadvertently, however, I think this reinforced his conclusion to whatever degree that the study was flawed. He may, however, have understood what I was trying to intimate, because he didn't seem to rest most of what he said on that fact. He continued to refer to the choice of period to study by the authors as the definitive problem that he had with the study.

341. Orme-Johnson, David, et al. differ from Sampson on this point. In a telephone conversation, Charles Alexander pointed out that if the research had been comparing an earlier high level of violence with a lower level, Sampson would have had a point. In fact the research was comparing daily fluctuations of group participation with fluctuations in the dependent variables. He also pointed out that there was significant violence with the period studied, when for example, the Israelis withdrew the troops from Lebanon. This is born out in Thomas L. Friedman's recounting of events, 1989, 179,199.
342. John Mack, M.D. is a Harvard psychiatrist who has published accounts of individuals who said they had been abducted by aliens. A long piece by Mack appeared in the *New York Times Magazine* within the months of our interviews.
343. It is important to note that the conditions of Hall's interview varied slightly from the norm. Whereas most of my interviews were arranged with a cold call directly from me to the respondent or an assistant, Hall told another diplomat's assistant that he would be happy to be interviewed. He, perhaps a little more than some of the other respondents, sought to instruct me in what he thought I should consider in terms of his and his agency's information use. He offered to think of concrete examples. I conducted a second Part I interview in order to consider his examples. The interview centered on factors in his organization that I would not have understood so clearly otherwise -- especially obstacles that he and his colleagues must overcome re: governmental policy constraints. He described the challenges of counter-intuitive information within that context. When I called to set up the second of three interviews he said, "You know, the handout's not going to be relevant." When I told him it was important to my dissertation he said that either he would give me the examples we talked about or consider the handout. He had only half an hour to offer. I convinced him to give me both.

Several times in the interview Hall said that reading IPPME encouraged him to wonder, "Why did Carla Brown bring me this?;" to ask, "Is this some sort of experiment?" and to imagine that "you were fishing for was precisely this kind of reaction for some reason that I'm not aware of." In addition to feeling somewhat set up for the third interview, Hall may have felt affronted because he and his colleagues were involved in a breakthrough devoutly sought within the Middle East for four decades at least. In this kind of situation, when one is dealing with high level negotiations, "plugging the latest hole that has sprung up in the dike of the peace agreement" as Sampson put it, you are "missing," "talking past each other." Hall works on and off under tremendous time constraints and other pressures, having "too much information already to assimilate that is serious in nature." He had, again perhaps more than other respondents, a view about who I was that prompted him in the third interview to say, "If you -- who have a certain amount of credibility -- didn't bring this to me I wouldn't have gotten -- I would have seen the words 'Maharishi', 'TM,' 'Arab/Israeli Conflict' and I would have tossed it [sound of paper flying]."

344. When a neuro-psychologist friend of mine explained this dissertation project to a fellow neuro-psychologist before I had conducted the interviews, he predicted that the study would evoke a limbic scream from some respondents. 'Limbic' refers to the area below the cortex, a primitive part of the brain. The neuropsychologist felt that the unorthodoxy of the paradigm presented by IPPME might evoke something so primitive and instinctual that it evokes such a scream, i.e., a shut down before the level of conscious processing is achieved.

345. In fact Davies suggested in private conversation that it would be feasible to look for a correlation of the between 3-4,000 meditators in one group practicing the TM-Sidhi program at the time of the breakthrough in Oslo. He said there were not enough meditators to correlate with the Middle East, but possibly with negotiations held in Norway.
346. Fisher, who also demanded randomization, acknowledged that the Davies/ Alexander paper contributed more, i.e., described the relationships between the intervention and effects more clearly.
347. Weiss, Carol, 1980b, 250.
349. Friedman, Thomas, 1989, 428-429.
350. Chief of staff Kevin Dunfey, who could not devote enough time to finishing our interview, also commented on this point.

"The problems of modern society in this region are so great and the stresses on the individual so enormous that I look on it, on a much more sort of macro-level. And, is this one way of helping people cope and one way of reducing stress? Well, maybe it is, but we've not mentioned the problem of five-year out, problem of water, the more immediate problem of unemployed youth. There, there's a much deeper crisis going on in this region which is that... there's an enormous, I would almost say, schizophrenia or conflict between the life in the family and the life in the work place. And this leads to a built-in inner tension which I'm not sure how you relieve and I'm not sure that the techniques here are going to help.

"I almost look upon the fighting in Lebanon as an outward manifestation of something much, much bigger and in Lebanon it has to do with traditional society; the re-birth of one aspect of that traditional society, which is essentially the Shiite. The guy, the community, that has been put down for so long, bursting out, and bursting out in some bad ways, from our viewpoint and some not so bad ways but trying to deal with centuries of deprivation. I look upon this [referring to IPPME], this kind of exercise as an effort to deal with very large problems in a very narrow framework. I'm looking at a much broader array of problems and I sort of feel that this is like putting a tea strainer on top of a smoke stack that's billowing awful things. I mean it's going to catch something, it's going to deal with part of the problem but I'm looking at a much broader problem."

"I look at Tadzhikistan where 30,000 people are killed a year, I look at Angola where we've been averaging 11-1200 deaths a day for the last eleven months. I look at Lebanon and I'm just not sure that this kind of technique is going to help much. In the Lebanon case, given where the Israelis are coming from, given where the Shiite community is coming from, given the deep divisions and various players in Lebanon."

As regards the science, Dunfey said,

My audience has no time for this kind of stuff [gesturing toward the figure on the front page]; absolutely none. Partially because I don't understand it but partially because it's on a different kind of level... I think that the packaging is terribly

important. The packaging of something like this is going to be terribly important. I mean, if anybody feels that this is-- has utility for the policy maker, on a very important problem, it's *got* to be packaged an awful lot differently than it is here."

- 351. Maharishi and teachers of Transcendental Meditation have always maintained that the techniques that they teach will only enhance the religious traditions of the world. Organized resistance to them from people with fundamentalist religious beliefs, however, has been common for a couple of decades.
- 353. Laudan, Larry, 1983, 125, *fn.* 127.

***Appendix A: International Peace Project in the Middle East:  
The Effects of the Maharishi Technology of the Unified Field***  
*Journal of Conflict Resolution*, 32 (4), Dec. 1988, 776-812.

D.W. Orme-Johnson, C.N. Alexander, J.L. Davies, H.M. Chandler and W.E. Larimore

**SUMMARY<sup>7</sup>**

Prepared by J.L. Davies. See original paper for research citations and references

This social experiment tests a new theory and technology for alleviating violent conflict on a national and international scale through reducing societal stress. Many theorists in the field of conflict resolution identify stress as a primary source of violent conflict. For example, White (1984) argues that in an environment where tensions are high, efforts for peaceful resolution of conflict can succeed only in the context of concurrent steps for "drastic tension reduction."

Transcendental Meditation (TM) and the TM-Sidhi Program have been found effective in reducing stress in the individual. These are mental techniques introduced by Maharishi Mahesh Yogi for experiencing more silent, less-excited levels of awareness. This experience in TM has been characterized by Wallace (1970) and subsequent researchers as a psychophysiological state of "restful alertness" distinct from the ordinary states of waking, dreaming, and sleeping.

Marked stress reduction and enhanced personal development as a result of TM practice have been described in several meta-analyses comparing TM with rest and other forms of relaxation or meditation (Eppley et al., 1984; Ferguson, 1981). Long-term reductions in physical illness rates, more coherent psychophysiological functioning and enhanced capacity to cope with environmental stressors have also been reported (Alexander, Langer, et. al., in press; Brooks and Scarano, 1985; Orme-Johnson, 1973, 1987).

Given the reported pragmatic outcomes of the TM techniques in reducing stress and fostering development and coherence in individual functioning, an important empirical question is whether groups of individuals create similar effects of reduced stress and increased coherence in a larger population.

**PRIOR RESEARCH**

Thirty-eight studies <sup>1</sup> have indicated that when a relatively small number of individuals (approximately 1% of a population) practices the TM technique, or when an even smaller group (as little as the square root of 1% of a population) practices the more advanced TM-Sidhi program together, the combined coherent influence is enough to alleviate built-up stress, as reflected in reduced violence and more positive trends for the society as a whole. Significant impacts of the TM and TM-Sidhi program on quality of life on the city, state, national, and international levels have been reported in indices such as decreased political and criminal violence, accidents, unemployment, and suicide rates.

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<sup>7</sup>

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## THE STUDY

In August and September, 1983, when Israel was heavily involved in fighting throughout the southern half of Lebanon, it was predicted that collective practice of the TM and TM-Sidhi techniques in Jerusalem would generate such an influence of coherence, varying in extent with the size of the meditating group, to cover Jerusalem, Israel, and even Lebanon, resulting in a calming of the conflict there. Consistent with prior studies, we hypothesized that the group practice would influence the coherence of a population equal to 100 times the square of the number of participants, plus 100 times the number meditating independently of the group in that population. The number of people participating daily in the group TM and TM-Sidhi practice in Jerusalem ranged from 65 to 241.

The effect of their group practice was tested for three different group and population sizes with respect to the following variables: for Jerusalem -- automobile accidents, fires, and total crime; for Israel, including the occupied territories, -- crime, stock market, and national mood; and for Israel and Lebanon combined -- war deaths and war intensity.<sup>2</sup>

Dependent variable measures were based on publicly available data sources. These and the experimental hypotheses were chosen before the experiment, and recorded with independent review boards of scientists in both North America and Israel. The experimental period -- Aug. 1-Sept. 30 - was arbitrarily selected with respect to the social variables studied. At the end of the first and second month of the experiment before any analysis of substantive data, the daily numbers of participants in group practice of the TM and TM-Sidhi program were reported to review boards. The level of participation in the group rose gradually over the first two weeks of the experiment, remained high for the next 13 days, and then fluctuated at a generally lower level for the second month.

## ANALYSIS

In order to control for pre-existing trends, cycles, and seasonal effects, including the effects of temperature and holidays, Box-Jenkins time series analytic techniques were used.<sup>3</sup> The effects of changes in the size of the group on eight dependent variables were studied as follows, for:

<u>Location</u>	<u>Dependent Variables</u>
o Jerusalem:	automobile accidents, fires, and total crime;
o Israel:	crime, stock market, and national mood derived from news content analysis; <sup>4</sup>
o Lebanon:	war deaths of all factions and war intensity derived from news content analysis.

The above variables were combined into equally weighted composite indices for Jerusalem, Israel,<sup>5</sup> Lebanon, and further into a single overall composite index, since it is generally the practice in quality of life research to combine variables into composite indices to provide a more reliable, broader based measure of quality of life.

## RESULTS

Consistent with the experimental hypotheses, increase in the TM-Sidhi group size was followed by significant changes in all dependent variables using at least one of the two methods of assessment. All four composite quality of life indices changed significantly in the anticipated direction. As predicted, the composite indices for Jerusalem and Israel were influenced by smaller TM-Sidhi groups (122) whereas Lebanon was not significantly affected until the group size reached 197.

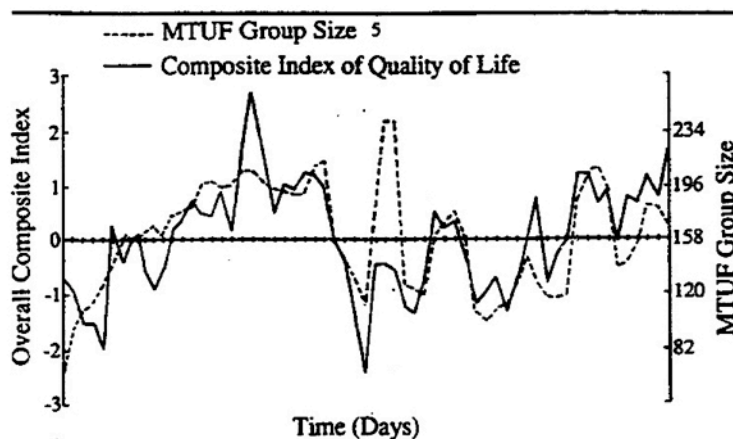


Figure 3: Standardized Daily Time Series of the Overall Composite Index Plotted Against the MTUF. An Overall Composite Index composed of six variables was constructed so that a positive deflection on the graph represents global improvement in quality of life. It can be seen that the index showed a positive correlation with the MTUF ( $r = .57$ ).

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Impact assessment analyses comparing days with the lowest and highest levels of participation within the two month period indicated, for example, that war intensity in Lebanon decreased by an estimated 45% ( $p=.0045$ ), war deaths by an estimated 76% (from 40.1 deaths to 9.7 deaths per day,  $p=.019$ ), and total crime in Israel decreased by an estimated 12% ( $p=.0016$ ). In Jerusalem auto accidents decreased 34% from 3.9 per day to 2.5 ( $p=.024$ ). For 13 days when the group size was experimentally raised to a high level the average of war deaths per day was 1.5 compared with 33.7 for the period preceding and following.

If a common effect was being generated simultaneously across all these different measures, then the signal-to-noise ratio would be increased by aggregating the variables. Averaging the standardized variables would add the common variance among the variables and, on the average, cancel to zero the random components. As predicted, the mean effect size for the individual variables (.26) was less than for the same variables aggregated into three composites (.43), and the aggregation of all variables into one composite variable produced the largest effect size (.69). This finding supports the hypothesis that the TM-Sidhi group was creating a generalized, underlying coherence effect common to all measures.

In every case transfer function analysis indicated that changes in the war and quality of life variables occurred immediately or shortly after changes in group size -- never the reverse. That is, group size did not change in response to improving or deteriorating social conditions. This pattern of results suggests that increases in group size were responsible, as predicted, for the observed

beneficial impact, with the primary changes being immediate (a lag of 0 or 1 days), and lesser, delayed effects being presumably due to the indirect impact of actions and decisions taken earlier.

The effects of holidays and temperature as well as all seasonal components such as weekend effects, drifts and trends in the dependent variables were explicitly controlled for using standard time series analytic procedures. While formal randomization of high participation days was not possible for practical reasons common to all sociological experiments, high and low participation days were found to be, in effect, randomly spaced. It is therefore unlikely that the results could be explained by any unknown variable following the same irregular pattern as high and low participation days.

Since completion of this study, seven additional groups using the TM-Sidhi techniques, and large enough for a predicted reduction in armed conflict in Lebanon over several days or weeks, have been brought together. In every case, a significant positive impact in alleviating violence has been observed.

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1. Ten of the 38 have been reported in four refereed articles in the following journals:  
*Journal of Crime and Justice*. 1981, Vol. IV. 25-45.  
*Journal of Mind and Behavior*. 1987, Vol.8, N.1. 67-104; 1988, Vol.9, N.4, 457-485.  
*Social Indicators Research*. 1990, 22, 399-418.
  2. The population sizes were: 1) Jerusalem--429,000, including the Arab population; 2) Israel--5,304,000, including the occupied territories; and 3) Israel and Lebanon combined--7,905,000. The critical group size for these three populations was 65, 122, and 197 respectively, taking into account the 38,000 independent TM participants throughout Israel and 2,000 in Lebanon.
  3. Two Box-Jenkins ARIMA time series methodologies were used: impact assessment analysis to study the predicted impact of various group sizes and transfer function analysis to address the question of causality -- whether changes in group sizes led changes in quality of life or vice versa. These techniques have been used for two decades in business, engineering, and economics and are now the most widely used and accepted in the social sciences for statistical analysis of the inter-relationships among variables changing over time.
  4. News content analysis was accomplished using the *Jerusalem Post*, by independent raters unaware of story dates, using a 7-point Likert scale for affect and 5-point Conflict Intensity Scale based on Azar's COPDAB scales.
  5. MTUF refers to the Maharishi Technology of the Unified Field, which includes the Transcendental Meditation and TM-Sidhi techniques.

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## ***Appendix B Maharishi Effect Research Publication History***

References, 38 studies investigating the Effects of Practice of the TM and TM-Sidhi Program on the Quality of Life in Cities, States, Nations, and the World <sup>1</sup>

1. **Borland, C. & Landrith, G. 1976.**
2. **Hatchard, G. 1977.**
3. **Dillbeck, Michael. 1978.**
- \* 4. **Dillbeck, M.D., Landrith III, Garland., and Orme-Johnson, D.W. 1981.**
5. **Landrith, Garland S. III. and Dillbeck, Michael C. 1983.**
- \* 6. **Dillbeck, Michael C., Banus, C.B., Polanzi, C., and Landrith, III, G.S. 1988.** ("A Random sample of 160 U.S. cities, 40 in each of 4 population groups: greater than 250,000, 100,000-250,000; 50,000-100,000; 25,000-50,000.")<sup>2</sup>
- \* 7. **Dillbeck, Michael C., Banus, C.B., Polanzi, C., and Landrith, III, G.S. 1988.** ("A random sample of 80 standard metropolitan statistical areas, 40 in each of 2 population groups: greater than 500,000 and 200,000-500,000. the sample of 80 metropolitan areas included 47% of the total U.S. metropolitan population.")<sup>3</sup>
8. **Abou Nader, T.M., Alexander, Charles N. & Davies, John L. 1984.**
9. **Lanford, A.G. 1984a.**
10. **Lanford, A.G. 1984b.**
- \* 11. **Dillbeck, Michael C., Cavanaugh, Ken L., Glenn, T., Orme-Johnson, David W., Mittlefehldt, V. 1987. (Metro Manila, Philippines 1983-1985)**
- \* 12. **Dillbeck, Michael C., Banus, C.B., Polanzi, C., and Landrith, III, G.S. 1988. (Washington, D.C.)**
- \* 13. **Dillbeck, Michael C., Cavanaugh, Ken L., Glenn, T., Orme-Johnson, David W., Mittlefehldt, V. 1987. Union Territory of Delhi (capital district), India. Dillbeck, M.C., Cavanaugh, K.L., & Berg, W. P. van den, 1983.**
- \* 14. **Dillbeck, Michael C., Cavanaugh, Ken L., Glenn, T., Orme-Johnson, David W., Mittlefehldt, V. 1987. (Rhode Island, U.S.A.) Dillbeck, Michael C., Foss, A., & Zimmermann, W.J. 1983.**
- \* 15. **Dillbeck, Michael C., Cavanaugh, Ken L., Glenn, T., Orme-Johnson, David W., Mittlefehldt, V. 1987. (Puerto Rico, U.S.A.)**
16. **Reeks, D. 1990.**
17. **Burgmans, W.H.P.M., Burgt, A. T. van den, Langenkamp, F.P., & Verstegen, J. H. 1983.**



18. Davies, John L. and Alexander, Charles N. 1983.
19. Beresford, M.S., & Clements, G. 1983.
20. Dillbeck, Michael C., Larimore, W.E., and Wallace, R.K. 1984.
21. Lanford, A.G., Dixon, C.A., and Reeks, D.L. 1984.
22. Gowing, S.G. 1986.
- \* 23. Dillbeck, Michael C., Cavanaugh, Ken L., Glenn, T., Orme-Johnson, David W., Mittlefehldt, V. 1987. (Philippines December, 1979)
- \* 24. Orme-Johnson, David W. Gelderloos, P, & Dillbeck, M.C. 1988.  
Orme-Johnson, David W. and Gelderloos, P. 1984.
- \*\* 25. Cavanaugh, Ken L. & King, K.D. 1988.  
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- \* 29. Orme-Johnson, David W., Alexander, Charles N., Davies, John L., Chandler, H.M., and Larimore, W.E. 1988 (12/01).
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32. Orme-Johnson, David W., Dillbeck, M.C., Bousquet, J.G., and Alexander, C.N. 1985.
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- \* 34. Gelderloos, P. Frid, M.J., Goddard, P.H., Xue, X. & Loliger, S. 1988.  
\*\* Gelderloos, P. Frid, M.J., & Xue, X. 1989, April.
35. Alexander, Charles N., Abou Nader, T.M., Cavanaugh, K.L., Davies, John.L., Dillbeck, M.C., Kfoury, R. J., and Orme-Johnson, D.W. 1984 (7)
36. Davies, John L. 1988. Davies, John L. and Alexander, Charles N. 1989.

37. Orme-Johnson, David W., Cavanaugh, Ken L., Alexander, Charles N., Gelderloos, P., Dillbeck, Michael C., Lanford, A.G., & Abou Nader, T.M. 1987.
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Note:       \*       Published.  
               \*\*       Published in proceedings.  
                       Note that 6, 7, and 12 are distinct inquiries published in the same article.  
                       11,13,14,15,and 23 are also separate studies published together in another  
                       single article.

End Notes:

1.       *The Maharishi Effect*, 1990.
2.       *Ibid.* 29.
3.       *Ibid.* 30.
4.       *Ibid.* 68.
5.       *Ibid.* 69.

## *Appendix C Respondent List and Biographies*

### Scholarly Reviewers

Fisher, George  
Gurney, Kevin  
Harrington, John  
Lambert, David  
Moore, Lawrence  
Walsh, William

### Reporters

Berman, Barry  
Gaines, Judith  
James, Jennifer  
Klein, Rubin  
MacBain, Bruce  
Nicosia, Nunzio  
Nielsen, Paul  
Nolan, Francis  
Rohan, Christopher  
Taft, Kingsley

### Congressional Members and Staff

Banks, Brian  
Glaser, Alan  
Marcheselli, Sen. Ralph  
Roach, Anna  
Rumar, Joseph  
Thomas, Patricia  
White, Rep. Jack

### Decision Makers Outside of Government

Cox, Donald  
Crawford, Dennis  
Golden, Karen  
Kaplan, Leon  
Kern, Adam  
Krachon, Michael  
Neumann, Peter  
Roth, Barbara

### Officials with Diplomatic Responsibilities

Hall, Maxwell  
Paris, Walter  
Sampson, Craig  
Tasman, Mitchell

**List of Respondents in Alphabetical Order (including respondents who were not able to finish their interviews, but who are mentioned) <sup>8</sup>**

***Brian Banks***

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<sup>8</sup> Please note, respondents are listed with their pseudonym assigned at random (with the exception of Ms. Judith Gaines who asked to retain her name). The biographical sketches are written to maintain the respondents' anonymity.

recently completed a Master's Degree in Middle Eastern Studies; was a Congressional staffer for about two years.

***Rep. Noah Berger***

a lawyer, has held elected office for more than twenty years. His committee work has focused in part on the Middle East. [did not complete his interview]

***Barry Berman***

who has an undergraduate degree in economics and political science, also completed a one-year graduate-level journalism fellowship. He was a diplomatic correspondent who had worked in Washington D.C. for thirteen years covering primarily foreign policy. He did not cover the Middle East exclusively and was not a Middle East specialist, but is "reasonably informed about the Middle East and the nature of the debate," he said.

***Donald Cox***

Ph.D., has been a political science professor for about 30 years. He wrote and edited books on U.S. policy toward the Middle East and on international politics and conflict and has contributed to numerous scholarly journals. He was the director of a Middle East institute and a presidential advisor.

***Dennis Crawford***

Ph.D., has been a senior Middle East analyst for 25 years in government, universities, and think-tanks. He has published several books on the Middle East and was frequently quoted by other analysts.

***Kevin Dunfey***

Ph.D., has served as a committee chief of staff specializing in foreign affairs -- and particularly in U.S. policy toward the Middle East -- for 20 years. [He was not able to complete the interview.]

***George Fisher***

completed his Ph.D. in political science. His master's degree was in mathematics. He also earned a minor in religion. Known as a rigorous mathematician, he has taught international relations, mathematical modeling and other courses for 18 years. His applied work has dealt mostly with quantitative models of international behavior. He said he was interested in using mathematical modeling and computer technology to predict outbreaks of war or famine.

***Judith Gaines***

a former Woodrow Wilson Scholar and Oxford graduate in Political Philosophy, was a reporter for large regional newspaper. At the time that she wrote stories about the Maharishi Effect she was a free-lance reporter, covering "what the Associated Press would not cover," what she described as the "offbeat beat."

***Alan Glaser***

majoring in South Asian studies in college. He was a foreign policy legislative assistant who focused on various regions. Before joining his Senator's staff he worked on Asian issues with "public interest groups that lobby on Asia." His work depended on "the big foreign policy issues of the day."

***Karen Golden***

B.A. in political science had been working for four years as a senior lobbyist, had served as a legislative assistant for two years in the Senate and three years in the House. She was in constant contact with members and staff in a hundred House and Senate offices and responsible for relations with major foreign policy committees.

***Kevin Gurney***

with an undergraduate major of mathematical statistics and Ph.D. in psychology, was a retired social psychology professor who continues to be involved in a dozen research interests. He had taught statistics to graduate and undergraduates in psychology for 42 years and had published in a wide range of fields.

***Maxwell Hall***

Ph.D. in Middle Eastern history and American diplomatic history is an author, former professor, and was serving as a senior diplomat.

***John Harrington***

Ph.D. in political science has been a political science professor for over thirty years. He also trained in economics. He has published extensively, has received numerous awards, continues to serve on numerous editorial boards and has served as a consultant to many government agencies.

***Jennifer James***

Diplomatic correspondent for a major regional newspaper, was bureau chief for 6 of 10 years stationed in the Middle East.

***Leon Kaplan***

an Israeli and law student at the doctoral level, has worked as a human rights attorney in Israel.

***Adam Kern,***

Ph.D. in International Relations was a Middle East scholar who taught international relations at a major university, was associated with several think tanks and was working as a reporter. He had been New York and Washington correspondent for several foreign

newspapers as well as a U.N. bureau chief. He recently published a book on Middle Eastern affairs, has written articles for many U.S. and foreign newspapers, and has appeared frequently on radio and television.

***Rubin Klein***

B. A. in Political Science, Economics, and Psychology, was a Washington diplomatic correspondent for an important Israeli newspaper. He has been a reporter for 25 years. Before that he was a spokesman for an Israeli politician.

***Michael Krachon***

interrupted Ph.D. and academic teaching in political science and Middle East studies to become executive director of an organization representing Americans of Middle Eastern descent. He was born in the Middle East.

***David Lambert***

Ph.D., was a policy specialist at a U.S. think tank and an applied mathematician interested in large-scale problems. He has published in numerous technical journals and has been involved in shaping U.S. policy.

***Bruce MacBain***

senior regional Middle East diplomatic correspondent for a pace-setting U.S. newspaper, who has specialized in Middle Eastern affairs for the past 20 years.

***Sen. Ralph Marcheselli***

an attorney who had served in the House and Senate for a total of 20 years is known for his independent and principled stands, although he can also be loyal to his party. He had served on several foreign affairs committees.

***Mark Mazzella***

Ph.D., was a prominent conflict resolution scholar; president of an association who has written 16 books and monographs. He reviewed the Davies/Alexander paper. [Was not interviewed.]

***Lawrence Moore***

who studied physics and majored in philosophy as an undergraduate, earned a Ph.D. in international relations. He has been a professor of political science for 11 years. His doctoral dissertation focused on statistical techniques, especially time-series statistical methods.

***Peter Neumann***

who has a Master's degree in international affairs, directed research activities and advocacy work for a human rights organization on behalf of four countries including the Israeli occupied territories.

***Nunzio Nicosia***

a diplomatic correspondent specializing in the Middle East for 25 years has lived and studied in the Middle East for 20 of those years.

***Paul Nielsen***

senior diplomatic correspondent at a pace-setting U.S. newspaper for over 30 years.

***Francis Nolan***

was a diplomatic correspondent for a pace-setting U.S. newspaper; after having been stationed in Jerusalem for 4 years, he specialized in Middle Eastern affairs.

***Walter Paris***

Ph.D., former university professor, strategist, and negotiator has had a leading role in formulating and implementing U.S. policy in several areas for over a dozen years; has published extensively. Described by one journalist as "as much of an expert in the area as there is."

***Anna Roach***

chief of staff for Congressman White since 1991. She had worked as a college intern for another representative. She spent 10% of her time on Middle Eastern issues and directed 6 1/2 staff members and two full-time interns.

***Christopher Rohan***

bureau chief for 5 years for a major U.S. newspaper; based in Jerusalem since 1990 and covered Washington, D.C. from 1985-90.

***Barbara Roth***

co-director of a lobbying organization had worked with notable success in related areas since 1977 and for the current organization since 1990. She received an award for her work in the Middle East and co-authored a book on peace in the Middle East. She said, "Because I got involved in the way I did in talking to the principals on both sides and talking to people both at the elite level and at the grassroots level, I'm very interested not only in what goes on at the top level of negotiations but also day-to-day life."

***Joseph Rumar***

who has a Master's degree in international relations had worked in the Senate for six and a half years, first as a legislative correspondent and eventually with primary regional responsibility on a Senate committee staff working for the Chairman.

***Craig Sampson***

Ph.D., a former university lecturer, who has often been honored for his work in various management capacities related to Middle East diplomacy over 20 years.

***Kingsley Taft***

who at the age of 30 earned his M.A. in journalism, was a diplomatic correspondent at a pace-setting U.S. newspaper, had written for all of the other pace-setting papers and was at his current paper for 16 years. He covered economics initially, but had been "watching the Middle East closely for 6 or 7 years," as a member of a news team.

***Mitchell Tasman***

Ph.D., is a former university administrator who had served in various capacities in his organization since 1977. He oversaw relations with many countries, managed hundreds of people, and was considered an effective and pragmatic diplomat and negotiator, with particular expertise in the Middle East.

***Patricia Thomas***

majored in history and earned a Master's degree in political science and international affairs. She had served as a legislative assistant to one representative from 1979 to

1985 and was working for Representative Berger as a professional staff committee member.

***William Walsh***

earned his Ph.D. in social psychology in 1976 and has worked as a professor since 1979. He is known for creatively using alternative research approaches. He is interested in macro as well as micro issues in political science and has published extensively.

***Rep. Jack White***

law school graduate, was in his fourth year in the House. Prior to being elected, he was a practicing attorney and law school professor.





***Appendix D Interview Guide -- Part I***

General approaches the respondent takes in working within that framework, how he/she responds to and uses information -- the way in which he/she creates a context for understanding and making professional judgments in his/her work.

**Areas of Consideration**

**Background / Education**

Work background.  
Educational background.  
How much social science training undertaken.

**Role / Orientation of work**

Operating philosophies/values, principles that figure prominently in the course of work.  
Questions considered relevant to explore. Why these rather than others?  
Nature of the agenda/radar screen with which the respondent works.

**Relations**

Associations, colleagues, audiences.  
Interests in relation to power, status.  
Respondent's orientation towards elites/power/the status quo.

**Descriptions of important information use (a detailed investigation)**

Sources -- distinction between people/written documents.  
Kinds of information generally sought.  
How priority is given to information.

**Within the total body of information how particular information is used. (detailed)**

How information dealing with competing or divergent views is used.

**Specific criteria for assessing the likelihood of using information -- how respondent decides within range of information received, what will be useful or not.**

Role of professional "instincts"/ experience/judgment, using several examples -- not limited to Middle East information.  
Role of conventional wisdom, prevailing mind set.  
Role of other criteria.

<b>Appendix D -- Interview Guide -- Part II</b> <b>How do policy makers and other member of the Middle East policy community determine whether <i>International Peace Project in the Middle East</i> research information is trustworthy and useful?</b>	
<b>Areas of Consideration</b>	* Crucial points.
A.*	Likelihood that the respondent will take the research into account in the future. -- whether or not and how far respondent might think about or draw on the study in the course of his/her work.
B.*	How does respondent weigh the research premises and results against their experience, expectations and values? How does the IPPME study tally with experience? How does it square with deeper ontological commitments? Does the interviewee describe related schema or frames of reference? Why or why not respondents refer to related schema or frames of reference; where do these fit within other schema? What are the implications of IPPME research for daily ongoing activities?
C.	How does respondent ascertain whether and to what degree the research information conforms to scientific standards? * Respondent's judgment as to his/her competence in evaluating methodology vs. Importance of reliance on social scientists for assessment of methodology. * Scientific criteria for assessment. * Importance of scientific outcomes in assessment. How methodology is examined? Place of methodology in the assessment? Consideration of how science works. * Perceptions re: place in science of the IPPME world view. Perceptions re: the concept of science employed in IPPME. Reactions to IPPME language. Perceptions about variables.
D.*	How important are B (assessment of experience) and C (assessment of scientific quality) -- how is each overall assessment weighed in relation to the other -- in determining whether respondent will give the study further consideration or whether the research ideas and information would substantively contribute to his/her work?
E.*	Perceptions about whether the IPPME study is feasible and practical -- or would be considered as feasible and practical by others. Does the research indicate how to make feasible changes in policy and other things that can feasibly be changed?
F.	Significance of the study's perceived challenge to the status quo * Concern for what significant others think about IPPME. * Importance of opinions of others within the profession, within the foreign policy communities (including peer review). * Appraisal in relation to conventional mind sets. * Perceptions re: one's status in considering response to research. * Interpretation of controversy -- effect of controversy on decisions to take research into account. Importance of considerations of competitive funding and related resources (like

	publishing) in assessing information and the IPPME research.
G.	Value respondent places on challenge to the status quo in assessing likelihood of considering the research further.
H.	<p>Use of Factors Peripheral to, but related to Scientific Quality.</p> <p>What is the <i>meaning</i> of such factors as:</p> <p>    Assumptions, rationales related to assessment of scientific quality.</p> <p>    Distinctions between scientific and extra-scientific factors.</p> <p>* Responses to JCR peer review.</p> <p>* Importance and meaning of the source -- of IPPME scientists' credentials/institution.</p>
I.	Effects of Prior Exposure to IPPME and IPPME-related research.
*	Exposure to the techniques and programs described in the IPPME research.
*	Familiarity with mention of the programs or research in the media.
*	Level of interaction with any others between Pt. I interview and Pt. II.
J.*	Which aspects of the unorthodoxy of the IPPME study influence respondent's response to it -- and how do these aspects interact with the other considerations he/she takes into account?

***Appendix E Chart I-A and Chart II-A***

Appendix E -- Chart I-A Likelihood of Future Consideration of Maharishi Effect Research														
Highly unlikely	I	II	Unlikely	III	IV	Likely	V	Most Likely	VI					
Scholarly Reviewers Fisher, <sup>600K</sup> Gurney, <sup>600K</sup> Walsh, <sup>600K</sup>			Scholarly Reviewers Lambert <sup>600K</sup> Harrington('94) <sup>600K</sup>		Scholarly Reviewer Moore ('94) <sup>600K</sup>			Scholarly Reviewers Harrington('87) <sup>600K</sup> Moore ('87) <sup>600K</sup>						
Reporters MacBain, <sup>600K</sup> Klein <sup>600K</sup>	Reporters Taft, <sup>600K</sup> Nicosia <sup>600K</sup>	Reporters Taft, <sup>600K</sup> Nicosia <sup>600K</sup>	Reporters Nielsen, <sup>600K</sup> Nolan, <sup>600K</sup> James <sup>600K</sup>	Reporter Rohan <sup>600K</sup>	Congressional Staff Rumar <sup>600K</sup> , Glaser <sup>600K</sup>	Congressional Staff Banks <sup>600K</sup>	Reporter Gaines <sup>600K</sup>	Reporter Gaines <sup>600K</sup>						
Congressional Staffers Thomas, <sup>600K</sup> Roach <sup>600K</sup>	Lobbyists Krachon, <sup>600K</sup> Golden <sup>600K</sup>	Congressman White <sup>600K</sup>	Analyst Crawford <sup>600K</sup> Human Rights Research Director Neumann <sup>600K</sup>	Diplomat Paris <sup>600K</sup>	Presidential Advisor Cox <sup>600K</sup>	Human Rights Attorney Kaplan <sup>600K</sup>	Senator Marcheselli <sup>600K</sup>	Senator Marcheselli <sup>600K</sup>						
Consultant/Reporter Kern <sup>600K</sup>							Center Director Roth <sup>600K</sup>	Center Director Roth <sup>600K</sup>						
Diplomats Hall, <sup>600K</sup> Tasman <sup>600K</sup>						Diplomat Sampson <sup>600K</sup>								
Science not relevant					Science useful									
A. Social science irrelevant or impossible to conduct. B. IPPME irrelevant because of way policy conducted, agendas, etc. C. Conflict too intransigent to be affected by IPPME. D. IPPME illegitimate. E. Describe prejudice. F. IPPME considered spiritual or religious. Either anathema to my religion or to my scientific or other perspective because of spiritual implications. G. Would prefer other explanations of the data or conflict resolution. H. Too challenging to the status quo, e.g. too risky. I. Can't imagine how it would work.					J. Needs different research design. K. Question source. L. Concerns about feasibility. M. Not interesting. N. Ideas interesting/lively. O. Important to be open-minded. P. More research is needed. Q. People (Orme-Johnson et al.) are credible. R. I believe it. S. Science is persuasive.									

Here>

**Appendix F Documentation of Interview and Analysis Audits**

**Example of Audit Form Completed by Rose Zimering, Ph.D.**

Observing the Assessment of Research Information by Potential Users:  
International Peace Project in the Middle East  
**Interview Transcript Audit**

Interview #: *JSTF* Date of Interview: *1/18/94 & 1/21/94*  
*Part I & II*

Did you detect any evidence of the interviewer's bias unduly influencing or intruding on the interview? Please note where; please give page number and paragraph number (instead of giving paragraph number you may also mark the sections concerned).

*No* *No*

Did you encounter evidence of leading questions or evidence of the interviewer either directly or indirectly influencing the respondent? Please note where.

*No* *No*

Did you notice a change in the respondent's stance or tone with him or her becoming suddenly more circumspect or more defensive? Please note where.

*No* *No*

**Note confirming telephone conversation with Dr. Susan Markowitz.**

*Carla -  
I didn't find any evidence  
of bias. However, there are a  
few comments which I started  
because I had questions about  
what was going on in the  
interview.*

*7/8*

*Susan Markowitz*

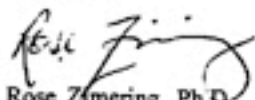
March 26, 1995

Dear Ms. Brown,

I have completed the audit on several randomly selected participants from your dissertation study. After an initial discussion with you regarding the overall objective of the study I reviewed the conclusions that you drew from interview data of two participants (one journalist and one reviewer). I then read the primary sources of information (i.e. the summary chart plus the transcript of the interview) and confirmed your content conclusions of each interview. Furthermore, I did not detect any evidence of an investigator bias that may have influenced or skewed your content coding.

Thank you for this opportunity to participate in your interesting dissertation study.

Sincerely,



Rose Zimering, Ph.D.  
Clinical psychologist  
Boston V.A. Outpatient Clinic

March 29, 1995

Ms. Karla Brown  
78 Auburn St.  
Newton, MA 02166

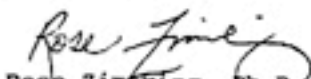
Dear Ms. Brown,

I have completed my review of seven interview transcripts from your dissertation study. I read each transcript carefully in order to determine whether you as the interviewer asked leading questions or influenced the respondent's answers with any personal bias or demand characteristics.

I can report with confidence that I detected no such bias or influence on the interviewer's part in any of the seven interviews. On the contrary, I perceived the interviewer to be very neutral and non-judgmental in all interviews. Furthermore, there was no evidence of the respondent's replies shifting from the subject matter of the interview to a more defensive or threatened tone.

The transcripts were very interesting which made my participation in your study a pleasure. Please feel free to call on me again if I can be of further assistance.

Sincerely,



Rose Zimering, Ph.D.  
Assistant Chief, Psychology Service  
Boston VA Outpatient Clinic



## ***Appendix G Biographical Information, John Samuel Hagelin***

### **Biographical Sketch**

Dr. Hagelin is Professor of Physics and Director of the Institute of Science, Technology and Public Policy at Maharishi International University. Dr. Hagelin received his A.B. Summa Cum Laude from Dartmouth College in 1975. After completing his Ph.D. at Harvard University in 1981, Dr. Hagelin joined the theoretical physics groups at the European Laboratory for Particle Physics (CERN) and the Stanford Linear Accelerator Center (SLAC), where he did pioneering research at the forefront of unified quantum field theories. Following his positions at CERN and at SLAC, Dr. Hagelin joined the faculty of Maharishi International University, where he established a doctoral program in elementary particle physics and unified field theories.

Dr. Hagelin is a leading authority in the area of unified quantum field theories. He is the principal investigator of a National Science Foundation research grant on the theory and phenomenology of unified field theories. His articles on the theory and phenomenology of electroweak unification, grand unification, supersymmetry and cosmology include some of the most cited references in the physical sciences. Dr. Hagelin is responsible for the development of a highly successful grand unified field theory based on the superstring. He has traveled to 35 countries during the past five years, speaking to scientists and government leaders on the subject of science, technology and public policy.

In 1992, Dr. Hagelin was named winner of the prestigious Kilby Award, which recognizes scientists who have made "major contributions to society through their applied research in the fields of science and technology." The award acknowledged Dr. Hagelin's "pioneering work in the field of supersymmetric unified quantum field theories, his relentless pursuit of the answers to compelling global issues, and the unlimited potential which he has exhibited through these efforts." The award recognized Dr. Hagelin as "a scientist in the tradition of Einstein, Jeans, Bohr and Eddington."

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1. Weak Mass Mixing, CP Violation, and the Decay of b-Quark Mesons, J.S. Hagelin, Phys. Rev. D20 (1979) 3893.
2. Phase of  $\epsilon'$  and the Sign of  $\sin \delta$ , J.S. Hagelin, Phys. Rev. D23 (1981) 119.
3. Radiative Corrections to  $\sin^2 \theta_W$  to Leading Logarithm in the W Boson Mass, S. Dawson, J.S. Hagelin and L. Hall, Phys. Rev. D23 (1981) 2666.
4. Comment on CP Violation in  $B^0 - \bar{B}^0$  System, J.S. Hagelin and M.B. Wise, Nucl. Phys. B189 (1981) 87.
5. Mass Mixing and CP Violation in the  $B^0 - \bar{B}^0$  System, J.S. Hagelin, Nucl. Phys. B193 (1981) 123.
6. Higgs Boson Exchange Models of CP Violation and  $K \rightarrow 2\pi$ , J.F. Donoghue, J.S. Hagelin and B.R. Holstein, Phys. Rev. D25 (1982) 195.
7. What Does b-Quark Decay Tell Us About CP Violation, J.S. Hagelin, *Proceedings of the 2nd Moriond Workshop (1982): New Flavours*, edited by Tran Thanh Van, Editions Frontieres, Gif-sur-Yvette, France.
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***Appendix H Newspaper reports by Judith Gaines***

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