

**POLITICKING AND PARADIGM SHIFTING:
JAMES E. McDONALD AND THE UFO CASE STUDY**

A DISSERTATION SUBMITTED TO THE GRADUATE DIVISION OF THE UNIVERSITY OF
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By
Paul E. McCarthy

Dissertation Committee:

Robert S. Cahill, Chairman
Deane Neubauer
Robert B. Stauffer
Sanford M. Siegel
David W. Swift

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—Tom Tulien, Sign Historical Group.

**Politicking and Paradigm Shifting:
James E. McDonald and the UFO Case Study
by Paul McCarthy, Ph.D.**

Introduction to the Internet Edition|

John Stepkowski, Sign Historical Group

Jim Giglio, National Capital Area Skeptics

Preparation of this on-line version is a collaboration between the ([SHG](#)) and National Capital Area Skeptics ([NCAS](#)). SHG and NCAS have joined forces here since both share an interest in presenting primary source documents relevant to the history of the UFO issue.

Politicking and Paradigm Shifting chronicles the activities of Dr. James E. McDonald in a series of UFO-related controversies between 1966 and 1970 that mark a critical period of scientific and political decision-making concerning the UFO issue. This was the period when the Colorado UFO project was being conducted in which the final report to the Air Force recommended that further scientific study of UFOs should not be supported. Also, on July 29, 1968, the Committee on Science and Astronautics of the U.S. House of Representatives organized a public hearing on the subject of Unidentified Flying Objects. The Symposium represented a variety of diverse views on UFOs by a number of scientists who had taken a serious interest in the phenomenon.

The two consequential documents mark opposing positions:

1. The report of the Colorado project, *Scientific Study of Unidentified Flying Objects*, ^[1] often referred to as the "Condon Report" after its Scientific Director, physicist Edward U. Condon.
2. The record of the *Symposium on Unidentified Flying Objects*, ^[2] a hearing conducted by the Committee on Science and Astronautics of the U.S. House of Representatives. This record is often referred to as the "Roush Symposium" after Rep. J. Edward Roush of Indiana, who chaired the session.

From the historical perspective, Dr. McCarthy's dissertation provides considerable background to these documents and essentially links them by telling the story of Dr. McDonald's involvement in both. For example, in Chapter three we find Dr. McDonald working to influence the Air Force sponsored Condon Project at the University of Colorado and in Chapter four engineering the 1968 Symposium by the Committee on Science and Astronautics of the United States House of Representatives.

In telling the story, Dr. McCarthy relies primarily on the voluminous correspondence of Dr. McDonald, supported by additional papers and correspondence of other participants in the events of 1966-70. These documents do more than simply chronicle who communicated what to who at any particular time, but reveal a thoroughly human "cast of characters" who are by turns inquisitive, passionate, ambitious and on occasion, stubbornly devious. Dr. McCarthy presents

them "warts and all."

In the preface, Dr. McCarthy describes how he began as a self-described "believer" with a perspective that led him to accept the "Extraterrestrial Hypothesis" (ETH); that this planet is regularly visited by extraterrestrial vehicles under intelligent control. He alerts the reader to his support for the ETH while laying out how he structured the work in an effort to prevent this bias from influencing his approach to the issues in political science and history of science that lie at the center of the study.

Further, Dr. McCarthy proposes the intriguing thesis that what Dr. McDonald attempted was nothing less than a fundamental "paradigm shift" in the Kuhnian sense, ^[3] in which an entire field of science goes through a revolutionary alteration of intellectual outlook and theoretical structure.

It remains for the reader to judge whether or not Dr. McCarthy supports his thesis adequately. But whatever the reader's decision, we are confident that the presentation of the story will be of considerable interest to all serious students of the UFO issue.

Both Sign Historical Group and National Capital Area Skeptics wish to express our appreciation to Dr. McCarthy for allowing us to bring his dissertation to the Internet.

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1. Condon, Edward U. and Daniel S. Gillmore, *Scientific Study of Unidentified Flying Objects*. New York, Bantam Books, 1969 (paper), New York, E.P. Dutton Inc., 1969 (hardcover). On the web at www.ncas.org and other mirror sites. - [Back to Text](#)
 2. U.S. House of Representatives, Committee on Science and Astronautics, *Symposium on Unidentified Flying Objects*, July 1969. Washington, D.C., U.S. Government Printing Office. On the web at www.ncas.org and other mirror sites. - [Back to Text](#)
 3. Kuhn, Thomas S., *The Structure of Scientific Revolutions*. Chicago, University of Chicago Press, 1970. - [Back to Text](#)

Note on Commercial Availability: The Colorado report can be found for sale on internet auction sites, as well as other sites specializing in out-of-print works, with some frequency. Kuhn's work is available both used and new, on-line and in stores; it is still in print. The Roush Symposium appears on auction sites that feature out-of-print works, but only rarely.

Editorial Notes

To assist online readability and navigation through Dr. McCarthy's original dissertation, hypertext enhancements were made in this web-based presentation that have met with Dr. McCarthy's full approval.

- The table of contents is presented as a set of clickable links to every part of the dissertation, with convenient "BACK To" access points.
- Not all the chapters in Dr. McCarthy's original work had sub-headings easily convertible to internal links. For example, in Chapter 3 "History of Events", we examined the text carefully for logical groupings in order to create clickable sub-headings.
- Itemized lists have been made more prominent.
- On page 5, we reorganized Table 1, ANNUAL UFO REPORTS 1947-1965, so it scrolls vertically on the screen rather than horizontally.

Researchers who have obtained access to the dissertation in previous years via the University of Hawaii library will have noticed gaps in the text beginning on page 170. Thanks to Dr. McCarthy's foresight in donating his original source materials to the Center for UFO Studies (CUFOS), The Sign Historical Group was able to obtain a complete draft of the dissertation for this online presentation. CUFOS is to be commended for preserving Dr. McCarthy's source materials—approximately eight 3" binders—and we thank Dr. McCarthy for ensuring this valuable resource will remain available to historians.

The scanning and OCR conversion of the paper copy of the dissertation was carried out by Jim Giglio of National Capital Area Skeptics (NCAS). Most of the HTML coding was performed by John Stepkowski of Sign Historical Group (SHG), the remainder by Jim Giglio. Proofreading the finished product was carried out by Candy Peterson, Tom Tulien, and John Stepkowski of SHG.

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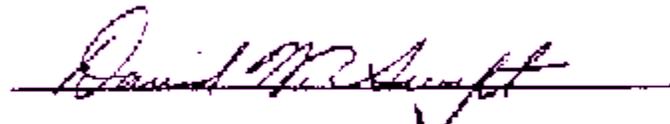
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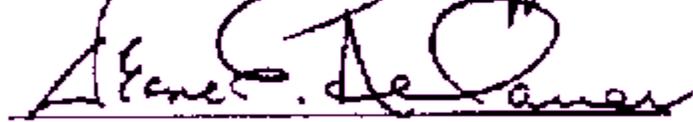
We certify that we have read this dissertation and that in our opinion it is satisfactory in scope and quality as a dissertation for the degree of Doctor of Philosophy in Political Science.

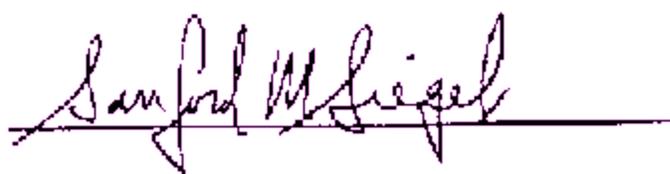
DISSERTATION COMMITTEE


Chairman









PREFACE

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ORIGINS OF THE STUDY

The origins of most research projects and the accompanying predispositions of the researcher(s) are seldom disclosed in the final report. However, in this instance I believe a discussion of the above matters, as well as the substantive UFO question, is in keeping with the spirit of the research in general and deserves inclusion. Moreover, it will aid the reader in two important respects.

First, the reader will naturally wonder how I became involved with the subject of Unidentified Flying Objects and, in particular, the chronicling of five years in the career of a scientist who spent this period of his life on what most members of the scientific community would consider a nonsense subject. Secondly, although my bias will be self-evident in the fashion in which I present my material, I believe it is appropriate to indicate the evolution of my beliefs about the UFO question and why I hold them; this will in turn benefit the reader in his attempt to evaluate the work as a whole.

As the title of the dissertation suggests, the research focuses on the politics of science. Not the usual politics of science (usual in the sense of what is written about) which generally speaks to questions of how government interacts with the scientific community, but rather what I will call the "personal politics of science." This consists primarily of how scientists interact with one another, and an explication of the personal strategies they adopt in the pursuance of their science-related goals.

The research did not begin with this objective. It started in early 1971 when I decided that I would do my dissertation on the only

topic for which I had a burning substantive interest -- Unidentified Flying Objects. In the jargon of the field of ufology I was a believer. By this I mean I felt that the Earth was experiencing extraterrestrial visitation. The question of what political framework would fit my subject matter, or indeed, what my subject matter really consisted of, did not disturb me. Everything is political, or historical or psychological, etc., depending on one's field of study. It was only a question, I felt, of getting the proper handle on the data. And so I proceeded.

The first handle I attempted to grasp was the conspiracy hypothesis. Based on reading twenty or so volumes on the UFO problem I concluded the government and/or the Air Force intentionally covered-up the fact that the Earth was experiencing extraterrestrial visitation by what appeared to be intelligently controlled space vehicles. I spent six months reading the UFO literature in an effort to glean conspiracy information from it. During this initiation into the field a number of things became apparent. First, as with any borderland field of inquiry, I found books and articles difficult to acquire. The University of Hawaii Library collection, for instance, is limited, as is that of every other library in the country. I obtained most of my material on interlibrary loan. Moreover, there are no UFO journals that, for lack of a better term, could be called "professional," i.e., exhibiting the standards of scholarship expected in an academic journal. Secondly, the quality of the scholarship in the field is highly variable. This is something I cannot easily demonstrate, and will not take the time to try, but let it suffice to say that when one reads about trips that an author took to Venus, how Martians engineered the Northeast power

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blackout of 1965 and that UFOs are spaceships returning to the long lost continent of Atlantis it takes no great acumen to realize that the ground on which one is attempting to build needs shoring up. Yet this proved to be only the beginning of a problem that had a way of coming full circle. Initially I thought I could believe anyone who appeared articulate, well-schooled and sincere. However, as I continued to read I came upon contradictions between and within camps of UFO authors. This proved disquieting and led me via this or that circuitous psychological predispositional route to accept some and reject others. Not being either sufficiently perspicacious to see beyond my own set of assumptions or to kick one of them out from under my perceptual filters I decided to restrict myself to what seemed to be, given the literature, a conservative stance. Namely, that UFOs were extraterrestrial in nature and the government and/or the Air Force conspired to keep this from the public.

INITIAL STRATEGY

My strategy became one intended to circumvent all of the "suspect" literature. At this point, the summer of 1971, it seemed that a circumstantial evidence approach to the conspiracy hypothesis, largely using indirect government documentation, might be the answer. To make a long story short, I developed a design, the output from which suggested several alternative interpretations. My committee recognized this and subsequently I dropped the conspiracy hypothesis from my research plans, although not from the back of my mind.

Continuing my search for the doable my committee chairman offered the suggestion that I might pursue government policy toward the UFO question since it was political and probably could be nailed down. This appeared to be a worthwhile idea because at the time I wanted to

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develop a proposal which made the UFO problem a question of foreign policy -- they weren't from here were they? Of course, this conception depended for its success on the acceptance by my committee that the UFO problem was what I said it was, namely extraterrestrial visitation, and that this neglected aspect of the American Foreign Policy literature deserved study.

At this time the Aerial Phenomena Research Organization (**APRO**) ^[1] of Tucson, Arizona, announced its first academic UFO symposium to be held in November of 1971 at the University of Arizona. APRO or fifteen academics were scheduled to speak and it seemed a fine opportunity to determine what the "Invisible College" did. ^[2] So on November 19, 1971 off I went with vague notions of government policy/conspiracy floating in my head and high hopes of learning about the inside knowledge developed by the participants in the symposium.

As it turned out the symposium speakers did not know much more about UFOs than I did. Most tried to focus their field of expertise on the subject, but the efforts, even then, seemed primitive to me at best. However I did meet Coral and Jim Lorenzen who formed APRO in 1952 and continue to keep it going. And most fortunately I met Richard Greenwell, a British resident alien by way of Peru, the assistant director of APRO. Being of the same age and having similar temperaments and interests enabled us to quickly become friends. This proved most propitious because the Lorenzens were older, more distant, and consequently more difficult to talk to.

One of the reasons I came to Tucson was to look at the APRO files which I thought might help me get a feel for the government and the Air Force policy. I also hoped to acquire copies of some of the

rare Air Force reports I expected APRO to have. I did that and more. The volume of available periodical literature I found overwhelming. I examined more material in three days than in the previous eight months. When I finished Greenwell asked me if I would like to go with him on his weekly trip to straighten up the files of the late Dr. James E. McDonald. I casually agreed. I had heard of McDonald, a noted atmospheric physicist at the University of Arizona who did UFO work, but I did not want to see his files. As far as I knew he never wrote anything relating to government policy.

THE MCDONALD PAPERS

Once I began perusing his papers, however, it became obvious they were a significant find. Unfortunately I didn't know quite what to do with it. My government policy notions made me select some material, while the politics of science perspective began creeping in and influencing my choice of other data. I half-heartedly changed research designs in midstream. I Xeroxed about one-thousand pages of McDonald correspondence, but remained uncertain what I would do with it.

After sixteen days in Tucson I went on to Washington, D.C., where I spent two weeks at the National Investigation Committee on Aerial Phenomena (NICAP).^[3] There Stuart Nixon, the Executive Secretary, opened his files to me also. Again it was an unbelievable experience. The inside of the UFO world seemed a fascinating mix of plot and counterplot; the politics of science again.

I returned to Hawaii full of enthusiasm with my two-thousand or so Xeroxed pages. The politics of science appeared to be the most intriguing aspect of the UFO question I could pursue, yet I didn't feel I obtained all the material I needed to examine the UFO controversy as a whole. I thought if McDonald did so much, surely other scientists

made similar contributions. My new goal became another trip. In my naivete I assumed all (or at least most) of the scientists in the controversy would like to get their points of view represented in my dissertation.

The trip considerably influenced the credibility I imputed to various investigators and

hypotheses. For instance, there is really no way to convey how impressed I became with McDonald. If there were some doubts in my mind about the extraterrestrial hypothesis (ETH), after going through his files they were alleviated. As a respected atmospheric physicist who examined the problem for almost five years, who was esteemed in his own field, sat on NAS panels, and spoke to Navy, Air Force, NASA and aerospace groups he seemed to personify credibility.

A further benefit which accrued to me as a result of going through the files at APRO and NICAP was the opportunity to observe the style in which many of the authors I previously read, communicated, both without an intervening editor and when they thought their true views would not lose them readers. For me this proved a useful exercise. When I returned to Hawaii I felt like an insider. However, I also was aware of the fact that in terms of overall knowledge of the scientific controversy surrounding the UFO phenomenon my understanding left much to be desired.

PROPOSAL TO NSF

Therefore, 1972 became the year to write a proposal to finance a second trip. The task became formidable, both because of the political nature of my topic, namely, how scientists interact over a potentially anomalous phenomenon, and because of my total lack of experience in proposal-writing. I felt that I had to disguise what I was doing because the scientist readers at the National Science Foundation would

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not want to admit science is political and by implication the NSF and its funding policies. Yet, at the same time I needed to score enough points to make my work look worthwhile in social science terms.

During the proposal-writing period I began making plans for my prospective trip. This meant writing those scientists and laymen who I thought could be of assistance. The result proved mixed, but further honed my critical faculties. My expectations were that a letter, possibly two, would enable the making of introductions, the laying out of my research design and the agreement, or lack of it, on the part of the correspondent. This is, in fact, the manner in which it occurred, except with Donald Menzel, an astronomer at Harvard, and Philip Klass, Senior Avionics Editor of *Aviation Week and Space Technology Magazine* -- the two foremost anti-UFO authors. Both were very cordial at first, but by the second letter Menzel became upset.

Initially he wanted to obtain funding for my work, but when he better understood my intentions via my second missive, I suddenly became a "cultist" and "incapable of thinking logically." However, the important point about these exchanges and those which followed over the next year is that they confronted me with the other extreme and forced me to reconsider the positions which only months before appeared so tenable. Suddenly the ground under my feet was shifting again. It wasn't that Menzel and Klass were necessarily correct in what they said, it was that I could not demonstrate that they were incorrect. In addition, the communications exposed me to a perceptual distortion which I had never before experienced. Rereading their letters in an attempt to make sense out of some paragraphs which at the time seemed random, it finally occurred to me that because our assumptions were so divergent and most of the time

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left unstated, that we often spoke totally by one another, we were in different worlds. As a result, we each filled in the blanks in such a way as to make meaningful communication virtually impossible. At any rate, as the eternal verities of my UFO world began to crumble and the demands of proposal-writing increased, I removed myself further and further from the disconcerting spaceship aspects of the UFO problem and became immersed in the procedural questions of the politics of science of the issue. Also I started to think about the psychological makeup of extremists, such as Menzel and myself, and why we do what we do and think what we think.

In my NSF proposal I tried to be neutral, jargonish and in good form, but I could not manage. In looking back on it the proposal contained numerous methodological problems. However, above and beyond that, my framework for analysis (from DeGrazia's book on Velikovsky), my orientation (only poorly disguised), the case study material, and lastly the personal politics of science label probably made the proposal one of those least likely to succeed. In May of 1973 I received formal word of the rejection.

However, my plans were already taking shape and I felt I could not turn back at that point. So I took out a student loan and made the trip anyway. It required ten weeks of travel and proved most productive. Nevertheless, I did encounter a few setbacks.

INITIAL RESEARCH ORIENTATION

When the trip began I conceived of the project as one in which I would try to present both sides of the UFO controversy in as unbiased a manner as I could. To me it appeared that

extraterrestrial visitation might be occurring, but my ardor and conviction for that position waned over the previous year. My ostensible orientation was towards

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determining if scientific due process had been accorded the UFO phenomenon. Of course, I felt scientific due process was an ideal type which neither side in the controversy would live up to and so I would use it as a straw man to give me an opportunity to investigate and chronicle the activities of the scientists who studied UFOs. In short, to tell a political story with a modicum of framework and social science jargon.

However, I found after a short time on the road that the negative position would be difficult to obtain. First of all, only a few people represent that position. By this I mean that they write about and/or investigate UFOs and debunk the subject. Dr. Donald Menzel, formerly head of the Harvard Observatory is one of those men. He gave me access to his papers, which contained a wealth of information, at the American Philosophical Society Library in Philadelphia. I asked that some eight-hundred pages be Xeroxed by the library and sent on to me in Hawaii. What I did not realize, and was not told until later, was that I only had note-taking privileges. When the Librarian contacted Menzel about the Xeroxing, he refused permission. Why did he do so? As he put it, he felt I intended to portray him as the "arch demon of saucerdom" and also mentioned something about Xeroxing tending to reduce the value of the collection in Philadelphia. He drew the arch demon conclusion for a number of reasons. First, in my previous trip I visited both NICAP and APRO, but not him. He felt both organizations consisted of kooks, crackpots and cultists. Secondly, for a year-and-one-half prior to my second trip we corresponded and disagreed on many issues related to the UFO question and research methodology. I never took a positive position on the ETH, but he always attacked me as if I did.

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I played devil's advocate and countered some highly insulting letters with equally scathing retorts. I am afraid this proved my undoing. As I look back in dismay and embarrassment, I don't believe I can begin to conceive of, much less convey to the reader, what the former head of the Harvard Observatory and paragon of twentieth century astrophysics must have felt when he read counter-argumentation from an upstart graduate student -- in the social sciences at that. Thirdly, immediately after perusing Menzel's papers, I visited Philip Klass in Washington, D.C. Klass and I were at loggerheads almost from the moment we met. I came to get data, but he insisted on drawing me into arguments to convert me to his position. Consequently, my three-day visit with

him was difficult. I attempted to avoid offending him, but lost my temper several times and almost lost the data I came for. As it was I think I acquired only the tip of the iceberg. But more importantly I believe he informed Menzel, who I had not met, that I could not be trusted. Subsequently, Menzel indicated that I could have the APS material if I could demonstrate that "I am a fair and objective researcher" -- a concept I don't believe in and which he knew we discussed in previous correspondence.

It is very easy to fall into the perceptual trap which Menzel used to filter my position. Namely that I want to settle the UFO controversy, that is, speak to the reality of the UFO data, and assess blame to those members of the scientific community who acted improperly. This is not the case. Of course, there is good reason for Menzel to believe this, for at one time, in my zeal, I did intend to do so. This undoubtedly was indirectly communicated in our correspondence, although I tried to downplay the issue. In addition, Menzel did receive a copy of my NSF

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proposal which I intended to be a neutral document, but which I suspect was not construed as such by the disinterested observer and, therefore, to Menzel it must have appeared a polemic.

Besides Menzel two other people who might have provided insight into the negative position refused to see me or permit me to examine their files. Most importantly Dr. Edward Condon (recently deceased), who had been the principal investigator in the Air Force sponsored 1966-68 University of Colorado UFO study. At the time his refusal was understandable since he underwent heart surgery only two months prior to my visit. However, over the phone he made it clear that the UFO phenomenon was a crackpot subject and I should give up my research.

Robert Low, the administrator of the Condon Project, was of the same opinion. In his first letter to me he indicated that my dissertation did not merit the approval of my department and that he would not help me. In my second letter to him I pointed out that if he thought my proposal would not be accepted, he labored under a misconception because acceptance had already taken place. Moreover, I explained that a number of academics had agreed to cooperate and were enthused by the idea. I also let it be known that Low's name had come up many times, in the course of corresponding with other participants in the controversy, but I hoped to hear his role described as only he could recount it.

Low's reply was not what I expected. He said there was nothing befitting a Ph.D. thesis in the physical or social sciences in the UFO phenomenon and that acceptance on the part of my department reflected adversely on its members. He said I could learn all he could tell me in the Condon Report and my research was "a waste of time."

My surmise is that both Condon and Low suspected my work would turn out to be something similar to the volume written by David Saunders ^[4] whom Condon fired from the Study and who later wrote an expose of the project's inner workings.

Of course, it would be unfair of me to suggest that only the anti-UFO scientists were uncooperative. For instance, Norm Levine, also fired from the Condon Project, did not respond to my letters. When in Boulder, primarily to see Saunders and Condon, I decided to call Mary Lou Armstrong, Condon's administrative assistant, who resigned in vehement protest over the handling of the project in general, and the firing of Saunders and Levine in particular. She too refused to see me, referring me to Saunders. Also Jacques Vallee, presently at Stanford's Institute for the Study of the Future, and one of the foremost contributors to the field of ufology, refused to respond to my letters. In a round-about, fortuitous fashion I managed eventually, arriving unannounced, to spend two informative hours with him, but he said he had no data to contribute. Lastly, James Harder of the Berkeley Engineering Department was another potentially good source who showed no interest in becoming a contributor to, and therefore an actor in, my dissertation.

In defense of everyone cited, both pro and con, I can understand their various desires for anonymity, to avoid being quoted out of context and just generally to keep an outsider from reading their mail. Yet, let's call these decisions by their proper names. They are political decisions, i.e., to insure tenure, escape controversy, retain reputations, keep some "so-called" extrascientific behaviors secret, obviate the possibility of inadvertently contributing material to the "wrong side" of the controversy, and to evade any responsibility for furnishing data to the personal politics of science literature.

RESEARCH ORIENTATION RECONSIDERED

Nevertheless, a positive outgrowth of this experience is that it forced me to move away from consideration of my scientific due process orientation, i.e., did the UFO phenomenon receive scientific due process, which required a strong two-sided presentation toward a conception of the scientific process as a political process. The political process phenomenon can be amply demonstrated from one side of the controversy alone, or as in this case, from the standpoint of the correspondence constituting one scientist's communication net, and a bit of associated

background and foreground information. In a sense, although the delimitation of my personal substantive goal of learning more about the politics of science of the total UFO controversy is involuntary, I have been forced to look beyond the "them" and "us" perspective that permeated my earlier thinking and am now in a better position to view all the actors in the controversy as "them." This is crucial because altering the research orientation means that a greater emphasis is placed on the political maneuverings of the participants in the controversy and none on which group is substantively or procedurally correct. Probably none of the protagonists desires to be portrayed as political in his scientific dealings; however, if this is true, then I have robbed Peter to pay Paul by my exchange of assumptions, but it is an exchange most political scientists should find tolerable.

Yet this does not mean my substantive UFO bias will not be a factor. I cannot help but filter my data through it, but at the intellectual level I am at least aware of this problem and to some degree am able to rectify it and alert the reader. Now may be an appropriate point to explain the nature of this bias.

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I believe there are five ways in which one can reach a conclusion on the substantive aspects of the UFO question. First, one can hear about someone else's UFO sighting. Second, one can read about sightings and investigations performed by others. Third, one can make use of other investigators case reports to reach a conclusion. Fourth, one can go out into the field and do case investigations. Fifth, one can make his own sighting. Each of these approaches, or any combination of them, is subject to bias, but as one moves along the scale from one toward five I think that the credence that can be placed in one's own belief increases. (It should be noted that five demonstrates my bias that one phenomenon, a real UFO so to speak, exists. A skeptic would be quick to jump on the fact that UFOs consist of many kinds of natural and man-made phenomena which could not justify a generalization from a single case.) Most individuals opt for numbers one and two. A few opt for three and four. And we don't know how many have had five imposed upon them.

My research does not demand that I resolve this substantive UFO issue, but it is in the nature of a subject such as this that one does not get involved unless one has an axe to grind. My experience has been with people in all five of the above categories. Each group contains individuals of impressive intellectual and technical stature clinging to either a pro or con position for reasons he considers valid. Not being a case investigator and not having made a sighting my position is a function of the faith I put in the credibility and expertise of the witnesses and investigators who I have spoken with and read about. At this point in time it is my belief that the person who does

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investigate cases and has not had an alleged UFO sighting is taking a strong emotional position on whatever side of the issue he stands. This is also true, but to a lesser degree, of approaches four and five, given the nature of interviewer bias, the lack of intersubjectively verifiable evidence and the complexity of perceptual distortion.

Consequently, because I have not investigated cases nor made a sighting, my belief that people are observing anomalous data is very much a function of my psychological predisposition to believe one group of investigators over another. Not necessarily because they are correct, but because I would like to think they are correct. Naturally I believe that my analysis of their analyses has produced the truth. That is, which group is correct. However, at a meta-level of analysis, I know that psychological predisposition is playing a larger role in my conclusion than is intersubjectively verifiable evidence. So, just as the investigator musters as much evidence as he can to make his pro or con case vis-a-vis the UFO data, I do the identical thing with respect to the investigators.

Although I do not intend to speak to the question of the reality of UFOs in the course of this dissertation, the present explication should better enable the reader to understand the perceptual filter through which I view my subject. This is by way of saying: reader beware. I don't accept the extraterrestrial hypothesis without reservation, but my bias is in that direction. Therefore, this bias may subtly influence the manner in which the data in this study is arrayed. It may make one group of scientists appear to be "good guys" and the other group not so good. Forewarned is forearmed.

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FOOTNOTES

1. The oldest American UFO organization. It has a membership, publishes a bulletin and investigates sightings through its worldwide field investigator network. - [Back To Text](#)
2. A term coined by J. Allen Hynek, Department of Astronomy, North western University, to describe those academics who pursue the UFO subject in their spare time, but shun publicity and do not publish in the area, although prepared to "come out" if the subject area should become legitimate. - [Back To Text](#)
3. NICAP was formed in 1956. It is similar to APRO although since its inception it has been better funded and staffed. Moreover, it has vigorously attacked the Air Force, fostered the conspiracy

- hypothesis and worked for Congressional hearings on the UFO problem. - [Back To Text](#)
4. Saunders, *UFOs; YES*. New York: New American Library, 1968. - [Back To Text](#)

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OBITUARY OF JAMES E. MCDONALD

(*Bulletin American Meteorological Society*, 1971, p 735.)

James Edward McDonald
1920-1971



James E. McDonald, professor of atmospheric sciences at the Institute of Atmospheric Physics at the University of Arizona and AMS Fellow, died on 13 June 1971. Dr. McDonald was born on 7 May 1920 in Duluth, Minn. He earned the B.A. degree at the University of Omaha in 1942, the M.S. degree at Massachusetts Institute of Technology in 1945 and the Doctor of Philosophy degree at Iowa State University in 1951. During World War II he served in the Navy as an aerologist, was a meteorology instructor at MIT, and reached the rank of Lieutenant Junior Grade. Dr. McDonald spent 1953 at the University of Chicago before joining the staff of the Institute of Atmospheric Physics in 1954 as the associate director. He later relinquished this post to devote all his time to teaching and research.

Dr. McDonald was a scientist and a person of most unusual qualities. He was a voracious reader and had an encyclopedic memory. There were few scientific subjects which escaped at least partial scrutiny and in many areas, his knowledge made him an authority. Anyone who encountered Dr. McDonald in his pursuit of information will surely recall his relentless pursuit of knowledge and understanding.

Dr. McDonald's contributions to atmospheric sciences extended far beyond the contents of his published papers. He was a brilliant teacher and lecturer. His courses in cloud and precipitation physics were outstanding in content, challenging in presentation, successful in exciting interest and curiosity, sometimes sprinkled with sarcasm and often lightened with humor. In his lectures,

he invariably impressed audiences with his attention to details and his extensive documentation.

One of Dr. McDonald's early important papers published in 1954 dealt with the shape of raindrops (**JOURNAL OF METEOROLOGY**). During the Fifties and Sixties he published a series of articles dealing with problems of cloud physics, particularly nucleation and other aspects of physical meteorology. The subject of radiation and atmospheric optics was one of his favorites. Of particular note are his works in the field of weather modification. In 1958 he wrote a lengthy article entitled "The physics of cloud modification" (*Advances in Geophysics*, Vol. 5) which serves as a standard reference on this subject.

Dr. McDonald had a major impact on the atmospheric sciences as a result of his service on the Panel on Weather and Climate Modification of the National Academy of Sciences. His work with this group tells much about the man. He joined it just after an early preliminary report gave a pessimistic view on the status of the subject. Dr. McDonald tended to share this view with the general community of atmospheric scientists. On the other hand, certain scientists, particularly those associated with operational cloud-seeding organizations, strongly objected to the negative tone of the report. Dr. McDonald agreed to carry out a careful examination of data and reports of a large number of cloud-seeding projects, particularly those conducted by commercial operators. As a result, he convinced himself and the Panel that there was reason for optimism about the efficacy of procedures for increasing precipitation by means of ice-nuclei seeding. This notion, expressed in the Panel's final report in 1966, had a marked effect on the progress of weather modification in the United States. Subsequent research has supported and extended the earlier optimistic views.

During the past year he played a crucial role on the Project Stormfury Advisory Panel as it sought to evaluate the results of experiments aimed at reducing the intensity of hurricanes.

A member of the AMS since 1944, Dr. McDonald served on the Publications Commission from 1958 to 1961 and during 1966. He participated in the Committee on Water Resources from 1961 to 1965 and the Committee on the History of the Atmospheric Sciences from 1967 to 1970.

Not surprisingly, Dr. McDonald had strong opinions on the problems of society and was often involved in public debates on such important questions as those dealing with civil rights and racial problems. Most recently, he was deeply committed to the analysis of the effects of SST emissions on the atmosphere. He spent hundreds of hours studying the relevant printed material and consulting with experts in many related disciplines. Having concluded that SST emissions could reduce ozone concentrations, increase ultraviolet radiation reaching the Earth and lead to increases in the frequency of skin cancer, he did his utmost to convince other scientists, laymen and politicians that the SST represented a danger to society.

Over the last few years, Dr. McDonald gave considerable attention to the analysis of reports of Unidentified Flying Objects. As was his custom, he approached the problem as a scientific one, he studied all available information and evidence, he interviewed hundreds of people and made detailed analyses of possible explanations. This investigation brought him into conflict with certain scientists and engineers. He maintained that the pursuit of satisfactory explanations for

UFO reports represented an important scientific problem which was not getting adequate attention. He found few allies in this scientific quest, and many of the relevant questions still remain unanswered.

Dr. McDonald was a man of integrity and great courage. He was loved and admired by a great many people, he was respected by everyone who stopped to listen, he made a lasting impact on many facets of the atmospheric sciences and society, and he will be missed much more than we now realize.

- Louis J. Battan

INTRODUCTION

The primary focus of this study is the personal politics of science which tends in this instance to cast light upon the reception of potentially new observational data in science, Thomas Kuhn's notion of paradigm shift, and the history of the UFO controversy itself. Moreover, through a decidedly different look at the scientific process than that which is traditionally put forth, an effort is made to demonstrate that the scientific process can be profitably regarded as a political process.

Ideally, a more complete reconstruction of the scientific controversy surrounding the UFO problem is the proper foundation on which to build. Unfortunately, some participants in the affair refused to, or could not for various reasons, cooperate. Consequently, this study is based on a subset of information, the core of which I believe is virtually complete. By this I mean that the nucleus of this research consists of the complete work, and correspondence where relevant, of the late Dr. James E. McDonald who was the most outspoken academic in the UFO debate from 1966-71. Concentrating on McDonald, for my purposes, is the wisest strategy to pursue, for it will permit me to examine those issues referred to above, with the knowledge that only a limited amount of material pertinent to the McDonald experience has escaped my scrutiny. Yet, this should not be misconstrued to mean that McDonald alone will receive attention; for in his efforts to resolve the UFO riddle McDonald interacted with virtually all the prominent names in what has come to be referred to as the Field of Ufology. As these interactions arise I will chronicle them. As a result the reader will

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obtain an understanding of the personalities, issues, and strategies which compose the context of the UFO controversy. More importantly, however, this will convey the politics of being a

scientific frontiersman. That is, attempting to make knowledge claims beyond those generally accepted by the scientific community.

The study is organized into seven chapters. In chapter one a framework for analysis is developed after reviewing some of the literature in the fields of Ufology, the politics of science, the sociology of science and the history of science. Within this framework the distinction is made between the scientific method and the scientific process; the former being a subset of the latter. Chapter two concentrates on McDonald's activities in 1966, the first year of his UFO involvement. He is followed intensively in order to capture the flavor of the highly political milieu in which he functioned and accordingly to make the case that the scientific process, at least in the potentially revolutionary instance, is a political process. Having demonstrated that, chapters three, four, five and six are less intensive, but are more substantively focused on single events as they highlight various aspects of the borderland science endeavor. In chapter three McDonald is observed meddling in the Air Force sponsored Condon Project at the University of Colorado, while in chapter four he engineers the 1968 UFO Symposium before the Science and Astronautics Committee of the U.S. House of Representatives. A few of the pitfalls of engaging a borderland subject are illustrated in chapter five by the battle which ensued over McDonald's spending of Office of Naval Research atmospheric physics research funds for UFO studies. This is followed by an examination of the 1969 American Association for the Advancement of

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Science (AAAS) UFO Symposium preparations in chapter six. Although McDonald did not play a role as a symposium organizer, the event itself -- getting established science to confront the UFO problem -- was the primary goal of his campaign. It is therefore instructive to view the confrontation. Lastly, chapter seven speaks to the concept of borderland science activity and an attempt is made to flesh out Kuhn's notion of a paradigm shift by discussing it in terms of a revolutionary political process.

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I. BACKGROUND AND FRAMEWORK

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PERSPECTIVE

In order to put McDonald's involvement in the UFO controversy in proper perspective it is necessary to view the controversy from its inception. Yet this itself is a difficult task because the matter is complex, convoluted and strewn with half truths and falsehoods; there is really no adequate way in which to adequately convey the flavor of the period from 1947-65 in an introductory chapter. ^[1] But it is possible to cover some of the important events and in the process attempt to communicate something of the atmosphere which pervaded the subject at the time McDonald began his work in 1966. It would appear that the best way to briefly go about this is to first present a discussion of the general nature of the phenomenon reported since about 1947 and then indicate the response to it by the Air Force, the scientific community and the layman.

THE PHENOMENON

Although there have been efforts to relate the post World War II UFO phenomenon to the foot-fighters (balls of light seen by fighter pilots) of World War II, the airship wave of 1897, Medieval accounts of things seen in the sky and the miraculous events recounted in the Bible, ^[2] for our purposes this kind of information is too esoteric and the links too tenuous for it to be relevant to setting the stage for McDonald's entrance. We can be content to engage the subject in 1947 when it first came to the attention of the American public as a result of the publicity afforded the sighting made by Kenneth Arnold as he flew his plane over Mt. Rainier, Washington, on June 24, 1947. Arnold allegedly saw eight disc-shaped objects flying at an estimated 1500 miles per

[[1]]

hour. The disc shape, somewhat similar to an inverted saucer on top of an upright saucer, soon became the prototype description as reports accumulated. Of course, many other shapes and variations on the saucer prototype, as well as the classic unidentified "light in the sky" have also been described.

J. Allen Hynek, Chairman of the Department of Astronomy at Northwestern University, has provided us with a typology of UFO reports which is useful for obtaining an initial grasp of the data. His typology consists of: nocturnal lights, daylight discs, radar-visual reports, close encounters of the first kind, close encounters of the second kind and close encounters of the third kind.

Nocturnal Lights are the most common UFO report and consist of unidentified lights in the night sky. As a class of reports they are numerically large, but evidentially not as significant as the other types. Hynek argues that: ^[3]

The typical Nocturnal Light is a bright light, generally not a point source, of indeterminate linear size and of varying color but most usually yellowish-orange, although no color of the spectrum has been consistently absent, which follows a path not ascribable to a balloon, aircraft, or other natural object and which often gives the appearance of intelligent action. The light gives no direct evidence of being attached to a solid body but presumably may be.

The Daylight Disc, on the other hand, provides much more information to the researcher. ^[4]

The object (often objects in pairs) is variously described as oval, disc-shaped, "a stunted dill pickle," and ellipsoid. It generally is shiny or glowing (but almost never described as having distinct point source lights), yellowish, white or metallic. It exhibits in most cases what we would anthropomorphically describe as "purposeful" directed motion, with the ability to accelerate extremely rapidly. No loud sound or roars seem to be associated with Daylight Discs; sometimes there is a faint swishing sound.

[[2]]

As we move through the typology the evidence claims UFO researchers are prepared to make for their data increases. The Radar-Visual report consists of an observation made both with instrumentation and visually by one or more witnesses. ^[5]

. . . it can be said that the radar operator observes a blip on his screen that, he avers, is definite, is akin to the type of blip given by a large aircraft, is not the result of malfunction, and does not resemble "weather phenomena." A visual sighting is characteristically a light, or possibly a formation of lights strikingly unfamiliar to the observer, with generally only a suggestion, if that,

of an object dimly outlined by the brightness of the lights. The speeds involved are invariably high, but combinations of high speeds at one time and hovering at another are not uncommon. Reversals of motion and sharp turns, not abrupt 90-degree turns, are characteristic of Radar Visual cases.

The Close Encounter cases are the most impressive from the viewpoint of the researcher. A brilliant light or object is seen within 500 feet, sometimes much closer, to the observer, although in Close Encounters of the First Kind there is no interaction between the percipient and the stimulus. Hynek describes a prototype thusly: ^[6]

Brilliant luminescence, relatively small size (of the order of tens rather than hundreds of feet), generally oval shape--sometimes capped with a dome-absence of conventional wings, wheels or other protuberances, and ability to hover and to accelerate very rapidly to high speeds characterize the UFO at close encounter.

The Close Encounter of the Second Kind does not differ from the Close Encounter of the First Kind except that the stimulus interacts with the environment in some way and leaves physical effects. Hynek States: ^[7]

The physical effects reportedly include tangible marks on the ground that can remain in evidence for days or even months and come ostensibly from physical contact of the craft with the ground, the scorching or blighting of growing things (particularly plants and trees), discomfort to animals as evidenced by their behavior, and such physical

[[3]]

effects on the human observer as temporary paralysis, numbness, a feeling of heat, and other discomfort. Interference with the local gravitational field is sometimes also reported, as evidenced by the reports of some observers of temporary feelings of weightlessness or other inertial effects, as though the well-known laws of inertia had been temporarily abrogated.

One remarkable reported physical effect involves interference in electrical circuits, causing car engines to cease functioning temporarily, radios to cut out or to exhibit uncommon static, car headlights to dim or be extinguished for a short while, and, on occasion, car batteries to overheat and deteriorate rapidly.

The Close Encounter of the Third Kind is even more bizarre, for it involves the reporting of the presence of an animated creature as part of the UFO observation. Hynek does not provide much of a prototype, he says: ^[8]

They differ from other close encounter cases only by definition, by the reported presence of

occupants, (in or about the craft) and by the fact that these encounters are not as frequently reported by highly trained and sophisticated people as are other close encounters.

Up to this point the qualitative aspects of the phenomenon have received attention. Let's now turn to the quantitative. Below is a table indicating the number of sightings of all types reported to the Air Force from 1947-65. These reports are almost exclusively domestic, but a few from American bases abroad are also included ([see Table I](#)). This does not mean the phenomenon itself is primarily domestic, on the contrary it is global in its scope. However, international communication about the problem and knowledge of foreign research itself is so limited that it is wise at this point to treat the American experience in isolation. Suffice it to say that reports of the same sort of data exist throughout the world.

Although Hynek's typology encompasses that aspect of the UFO phenomenon which he believes represents legitimate data, there is

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TABLE I
ANNUAL UFO REPORTS 1947-1965

Years	Reports
1947	122
48	156
49	186
50	210
51	169
52	1501
53	509
54	487

55	545
56	670
57	1006
58	627
59	390
60	557
61	571
62	474
63	389
64	562
65	887

SOURCE: *Project Blue Book,*
Project Blue Book Information Office,
SAFOI, Washington, D.C.,
August 1, 1967, p. 7.

[[5]]

another component to the problem which most investigators conceive of as illegitimate, but because it is so bizarre it has received more than its share of attention and consequently, according to many UFO researchers, has had a disproportionate and unfavorable effect on the study of the UFO problem. It is the contactee issue.

THE CONTACTEE ISSUE

Hynek spoke of occupants in his prototype of the Close Encounter of the Third Kind. However, he was unwilling to entertain contactee reports. In the lexicon of Ufology the former consists of observations of, but not interactions with, alleged UFO occupants, while the latter consists of definite interaction with the occupants, often of a religious or semi-

religious nature and often including claims of trips to other planets and/or assertions that the observers themselves are from other planets. The circus-type atmosphere which these reports created flourished in the 1950s and continues to exist to some degree. A few of the better known of the contactees were George Adamski, Truman Bethurum, Daniel Fry, Orfeo Angelucci, Howard Menger and Gabriel Green. They all wrote books and/or had followings based upon the extraterrestrial messages of doom or salvation which they proffered. ^[9] They attracted a great deal of attention and regardless of the merits of the data enumerated by Hynek, data which serious UFO investigators claimed was, and is, anomalous, the contactee cults distracted interest from those data and increased the probability that the scientific community would view the UFO question as a nonsense problem and not submit it to rigorous scrutiny.

[[6]]

THE AIR FORCE RESPONSE

Soon after the initial American UFO observations in June 1947 the Air Force as the defender of American air space took the responsibility to look into the problem and determine whether these events jeopardized the national security of the United States. Because earlier observations made in 1946 were over Sweden the fear existed that they were actually tests of Russian missiles which could, if they had intercontinental range, change the complexion of the embryonic cold war.

The Air Force maintained the sole official responsibility for explaining UFO data between 1947 and 1969. Volumes could be written on this subject alone. However, such detail is not necessary for the purposes of this study. What is important is that the Air Force staffed a small investigatory project which from 1947-49 was called **Project Sign**, from 1949-51 was named **Project Grudge** and from 1952-69 bore the code name **Project Blue Book**. The reports and press releases of each of the projects assured the public that the sightings were not a threat to the national security and could be explained as misidentifications of stars, planets and man-made objects, or were natural atmospheric phenomena, or hoaxes. ^[10] The Air Force adhered to this public position for 22 years until the closing of Blue Book in 1969.

The Air Force made its pronouncements against a background of claims that it did so after careful investigation of sightings with the aid of some of America's finest scientific talent. The implication for members of the scientific community became obvious. The Air Force, through the use of the ample scientific muscle at its disposal, could readily explain the UFO sightings which perplexed the untrained layman.

THE ACADEMIC RESPONSE

To the practicing scientist this meant that further work on the problem was unnecessary. This attitude received further reinforcement in the 1950s by the only academically produced treatise on the subject. A noted Harvard astronomer, Donald Menzel, produced a volume entitled *Flying Saucers*.^[11] Menzel pursued his UFO work very differently from the way in which he did his astronomy. Rather than trying to explain sightings by investigating them, i.e., interrogating witnesses, looking for possible causes for each event, etc., he authored a book containing examples of the various ways in which the atmosphere could produce a shining sphere-like or elliptical-shaped phenomenon which could fool the untrained observer. In so doing he produced the equivalent of an atmospheric optics text for the layman. However, he never confronted the data; he only offered possible explanations while assuming that UFOs were a nonsense problem. Nevertheless, his book became the definitive academic work on the subject during the 1950s and as such carried considerable weight among scientists.

In 1963 he followed it with a second book entitled *The World of Flying Saucers* written with Lyle Boyd.^[12] He drafted it in the same vein, treating the entire UFO phenomenon as preposterous pseudoscience. However, Menzel did get closer to the data, not through personal investigation of cases, but by gaining access to and explaining away official Air Force UFO reports from the Project Blue Book files. This book further ensconced Menzel as the definitive academic word on UFOs.

Consequently, prior to 1966 academics did very little to elucidate the UFO problem. Two UFO groups, the National Investigations Committee on Aerial Phenomena (NICAP) and the Aerial Phenomena Research Organization

(APRO) used some scientists and engineers as investigators, but no one made a concerted effort to examine the data. Two possible exceptions to this generalization, however, were J. Allen Hynek, at Northwestern University, and his protege Jacques Vallee. Because Hynek began his Air Force consulting with Project Sign in 1948 and maintained that relationship through 1969 he could claim closer ties to the UFO phenomenon than any other scientist. Yet, regardless of what he believed about the significance of the observations during those years. It was not until 1966 that he began publicly to take the position that the data deserved

closer scrutiny. On the other hand, Jacques Vallee while ostensibly Hynek's protege wrote considerably more about the UFO phenomenon. In 1965 he published *Anatomy of a Phenomenon* ^[13] and in 1966 with his wife Janine he penned *Challenge to Science; the UFO Enigma*. ^[14] In both volumes he took a more systematic look at the UFO problem than had been previously attempted and tried to interest the scientific community in looking at the data. Nevertheless, for all practical purposes there was no UFO research taking place in the academic community prior to 1966.

THE PUBLIC RESPONSE

The response which the phenomenon evoked from the public proved detrimental to scientific activity in the short run, but if the data should prove anomalous, will be very important to the scientific response in the long run. This is because the public in the form of various authors, UFO groups and charlatans initially helped to frighten off rigorously trained investigators, yet these same individuals kept the UFO question alive, chronicled the data, and overcame Air Force efforts to inter the subject.

[[9]]

Former Marine Major Donald Keyhoe began it all in 1949 and developed into the most influential layman in the area. He published an article in *True Magazine* for December 1949 in which he claimed there was more to the UFO problem than the Air Force claimed and hypothesized an extraterrestrial explanation. ^[15] Thus Keyhoe gave extensive exposure to both the extraterrestrial and conspiracy hypotheses. He elaborated on these ideas in books which appeared in 1950, ^[16] 1953, ^[17] 1955, ^[18] and 1960. ^[19] Each one enjoyed good sales which helped make the UFO problem a topic of general conversation. Of course, Keyhoe was not alone. Scores of authors wrote UFO books in the period 1950-1965, but Keyhoe remained the most authoritative sounding as a result of his "Pentagon connections," and probably the most widely read.

Many privately funded UFO organizations were formed during this time, however, only two still survive. The Aerial Phenomena Research Organization (APRO) began in 1952 under the direction of Coral Lorenzen, while the National Investigation Committee on Aerial Phenomena (NICAP) formed in 1956 with Harrison Brown as its head. Both functioned primarily as repositories for UFO sighting reports while they attempted to obtain a hearing for the data before the scientific community. Each recruited individuals on an international basis to investigate sightings and forward the ensuing reports to national headquarters; for APRO Tucson, Arizona, and for NICAP Washington, D.C. The NICAP group reorganized under the leadership of Donald Keyhoe (1956-69), attacked the Air Force vigorously and

constantly lobbied on Capitol Hill for Congressional Hearings on the UFO question. On the other hand, the APRO leadership, recognizing the inadvisability of trying to attack the Air Force from its remote Tucson base of operations,

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concentrated instead on fostering an image of a research-oriented organization. In fact, neither group did much research other than that done by the field investigators. Investigators wrote up and filed reports running into the tens of thousands over the years, but due to a lack of funds the research endeavor never went much further. Consequently, the groups developed their data bases, published newsletters and made very few converts in the scientific community.

The contactees referred to above, along with various charlatans and hucksters, did much harm to the attempt to acquire scientific legitimacy for the UFO phenomenon. The relatively quiet, by and large non-sensational, efforts of NICAP and APRO took a back seat to the bizarre tales of those individuals only interested in making a fast dollar from the excitement generated by UFO reports. As a result as time passed the first thought that crossed people's minds when the subject of UFOs arose was "little green men." This was obviously the last thing on the minds of the few serious researchers in the field, but it is representative of the type of image that had to be altered if the scientific community was going to consider the subject a legitimate topic of inquiry.

Therefore, we can see that between 1947 and 1965 there were a number of factors working to keep UFO data from undergoing scientific scrutiny. The Air Force had consistently written the matter off for 18 years. With these assurances, as well as those from Donald Menzel of Harvard, most scientists found no need to delve further into the problem. Moreover, this conclusion was additionally reinforced by the conspiracy claims of Keyhoe and various contactee and charlatan assertions which most reasonable men viewed as fraudulent.

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THE PROBLEM

Briefly elucidating these salient aspects of pre-1966 UFO lore prepares us to embark on our case study in the strategy and practice of borderland science. I suspect what is observed in this instance is a slightly exaggerated version of the manner in which "science as usual" is

conducted. This exaggeration appears to have two causes. The first concerns the perquisites of the scientific profession which are at stake. That is, if UFO data achieved respectability the scientists responsible, particularly if the extraterrestrial hypothesis (ETH) were assumed, would never have to go begging for research funds again. The second cause for exaggeration involves the restructuring of world views necessary to accommodate the hypotheses generally advanced to explain UFO data by those who believe it has significant scientific and social import. In other words, the ramifications of accepting UFO sightings as new observational data are much greater than those customarily encountered by the scientist in his daily work. Viewed as a political problem it is concerned with how scientists interact when confronted with a potentially anomalous phenomenon and at the same time it raises the issues both of how knowledge grows and of who gets what, when, where and how in the scientific community?

The term "anomalous phenomenon " is used here for lack of a better one. Ordinarily an anomalous observation is one which does not fit into existing theories or frameworks of analysis. What is denoted here is a special case within this class. It is an observation which, according to Kuhn, is potentially a cause of a paradigmatic shift in whatever discipline it occurs. ^[20] To accommodate it, entire world views must be revised, it is not enough to "fudge a little" or extend already extant

[[12]]

conceptions of reality. These conceptions must undergo extensive revision or be shelved entirely. This distinction between the routine and the revolutionary discovery is made explicit by Blackwell. The routine discovery is made within the confines of previously established knowledge, while the revolutionary discovery is made outside these confines and results in a genuine upheaval of scientific thinking. ^[21] When the possibility of such an event is in the offing the politics of science associated with it tends to become more visible.

THE POLITICS OF SCIENCE

Political scientists generally conceive of the politics of science in terms of the relationships between government and science. The areas of principal emphasis have been the impact of government on science, the scientist as decision-maker, and the scientist and foreign policy. A few examples should serve to illustrate this.

A number of studies speak to the question of government impact on science. Price discusses the problems arising from the growth of the federally funded post World War II scientific

establishment, ^[22] while Reagan examines the funding policies of the federal government with respect to scientific research. ^[23] In the same area Knorr and Morgenstern address themselves to policy questions related to the management of military research and development in the United States. ^[24]

Several authors treat the scientist as a decision-maker. In this respect Wohlstetter considers the effect scientists have on decisions concerning national and international security ^[25] and Schilling explores President Truman's decision to pursue development of the H-Bomb. ^[26] Gilpin directs his inquiry to the dispute which developed in the

[[13]]

scientific community over the nuclear test ban treaty, ^[27] while the influence of scientists on policy-making in the executive branch is appraised by Schooler. ^[28]

With regard to foreign policy Schilling outlines the recent history of the scientist in the policy-making process ^[29] and Skolnikoff illustrates the importance of scientific input to the Department of State decision-making process. ^[30] Lastly, Nelson uses a study of the Pugwash Conference to indicate the increased sensitivity on the part of scientists to international politics. ^[31]

THE PERSONAL POLITICS OF SCIENCE

On the other hand, "the personal politics of science" encompasses the everyday interaction of scientists. This approach stresses the interpersonal strategies invoked by scientists in the pursuit of knowledge. It fleshes out the skeleton of scientific research to provide insights into the social processes underlying the formal outcomes which are found in scholarly journals and texts. In so doing this personal politics of science brings one to a better understanding of both the context of discovery and validation. Unlike the traditional politics of science it is concerned with the micro-analysis of the behavior of scientists. Although governmental and scientific institutions play a role, the primary actors in such analyses are the scientists themselves. The point of such endeavors is to break out of the time-honored myth which portrays the scientist as a disinterested observer who, with respect to his research, is neutral both in the laboratory and in the world.

[[14]]

For example, DeGrazia chronicles the treatment given Emmanuel Velikovsky and his work by the scientific community. He concentrates on the interactions of Velikovsky with his critics, the scientific journals, book publishers and other academics. ^[32] This personal approach to the politics of science is also utilized by Greenberg. He takes advantage of the knowledge he acquired as an assistant editor of *Science* to explicate the history of the politics of what he calls "pure science" in the United States. He places emphasis on the period just prior to World War II through 1965 in a discussion directed toward which scientists wanted what projects, where, and why. ^[33] In a similar (personal) vein Barber points out that while literature exists which explores political, technological, economic and religious resistance to new ideas in science, virtually none probes resistance to new discoveries on the part of scientists themselves. He goes on to provide examples of this phenomenon. ^[34]

THE GROWTH OF KNOWLEDGE

There is another stream of literature which converges on these same problems from the perspective of the sociology and history of science. It is concerned with the manner in which knowledge grows. ^[35] The building bloc notion has been generally accepted until quite recently. In essence this hypothesis suggests that knowledge grows progressively and incrementally, each new idea following logically from that which has gone before. However, this is not the only position in the literature. According to Kroeber it is the exhaustion of ideas in one area of research which leads to new problems or orientations and the concomitant growth of knowledge. ^[36] The identical outcome is produced by a series of

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"micro-revolutions" if the argument of Toulmin is accepted. He claims that old theories are not discarded in crisis periods, as Kuhn would have us believe, but rather changes are made in the basic assumptions. Later the "so-called" old theories are reintroduced or surface in other disciplines. ^[37]

The concept of growth accepted by Crane ^[38] and Ziman ^[39] is that of Kuhn ^[40] which was later reinterpreted by Masterman ^[41] and thus accepted by Kuhn. ^[42] Kuhn asserts that periods of science-as-usual, what he calls "normal science," are interspersed with periods of crisis and then revolution. Once a revolution occurs a paradigm develops in a discipline, or a part thereof, which attracts scientists to it and permits normal science to continue again. Crisis is precipitated by problems insoluble within the old paradigm. Revolution is the "breaking out" of the old paradigm and the formation of a new one in order to resolve crisis-

causing problems. The definition of paradigm is not clear, but Masterman presents three usages gleaned from Kuhn's work:

1. metaphysical, a world view
2. sociological, a universally recognized scientific achievement
3. an artifact or construct which provides tools for specific problem-solving

Beyond the range of all of these usages anomalies may occur which could lead to crisis within a discipline and eventual revolution. The growth of knowledge, then, is portrayed as a cyclical process.

An aspect of this process which the proponents of all of these hypotheses fail to entertain is the political component. Although Crane comes close, by citing the cognitive and social constituents of knowledge growth, ^[43] and Kuhn even uses the revolutionary analogy to describe his paradigm shift, ^[44] they nonetheless neglect the political.

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It is at that point where this research takes advantage of both the personal politics of science literature and the growth of knowledge hypotheses to suggest how scientific growth, of a revolutionary nature, occurs, not only at the abstract level of theory, but also at the real world level of the scientific arena. The Kuhnian model of growth seems potentially the most appropriate to the UFO phenomenon. It has an appeal regardless of which conception of paradigm one chooses to adopt. For it is a paradigm shift which is necessary, if the UFO phenomenon is truly anomalous, to transform the scientific climate of opinion toward the subject and foster interest in it. Either a change in world view, a critical experiment, or some application of new, or heretofore unused, instrumentation for observational purposes, is required to change the research picture vis-a-vis the subject. This is necessary because research cannot go forward as long as the subject is deemed illegitimate.

THE SCIENTIFIC PROCESS AS A POLITICAL PROCESS

To understand the efforts which have been made in this direction it is constructive to conceive of the scientific process as a political process. While this is heresy to the positivist, it is fundamental to bridging the gap between paradigms. As Kuhn points out there is always resistance to new ideas from the old guard. In some instances it is overcome, in others the younger

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generation of scientists is forced to wait for the older generation to die off before acceptance of the new ideas can occur. However, Kuhn does not give much consideration to the nature of the struggle to obtain acceptance. The struggle results from the incommensurability of the old and the new paradigm. Each carries with it a set of criteria for evidence claims and acceptable methods of validation. Unfortunately, the criteria and methods of the new paradigm may not be admissible under the standards of the old. Since the latter has proven itself many times in the past and its adherents, who are legion, have world views and careers securely anchored within it, resistance to the new paradigm develops. Yet, because the standard bearers, of each do not accept one another's basic assumptions about the world and/or how to do research they can only talk at, not with, one another; intersubjectively verifiable claims are not possible because the followers of each approach speak different languages. When such a situation exists the outcome becomes a matter of persuading those of the old school to accept the new. Kuhn is cognizant of this and points out that proponents of the new paradigm try to make their case by showing that their paradigm solves the problems which had previously caused a crisis, predicts further unexpected solutions to other problems and is more aesthetically pleasing than its predecessor. However, Kuhn does not see this as a political process and does not elaborate on it.

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In fact, he admits that the research which could throw light on the conversion question has not been done. I believe this attempt to gain acceptance can usefully be viewed as politicking, often in a revolutionary way, for purposes of securing an assenting constituency. These behaviors, and those related to them, constitute the personal politics of science of this study.

THE SCIENTIFIC PROCESS AND THE SCIENTIFIC METHOD

We are well under way to grasping this concept if we define the scientific process as any and all behaviors engaged in by scientists to further their science-related interests. This is a considerable embellishment of the concept of scientific method which is usually used interchangeably with scientific process. When traditionally used it consists of an objective scientist hypothesizing an outcome, doing an experiment, collecting the resulting data, interpreting the outcome and writing up the findings for publication. The result of the above

expanded definition, on the other hand, is to suggest that science consists of much more than what scientists do in their labs or report in books and journal articles. The scientific process viewed as a political process does not conceive of the scientist as neutral, but rather as an advocate. He has a position with respect to the problems he studies, possibly to a greater extent on controversial issues, and he desires to promulgate this position. Therefore, he engages in

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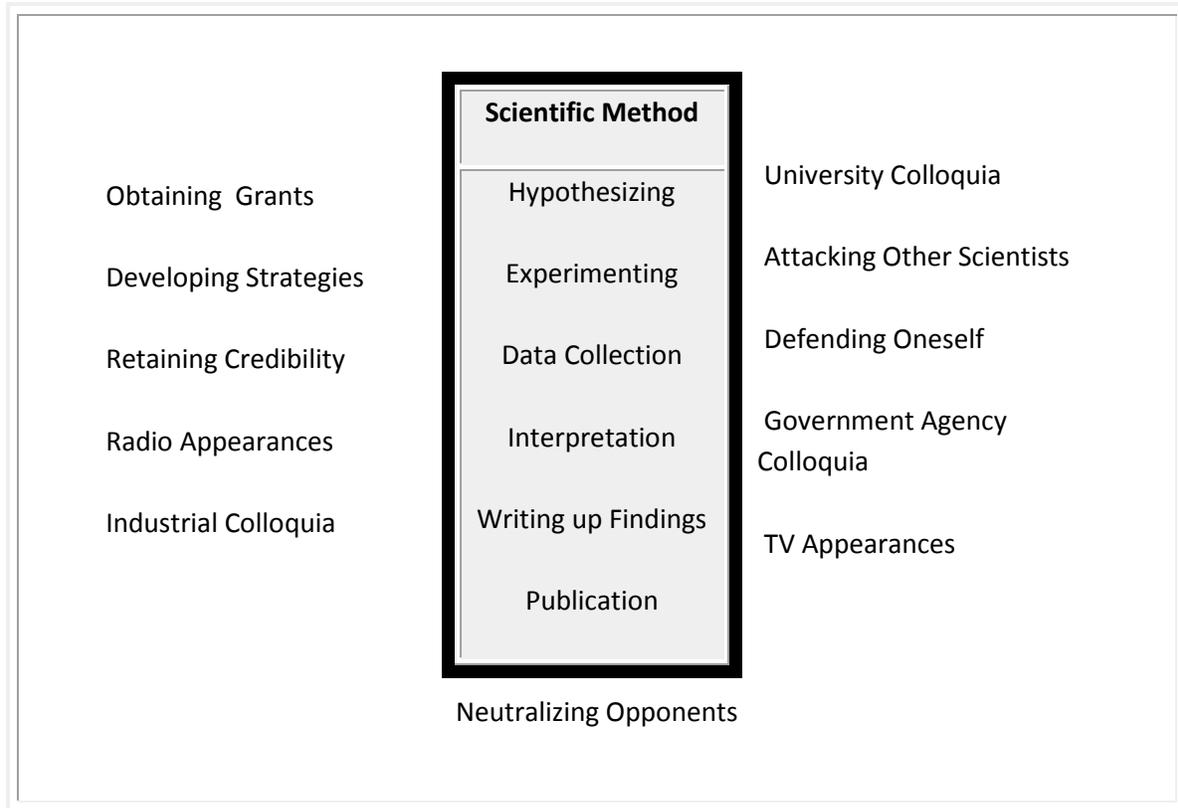
behaviors which he believes will foster his substantive findings, argumentation and other research ends. These behaviors are political and constitute the tactics of the strategy which he feels will best serve to resolve the issue which is at stake. This view, then, enlarges the repertoire of activities which comprise the scientific process and makes it possible to speak of the scientist as a political actor.

This orientation has certain distasteful aspects to some. For instance, it makes the behaviors associated with the scientific method, namely those enumerated above, a subset of the behaviors included in the scientific process. The figure which appears below serves to illustrate this. The schematic itself is the scientific process. It consists of the scientific method represented by the behaviors in the inner box and the associated political activities shown peripherally outside the inner box. The latter are not intended to be all inclusive, but should serve as a useful heuristic.

Another difficulty with this enlarged conception of the scientific process is that it permits individuals other than scientists to participate. This implication should not be interpreted as a covert means of legitimating as scientific the actions of those who take part in the scientific process, but who are not scientists; for this is not the intention of the definition. An individual can be a party to the scientific process without the

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FIGURE I
THE SCIENTIFIC PROCESS



[[21]]

necessity that the inference be drawn, either that he is a scientist, or that his work is scientific. For these labels are reserved for the professional scientist in the first instance and for anyone who adheres to the scientific method in the second instance.

The elaboration of the normal scientific process definition is a means to a better understanding of the personal politics of science. Informally it has existed since the first scientist penned a biography. In reading *The Double Helix* ^[45] or *Lawrence and Oppenheimer*, ^[46] for instance, all of the above is implicit, but not elaborated. The McDonald UFO case study which follows is a detailed examination of selected aspects of one man's attempt to force a paradigm shift on the American scientific community. The ramifications would have been of major import to society if McDonald had proved successful. Few areas of life would have remained unaffected, at least in the industrialized world; space budgets, military budgets, priorities at all levels would have undergone reassessment along with life styles, careers, and cosmological orientations. That

McDonald failed is not to say that he was wrong, nor that the issue is no longer joined. Neither is it to imply that he was correct and championed an idea whose time had not come. The UFO phenomenon is a thorny issue, not readily amenable to scientific investigation; the controversy continues, but is smoldering, rather than burning. Only time will tell if James McDonald pursued, in his own words, "the most important scientific problem of our time," or a will-o'-the-wisp.

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In either case, a great deal can be learned about the behavior of scientists when paradigms are at stake from the analysis of his attempt to legitimate the study of the UFO phenomenon. In particular, we can better understand, at least in this case, how a scientist tried to bridge the chasm between an old and a potentially new paradigm.

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FOOTNOTES

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4. *Ibid*, p. 67. - [Back To Text](#)
5. *Ibid*, p. 79. - [Back To Text](#)
6. *Ibid*, p. 108. - [Back To Text](#)
7. *Ibid*, p. 110. - [Back To Text](#)
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II. INTRODUCTION

Introduction	October
Background	November
March 1966	December
April	Summary and Conclusions
May	The Scientific Community
June	The Government
July	The Military
August	The Public
September	Summary of Tactics

[Footnotes](#)

Introduction

The McDonald campaign to force a paradigm shift on the scientific community with respect to the interpretation of unidentified flying object data had a duration of almost five years. There is sufficient information available to permit a detailed look at the entire process. However, this is not necessary both because it would make for a tedious presentation and due to the fact that the central theme of the study -- namely that we can view the scientific process as a political process -- is demonstrable without resorting to such a complete rendering. Therefore, only the first year (1966) of McDonald's involvement in the controversy is presented in full.

McDonald is followed in his day-to-day interactions with those within and without the scientific community in an attempt to recreate the hectic atmosphere which developed around him as he tried to reason and cajole his way to a paradigm shift. The presentation of his first year's campaign, then, is not a substantively focused discussion, but rather reflects the helter skelter strategy and tactics which McDonald employed. This does not mean that his behavior lacked direction, however, it is indicative of the fact that he did considerable spade work to determine the parameters of the phenomenon and the most appropriate tactics to implement his strategy. Oftentimes it seemed that he endeavored to fight some amorphous multi-faceted foe (the old paradigm) which would give way in one area only to become more intransigent in another. Hopefully, much of this is conveyed in the following chapter. Although regrettable, the detailed form of presentation is an unavoidable necessity in making my case. For it is

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only through the reconstruction made possible by the use of most of the pieces, that the mosaic called the scientific process will take form.

To further flesh out the context various bits of background are presented intermixed with the ongoing story. In some respects this serves to break the continuity of the events, but on the other hand it provides an increased understanding of the UFO controversy in general and that knowledge facilitates a better grasp of the role McDonald played and the rationale for the tactics he used. Moreover, by proffering this background data now, it will not be necessary to present it in the chapters which follow and consequently they can focus on elements of the politics of science surrounding the paradigm shift attempt without annoying digressions.

BACKGROUND

It is difficult to say when Dr. James E. McDonald first became interested in the UFO phenomenon, but it is probably safe to conjecture that his curiosity was aroused by a sighting he

and his wife made while driving in the Arizona desert in the early 1950s. Nevertheless, it would appear that he did not take the problem seriously until much later. When he did so he often said, when asked why he waited so long, that he relied on the statements of the United States Air Force and in particular on the word of the only Air Force scientific consultant who claimed over-time familiarity with the problem, J. Allen Hynek. At any rate, McDonald remained content until the spring of 1966 to pursue the problem on a very low-key basis.

This amounted to some case investigations in the Tucson area and limited interaction with Richard Hall, the Assistant Director of

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the National Investigations Committee on Aerial Phenomena (NICAP). As early as April 1960 NICAP contacted McDonald in the process of soliciting the signatures of scientists on a statement which would eventually take the form of a press release indicating the significance of the UFO problem. Apparently McDonald wrote Hall to the effect that he would sign, but expressed qualms about several of the points. Hall responded that McDonald's criticisms were well taken, but that the statement could not be changed at that late date, although McDonald was welcome to add anything he wanted at the end of the statement which would be solely attributable to himself. ^[1]

Hall also enclosed case material on an ice-fall (a term used to designate the falling of a large chunk of ice from the sky) which took place May 10, 1959 in Smithtown, Long Island. Probably McDonald agreed to consult on such cases because his specialties of meteorology and cloud physics seemed related to them. Many researchers persuaded of the extraterrestrial hypothesis (ETH) felt that the mysterious ice-falls emanated from space craft, notably because reports of them existed long before the flight of the first airplane.

Later that year Hall again wrote McDonald on the subject of the joint statement of scientists. McDonald, already deeply embroiled in a controversy concerning the placement of ICBM missile sites around Tucson, earlier asked Hall in a letter of June 12, 1960 not to use his name on the UFO statement unless it came out in September or later because he feared the bizarre nature of the issue could redound to his detriment in the missile site battle with the Air Force. ^[2] Therefore, Hall told McDonald that his signature would not be used since NICAP wanted to get the joint statement out in July. ^[3]

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In addition Hall outlined part of his position on the UFO issue. He said that he found the "don't panic" Air Force line on the Titan missile question analogous to the "don't panic" policy on UFOs. He indicated that unlike McDonald he did not consider UFOs an unusual scientific problem, but rather physical objects readily amenable to investigation via instrumentation. The problem, in Hall's view, appeared psychological and political; an unwillingness to launch an investigation. He felt the answer would be forthcoming if the scientific method were applied. Hall defended Major Keyhoe and NICAP against McDonald's criticisms of Keyhoe's writing style for its lack of rigor, and NICAP for its unscientific ways. Hall claimed Keyhoe's facts were the important issue, not whether or not he could write in an acceptable scientific mode. With respect to NICAP, it didn't have the funds to do science, it needed to act as a propaganda agency to thwart the Air Force, the doing of science would have to wait for funding.

McDonald wrote back that he was in agreement with Hall "on all the broad issues raised by UFOs." He made it clear that his name should not appear in the joint statement because the Air Force would use it to discredit his position in the missile site controversy. He said he would follow NICAP's efforts to get a congressional inquiry with great interest. ^[4]

So we can see that by 1960 McDonald exhibited more than a casual interest in the UFO problem. He functioned as an anonymous consultant for NICAP, he looked into cases in the Tucson area, and he started a search of the UFO literature in an attempt to determine for himself the scope of the phenomenon, and if there existed a solid foundation on which he might build. Perhaps equally important for his future work, he

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established contact with Richard Hall, who would be his coworker and close friend in the hectic years from 1966-1971.

McDonald's files do not indicate that he did any UFO work between 1961, when he discussed a sighting photograph with Hall, and 1966 when he made the decision to devote considerable energy to the subject. ^[5] I think we can surmise that he used the intervening years to pursue the problem in a subdued fashion by continuing with Tucson area investigations and by following the UFO literature.

He indicated in a letter to Hall in 1966 that before he "came out" on UFOs he proceeded in this manner for about ten years. He considered the evidence gleaned in this way disturbing but not decisive. Moreover, in the case of the UFO literature, Keyhoe's books for instance, he could not determine what to accept as fact. What made him decide to try and satisfy his curiosity on the subject with a summer study in 1966 were a few unexplained local incidents in 1965 and the

March 1966 wave (a large number of sightings) in Michigan. [\[6\]](#)

MARCH 1966

This is partially confirmed by a letter McDonald wrote to Tom Malone, Vice-President and Director of Research Meteorology for Travelers Life Insurance Company and Chairman of the National Academy of Science (NAS) Committee on the Atmospheric Sciences (CAS) at the time of the March Michigan sightings. He expressed his belief that the scientific community failed to respond adequately to the UFO phenomenon, that the history of science was strewn with stories of similar oversight and that to avoid the problem on the grounds that the data were too messy smelled of scientific arrogance.

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He went on to indicate his increasing dissatisfaction with the treatment accorded the problem by scientists and also the Air Force. He stated that he contacted Representative Morris Udall of Arizona asking him to suggest to Representative Ford of Michigan a small panel investigation by scientists.

In addition, he asked Malone if a small panel could be set up within the CAS, NAS or some other body. It would have to be done without any publicity and the panel would need access to the Air Force files. Federal Aviation Administration files, etc. If nothing came of it such a summer study could be quietly dissolved, if it bore fruit the study could be expanded.

McDonald asked Malone to bring it up to the CAS and to Philip Seitz, President of the NAS. He assured Malone that he gave great thought to the question and that this seemed to be the best approach. He closed with a remark which is typical of his style, "I know that it probably strikes you as a bit off the main path but I do hope your scientific intuition convinces you that that alone may be a good reason to give the idea a whirl." [\[7\]](#)

On the same day as his letter to Malone he sent a note to Representative Udall, who I would presume he worked with on the Titan missile affair because he addressed his letter "Dear Mo." [\[8\]](#) In his remarks he pointed out his growing interest and concern with the UFO issue and brought up a March 27 news story which intimated that Gerry Ford might call for major UFO hearings. McDonald pointed out the difficulty of assembling reliable information on the problem and emphasized the "journalistic fun-poking" which would occur were a hearing held. He presented as an alternative a small two- or three-man

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study through the CAS or NAS which would probably accomplish more and do it in an unobtrusive fashion. He asked Udall to pass the letter on to Ford, but both were to keep the idea confidential.

A separate note went to Udall to explain the overture already made to Malone and to counsel Udall not to forward the initial letter to Ford if he (Udall) felt it would not remain confidential McDonald, as he put it, didn't want a headline saying, "University of Arizona Scientist Pleads with Congressman to initiate Flying Saucer Probe." [\[9\]](#)

In light of the NICAP efforts to obtain a Congressional probe of UFOs since the late 1950s McDonald's remarks to Udall suggest that he was not in tune with NICAP policy at this point. His plans for a quiet study through the NAS further imply that he believed the normal scientific channels were adequate to resolve the issue.

Two days later McDonald began to have second thoughts on the procedural acceptability of his plan to study UFOs through the CAS. He zipped off another letter to Malone. He intermixed apologies for causing Malone problems with more ideas on how to pursue the UFO issue. First he mentioned the possibility of a background study to provide information for the committee to decide if it wanted to take up the problem. Also he presented the concept of a one-man project reporting directly to Seitz. McDonald made it clear that entree was needed in such a situation, so that doors would open. The NAS affiliation, he felt, would provide this and a one-man shoestring study might be useful. He further liked the one-man approach because if fruitless it could be terminated easily. But, on the other hand, if Malone liked the two- or three-man panel concept he (McDonald) would speak for twenty minutes on the proposal at the April CAS Meeting in Boulder. [\[10\]](#)

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APRIL

On April 1, 1966 McDonald heard from Udall that Gerry Ford received the letter and it would be kept in strictest confidence. Udall said he felt the idea of a quiet study an excellent one and offered his assistance to Ford. [\[11\]](#)

This interchange is typical of McDonald's strategy on any given issue. First he would formulate the problem, then choose those individuals most likely to be of assistance, and finally start a

barrage of letters intended to move those people to action. We will find this pattern exhibited on numerous occasions in the following pages.

During this early period McDonald was in the process of getting his feet wet on the UFO issue, but he also began to show the pugnacity and dogged tenacity which were both his best and most irritating qualities. He wrote Hall to ask for back issues of the Investigator (the monthly NICAP newsletter), and a list of NICAP publications. This tends to suggest that he started to do his homework on the subject. Nevertheless, he told Hall he looked into the now classic swamp gas explanation rendered by Hynek at the behest of the Air Force for the March Hillsdale, Michigan, sightings and found that the luminosity produced by methane gas escaping from a pond would not last long enough to account for the reports. [\[12\]](#)

This is the first indication of a disagreement between McDonald and Hynek, however, it will not be the last. They were totally opposite in temperament and style. Hynek was cautious, career-oriented and well aware of all the difficulties associated with grappling with the UFO problem after eighteen years of part-time consulting for the

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Air Force on the astronomical aspects of the phenomenon. Not a man who would go looking for trouble, he just happened to be close at hand when the Air Force needed a consultant in 1948. Then, very slowly at first, but with ever greater rapidity in the late 1960s, the storm drew him toward its center. McDonald, to the contrary, was volatile and enjoyed a fight. Although politically savvy, as will be shown in the following pages, he could also be obtuse when least expected. Accordingly he could make demands in the name of science befitting a puritan in their stringency. He could not understand Hynek, so he claimed, and this proved the basis for numerous squabbles over the five-year period of McDonald's involvement in the UFO controversy.

On April 10, 1966, however, McDonald remained uncommitted to the UFO problem. In corresponding with Malone, after the Boulder meeting, he talked of the conflict between chairing a CAS weather modification panel and pursuing the UFO question over the summer. He wanted to do both, but realized he did not have the time, yet showed concern for the fairness of the rumored panels which might be established by the Air Force. Secretary of the Air Force Brown mentioned this panel possibility in a three-man hearing featuring himself, Hynek and Major Hector Quintanilla, the Blue Book officer, before Mendel Rivers' House Armed Services Committee on April 5. McDonald concluded that a way out might be to sit on Secretary Brown's Department of Defense panel which would require less of his time than a one-man study, but accomplish the same thing. To that end he wrote John Coleman at NAS to ask if this could be

arranged. [\[13\]](#)

Coleman responded to this letter with a phone call in which he briefed McDonald on the latest NAS thoughts on the UFO problem. They

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were, that if the recommendations of the Air Force Scientific Advisory Board (**AFSAB**) for a university study were followed, there would be no need for NAS involvement. McDonald responded with a letter in which he agreed with Coleman's remarks, and stated that his proposal made to the CAS at Boulder could now be ignored.

Nonetheless, he did have reservations which stemmed from several sources. First, he spoke with the head of the University of Arizona Astronomy Department, Aden Meinel, who formerly sat on an ad hoc Air Force advisory panel on UFOs with Donald Menzel of Harvard a few years previously. According to Meinel, accounts of sightings, what McDonald considered the heart of the problem, were not stressed. McDonald gathered this would not be the case in the university study, but it concerned him that a clinical psychologist, so he heard, would be featured on the investigating team. He felt that based on the 150 or so persons he interviewed in 30 or 40 cases that very few witnesses needed psychological analysis. The fringe groups who made bizarre claims needed it, but they did not, except in a small percentage of cases, report sightings. He thought scientists like Edward Teller, Donald Menzel and Gerard Kuiper who dismissed good sightings with psychological explanations did so because they did not go out in the field to interview witnesses.

He went on to point out the difficulty of getting people to report sightings and then argued that the public announcement of the fact that each investigative team contained a clinical psychologist would be tantamount to saying people who make UFO reports are unbalanced. The result would be to significantly reduce the number of reports.

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He said he spelled out these points in some detail in the hope that the letter could go via the NAS to the AFSAB and that this might make it possible for him to assist the group preparing plans for the university teams. McDonald toots his own horn quite blatantly at this point by discussing his UFO work and the academic areas of specialization which would make him an asset to a UFO study. It seems obvious at this juncture that he badly wanted to be a part of the forthcoming study or perhaps a pilot study. For in a P.S. he lauds a plan put forth by Hynek to study the 25

best authenticated cases and states that this should really precede the university project. [\[14\]](#)

Even though McDonald's involvement increased he took great care to avoid making a public issue of it. Apparently as late as April 20, 1966 he remained uncertain about NICAP and did not keep Dick Hall informed of his machinations. For Hall wrote on April 20 to ask for some weather maps and began his letter, "There is one way that you could be particularly helpful to us without your name being involved." [\[15\]](#) So we can conclude that at this time McDonald continued to play his cards close to his vest and was chary with regard to NICAP and the UFO phenomenon in general.

Ten days after Hall's letter to McDonald the latter wrote to Jim Hughes, his Office of Naval Research (ONR) project monitor, on the subject of UFOs. He told Hughes that Charlie Moore, of the New Mexico School of Mining and Engineering and Martin Uman, a physicist with Westinghouse Research Labs, were in town for an Institute of Electrical and Electronic Engineers session on atmospheric electricity and that they pursued the topic of UFOs over dinner. Ball lightning received considerable discussion because Uman showed interest in the similarity

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between it and UFOs. Because Moore just talked at length with Hynek he provided a summary of Hynek's UFO views.

McDonald discussed his NAS-CAS UFO activities in the letter and mentioned the rumor that the Department of Defense went to NAS for advice on universities and investigators for the proposed study. He said he heard he was at the head of the list under consideration "to tilt with the little green men." However, he felt his Titan activities might hurt his chances with the Air Force. At any rate he seemed pleased that the problem would be joined, regardless of who did the research. [\[16\]](#)

MAY

Dr. Brian O'Brien was a member of the NAS and Chairman of the AFSAB ad hoc panel on UFOs which in February of 1966 recommended a university study of UFOs to Secretary of the Air Force Brown. McDonald knew him through his work in atmospheric physics. Apparently they talked over the UFO problem in early May because on the 14th McDonald wrote O'Brien a letter which shows that O'Brien agreed to raise the summer study panel idea, which McDonald wanted, at the Air Force Systems Command (AFSC) on his (O'Brien's) next visit. Furthermore, McDonald now wanted O'Brien to raise the issue of him (McDonald) visiting the Blue Book

offices at WPAFB in Dayton, Ohio, to view unclassified materials on one leg of a trip which would take him to Washington, D.C., for a meeting of the Project Stormfury Panel under the auspices of the Environmental Sciences Services Administration (ESSA). McDonald emphasized his willingness "to put a lot of effort" into a summer study of UFOs done with a group or alone. [\[17\]](#)

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A cover letter written by Dr. A. Richard Kassander, Chairman of the Institute of Atmospheric Physics, for an NASA Institutional Grant to enable McDonald to do a summer study of UFOs is dated May 18 and is evidence for the fact that by this time McDonald was quite serious about looking into the phenomenon. Kassander's letter is also interesting because of its apologetic tone. He stated that he regretted the existence of the data, but it would not go away and as a result of conversations with McDonald he felt the observations could not be dismissed lightly. He recommended the proposal because it could provide guidelines for a future large-scale study. [\[18\]](#) In the proposal McDonald asked for a modest \$1300 in seed money to cover phone interviews and travel expenses.

A week later Jim Hughes of the ONR informed McDonald that Jim Kearney at Pasadena Naval Research did interesting work on laser detection of visible and subvisible clouds. This type of observation, thought Hughes, might account for some of the inexplicable high-speed radar tracks of UFOs since a cloud could appear, dissipate and reappear in another place providing the illusion of a high-speed object. He suggested McDonald go to WPAFB to examine the Blue Book files for cases which might fit this category in order to develop an assessment of the problem. Because the work was for the ONR Hughes said it would pay the trip expenses. [\[19\]](#)

This is the beginning of what would become a controversy within a controversy. Was it pure coincidence that Hughes suggested McDonald make the WPAFB trip at the exact time McDonald wanted to make it? It would seem to stretch the limits of credulity, but we will see the arguments on both sides unfold because this won't be the last time

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McDonald uses ONR atmospheric research funds for what appears to be UFO research.

On May 26 McDonald penned a note to Hughes to thank him for the trip and to recount an unusual call he received from WPAFB asking for information that would enable the Air Force to obtain a security clearance for him to view classified cases. McDonald assured the secretary that

when he spoke to General Cruikshank, Commander of the Foreign Technology Division (FTD), that the general stated all cases were unclassified. The secretary said no, she listened on the extension phone and heard McDonald ask to see the classified cases. McDonald claimed to Hughes that he did not make the query and felt uneasy over the eavesdropping. He said he would give serious thought to viewing classified material before doing so.

Although he did not say so this undoubtedly reflected a concern which exists in the UFO field as speculation and rumor, i.e., that if the Air Force is covering up and is worried about an investigator getting too curious it will permit the person to see classified material which will tell all, so to speak, but leave the investigator silenced unless he wants to risk prosecution for revealing classified information. Probably as early as May 1966 McDonald considered this possibility.

This is further indicated by a discussion of the cover-up hypothesis of Donald Keyhoe, in which McDonald told Hughes that it would make no sense for the Air Force to staff Blue Book with incompetent scientists if a cover-up existed. But McDonald concluded he would only provisionally reject the cover-up notion. This is the first sign of an internal debate which would plague Hall and McDonald for the next

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several years once McDonald reached the ETH conclusion; was the Air Force study a cover-up or a foul-up?

In the same letter McDonald explained that when in Washington he wished to get a briefing on the rumored Navy studies of UFOs and he wanted to know if ONR might fund him, in terms of travel money, for his UFO research. With respect to the NASA Institutional Grant proposal for \$1300, he believed the chances of obtaining it were poor because Gerard Kuiper and Aden Meinel, the only astronomers on the committee, thought little of the UFO problem. McDonald left the impression that he intended to speak with them privately. He suggested that Hughes pick up a copy of the April 5 House Hearings on UFOs, probably to get Hughes thinking positively on the UFO subject.

Also in this letter McDonald casually mentioned that he would spend a lot of time at NICAP checking library items. This is his first visit to NICAP and must be viewed as a considerable escalation of his past position which consisted solely of corresponding with Dick Hall. McDonald communicated another first to Hughes, the belief that the UFO problem belonged at NASA. Al Eggers, an aerodynamicist at NASA, is named as the only possible entree who might be able to bring the matter to the attention of individuals in higher places. [\[20\]](#)

On May 27 McDonald spoke with Gerard Kuiper and found him receptive to, or at least persuaded that, the \$1300 NASA Institutional Grant was reasonable. Later that day McDonald jotted him a note, after he learned that the Air Force plan to set up university investigative teams was in progress, in which he cited his concern that this would make it more difficult to get the problem under the aegis of NASA. He said that if Kuiper deemed it wise he (McDonald) would contact NASA while in Washington

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from May 29 through June 5. However, he thought that the best strategy would be to wait and discuss the problem with Kuiper and a few of his colleagues when the former returned, since Kuiper's word would carry more weight than his own in NASA circles. McDonald also felt that his side trip to WPAFB would put him in a better position to speak to the pros and cons of the question of Air Force involvement. [\[21\]](#)

He made his first trip to WPAFB to examine the Blue Book files on June 6, 1966. At that time he viewed the 1953 Robertson Panel report, prepared under the auspices of the CIA. The report received routine declassification after 12 years by Major Quintanilla, the Blue Book officer, and McDonald's note taking received approval. In a small paragraph (written in October 1966) at the end of these notes McDonald discussed his experience with the document. This paragraph indicates that when he returned to WPAFB on June 30, 1966 and asked for a Xerox of the report a Colonel Louis DeGoes informed him that it would have to receive an authorization. On McDonald's third visit, July 20-22, DeGoes told him that the CIA decided to reclassify the document. Although McDonald indicated that he discussed the CIA involvement with many colleagues and took notes on the report, neither DeGoes nor Dr. Cacciopo, Chief Scientist at FTD, showed any interest. So in October McDonald began to expound on the Robertson Report in his talks. [\[22\]](#)

For McDonald this was probably the beginning of his concern for a cover-up of the UFO phenomenon. To officially confirm CIA involvement seemed a revelation in 1966 and McDonald's lengthy note on how it came to pass that he made the Robertson material public suggests the gravity with which he viewed this information.

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JUNE

Two weeks after he spoke with Kuiper McDonald sent him a memo just prior to Kuiper's

chairing of a meeting of the Space Sciences Committee one morning at the University of Arizona. In it McDonald stated that he wanted to address the Committee on his conclusion reached while at WPAFB, namely that UFOs were extraterrestrial. ^[23] Whether or not the address took place is not clear from the correspondence, but this represented a critical turning point for McDonald. He had both become convinced of the validity of the ETH and committed himself to his peers. From this time on the topic consumed him. Except for short periods of time such as his participation in the SST debate, he devoted essentially full time to the problem until February of 1971.

Shortly after the memo to Kuiper he wrote to Tom Malone enclosing an Oklahoma Department of Public Safety report on an August 1965 wave of sightings and a copy of a Captain Holder's report on the Socorro incident, an alleged landing-occupant case which took place in New Mexico on April 24, 1964. ^[24]

Of greater interest, however, is a letter from Hynek to Secretary of the Air Force Brown which McDonald also passed along. ^[25] His comments on it were, "Hynek felt this was a rather daring step to write directly to Brown. I find the letter disappointingly full of equivocations."

This is perhaps a good point to take a look at Hynek's strategy through the use of the above-mentioned letter. It will enable us to better understand both him and McDonald. First let us consider the boldness of Hynek's action. Since the mid 1950s Blue Book consisted of one officer and two enlisted men. The Air Force viewed it primarily as a public relations effort and a low priority project. Hynek, as the

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astronomical consultant for 18 years, knew that and knew also of the difficulty of convincing the Air Force of the anomalous nature of UFO data. Now, as consultant to this laughing stock project, he wrote the Secretary of the Air Force. He did so in an indirect style which McDonald either failed to appreciate, or refused to acknowledge.

Hynek tried to build a case that the Air Force image would be tarnished by the abundance of UFO books about to appear and the inadequate investigatory methods at Blue Book. He argued that to counter NICAP and others, the best approach would be to carry the AFSAB recommendations further and give the task of all major UFO investigations to "nationally respected scientists." This scientific "back up" to Blue Book, he claimed, would get the UFO monkey off the back of the Air Force. So what Hynek felt could not be done by direct means, i.e., the justification of a large scientific project on the grounds that UFO data appeared significant, he did feel might be accomplished by appealing to what concerned the Air Force, its

image.

JULY

On July 1 McDonald received a four-page letter from Herb Roth of United Airlines (UAL) who directed the Voluntary Flight Officers Network (VFON). This group, formed in 1963 at UAL to monitor the reentry of space satellites, consisted of volunteer pilot members. In January 1966 it expanded to include over 30 of the world's largest airlines and worked in concert with the Satellite Reentry Program of the Smithsonian Astrophysical Observatory. Roth wrote McDonald because he heard from Richard Hall that McDonald advocated a program consisting of a camera in every commercial airline cockpit to photograph UFOs which

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might be encountered. Roth considered this a good idea, but made it clear that the matter needed to be handled tactfully. He explained how he already enlarged reporting duties to include meteorites and any unidentified atmospheric phenomena, but that he was careful to avoid the term UFO. Now he thought he could get some cooperation in the carrying of cameras if he asked the Smithsonian to recommend it on a voluntary basis to photograph unusual atmospheric phenomena. He closed by asking for McDonald's opinion on this plan. [\[26\]](#)

July 3 found McDonald writing Colonel Louis DeGoes a quick Sunday morning note. De Goes was with the FTD at WPAFB. McDonald met him, along with Majors Bruce and Boyce, on his June 30 visit. They comprised the Air Force team investigating Blue Book procedures. This investigation was the natural outgrowth of the poor press the Air Force received from the April Congressional Hearings, the Michigan "swamp gas" sighting explanation, and the proposed university-sponsored study of UFOs already in the planning stages. McDonald seemed pleased with the intended DeGoes review, enclosed several papers and references on ball lightning (thought to be a possible source of some sightings) and extended his help if he could be of assistance. [\[27\]](#)

Further indication of McDonald's favorable impression of Colonel DeGoes is found in a letter written four days later to Isabel Davis, an old UFO investigator from the days of Civilian Saucer Intelligence (CSI) in the early 1950s, who in the 1960s worked with NICAP. He asked Davis if she could be persuaded, having battled with the Air Force for some 15 years on UFOs, to make her clipping files of sightings available to DeGoes if he requested them. McDonald felt DeGoes and his men looked into the UFO problem much further than Major Quintanilla, the Blue Book

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officer, and he said, "I see real hope that within the FTD itself, there is now a chance that the real nature of the UFO problem may be discerned." He went on to state that DeGoes asked him to return in a consulting capacity, which he considered a good idea. Because the rumor existed that the Air Force was having difficulty getting scientists interested in the independent university approach McDonald speculated that the DeGoes effort might come up with new findings first.

McDonald also gave Davis a review of his recent activities. He pointed out that unlike Davis he considered it necessary to explore the "one-shot hallucination hypothesis" and to that end discussed the possibility with Nell Bartlett the chairman of the University of Arizona Psychology Department. Bartlett, however, found the hallucination hypothesis untenable in light of some of the apparently structured, coherent UFO reports spanning extended periods of time.

This was an important issue because it hovered around the periphery of the contactee cases (a contactee being a person claiming contact with extraterrestrial beings). NICAP would not touch such cases, nor at this time would it consider occupant cases (a case where a UFO report included an alleged description of an occupant inside a craft or on the ground, but where no contact occurred). McDonald encountered several occupant cases and therefore deemed it wise to examine the possibility that they might lend themselves to a psychological explanation. As with most new investigators he was reluctant to entertain what he considered an exceptionally bizarre aspect of the data, but as a scientist he also regarded it as his obligation to scrutinize all elements of the reports.

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Lastly, McDonald said he looked upon his Washington, D.C., trip of June 29 as productive even though he did not get to brief a group at NASA. He emphasized that he wanted to set up such a briefing for the following week and considered it imperative to get as many government agencies as possible interested in the problem to avoid further future bottle ups. Results were already visible, he claimed, in the form of rumblings from a two-hour briefing he gave Donald Hornig, the Director of the Office of Science and Technology in the Executive Branch. ^[28]

On the same day McDonald got off a letter to Dick Hall also informing him of the latest progress with DeGoes. He found encouragement in Bill Weitzel's (an NICAP investigator) 128-page report on the Ravenna, Ohio, sightings and intended to pass it on to DeGoes. He suggested to Hall that a rapprochement between the Air Force and NICAP could be worked out to the benefit of all concerned. McDonald expressed the hope that he would be able to meet with Dr. Brian O'Brien while in Hartford seeing Tom Malone. McDonald felt strongly that O'Brien needed to be

neutralized by opening his eyes to the facts about UFOs since O'Brien's word pulled a great deal of weight in Washington. O'Brien previously indicated he thought "McDonald belonged on a couch." To this McDonald replied he did not care what O'Brien thought except as the side effects might impede the progress of UFO research. [\[29\]](#)

The following day Hall sent off a quick note to McDonald to report on a conversation between Lee Katchen (an atmospheric physicist at NASA and also an NICAP Investigator) and Hynek. According to Katchen, his call to Hynek came at a very opportune time because Hynek claimed he had been struggling for months with his conscience, and fears of losing his job, over calling attention to the significance of the UFO

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phenomenon. Katchen alleged his call turned the tide and as a result Hynek planned to send a letter to *Science*. Hall thought It would help if some scientists who were not affiliated with NICAP were to call Hynek and encourage his letter-writing effort.

In this regard it must be remembered that at this time all concerned considered Hynek the most experienced academic UFO investigator. Consequently they believed that his 18-year consultantship with the Air Force and chairmanship of the Northwestern Department of Astronomy would lend considerable weight to the UFO cause were he ever to make his private position a matter of public record.

Hall also confirmed the fact that Hynek spoke with U. Thant, along with journalist John Fuller, who wrote two books on UFOs, and Thant proved receptive to the formation of an international study group to look at the UFO question. As an aside Hall mentioned Hynek recently showed an interest in the relationship of UFO sightings to power blackouts. Since the great Northeast blackout of 1965 when concomitant UFO sightings were made and the Exeter, New Hampshire, sightings, where UFOs were reportedly seen hovering over power lines, many investigators found the possible linkage intriguing. The fact that Hynek wanted to pursue it was a positive sign to Hall. [\[30\]](#)

McDonald provides an informative look at his perception of the developing UFO scene in a response to Herb Roth, coordinator of the VFON program at UAL. As you will recall, Roth was ready to attempt to get pilots to carry cameras to photograph UFOs. But McDonald cautioned him to wait a few months because in that period of time he felt a new "official line" would be taken. He asserted that through informal talks with people at Blue Book, the Air Force Office of Science and Technology, NASA

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and other agencies that he stimulated an uneasiness about UFOs which would bear fruit. [\[31\]](#)

McDonald was not the only one who sensed a change in the atmosphere surrounding the study of UFO data in 1966. Apparently in early July NICAP convened some sort of strategy session from which emerged a campaign plan. Hall wrote McDonald on July 12 enclosing a list of 60 scientists and engineers interested in UFOs. He argued the list could be used:

1. To help counter the claim that no scientists are interested in UFOs.
2. To seek advice and counsel from a broad spectrum of scientific disciplines.
3. To obtain in one way or another general scientific support for UFO investigations and study.

He also furnished McDonald with the information that Gene Rygwalski, a mathematician with the General Electric Space Technology Center in Valley Forge, Pennsylvania, and sub rosa NICAP subcommittee chairman, had a group of scientists ready to apply for funding to look at the UFO problem. However, Hall advised him (Rygwalski) to wait awhile because the official Washington position seemed to be in a state of flux.

In this communication from Hall we find several indications of the importance of recruitment of scientific talent to a borderland subject and the manner in which it is hoarded by the recruiters. The reason for the list was essentially leverage in argumentation and fund-raising, but Hall pointed out that Hynek could see it at McDonald's discretion, but not Jacques Vallee or William Powers, both protégés of Hynek. Hall believed cooperation with Hynek a possibility, but he felt

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Vallee and Powers were not interested in exchanging information and in fact might attempt to lure the "NICAP 60" into their own camp. [\[32\]](#)

Whether this concern was well-founded is not at issue here, however, it should be stressed that matters such as these occupied the thoughts of most of those addressing the UFO question at one time or another. I don't think it reflected so much a desire to be the first to make the "big breakthrough" everyone hoped for, as it did the fact that the symbols of authority -- scientists interested in UFOs -- were a scarce resource. Everyone realized this resource had to be utilized for its propaganda value to obtain converts, influence Congressmen, impress funding agencies and keep up the membership of NICAP and APRO. I don't wish to leave the impression that the technical skills these men possessed were not also esteemed, for they were and are, but to put

them to their best use it was felt the entire UFO issue needed to be catapulted into the realm of big time, big money science. This could only be done, given the past history of the phenomenon, if men of scientific stature could be convinced of the saliency of the matter.

Although he was often bold in his undertakings McDonald's response to Hall indicated he could be circumspect when necessary. He told Hall that neither APRO nor the Northwestern group would see the list of 60 scientists, however, he would show it to Tom Malone, but not for purposes of Xeroxing.

In the same communiqué McDonald outlined his travel itinerary for the following week. First it would be up to New York to see Ted Bloecher, Isabel Davis and Lex Mebane (old CSI researchers now aiding NICAP), next to Hartford, Connecticut, to see Tom Malone and hopefully get in that talk with Brian O'Brien, then down to Washington to see NASA people and

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Jim Hughes at the ONR, and finally over to Dayton, Ohio, to take another look at the Blue Book files. This epitomizes the McDonald style, always on the move, continually talking and knocking on doors. He felt he might kick himself for the trip, "But it's undesirable to let things cool off too much without making the rounds again and pushing people." [\[33\]](#)

Word of his campaign spread rapidly in the UFO field. As previously observed a certain friction already existed between himself and Hynek as well as between APRO and NICAP. As would only befit such a situation Hynek, while ostensibly neutral, seemed to be cooperating with the Tucson-based APRO group and McDonald worked through the Washington-based NICAP organization. Why they did this is debatable, but we might speculate that Hynek, as an Air Force consultant, would have found association with NICAP incommensurate because of its constant harassment of the Air Force. On the other hand, APRO learned early that it neither had the membership, funds, nor geographical location to tilt with the Air Force and so fostered the image of a research-oriented group which seemed more compatible with Hynek's position. On several occasions McDonald intimated that he avoided APRO most of the time because their investigatory work did not satisfy him (he later changed his stand on this), he couldn't get along with Coral Lorenzen, and he disliked the acceptance of occupant cases in his early days of research. This must have embarrassed the Lorenzens since they were ensconced only a few miles from McDonald's home and naturally a certain antipathy developed. It might also be appropriate to remember that McDonald had already jostled with the Air Force, enjoyed a good Donnybrook, and could get into one through NICAP, but not through APRO.

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At any rate, on a rare occasion when he called APRO for information on a case in early July 1966, he let it be known, perhaps in passing, but probably to stir up Hynek, that he visited WPAFB. Subsequently, in a letter to Hynek, Coral Lorenzen warned him that between McDonald trying to carve a niche for himself in the UFO field, the Air Force looking for a goat for its ineptness, NICAP moving toward a scientific foul-up stance, and McDonald accusing Hynek of timidity in his role as Air Force consultant, that Hynek could be left holding the bag when the issue finally broke. [\[34\]](#)

In his continuing effort to get the FTD at WPAFB to see the light about Blue Book inadequacies McDonald sent a summary of the April 17, 1966 Ravenna, Ohio, case to Dr. Anthony Cacciopo the scientist at FTD responsible for Blue Book. He also asked Cacciopo to read Bill Weitzel's 128-page report which NICAP forwarded and to peruse a 17-page tape transcript of Major Quintanilla, the Blue Book head, interrogating the witnesses. McDonald believed that the transcript in particular would show up the glaring incompetence typified by Quintanilla. [\[35\]](#)

This was a period of excitement for McDonald. He informed Hall of his travel plans which would eventually get him to Dayton. He said he viewed as positive the fact that DeGoes and his two Major review team would be out at Rand at the time of his visit. He felt it could be a significant development. It would be some time before he would learn otherwise. [\[36\]](#)

Five days subsequently McDonald received the first academic encouragement to continue his UFO work. Dr. A. B. Weaver, Chairman of the Space Sciences Committee at the University of Arizona, officially informed him of the positive recommendation given his \$1300 proposal for

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an NASA Institutional Grant. [\[37\]](#) This grant, while small, facilitated much of McDonald's early telephonic interview work on old cases.

At the time that the above announcement arrived McDonald was on his Hartford-Washington-Dayton trip. On July 20 he wrote up a summary of the trip's highlights while still at WPAFB. He preferred getting as many facts as possible on paper in order to jog his mind loose of details and to keep his friends informed of his progress. This letter provides insight into the planning of the Air Force sponsored university contracted UFO project, McDonald's perception of Blue Book, and the role Hynek played in the Blue Book investigation.

The letter reveals that during his stay in Washington McDonald met with Dr. J. T. Ratchford of the Air Force Office of Scientific Research (AFOSR) whose job it was to place the UFO contract

at a university and Dr. William Price, Executive Director of AFOSR, to whom Ratchford reported. The discussion, after McDonald explained his views on UFOs and Blue Book, and in the process watered down his ETH convictions, focused on the difficulties involved in placing the project at a university.

The projected study was to consist of a lead university with a principal investigator and scientific team in residence, but also field teams at other universities; they wanted a principal investigator with first-rate credentials who could spend full-time on the project. McDonald indicated that this would be an extremely hard post to fill because of the nature of the subject and because top quality scientists were usually up to their ears in their own work and would not have the time. Nevertheless, he offered, and according to him. Price accepted, his proposition to go on the road as a traveling salesman, so to speak,

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to cajole any lukewarm prospects for the principal investigator position into taking it.

After some talk by Ratchford about the response of groups like NICAP and APRO to a non-extraterrestrial explanation of UFO sightings and McDonald's assurance that he personally would get behind a well-done scientific negation of the data, the conversation turned to what McDonald deemed a strange topic -- the political leanings of the principal investigator. He wrote Malone, and told Hughes in conversation, that he never heard of avoiding a rightist or a leftist in choosing a project director, but realized that Ratchford and Price articulated such concerns because they misread (he felt) the nature of the UFO problem. They assumed that all the public discontent originated from kooks and cultists who somehow were also politically far left or right. Consequently, both Price and Ratchford believed that should an extremist head the project his conclusions would be written off by the public. McDonald indicated that he tried to convey to Price and Ratchford that there were good scientific grounds for criticism of the Air Force handling of the UFO matter and concluded to Malone that they (Price and Ratchford) were just naive about the problem.

The same day McDonald visited NASA and wrote, "I think we have now planted the seeds at NASA." He said he received a warm reception from the people at the Advanced Research and Technology Division: Ben Holzman, George Deutsch and James Danberg, Research Division, and Mason Charak and Conrad Mook, Space Vehicle Research and Technology. He discussed the possibility of the ETH with respect to UFOs and argued that the problem belonged at NASA, accentuating what it could mean for NASA appropriations. He told Malone the latter point did not go unappreciated.

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As it turned out McDonald also briefed a Joe Fletcher from RAND (the Air Force think tank) who he just happened to meet in the elevator at NASA and who Holzman belatedly, so he said, invited to the meeting. As fate would have it Fletcher knew that Colonel DeGoes and the two Majors from WPAFB were at Rand to discuss UFOs. McDonald regarded the coincidental meeting with Fletcher as worthy of rumination. Was it so coincidental? Regardless, it pleased him that NASA knew he briefed the Air Force (Price and Ratchford) and that the Air Force, through Fletcher, would know that he visited NASA. A little friendly competition, thought McDonald, might push the issue to a speedier resolution.

The next topic for Malone's information was McDonald's Blue Book visit. First a paragraph on how he managed to bully Major Quintanilla into agreeing to change the Ravenna, Ohio, case (the 128-page Weitzel reported sighting) to unidentified. Then it was time to pursue one of McDonald's preoccupations, the determination of Hynek's role in the UFO affair. He asked Quintanilla a lot of questions about Hynek because after his (McDonald's) June 7 trip to Blue Book he visited Hynek in a righteously indignant mood and harangued him concerning his past actions as Blue Book consultant and his failure to alert the scientific community to the significance of the UFO data.

Now through his questioning of Quintanilla he began to build a stronger case for Hynek's timidity. Quintanilla showed amusement at Hynek's claim that the Air Force compelled him to go along with its policy which, according to Hynek, stifled his initiatives. Quintanilla claimed that if he were the bottleneck to Hynek's plans, what about the other five or six Blue Book officers Hynek worked with over the years? As far as Hynek's claim that he couldn't get by Quintanilla to see

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Cacciopo (the chief project scientist) or General Cruikshank, Quintanilla said Hynek did see Cacciopo. Consequently, McDonald's suspicions of Hynek, based on his own look at the data, Hynek's remarks in Evanston on June 8 and now Quintanilla's rejoinders to those remarks, began to crystallize in a none-too-flattering portrait. McDonald even entertained the possibility that Hynek used the Air Force for the \$5000 per year consulting fee with no intent to pursue the scientific problem of UFOs. [\[38\]](#)

It should be emphasized, however, that at this stage of his campaign McDonald knew little about Quintanilla or Hynek, particularly Hynek's quiet behind-the-scenes attempts to obtain changes at

Blue Book without making too many waves. He was quick to condemn Hynek on grounds having to do with a scientist's public trust and obligation to science as an ongoing institution. Yet, what he failed to question were Quintanilla's own statements. Did Quintanilla, for instance, have his own reasons for wanting to show Hynek in a poor light? Was Quintanilla himself incompetent, as McDonald already alleged in another context and would this fact prejudice his remarks? Moreover, did a man such as Hynek, with at least a light hand on the pulse of the UFO issue for 18 years, have his own subtle strategy for dealing with the UFO problem?

Present at the June 8 Evanston meeting was Jacques Vallee, a protégé of Hynek's in the sense that he came to Northwestern as a student, but who in fact did much more published work on the UFO problem than Hynek himself. At this time Vallee was about to finish his Ph.D. in Computer Science. He wrote McDonald on July 20 to thank him for some material and to explain that he would return to Europe in a week to interact with scientists there who had investigated UFOs for years, but were even more reticent in their public statements than their counterparts

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in America. He told McDonald that his access to Blue Book through Hynek and his indirect knowledge of APRO and NICAP cases made him conclude that European data was more accurate and better defined.

On McDonald's view that the villain in all this was Blue Book, Vallee could not agree. He saw no reason to back the public attack on Blue Book that McDonald wanted because he failed to see the efficacy of it. He believed the lack of adequate study was largely the fault of the scientific community and therefore only agreed to cooperate in McDonald's strategy of informing scientists about the problem on his return in September. It would be up to McDonald to attack the Air Force. [\[39\]](#)

While Vallee penned his missive, McDonald rummaged around the Blue Book offices at WPAFB for the third time in a little less than two months. His certainty that something was amiss continually increased. Concurrently he believed that he could work hand-in-hand with Colonel DeGoes' review of Blue Book and get corrective feedback into the system. To that end he wrote a six-page single-spaced memo to DeGoes before leaving WPAFB in which he spelled out some of his complaints and suggestions.

In his first point he raised five cases which he looked at and felt deserved further investigation. Then in his second point he alluded to a radar case of August 1965 in which good returns were allegedly received on a number of military radar scopes only to be denied and called unreliable

the next day. McDonald asked, "Did someone remember AFR 200-2 the following day and clam up? Or was it really correct that no reliable Air Force radar fixes were obtained?" Here McDonald raised the old cover-up specter based on Air Force Regulation

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200-2 which made disclosure of a UFO sighting by Air Force personnel subject to ten years in prison and/or \$10,000 fine. This might not have been a wise course of action, for even if the Air Force did not have skeletons in its closet, they now know he suspected they might, and above all else they did have a public image to uphold which criticism from a respected atmospheric physicist could only tend to tarnish.

However, McDonald went further. He wrote several hundred words about the famous Socorro, New Mexico, report in his third point. Socorro was a touchy issue with the Air Force because it was an alleged landing of a craft, with occupants seen on the ground at a distance of less than 200 feet, by an on-duty policeman. Now McDonald wanted to exhume this 1964 case, which the Air Force wished to forget, based on further work done on it by Charlie Moore of the New Mexico Institute of Mining and Technology. So McDonald, in effect, suggested the revival of a case many UFO researchers considered one of the best ever recorded in the United States.

But this was not the end of his recommendations. He laid out the circumstantial evidence for the relationship between the 1965 Northeast power blackout and UFO sightings and stated that many similar incidents on a smaller scale needed probing. He made it clear that if the remotest possibility of the relationship existed the Air Force would be in default if it did not examine the matter.

His other points of interest included a proposal that DeGoes contact Vallee before the latter left for Europe so that DeGoes could obtain a pre- and post-trip briefing on the European UFO situation. Then he went into an extensive discourse on the "swamp gas" explanation proposed by Hynek for the 1966 Dexter, Michigan, sightings. McDonald provided

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considerable chemical detail to drive home his belief that the explanation lacked credibility and also tried to play upon the Air Force public image, as Hynek often did, as a reason for reanalysis of the case. He asserted the Air Force position stood on a sentence in a freshman chemistry text. If Hynek could not bolster it with substantial scientific documentation he argued the explanation

should be withdrawn, "because the Air Force looks pretty silly on this one."

McDonald closed by bringing up his meeting with Rand's Fletcher at NASA and his talks with Price and Ratchford at AFOSR, no doubt to put what he considered pressure on DeGoes. ^[40] He did not seem to realize that the positions he espoused, while held by a number of reputable scientists, would never be aired, particularly by people at Blue Book, which functioned primarily as a public relations project and not an investigatory body. ^[41] He would soon learn this the hard way and would adjust his strategy and tactics accordingly.

One obtains a good feeling for Hynek's position on all this in a July 26 letter to the Lorenzens. He thanked them for the information on McDonald's WPAFB activities, but said he knew about them and it pleased him to have McDonald running interference for him. This made it possible for him to make requests at WPAFB he could not have made previously. On the other hand, he deemed McDonald rude and incapable of scientific cooperation, so much so that after their June 8 meeting he said he would not give McDonald the time of day. Lastly, he asked the Lorenzens to dig around to see how McDonald managed to finance his research. ^[42]

So a definite rivalry had developed between Hynek and McDonald. Although Hynek moved toward a resolution of the UFO question in a

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circumspect fashion for many years. It was not his style to proceed as a bull in a china shop. Then along came the bull in the form of McDonald, who quickly became convinced that scientific pay dirt existed within the UFO data and that it was only Hynek's lack of intestinal fortitude which kept this knowledge from the scientific community. The further he went the more sinister Hynek appeared and the greater the clash in scientific styles -- their personal politics of science -- became.

AUGUST

Two letters from Martin Uman of Westinghouse Labs suggest that McDonald made a few inroads with him. Uman indicated his latest reading included Vallee's *The Anatomy of a Phenomenon*, NICAP's *UFO Evidence* and John Fuller's *Incident at Exeter*. Uman's interest in UFOs stemmed from his work on ball lightning, a phenomenon regarded by some, principally Philip Klass of *Aviation Week and Space Technology Magazine*, as the cause of many UFO reports. Uman suggested to McDonald that clear air ball lightning, that is ball lightning not associated with thunder storms, was a real possibility. ^[43] In his second letter he enclosed a wallet-sized slide-like device for identifying the color spectrum of UFOs. He called it the first

UFO experiment and said he could discriminate between types of streetlights with it. [\[44\]](#)

Although this study is on McDonald there are points where the temptation to follow J. Allen Hynek is strong as witnessed by a number of previous references. I am resisting this as much as possible because the material on Hynek, in my estimation, is incomplete and the scope of the work itself would become too broad were Hynek pursued closely. Nevertheless, there are instances where his actions are important to the narrative and in such situations they will be included.

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Hynek contacted the Lorenzens at what he judged an historic time in August 1966. He wrote a letter to *Science* on UFOs which was initially rejected and then accepted when Hynek, it is rumored, made a veiled threat to the effect that he might make a public issue of the rejection. [\[45\]](#) Moreover, he wrote the foreword to Vallee's new book *Challenge to Science* and wrote an invited piece for the British journal *Discovery* on UFOs. It tells us something about the nature of the scientific community, of Hynek himself and the atmosphere surrounding the practice of borderland science when Hynek states, "I feel like Luther nailing his theses to the Church door." It also pleased him that these public displays of his position would throw a wrench into McDonald's anti-Hynek campaign. [\[46\]](#)

As all the above took place another element in the scientific drama evolving around UFOs already simmered in Boulder, Colorado, home of the University of Colorado. The Air Force wanted to find a university to take on the UFO study advocated by the AFSAB in February 1966. The Air Force approached the University of Colorado and on August 9, 1966 the then assistant dean Robert Low wrote a memo to E. James Archer, Dean of the Graduate School, and Thurston E. Manning, Vice-President and Dean of Faculties, to report on the pros and cons of accepting such a contract. It eventually became known as the "Trick Memo" and an article appeared in *Look Magazine* by John Fuller devoted to it. [\[47\]](#) This would not surface until May 1968, but what is important now is not what many of the conspiracy oriented UFO investigators enjoy emphasizing, that the memo is proof the project was a whitewash, but rather the politics of science so blatantly evident in the memo.

For the memo Low queried a number of people at the university, at the Environmental Sciences Services Administration (ESSA) and at the

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National Council for Atmospheric Research (NCAR), all located in Boulder, and tried to encapsulate their views. Louis Branscomb a professor of physics at CU and now president of the American Association for the Advancement of Science (AAAS) seemed most concerned about the image of the university.

He opposed the project for a number of reasons, but the most important seemed to be that to approach it objectively the validity of the observations would have to be given credence. This, he thought, would call into question many established physical laws, put the work beyond the pale and lose the university more prestige than it could possibly gain by accepting the project. He believed one feasible strategy might be for the NAS to accept the Air Force contract and then subcontract the money to CU.

Gordon Little of the ESSA felt that sometimes a project like the one proposed by the Air Force had to be accepted because of national need regardless of the risks. However, in this case he failed to see the significance of the national need.

On the other hand, Walter Roberts, head of NCAR, tried to get Will Kellogg, the associate director, to take it on, but Kellogg felt too committed to assume the responsibility. Roberts thought the project was urgent, that CU should take it on, and that no kook stigma would be attached to doing so.

Low wrote up his own comments which in my estimation do not so much reek of a whitewash as they do of a promotional effort to convince Archer, Manning and the Board of Regents that the university could take the contract and not get burned. I think the infamous paragraphs deserve quoting: [\[48\]](#)

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The analogy with ESP, Rhine and Duke is only partially valid. The Duke study was done by believers who, after they had finished, convinced almost no one. Our study would be conducted almost exclusively by nonbelievers, who, although they couldn't possibly prove a negative result, could and probably would add an impressive body of evidence that there is no reality to the observations. The trick would be, I think, to describe the project so that, to the public, it would appear a totally objective study but, to the scientific community, would present the image of a group of nonbelievers trying their best to be objective but having almost a zero expectation of finding a saucer. One way to do this would be to stress investigation, not of the physical phenomena, but rather of the people who do the observing -- the psychology and sociology of persons and groups who report seeing UFOs. If the emphasis were put here, rather than on the examination of the old question of the physical reality of the saucers I think the scientific

community would quickly get the message.

There is another reason, it seems to me, to do this. Except in a field like optical meteorology, I can't imagine a paper coming out of the study that would be publishable in a prestigious physical science journal. I can quite easily imagine, however, that psychologists, sociologists and psychiatrists might well generate scholarly publications as a result of their investigations of the saucer observers.

I have not, of course, heard the story presented by the Air Force people. That comes Wednesday morning, the 10th. Ed Condon and Will Kellogg have heard it, however, and they say the project is presented in a very reasonable light.

It is premature to have such an opinion, but I'm inclined to feel at this early stage that, if we set up the thing right and take pains to get the proper people involved and have success in presenting the image we want to present to the scientific community, we could carry the job off to our benefit. At least, it ought not to be rejected out of hand.

It is important to present this now in order to obtain a sense of the thinking taking place at CU and, in particular, on the part of Bob Low who would eventually become administrator of the UFO project. The pot was beginning to boil with big money at stake. The UFO issue was getting hot thanks to prodding by McDonald and Hynek and the AFSAB recommendations stemming from the O'Brien Panel of February 1966. It

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would appear that to some no amount of money could make the risk of prestige necessary in accepting the project worth it, but others such as Low seemed to be planning on how to make the best of it. From this point until well after the project report became public in January 1969 the study remained a central concern to McDonald. He offered his advice, gave it, criticized the project administration, contributed to the firing of two staff members, participated in an expose article in *Look Magazine* on the "Trick Memo" and finally rechecked the investigatory work contained in the final draft -- and stumped the country speaking out against it.

However, at the time of the Low memo McDonald continued his efforts to stimulate interest in the UFO data. He spoke with people at NASA in the Office of Advanced Research and Technology on July 19, and, as promised, followed up his talk with recommendations to Dr. A. J. Eggers. He prefaced his remarks with statements about his own research and what he called the almost unavoidable conclusion that UFOs were extraterrestrial and probably under intelligent control. He suggested that a NASA panel: [\[49\]](#)

1. Do a thorough review of the literature (bibliography attached).

2. Take a thorough look at the Blue Book files at WPAFB.
3. Visit NICAP to study their reports and make the acquaintance of Dick Hall.
4. Conduct a stock taking conference on what had been learned.

Although McDonald began his campaign in a subdued fashion, by mid-August he threw some of his caution to the winds, for in a letter to Dick Hall he brought up the possibility of an article for *Look Magazine*. However, he said John Fuller's articles would come out in September so

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the *Look* editor would not be interested in other pieces for several months. He indicated exploration of other alternatives was already in progress.

Concomitantly McDonald began to branch out with his message at the University of Arizona. He mentioned a talk he planned to give to the Physics Department in a few weeks, to which he hoped to get several astronomers and psychologists with whom he previously spoke. [\[50\]](#)

SEPTEMBER

The first hint of what eventually proved to be a major fly in his ointment came in the form of a letter from Hall in which he discussed an editor of *Aviation Week and Space Technology Magazine*. Philip Klass, who interviewed Hall at NICAP and posited his own ball lightning hypothesis for the explanation, according to Klass, of 80 to 90 percent of UFO observations. Klass told Hall that he wanted to attract a lot of scientists to the study of UFOs and Hall felt because of Klass' prestigious position and connections it was worth cultivating him in order to dissuade him from the ball lightning theory.

The subject changed to Hynek in the same letter. Hall wanted to meet with Hynek and various sociologists and psychologists in the Chicago area now that Hynek had "come out" in his *Science* letter and asked for participation of social scientists in the UFO problem. This could easily be arranged because Bob Hall, Dick's brother, taught in the Sociology Department at the University of Illinois. From the tone of Hall's remarks it would seem that he and McDonald already discussed exposing Hynek's former position of circumspection, for now Hall argued it would be inappropriate to do so, but possibly later, should Hynek attempt an ex post facto doctoring of the historical record. [\[51\]](#)

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During the month of September McDonald made the decision to go public. Much of the planning

evidently took place over the phone because the first indication of this turn about, other than the August 25, 1966 letter to Hall, came in a September 27 strategy letter from Hall. McDonald did not know how to undertake his "coming out" and so it was quite natural to find that Hall, who managed various NICAP publicity campaigns providing advice. In using this tactic they wanted to focus as much publicity as possible on a reputable scientist, with well above-average credentials, who considered the UFO question a significant scientific problem. This is essentially what McDonald felt Hynek should have done many years before.

To make clear that this was not an amateurish affair on the part of NICAP or McDonald I think going into it in some detail is in order. The discussion revolved around the efficacy of a press conference versus interviews. In the above note Hall tried to present what he accomplished to that point and the available options. Speaker of the House McCormack, who conducted UFO hearings in 1958, received word to expect a letter from McDonald, Howard Simon at the *Washington Post* wanted to talk, Phil Klass desired an interview, as did Jean Smith of the local NBC-TV news. Hall said this was as far as he wished to go until he heard further from McDonald.

He would alert the *Christian Science Monitor* and the *Wall Street Journal* believing it best to concentrate on "major national newspapers, networks and syndicates," i.e., Bulkley Griffin of the Northeast syndicate, Reuters (a good contact there), UPI (their expert a personal friend), AP (the aviation editor has an interest), Mutual Broadcasting System radio (a good contact there). Locally McDonald could do Jean Smith's

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WRC-TV news. Herb Davis' WEEL show and spots on the ABC and CBS affiliates. So Hall left it in McDonald's hands, either a press conference or numerous interviews. [\[52\]](#)

Shortly McDonald got a response to the spade work conducted by Hall in Washington. He received a reprint from Phil Klass of his *Aviation Week and Space Technology Magazine* ball lightning article which appeared August 22, 1966. McDonald felt not only obliged to respond with a letter to Klass on the development of his own position on the UFO question, but also with a rebuttal letter to the editor of *Aviation Week* picking apart what he called flaws in Klass' argument.

In his first paragraph McDonald thanked Klass for opening up *Aviation Week* to a UFO debate, but then talked down to him by asserting his own expertise in "meteorological and physical matters" which by implication he intended as adequate rebuttal to Klass, for in the next sentence he made it clear that except for a very small percentage of cases UFOs could not be accounted

for by ball lightning or plasmoid processes.

He tried to impress Klass with the total scientific inadequacy of the Blue Book operation. He apprised him of his talks with Colonel DeGoes and Hynek, but showed concern that the DeGoes panel "had clammed up" and he didn't know their conclusions. McDonald asserted that shortly he would publicly make a statement writing off as meaningless the last ten years of Blue Book work. [\[53\]](#)

In the enclosed letter to the editor McDonald argued that an occasional UFO might be a case of ball lightning, but his extensive studies convinced him "that neither ball lightning nor meteoric events

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nor any other known geophysical or astronomical phenomena seem remotely sufficient to account for the UFOs."

McDonald continued with some debatable reasons why ball lightning was a poor UFO candidate, talked about the poorly labeled ball lightning cases at Blue Book and then launched into an excoriation of the Air Force handling of UFOs which was "seriously lacking in scientific content." Then he dropped his bombshell by saying "the hypothesis that these may be extraterrestrial objects engaged in something that might be regarded as reconnaissance operations slowly emerges as the most acceptable hypothesis." He closed by hoping that *Aviation Week* would call for a review of the available UFO evidence. [\[54\]](#) So as early as September 1966 McDonald was prepared to publicly endorse the ETH.

Aviation Week never printed the letter, yet it marked the beginning of the Klass-McDonald debate. McDonald felt it necessary to cut down an unqualified avionics editor, who though a scientific upstart, could have considerable literary clout through his space industry trade publication. Klass, on the other hand, viewed McDonald as a kook scientist who went off the deep end and might make an interesting story. To complicate matters even further, Klass' ball lightning-plasma hypothesis relied on a phenomenon itself only recently rescued from the waste bin of bizarre notions. And, needless to say, after expending extensive time and effort on his work, as had McDonald, Klass had a very real personal stake in the outcome of the debate. Much more will be heard from this duo as the story progresses.

On September 29 Hall again tried to firm up McDonald's "coming out" plans. It should be kept in mind that NICAP as well as APRO endeavored

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for years to obtain scientific recognition for the UFO phenomenon and continually fell short because, they felt, no well-known scientist was willing to touch it. Now, however, NICAP had found the man who might "blow it all open" in an atmosphere already charged with rumors of a university study of UFOs. It is easy to see, then, why Hall pursued the matter vigorously.

After enumerating several members of the media who would cooperate he cited other avenues which needed to be examined. First the staff at the *Saturday Evening Post* felt McDonald should submit an outline of his proposed article and second Hall believed, since McDonald already contacted *U.S. News and World Report* on the possibility of an article, that he should inform them of his plans to go public as well as other national news magazines. [\[55\]](#)

McDonald, although in high gear, retained some reservations about going public, which I think is a fair interpretation of his September 29 letter to Colonel DeGoes. I think he actually felt that he could threaten the Air Force sufficiently to blackmail Blue Book into mending its ways if its image risked being tarnished by rigorous criticism from a prominent scientist.

McDonald opened his letter casually asking for some ball lightning references he loaned DeGoes, then he expressed disappointment that the cases he suggested for reclassification did not get reclassified. Whereupon in one sentence he let out what galled him most, namely the fact that his July 22 six-page single-spaced memo to DeGoes remained unanswered.

He concluded that lack of communication meant the Air Force did not want changes at Blue Book. He said he would publicly make the same

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strong scientific criticisms of the handling of the UFO problem by the Air Force as he had made in private to Dr. Cacciopo and General Cruikshank and that he wanted this message passed along to Dr. Cacciopo. [\[56\]](#)

There is also the overtone that McDonald knew that Cacciopo didn't want to be left holding the bag if the UFO data actually contained any scientific pay dirt. Since Cacciopo was in charge, scientifically, of Blue Book and about forty other Air Force projects at WPAFB, McDonald probably thought making known his threat to take a strong stand on such a bizarre issue might light a fire under him.

OCTOBER

The next day McDonald sent off a six-page letter to Hall on the strategy and tactics of his

scheduled October 19 presentation as well as his future plan of attack regarding Congressional Hearings. The general points which he wanted to cover in his own words were:

1. My examination of the USAF program leads me to describe it as completely inadequate, from a scientific view.
2. Not only incompetence, but deception of public and Congress has been involved.
3. The percentage of Blue Book reports that properly belong in the Unidentified category appears to me to be low by almost one whole order of magnitude.
4. "Explanations" in terms of misinterpreted geophysical and astronomical phenomena fail to account for many hundreds of UFO reports (both in Blue Book and in outside files such as yours) and other hypotheses such as technological and psychological fail to explain the problem away.
5. After carefully considering such alternative hypotheses, I find from my study of the problem that the most acceptable hypothesis appears to be that of the extraterrestrial nature of the UFOs.
6. Although entirely unwilling to speculate on "reasons" and "motives," I subscribe to the view that certain parts of the total UFO pattern suggest something in the nature of a reconnaissance operation.

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7. I haven't the slightest idea where the UFOs may be coming from and will not speculate thereon.
8. Because of my belief that the UFOs constitute a scientific problem of first-order importance and because of having found from personal experience that the Air Force program is scientifically almost worthless (or worse), I believe that some radical change is now required in the manner in which the UFOs are studied. NASA seems a likely agency to take over the investigation free from all the errors and biases that run through the USAF effort.

On the nature of the press coverage involved, McDonald wanted Hall to decide on the best approach for NICAP and the UFO issue in the long run. Essentially on the horns of a dilemma, he didn't want to be branded a publicity seeker, and yet publicity was just what the issue needed. He said he didn't mind the large press conference approach because he thought he could control it, but he said, "It's the difficulty that if you begin to look like your goal is to get plastered over front pages you lose effectiveness with both scientists and Congress, the ones we want to influence."

With regard to radio and TV McDonald wanted to make appearances, but he wished to avoid any sensationalism or Johnny Carson type talk show formats. He felt radio and TV presentations would be criticized by many scientists and Air Force slanted officials and consequently needed

to be carried off with a low profile.

Then he went into his local Tucson tactics. He planned an October 5 talk in which he would make the same points as in Washington, D.C. He intended to do this on the advice of his local news bureau people. They contacted two newspapers in Tucson, one in Phoenix, a local UPI stringer and the local AP man, all of whom were very interested. McDonald preferred the press conference for reasons of efficiency, but

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felt it would "go over like a lead balloon" in Tucson and so decided on personal interviews.

He would follow the talk of the 5th by another, "Atmospheric Physics and the UFOs," on October 6 to a Department of Meteorology colloquium at the UA. Then there was a possibility of working in a talk while at the University of Washington where he was scheduled to discuss weather modification. As McDonald put it, "may not eventuate, but from here on out the objective will be to spread the word in any good way that affords itself." He regretted the responsibility of his cloud physics course because he believed he should be spending twelve hours a day "boning up on UFO case material."

He thought that the degree of press coverage obtained by his October 5 talk should determine the amount of energy he would put into a letter-writing campaign to Congressmen. He intended to write McCormack, Ford, Rivers, Hutchinson, Stanton, Vivian and most of the Arizona delegation. The thrust of the attack would be to get Congressional Hearings which would move the UFO question from the Air Force to NASA. He hoped NICAP would encourage its 10,000 plus membership to engage in a concurrent letter-writing campaign. ^[57]

At this juncture I think McDonald considered himself to be historically weak on the UFO phenomenon. His WPAFB exploits, trips to NICAP, talks with Hynek and case investigations led him to the ETH, however, he knew he was only familiar with the tip of the iceberg. In order to remedy his deficiency he tried to do his homework as rapidly as possible prior to his "coming out." He doesn't mention this, but I am certain it occurred him that he could look ridiculous if asked detailed questions on a "significant" case of which he had not heard. All of

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this, of course, at a supposedly momentous press conference where he was the touted authority on UFOs.

To this end he asked Hall some substantive questions in his letter of October 1. Hall's response consisted of considerable background material: [\[58\]](#)

1. Excerpt from Arnold and Palmer book re: Rhodes photos.
2. Current list of NICAP Board.
3. Two back issues of NICAP Investigator.
4. Wall Street Journal editorial on UFOs.
5. Date re: USAF unknowns.
6. The information that Brigadier General W. M. Garland was assistant to General Samford, Director of Intelligence, in 1951.
7. The assurance that the sanitized CIA UFO Report (minutes of the Robertson Panel) would be forwarded in a week once the Moss Subcommittee got it released.

The same day McDonald, still hoping I think to blackmail his way off the cross, wrote a letter to Tom Ratchford at AFOSR. He mentioned essentially the same things he put to DeGoes about being disappointed in Blue Book, the lack of communication with him (McDonald), the de facto rejection of his offer to sell the university team approach to prominent scientists, Ratchford's lack of contact with NICAP, and McDonald's belief that the problem belonged at NASA.

In conclusion he again stated that he felt a scientific obligation to speak out on UFOs, that this would entail strong criticism of the Air Force and include demands for radical changes in the handling of the entire UFO matter. [\[59\]](#)

More of McDonald's thinking falls into place when we view some of his comments to Tom Malone. He argued that the most recent Pentagon

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Public Information Office release on UFOs (September 1) showed absolutely no change in policy. The DeGoes panel seemed moribund and therefore he decided to give up on the Air Force. It seemed the only way to get the problem out on the table would be to make the Air Force dirty linen public.

He filled Malone in on his plans and then turned to Hynek's letter to *Science* which only asked for greater attention to be given UFOs and yet did not publish. McDonald heard Hynek went to a columnist with the letter and got it published in a batch of papers along with a protest to *Science* which resulted in Philip Abelson, the editor of *Science*, calling Hynek to tell him that if the letter were shortened it would be published. [\[60\]](#) Although Hynek did apply some pressure to get his letter into *Science*, in fact he did not get it into a newspaper, which illustrates the manner in which rumor spreads and is exaggerated when the scientific stakes are high. [\[61\]](#)

Although McDonald verged on going public, Hall dropped him an infuriated note to indicate his displeasure with the effort as a whole. Hynek, who seemed to be making a turnabout, was quoted in the NICAP Investigator as saying the Air Force research on UFOs did not stand up to scientific scrutiny. However, when the *Washington Post* queried him on his comments he said the quote was taken out of context. On another front Klass of *Aviation Week* appeared to be on the attack. This time it was Hall and NICAP, according to Hall, who were totally misrepresented in an October 3 *Aviation Week* article based on an interview Hall gave to Klass. ^[62] Probably at this point both Hall and McDonald realized that Klass was not just another journalist with a casual interest in UFOs, but an adversary of formidable proportions.

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McDonald gave his presentations on October 4 and 5. The only correspondence he received from interested scientists was a letter from Gerard Kuiper of the Lunar and Planetary Laboratory at the UA. He made several objections to McDonald's argument for the ETH. He first suggested that a few poor explanations of unknowns were hardly strong evidence. He then said that, given the difficulty of making one launch to the moon, the probability of multiple independent launches from other planets to Earth was unlikely. He further went on "tongue in cheek" to say he would prefer to believe that the ghosts of the Aztecs or Incas were returning to this planet than question the validity of the law of the conservation of energy on the basis of a few reports from untrained observers. He closed with a good conservative statement. "In my judgment the only defensible position a scientist can take here is that there are unexplained (terrestrial) atmospheric phenomena." ^[63]

In McDonald's reply he asserted that in a short talk it was not possible to present the "body of evidence" pointing in the direction of the ETH, which he emphasized was only an hypothesis. He must have shocked Kuiper considerably when he proposed the most likely alternative hypothesis to be an unexplained psychic phenomenon. McDonald showed his awareness of the precariousness of his scientific position when he said, "That publicly espousing such an hypothesis (the ETH) even in the pussyfooting language of 'least undesirable hypothesis,' is professionally risky, is very clear to me." From this comment it is probably safe to surmise that McDonald still did not see the UFO issue as becoming an all consuming endeavor which would result in his production of research papers dropping from 64 between 1951 and 1966 to zero between 1967 and 1971. ^[64]

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Although McDonald was familiar with press coverage, he expressed surprise at the results of his Tucson "coming out," and in fact began to reconsider his Washington plans. He wrote Hall that he groaned at the press errors, but that some of his points did come across in the Phoenix and Tucson stories; however, the *Los Angeles Times* included a statement that he believed in "persons from outer space" and the AP wire story asserted he believed in contemporary CIA involvement. This made McDonald wonder about the efficacy of using the mass media in the UFO campaign. He said he didn't mind standing up for a far-out view, but he didn't like being made a fool of through misquotes and slanted remarks.

Consequently, he stated to Hall that he refused all local media coverage for the past several days and did only one ten-minute spot for CBS News in New York. He said he wanted to reduce the number of the press at his Washington talk to good nationally syndicated people, but realized the paradox that almost all the people Hall suggested fitted that description. He concluded that he would leave the arrangements up to Hall who should tone down the CIA angle and make the discussion turn on the scientific inadequacy of Blue Book.

On the local scientific front feedback from McDonald's colleagues proved negative, so he said, and they showed concern for the reputation of the UA. Undeterred McDonald planned to give a talk to the Aerospace and Mechanical Engineering Seminar on October 11 and looked forward to a colloquium in the Psychology Department. It would appear that he wanted to cite the reactions, which he expected to be "snorts," from both groups in his Washington talk.

Also he raised an aspect of the possible, in his mind, cover-up problem. The negatives of some photos he knew of were allegedly

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confiscated by the FBI and some Polaroid prints from California were supposedly lost by the Air Force. McDonald thought NICAP should make a list of such incidents for future reference. Evidently the possibility of a large-scale governmental cover-up of the UFO problem was no longer a matter of idle curiosity for him.

Referring to Klass, McDonald felt that his article was full of holes. Based on Klass' memo McDonald said, "don't think I'm going to like him either." [165](#)

McDonald sensed a battle or at least a skirmish in the air. However, he did not know that Klass relished a good battle as much as he did and possibly more. Klass prepared for McDonald as McDonald prepared for him, but I believe Klass viewed McDonald as big game deserving of equally large bore ammunition, whereas McDonald seemingly underestimated Klass as an

upstart, but one who would adhere to the generally accepted forms of academic combat.

The following day McDonald submitted a report to Dr. A. B. Weaver, Chairman of the Space Sciences Committee, which provided him with the \$1300 seed money to begin his studies. McDonald informed the committee of the \$400 he spent on phone interviewing, his proposed talk at the D.C. chapter of the American Meteorological Association and his campus colloquia activities. Probably looking to future funding from the same committee he pointed out that he had not sought support elsewhere. ^[66]

On October 15 a press release announced that Dr. E. U. Condon, a respected physicist, past director of the National Bureau of Standards, past President of the AAAS and American Physical Society, would head the Air Force funded study which would take place at the University of Colorado. Although attempts to obtain a university and principal

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investigator began in May it seems more than coincidental that just as McDonald prepared to go public and prior to what he anticipated to be a significant Washington talk, that the Air Force took the wind out of his sails with its announcement.

No doubt Condon had little idea of what was in store for him. However, he probably received an inkling from the three-page letter Hall sent him on the nature of NICAP and its position on UFOs. In it Hall did a number of things:

1. he offered NICAP's cooperation,
2. provided a copy of *The UFO Evidence*, a fact sheet, staff biographies, names of board members and a background on NICAP,
3. listed the membership at 10,500,
4. asserted NICAP's commitment to the ETH,
5. dropped the names of Cape Kennedy, Huntsville, Goddard Space Flight Center, General Electric, Westinghouse and Bell Telephone as employers of scientists who were NICAP members, and
6. volunteered NICAP investigators for a UFO network.

Hall said that Condon's agnostic position was fine, NICAP only wanted the thorough and impartial investigation which the Air Force failed to provide. Then he went into the nature of UFO data problems and most importantly offered to provide evaluations of the kooks and frauds in the field. ^[67]

What Condon thought is anyone's guess, but based on his later statements it would seem likely that he found it a bit incongruous for a group supporting the ETH to offer its services to evaluate

kooks and frauds when the position of NICAP itself must have struck him as bizarre.

It pleased McDonald that the Colorado program appeared to be taking shape with the aid of NCAR and ESSA. He wrote Tom Ratchford at AFOSR to

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thank him for a copy of AFR 80-17, an Air Force regulation governing the reporting of UFOs. He expressed his delight that a scientist of Condon's standing would head the project, but of course felt compelled to proffer some suggestions.

First, he thought that Blue Book, given its past record, could not be entrusted to pick the sightings the CU project would study. He heard a rumor of such a plan and considered it his obligation to warn against it.

Second, he argued that the copy of AFR 80-17 which Ratchford sent him did nothing to insure that the university investigative teams could go "to air base level and talk to radar operators, pilots, tower personnel, etc." The AFR 200-2, predecessor to AFR 80-17, prohibited this and McDonald argued the need to remove such an obstacle to insure the highest quality investigatory work.

Finally, he discussed his campus colloquia and pointed out that the objections which he could not overcome concerned the impossibility of controlled laboratory experimentation with UFO data and therefore he believed the CU project should contain scientists accustomed to working outside a controlled situation, such as Geophysicists. McDonald asserted that scientists who normally read meters would balk at personal testimonials. He admitted the data was messy and not amenable to immediate plugging into a computer, but he said, "not all scientific problems come neatly packaged."

He again volunteered his six months worth of experience to the project and invited Ratchford to the October 19 Washington talk. He sent a carbon to Ed Condon. ^[68] Here I believe McDonald misread the situation and was obtuse to think that having publicly presented the ETH

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as "the least unlikely hypothesis" that the CU project or the Air Force would want to touch him with a ten-foot pole. At this point he was probably already branded as a fanatic, but unfortunately for the non-believers, a fanatic with credentials which opened some doors in high places and

enabled him to break down others.

Giving Klass rather short shrift, McDonald dashed off a condescending note on October 15. In the first paragraph he alleged the quantitative aspects of Klass' plasma hypothesis were untenable. In the second paragraph he said he refused to appear in a Washington radio debate with Klass because it didn't seem like much of a scientific nature could be accomplished. In closing he told Klass that he could be contacted at NICAP while in D.C. and he hoped to see him at the AMS meeting. [\[69\]](#)

This study deals with the issues, strategies, tactics, and personalities involved in McDonald's attempt to shift a paradigm, but there was also much going on in the way of investigation on the part of McDonald. It is an element of the context, but there is really no way to adequately convey the amount of time he put in, or all of the problems he encountered. As a sample of what continually passed between McDonald, NICAP and various individuals with whom McDonald corresponded let me present verbatim the first four paragraphs of a letter from McDonald to Hall.

Dear Dick:

I enclose miscellaneous items:

Letter on my phone interview with Uzunoglu re August 1, 1966 case.

Local press clip re APRO'S offer of aid to Colorado.

Letter from Madeleine Ward for your files on that case.

Copy of some notes on a phone talk with local FAA RAPCON man.

Copy of letter from M. Ortiz of Nogales re airship matter.

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Copy of letter from S. E. Palmer, concerning a 1948 sighting.

Copy of letter from R. L. Gray concerning a June 1966 sighting.

Copy of several transcriptions of NY Times stories on 1946 ghost rocket case. References are from your book, so you probably have them.

Thanks for all the reference material on the Cherry Creek case. It leaves me just a bit in doubt of the protocols of the case. Fred Fair's comments indicate mixed emotions, don't they? The mental level of the boy (16 yrs. in 7th grade) argues against clever hoaxing, but also leaves one uncomfortable about his veracity. Maybe we can discuss it further in Washington. I was hoping it would be one of the stronger cases, but it appears to have some thin spots.

I've been trying to dig out examples of good cases with animal reactions, as these have a bearing on the hallucination hypothesis. That was how I had become particularly concerned with Cherry Creek. Maybe we can run over the animal question next week. Do you think the Belle Glade,

Fla., cattle-stampede case is reliable? I found about a dozen animal cases including Le Roy Kansas 1897 and the Portland pigeons of 7/47.

Wm. Rhodes sent me his sole clipping of the July 9, 1947 *Republic* and I'm having it photocopied. Very faint vapor trails do seem to show on the halftone, streaming at sharp angles off either tail of the "heel." He also gave me a copy of Vol. I, No. I of *Fate Magazine*, which he'd had in his files for years. Has Arnold's original account of the Mt. Rainier case. I shall be talking about that case next week, since the radiosonde data don't bear out Menzel et al.

In the way of tactics McDonald made it clear to Hall that based on press accounts and comments from physicist colleagues he went too far in his October 5 and 6 talks. He tried to convey his position in terms of hypotheses, but in retrospect felt his real convictions were obvious to the audiences. Consequently, since the ETH could not stand by ordinary scientific demands he decided that he would word his Washington statements much more carefully.

He remarked that Colonel Steiner (AF) contacted him. Phil Klass spoke to Steiner knowing that he also served as program chairman for the Washington, D.C., chapter of the AMS. As a result Steiner knew about McDonald's refusal to appear in the radio debate, and asked if Klass

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could have dinner with them in D.C. McDonald said "not for his sake," that he felt Klass attacked the problem like Menzel of Harvard. McDonald averred that Klass' talk with Steiner, who was in a sensitive position with the Air Force, made Steiner concerned that McDonald planned a press staging event at the October 19 AMS meeting.

Even though McDonald intended to begin his Congressional letter writing campaign ten days earlier he did not do so. He said he wanted to discuss this tactic while in D.C., since it now seemed obvious the Air Force could assuage any attempt to get Congressional Hearings by merely pointing to the Colorado study as the answer. He proposed that probably the best thing to do now was to insure that the Colorado study was well conceived. [\[70\]](#)

This intruding on, and attempting to intrude on, the Colorado planning would occupy McDonald for almost another year-and-one-half. He offered much assistance, consulted a little, wrote numerous letters, developed an extensive intelligence network inside the project itself and generally made himself a thorn in Ed Condon's side.

The actual announcement of Condon's appointment to head the project received coverage in all the major newspapers and included quotes with respect to his position on the issues. From the standpoint of the politics of science and especially' with respect to his later quotes in various

speeches and interviews it is interesting to note a few of his remarks at this juncture.

He stated that he did not exclude the possibility that some of the UFOs contained outer-space visitors, but he said he would need incontrovertible evidence to hold such a position. He asserted that extremists of the debunking school as well as the ETH school were just as

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bad to him, but he did not consider all ETH proponents nuts. He went on to say the 5 or 6 percent of the Air Force reports which could not be explained over the past twenty years would have to be taken seriously, but he added that just because an object could not be identified it was not necessarily an extraterrestrial visitor; yet on the other hand, "There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy." Condon alluded to the persecution of various early astronomers such as Copernicus, Galileo, and Kepler and argued that those who took the UFO question seriously experienced similar ridicule. He claimed that an important candidate up for election told him privately that he saw a UFO, but he and his wife agreed that to report it prior to the election would open the candidate to derision.

This is perhaps the appropriate time to mention the ex post facto reconstruction of this interview by some of those who were eventually critical of Condon. They interpreted the election anecdote as a reminder that Condon himself was in the race for a board of regents seat. Some were even so cynical as to suggest he took the CU UFO position only to get publicity for his campaign.

In the same interview Condon criticized scientists who did not take the opportunity to examine the Hillsdale, Michigan, sightings earlier in the year. He went on to advocate the scientific method as the best means of dealing with the subject and intimated that the five or six hundred unidentified cases he would have to work with would be ample for investigative purposes. [\[71\]](#)

All things considered he gave an impressively fair interview. He neither came down on one side or the other and responded in the fashion one would expect from the objective researcher preparing to wade into a

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project. From those portions of the article which McDonald underlined it would seem that Condon's scientific style pleased him.

Nevertheless he went ahead with his scheduled AMS talk on the 19th. In it he discussed various UFO hypotheses, Blue Book handling of the problem, CIA involvement, and the "why"

questions associated with UFOs. With regard to the hypotheses explaining the phenomenon he laid out eight:

1. Hoaxes, fabrications and frauds.
2. Hallucinations, mass hysteria, rumor phenomena.
3. Lay misinterpretation of well-known physical phenomena.
4. Advance terrestrial technologies.
5. Poorly understood physical phenomena.
6. Poorly understood psychological phenomena.
7. Extraterrestrial probes.
8. Messengers of salvation and occult truth.

He argued categories 1-4 accounted for most sightings, that 5 and 6 probably explained a few more and that the adherents of 8 failed to shed scientific light on the problem and so he dismissed them. For him this left 7 (extraterrestrial probes) as 'the least unsatisfactory hypothesis.' He believed 6 was the only important alternative.

After disposing of all but hypothesis 7 McDonald went on to criticize Blue Book handling of the question, the Robertson Panel of 1953 and various Army, Navy and Air Force regulations which made studying the problem difficult. He concluded by saying he couldn't answer the "why" questions such as:

1. Why no contact?
2. Why so many sightings took place in remote areas?

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3. Why nighttime observations were more common than daytime? He felt it was too early for such speculation. ^[72]

It is clear from this talk that McDonald adopted essentially the NICAP position. However, he was sensitive to his station in the scientific community and to criticism by his peers, so he also developed the circumlocution that extraterrestrial probes were "the least unsatisfactory hypothesis" to account for the observations. This enabled him to argue that he was not an outright advocate of the ETH, but as a scientist who spent six months looking at the data this was his tentative conclusion.

The McDonald correspondence does not indicate much publicity came from his talk, although the *New York Times* did pick it up. It is not clear whether it was intentionally avoided by McDonald or if the announcement of the CU study made his remarks anticlimactic. There is one

reaction, however, which is worth noting.

Phil Klass seems to be the only one who responded directly to McDonald in written form. He asked for a breakdown of UFO cases from the categories of McDonald's AMS talk and then asked what McDonald thought should be done if the Colorado study found proof of extraterrestrial visitation:

1. Appropriate \$5-10 billion to the USAF to enable it to develop a defense in event such extraterrestrial visitors prove to have a hostile intent?
2. Seek United Nations action to develop an earth-wide defense against the possibility, however remote, of hostile action?
3. Attempt to "catch" an extraterrestrial visitor, however futile that might seem to be, for a first-hand examination?
4. Launch a campaign to "ignore them," stop looking, gawking, chasing after UFOs and simply ignore them until such time as they see fit to seek formal contact?

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He said he doubted McDonald's hypothesis, but that his questions were nevertheless posed in a serious vein. [\[73\]](#)

A few days later McDonald discussed the abstract of his talk in a communication to Hall. He explained that he now realized that too strong a statement on the ETH to ill-informed scientists scared them, so he decidedly toned down his remarks. He said he would run off 200 copies and send a few out, in particular to Ev Clark, Elliott Carlson (reporters), and Phil Klass.

To give some indication of where on the UFO spectrum McDonald fell at this point it is interesting to note that he spent an entire paragraph taking pot shots at the alleged seduction of a Brazilian farmer (the Villas Boas case) by extraterrestrials who purportedly conducted a cross insemination experiment with him. To quote McDonald, "it out-Barney's Hill" (a famous abduction and physical examination case alleged to have occurred in 1962 in New Hampshire). So McDonald, it would appear, remained circumspect even with Hall when it came to occupant cases at this time, which probably means he made light of them in general. This is a typical phenomenon within UFO research. The researcher starts off very conservative and slowly develops an ever more radical position with respect to the data.

McDonald also stated that Colonel DeGoes sent him a chilly note which made it appear that McDonald's assistance was no longer wanted. There was no comment on any of McDonald's suggestions concerning Blue Book, but he returned the ball lightning references. This brought McDonald back to Klass, who according to McDonald, became the target of considerable

laughter from his UA colleagues when McDonald raised the

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clear air ball lightning hypothesis. However, McDonald entertained it as an interesting argument, just not the answer. [\[74\]](#)

He further commented to Hall that Klass' letter full of questions was "not so annoying as his earlier stuff" and wondered if it might suggest a change of heart. He knew very little about Klass, this would seem obvious. However, McDonald considered questions 1 and 2 loaded and intended to approach them with that in mind.

He was again critical of the Villas Boas case then running in the most prestigious UFO publication available, the British *Flying Saucer Review*. He couldn't understand why they presented such junk along with what he considered good research. He marveled at how Jacques Vallee could recommend it as the best UFO periodical. Time would eventually change his scientific consciousness as he became more familiar with the data, the history of the UFO phenomenon and those individuals who had spent a good portion of their lives pursuing the subject.

An inkling of this is received in the same letter which is based on extensive discussions McDonald conducted the previous week at NICAP with Hall and Keyhoe. As a result he reread Keyhoe's book *Flying Saucers Top Secret* in a new light and now realized that the conspiracy experiences about which the Major wrote, and many others of which he spoke the previous week, were much more credible than he (McDonald) had thought.

As a result he could understand why NICAP took such a strong conspiracy stand, but he still could not accept the position. He felt that Blue Book would have been administered by clever top-level people if a conspiracy existed rather than the string of incompetents that held the post. He closed his conspiracy discussion on a note which I

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think expressed his real puzzlement, "Damnedest mess in the world, isn't it?"

In conclusion he mentioned his attempts to schedule talks at other universities and that he intended to pursue a number of cases which Menzel explained as atmospheric inversions and refractions by getting some part-time help to plot radiosonde data. [\[75\]](#) This was the start of the long drawn out historical search which all scientists new to the subject invariably undertake.

Who and what to believe? How to check old reports? What has the Air Force done or not done? If there is scientific pay dirt, can it be found? If it can be found, at what price in terms of one's academic status, emotions and regular work? It seems that just such attributes and conditions surrounding borderland science subjects make them too elusive and risky to examine at any more than a cursory level for most scientists.

This point received emphasis the next day in a long letter to Hall in which McDonald went into detail about a phone conversation with Clyde Tombaugh, the discoverer of Pluto, about a UFO observation Tombaugh made in August 1949. Leaving aside the case material, the interesting aspect of the conversation from the standpoint of this study is that Tombaugh, according to McDonald, knew that Hynek was changing his position and felt it would be good if a careful examination of the UFO data were undertaken by reliable people because he felt "that fanatics have scared most scientists off." McDonald decided to send him a summary of his AMS talk. ^[76]

Sometime in early October Keyhoe and Hall received invitations to provide briefings to the CU project staff. In a note to McDonald, Hall indicated that he would stop off in Chicago to see his brother Bob who

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taught at the University of Illinois and receive some pointers on briefing academics.

In the same communiqué Hall said he put off Klass who wanted to look at the Socorro, New Mexico, file. He said he didn't like the idea of withholding evidence, but he feared the manner in which Klass would distort the facts given his previous reportage. ^[77]

It is interesting to note that, at least as far as I know, neither Hall, nor anyone else of his persuasion, ever applied the same type of argument to the Air Force approach to non-release of information. NICAP, APRO and the other groups had and have a vested interest in keeping the UFO issue alive, leaving aside for the moment the actual question of whether or not the data is anomalous, and no one would deny that those groups engage in a bit of hyperbole on occasion. Consequently, it would seem very rational policy on the part of the Air Force to withhold information in the hope that this would reduce the grist for the UFO groups and pulp publishers. We must keep in mind, that at least at the Blue Book level, the Air Force people in charge considered UFOs a laughing matter, a public relations assignment. ^[78] And it should be remembered that it was at this level that the UFO groups addressed most of their anti-Air Force attacks.

By late October the UFO picture began to change for McDonald. The Colorado project got under way, his speaking engagements commenced on a national scale, his concern for what he

perceived as Hynek's about face on the issue crystallized and Klass began to dog his tracks. It might be well to remark on the intensity of McDonald's correspondence at this point. The dates bear out the volume of material sent and received. For he and Hall, to say nothing of the others in the field, the issue

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was white hot. It was finally to obtain scientific due process, which they felt would surely vindicate many years of heretofore unrecognized work.

In a missive to Hall, McDonald mentioned just receiving an advance copy of Vallee's new book with a foreword by Hynek. We can see McDonald's views are well formed on Hynek at this point as he comments that "Hynek's introduction is a masterpiece in trying to cover his rear."

McDonald showed marked concern about the CU project already. He enclosed clippings from the *Denver Post* and *Rocky Mountain News* which indicated such a scorn for the UFO study on the part of the project's chief administrator, Bob Low (author of the previously discussed Low Memo), that Condon needed to write a letter to the editor to clarify the position of the project.

Voice of America indicated an interest in doing an interview with McDonald and he accepted. They also planned spots for Donald Menzel of Harvard and Ralph Lapp of Quadri-Science Incorporated, which reminded McDonald to send a copy of his AMS talk to Lapp. Also Mike Levitas of the *NYT Magazine* phoned to ask McDonald to do a 3000-word story on UFOs. McDonald felt it an ideal place to make a few points on USAF mishandling of the problem.

McDonald did more ancillary work to prop up his position and while doing so convinced Hall that for future reference NICAP should have on hand such reference volumes as: MacGowan and Ordway, *Intelligence in the Universe* and Jackson & Moore, *Life in the Universe*. This was for purposes of showing how "so-called" authorities on life in the universe dismissed the UFO problem. McDonald, of course, assumed the problem was about to break and such remarks would be appropriate. Also prior

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to the "breakthrough" McDonald wanted as many negative citations on UFOs as possible by important scientists. These he could in turn refute in the cogent papers he intended to write urging further consideration of the UFO question as one of the most important scientific problems of our time. [\[79\]](#)

Meanwhile Klass began to spread his own gospel of the plasmoid and check on McDonald's reputation. He wrote Bernard Vonnegut, of Arthur D. Little, Inc., forwarding his *Aviation Week* article on UFOs and asking Vonnegut about McDonald's expertise in upper-air physics. Vonnegut said he was not sure. His impression was that McDonald had not been active in the area for some ten years, but that "most scientists share my good opinion of Jim McDonald." McDonald's commitment to the ETH surprised Vonnegut. ^[80]

Not one to permit an offer of proselytizing to go by McDonald contacted Mike Levitas of the *NYT Magazine*. He outlined the main points he wanted to make in his article, namely, how and who should study the UFO issue, an overview of the problem, his own work in the area, the nature of the reports, and the spectrum of UFO hypotheses. He said he believed the real problem would be keeping the article down to 3500 words, but if Levitas remained interested he would write it up. ^[81]

In a note to Hall, McDonald presented, at least from his vantage point, a new tactic. He wanted to check on scientifically trained UFO witnesses. Did Hall have a list? The object of this tactic would be to counter the argument that an untrained layman is easily fooled by natural phenomena of one kind or another.

He also mentioned his disappointment at not being able to address a group at the University of Washington at Seattle on UFOs instead of

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the scheduled topic of weather modification. He received a polite letter from an officer of the group, Richard Reed, who once wrote an article attributing all UFOs to lenticular clouds, "putting the nix" on the change in subjects. McDonald suggested to Hall, however, that he (McDonald) might speak to a small group of the Seattle NICAP Subcommittee if it could be arranged. ^[82]

Hearsay about the CU project was already beginning to fly. Hall wrote McDonald that a journalist friend who knew Condon said Ed planned an instrumented look at the phenomenon using fixed cameras, spectrosopes and magnetometers. Hall remained skeptical, considering the project budget, but pleased that Condon thought big.

In addition, Hall raised an issue always considered touchy by those who classify themselves as serious UFO researchers. ABC just produced a program on extraterrestrial life and, as Hall perceived it, linked NICAP to the cultists and the annual desert conventions of contactees and kooks at Giant Rock, California. ^[83] This infuriated him for he realized it did the cause immense

harm.

A few days later McDonald wrote a note to Ted Bloecher, a veteran UFO watcher, who, as a traveling stage actor, was in the process of combing microfilmed newspapers in each city he visited (eventually 90 cities and 140 newspapers) to ascertain the dimensions of the UFO wave of 1947 in the United States. [\[84\]](#)

McDonald again got off on Hynek. He said he could not understand how Hynek could condemn the Air Force after eighteen years of equivocating contact with the data himself. He expressed distress with Hynek, but was appreciative of the salutary effects his new position

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might have. He said that Vallee urged him to join forces with Hynek, but he found Hynek's past actions too disquieting to permit it. [\[85\]](#)

Now that McDonald did more talking on UFOs he also gave more consideration to how the talks were couched. In a letter to Bill Weitzel, the NICAP investigator who compiled the 128-page report on the Ravenna, Ohio, case, McDonald commented on Weitzel's critique of his October 19 AMS talk. He agreed that perhaps some of the "whats" and "whys" of the UFO phenomenon might deserve speculation in future talks for the benefit of some sincere doubters, but he cautioned that Menzel types could take quick advantage of this tactic to put the entire matter into the realm of science fiction and "smear one's whole approach."

McDonald indicated his satisfaction with the reception he received at the UA Psychology Department colloquium because no one yelled hallucination and good questions were asked. He said that he now planned to speak to the psychologists at Arizona State University and at the University of Washington. [\[86\]](#)

The quantity of talks increased as witnessed by a letter McDonald sent to Hall in which he discussed his tentative schedule. He intended to speak with a group of astronomers at UA in a few days and on November 9 to the astronomers at Kitt Peak National Observatory in Tucson. Then a colloquium before the UA Electrical Engineering Department, a talk to the Tucson Amateur Astronomers Association, and on December 6 to the local chapter of the National Pilots Association. Word of his talks spread rapidly so he also received invitations from civic groups like the Lions Club, however, he adopted a policy of turning down these latter requests to focus on those groups which he felt might bring scientific or observational weight to bear on the problem. [\[87\]](#)

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The tiff between McDonald and Klass acquired some positive kindling, from McDonald's point of view, with a letter from Martin Uman, whom Klass consulted on his (Klass') ball lightning hypothesis. Uman said Klass' first article wasn't bad, but in his latest one he misquoted Uman's remarks to make a stronger case. Uman considered it distorted and, in general, terrible. He concluded "Klass seems to really have gone off the deep end." [\[88\]](#)

Simultaneously NICAP prepared for the CU briefing. Hall informed McDonald that he spoke with Mary Romig of the geophysics and astronomy department of Rand Corporation and he detected a "believer." She provided Hall with various nonclassified Rand UFO reports, said she would digest and forward some "Official Use Only" material and would report on her trip to CU on November 21, a week prior to the NICAP CU visit. Hall asked McDonald for any advice he might have on tactics for the CU session, saying that he worried about conveying the signal to noise ratio problem to Condon and how the project might circumvent it. [\[89\]](#)

(It is a commonly held view that 80 to 90 percent of UFO reports represent noise and not signal. The possibility bothered Hall that the project, because the staff did not consist of scientists with backgrounds in the UFO field, might inadvertently concentrate on the noise at the expense of the signal.)

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McDonald was off on one of his favorite topics a few days later in a note to John Fuller, author of *Interrupted Journey*, namely, Hynek. After thanking Fuller for telling Dial Press to forward a copy of the new book McDonald commented on Hynek's shifting stand, but also ignored

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history to admit that Hynek's letter in *Science* "was good and will have its beneficial effects, without a doubt." [\[90\]](#)

The next day Hall wrote McDonald a letter which sheds considerable light on the extrascientific problem of cover-up versus foul-up which often occupied the minds of those convinced that UFO data deserved a fair hearing and were puzzled as to why it failed to obtain one. Was it a question of government conspiracy, or ineptitude on the part of the Air Force?

Hall first mentioned the international implications. He stated that an international foul-up seemed possible because the prestige of the United States Air Force appeared so great that it coerced any foreign interest underground. Talks with foreign military attaches, according to Hall, showed that

they ignored official Air Force pronouncements and pursued a quiet course of investigation.

However, in the mood for more speculation. Hall did so for the domestic situation. I think this is worth alluding to because all through McDonald's UFO campaign this cover-up versus foul-up question played a role and this is the point at which it began to occupy his thinking with increased frequency.

Hall explained that he thought possibly a high-level cover-up existed along with a low-level foul-up. The former, in the late 1940s, he believed due to the concern that UFOs were real and possibly Russian, and the latter to any one or combination of things ranging from high-level pressure for good public relations, to disinterest, incompetence, or following the path of least resistance. Hall generally avoided such speculation on what he considered an unanswerable question, but the

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previous weeks talk with McDonald and Keyhoe gave impetus to his thinking in the area. [\[91\]](#)

McDonald liked to keep Tom Malone informed of his progress. He wrote that things were moving along well and that the Psychology Department colloquium pleased him. So much so that he managed to set up a joint one with Psychology and Sociology at the University of Washington. He was displeased, however, that he had heard nothing from Condon even though he forwarded Condon and Will Kellogg, Associate Director of the NCAR, a copy of the October 19 AMS paper and Ratchford and Price assured him that Condon would be in touch. [\[92\]](#)

At this juncture I believe McDonald felt, as some of his letters suggest, that he was the most qualified scientist in the country to pursue the UFO problem and/or consult on it. I think he wanted to head the Air Force study and now that he couldn't, he believed he should make an effort to guide it, to give it the advantage of his past work and try to help them avoid the pitfalls he knew existed. Consequently, it would appear that his expressions of concern for lack of contact with Condon were indirect messages which he hoped would filter back to Boulder.

Because NICAP played such an integral role in the Colorado study and because the Colorado study took up such a significant part of McDonald's energies we must take into consideration, where relevant, some of the interactions between NICAP and the project in Boulder which don't directly involve McDonald. For instance, in early November Major Keyhoe, the Director of NICAP, wrote Condon to ask for assurances that the study proceeded completely independent of outside control. Keyhoe found this necessary because he wanted NICAP associated with it, as the largest (some 10,500 members) saucer-watcher group in the

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country, and he saw the possibilities of this project being the big breakthrough. On the other hand, he had battled with the Air Force over UFOs for sixteen years (under NICAP auspices since 1956) and now Air Force money funded the CU work. So he feared a whitewash and wanted assurances against that, but also showed anxiety over association with an Air Force sponsored project, and how the relationship would be construed by the faithful NICAP membership which kept the organization afloat. Moreover, by this time he received word about the Colorado press stories on Bob Low and probably put the worst construction on them.

Condon replied that he considered Keyhoe's request proper, that the study was totally free from outside influence and would act in the national interest. He said two ground rules were followed:

1. Existing interpretations be accepted to the extent that they are supported by all the evidence.
2. Existing fact -- i.e., files of sighting reports -- be tested, to the extent possible, for validity and internal consistency.

Condon argued the necessity to start from scratch, that Blue Book efforts could not have been much, given their staff size, and he felt no need to accept Blue Book, NICAP or APRO interpretations at that point. [\[93\]](#)

So Condon knew he needed help from Keyhoe and didn't write to him as he might write to a kook, even if he considered Keyhoe one. He probably realized the noise Keyhoe could make on Capitol Hill and having once jostled with Senator Joseph McCarthy's Senate Subcommittee in the early 1950s, I don't think he wanted Keyhoe to turn his scientific investigation into a circus. No doubt the Air Force provided him with a dossier of Keyhoe's past activities and consequently Condon knew exactly the kind of man with whom he dealt.

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The scope of the UFO problem increases when we look at a note Ralph Rankow, a New York based NICAP investigator wrote to McDonald. He said he spoke on UFOs at the United Nations (probably to some unofficial group). An international team approach interested McDonald so this undoubtedly excited him. Rankow said, however, that in reply to a follow-up letter to U Thant in which he (Rankow) offered to speak at greater length, probably to the General Assembly or Security Council, Thant indicated it would be necessary to get a "member state" to sponsor a resolution to that effect. Rankow did not see much chance of that happening, and agreed with

McDonald that Colorado remained the best bet for cracking the problem. [\[94\]](#)

It was not until 1967 and 1968 that McDonald became more interested in the international aspects of the UFO issue and its solution. By then the prospects of a positive reading from the Colorado Project appeared unlikely. As a consequence, McDonald addressed the Outer Space Affairs Group at the UN, wrote on the international implications of the UFO problem, investigated cases in the Australia-New Zealand-Tasmania area, tried to contact interested Soviet scientists and attempted to keep abreast of the European UFO situation through an extended correspondence with Aime Michel in France.

But that is getting ahead of the story. Toward the end of 1966 he was only beginning to lecture extensively on the domestic front and to do so he began to research classic cases.

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Now that McDonald lectured more he also did an increased amount of homework on classic cases. He wanted to be certain of his facts before going far out on a limb which some astute skeptic might attempt to saw off. In this regard, to illustrate one of the problems of borderland science, it would probably prove useful to look at a small bit of McDonald's thinking on the classic Kenneth Arnold sighting over Mt. Rainier, Washington. This is the sighting which set off the UFO wave of 1947 and brought the subject into the news. McDonald planned to speak at the University of Washington and cite the case because as a regional example it would have more impact. A short quote might help to exemplify the problem which he faced and brought up to Hall in a letter. [\[95\]](#)

Last nite I spent an hour on the phone with Kenneth Arnold -- who has not yet left for Australia, as I learned. Very interesting. Pulled out a lot of fine points that he'd not stressed in his accounts, but which have quite strong bearing on the optical absurdity of the Menzel-Blue Book mirage explanation. Since I'm going to discuss that one up in Seattle next week, wanted to have his own account. The one thing that surprised me is that Arnold speaks very favorably of Ray Palmer, and seemed to have quite high regard for him on several counts. Am I the victim of Menzelian heresies in thinking Palmer is a bit of a charlatan? Or is this some measure of Arnold's credulity -- or does the dollar sign perhaps enter here?"

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These are the kinds of data-related problems that anyone doing UFO research must grapple with.

It is no wonder that so few people will accept such a challenge either on a sub rosa basis or openly. It also raises the question of what type of individual will accept the challenge? Is it the ideal scientist who must follow his data, of whatever kind, wherever it leads him? Is it the intellectual or anti-establishment extremist who feels some need to play the role of the iconoclast? Is it the person who is enthralled by the unusual and bizarre? Alternatively, is it a combination of the above or an ideal type which I have not included? As the McDonald maneuvering unfolds perhaps we will gain at least a better idea of the type of individual he was, even if it would be inappropriate to generalize from him to all thoughtful UFO researchers.

Although McDonald intended to go to the University of Washington at Seattle to speak on weather modification he persuaded the Psychology Department and local AMS chapter to let him talk on UFOs. As mentioned earlier he also wanted to address the local NICAP Subcommittee if possible. In this regard he wrote June Larson, head of the Seattle subcommittee, informing her of his plans. Here we find another example of the politics involved in pursuing the UFO matter. McDonald told Larson that she and her husband could attend the AMS meeting with a few NICAP members, but he said he didn't want an open notice made up which would attract the cultist fringe and derail the discussion. But

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more importantly, as he understood it, the Seattle AMS membership was as ignorant about UFOs as most scientists and cautious about getting involved. Therefore, he suspected they would not put out a written notice on the talk, but would contact members by telephone. [\[96\]](#)

Returning to the subject of the Colorado project, it would appear that as early as November 9 Hall met with Dr. David Saunders, a psychometrician, who was to do the statistical analyses of UFO data in Boulder. Hall wrote McDonald that his impressions were favorable and discussed the various approaches the Colorado group, according to Saunders, intended to consider. As a result. Hall kept McDonald well informed of the progress at CU, which probably made him more anxious to make his views known. [\[97\]](#)

Ted Bloecher, who recently spoke with Hynek, provided more fuel for McDonald's anti-Hynek sentiments a few days later. Bloecher shared McDonald's view that Hynek was "undertaking his own public relations program to justify his past position." As far as Bloecher could see all Hynek could only talk about was his latest efforts to get into print on the UFO question and rectify his past sins of omission. [\[98\]](#)

In a note to James Hughes, McDonald laid out some of his plans. After the University of Washington talk he intended to visit the Los Angeles NICAP Subcommittee and the Rand

Corporation. He said that from talking with Mary Romig of Rand he already knew that Colonel DeGoes and the two Majors who composed the Blue Book evaluation group only went out to Rand for advice on the public relations aspects of the problem. Romig knew this because she was the only one at Rand working on any part of the UFO problem. This must have come as a shock to McDonald after the amount of effort he put into his sincere critiques

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of Blue Book and because of his previous expectations for DeGoes. He already knew nothing came of his efforts and so wrote off the ability of the Air Force to officially provide any self-corrective mechanisms at Blue Book, but the fact that DeGoes humored him, probably from the beginning, in all likelihood raised his hackles.

On Colorado, McDonald explained he heard both good and bad bits of information. What rankled him most, however, was that he wrote Condon twice offering help and received assurances at the October 19 AMS meeting from Price and Ratchford that Condon would contact him shortly, yet a month had passed without any word. He intended to write again in a week and did find it somewhat comforting to know Hall and Keyhoe would travel out to Boulder to provide briefings to the project staff. [\[99\]](#)

The subject of Phil Klass remained on McDonald's mind. He asked Hall if "you ever had more bugging from Klass?" He said it was remiss of him not to reply to Klass, but he did not wish to argue in Klass' litigious manner. However, on returning from Washington he thought he should respond to Klass' claims in order to avoid the rumors which Klass might start. Lastly, McDonald wanted to know if *Aviation Week* published anything more on UFOs? [\[100\]](#)

After returning from his swing along the West Coast McDonald dropped a line to Hall hitting the high points. For our purposes the high point was that WNEW-TV of New York City contacted him to participate in a one-and-one-half-hour panel discussion on UFOs. Although he began with misgivings, after finding out that the director, David Schoenbrun, was a man of some stature, and after persuading him (Schoenbrun) to

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tape a piece solely of his (McDonald's) own comments, McDonald decided to proceed to New York.

The low points of the West Coast trip consisted of finding a lack of dynamism among the NICAP subcommittees and a UFO briefing at Rand which indicated to McDonald that Mary

Romig knew little about the subject and that the people there felt it would be difficult to catch the ear of the Air Force on such a touchy matter. [\[101\]](#)

It seems that McDonald took the initiative and phoned Condon and/or Low on November 20 and then flew into Denver to speak with the staff. In a note to McDonald Low pronounced the meeting productive and suggested that McDonald return to Boulder in the future. Low especially wanted McDonald to make a list of cases:

1. Where data was lost, destroyed, obfuscated, etc.
2. Which McDonald would consider productive for CU investigation.
3. Which impressed McDonald.

Low said he would be down to Tucson to visit APRO and discuss these matters with McDonald in the near future. [\[102\]](#)

The trip to New York for the TV taping went well to hear McDonald tell it. He wrote Idabel Epperson, chairman of the Los Angeles NICAP Subcommittee, to fill her in on the details and to thank her for some photographs she permitted him to use. He said the studio crew seemed excited about the taping and consequently he expected the two-hour panel discussion and his own half-hour interview to be aired soon.

McDonald felt some of the panelists who came to scoff, and by this he referred to Dr. Carl Sagan, astrophysicist-exobiologist of Harvard. Ed Edelson, *World Telegram* Science Editor and Leon Jaroff, *Time* Science Editor, left with puzzled looks on their faces. It is difficult to say

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whether the bewilderment was a function of the evidence McDonald presented, or of the extreme position he took. [\[103\]](#) No doubt he was correct, however, in his assessment that Sagan, Edelson and Jaroff looked puzzled. It pleased him that David Schoenbrun demonstrated moderate familiarity with the UFO phenomenon, lived in France during the 1954 wave and knew military men who took the subject seriously. As a result McDonald confronted a sympathetic panel moderator and after his thirty-minute interview, Schoenbrun made a plea for an independent UFO study. [\[104\]](#)

The fact that the McDonald-Hynek rift existed did not go unnoticed by others in the field. In a note to Judi Hatcher, a UCLA graduate student in astronomy and NICAP investigator, Ted Bloecher discussed Hynek at some length. He indicated Hynek liked the idea that he (Bloecher) and Vallee might cooperate on a comparative study of the Scandinavian UFO wave of 1946 and the American wave of 1947. Hynek even volunteered to obtain 1947 material for Bloecher from

the Blue Book files.

Bloecher said he thought it wise to forget about Hynek's past sins of omission and begin to cooperate with him wherever possible, especially in light of the fact that Hynek intended to aid the CU study. According to Bloecher, "Hynek says his *Saturday Evening Post* article is highly critical of Air Force procedures in the past, and he is exceedingly sensitive about criticism of his own position -- particularly by Jim McDonald" (Jim, please note!). [\[105\]](#)

Low was cordial in his letter to McDonald concerning McDonald's one-day stopover in Boulder. Now in a letter to Hall McDonald provided his impressions of the project which were it "left me slightly uneasy." He felt ill-at-ease over the poor background displayed by the project

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staff, considering the several months they had to prepare. It also appalled him that the project lacked physical science talent. Dr. Franklin Roach impressed him, but he didn't think Roach could carry the project by himself. He said he did not voice his fears in Boulder, but left the message loud and clear that the real problems would arise when the project began to uncover the Air Force foul-up. McDonald was uncertain how to interpret Condon's response to this, which was "We're not interested in a renewal of this contract." Should he (McDonald) be reassured that Condon meant the remark as a sign of toughness or worried because it indicated a casual approach to the UFO problem? The latter interpretation received reinforcement when he spoke to the staff, primarily on meteorological optics, and came away amazed at the elementary topics they considered informative. He closed by saying, "all in all, let's cross our fingers and watch carefully." [\[106\]](#)

So McDonald was upset about the state of things in Boulder by November 29, 1966. Although he kept those feelings between himself and a few close associates, as time would pass, and he would continue to look at the project with a jaundiced eye, an adversary relationship would develop between himself and Condon.

This is a very interesting and I suspect unique phenomenon in itself. Probably one that can only take place in a borderland research area. When does a scientist meddle, and that is the only way to describe McDonald's behavior, in another scientist's research project? McDonald was certainly familiar with scientific protocol. Yet he must have felt the problem sufficiently significant to either be blind to his violation of protocol, or, after having given the situation consideration, concluded that it warranted atypical scientific practices.

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It would be well to keep in mind that one of the reasons which spurred him on, and is seldom present in other research, is that the CU scientists were supposedly chosen for their lack of interest in the UFO problem in order not to bias the study. Whereas scientists normally study that which excites them and in which they have some background. One therefore might argue that since McDonald was already somewhat of an authority on UFOs, that it was only natural that he, or anyone else who believed as he did, could not help but be drawn into the web being spun at Boulder when the general consensus of those close to the subject was that UFO research would rise or fall on the outcome.

DECEMBER

While attempting to push UFO research along McDonald began to find that certain methods worked better than others. In a letter to Hall he mentioned that Al Bland of the National Lecture Bureau contacted him to tape a show with Frank Edwards on UFOs. Frank Edwards was a radio news commentator, writer on the unusual. Board member of NICAP and general raconteur. McDonald shied away from doing the program with Edwards, because he wanted to avoid the sensational tone which he thought could develop through Edwards' approach. So in talking with Bland he stressed the seriousness of the UFO problem and the importance of reaching members of the scientific community who possessed the skills to study it. He found Bland warmed to this tactic as did Ted Kavanau and Mel Bailey prior to the WNEW-TV taping in New York. McDonald made a note of this tactic as being the proper way to elicit interest and obtain a hearing. He asked Hall if they might be able to get together with Vallee and do the taping in Chicago.

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In the same letter he expressed his disgust with Gerard Kuiper, noted UA astronomer, who took it upon himself to begin speaking out against the study of UFO data without, in McDonald's opinion, any knowledge of the subject. He felt he aroused Kuiper's interest the previous June, but now it seemed to no avail. He thought he should address Kuiper's research group and inform them of the real dimensions of the UFO problem because of Kuiper's influence and influential friends. Moreover, since Kuiper traveled a great deal his coast-to-coast pontifications could hurt the chances for future UFO studies. [\[107\]](#)

McDonald acted on these sentiments the same day by getting off a letter via campus mail to Kuiper. First he asked if the November 30 *Citizen*, a Tucson paper which covered a talk he gave on UFOs, correctly quoted him. McDonald wanted to hear Kuiper's argument if Kuiper felt that

all UFO phenomena had conventional explanations.

In order to impress Kuiper with his own perception of the import of the issue McDonald told him about the various speaking engagements and research efforts he undertook beginning in June. He emphasized that he engaged in this activity to bring the UFO problem out of scientific disrepute. He threw a barb at Kuiper, perhaps unwisely, by saying no one he knew who looked at the phenomenon closely did not consider it a bewildering scientific problem.

He further said he was troubled by the question, "Have you really dug into this problem any more than you had at the last time we talked about it?" McDonald expressed concern over the impact talks such as Kuiper's Bar Association speech could have, especially in scientific circles. He followed this up by asking if he could address Kuiper's laboratory staff on the problem. [\[108\]](#)

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McDonald did not let up on Kuiper nor, in the process, on Hynek. Five days after his above letter to Kuiper he sent off a further memo with a copy of Hynek's December 6 *Saturday Evening Post* article. This article, which took some good swings at the Air Force investigation of UFOs, by implication, did much to strengthen McDonald's position. Nevertheless, he could not resist this opportunity to inform Kuiper of what he called Hynek's past complicity with the Air Force in obfuscating a potentially important scientific problem.

After this introduction McDonald again made an overture for an opportunity to speak with Kuiper's Lunar Planetary Lab staff. He argued that as absurd as the ETH seemed, all the evidence appeared to point to it, and he felt it "undesirable and unwise" not to share this information with his colleagues. [\[109\]](#) He was probably hinting here that once the UFO matter broke LPL could be in for considerable funding if it already maintained a good grasp of the problem.

The next day Hall wrote McDonald, who apparently decided to do the National Speakers Bureau UFO taping, that they could not set the schedules up in such a way as to coincide. Therefore, Hall would be in Chicago on December 19 and there would be no chance of getting together with Vallee because Vallee thought Hall intentionally tried to avoid him on his (Hall's) last trip to Chicago. At this point there seemed to be no way of establishing a rapport which might lead to cooperation between the Northwestern group and NICAP. [\[110\]](#)

As was his habit, McDonald got off a letter to Tom Malone apprising him of his past month's activities. He included a paragraph on Hynek's fancy footwork à la the *Saturday Evening Post* article and referred to the entire affair as laughable. He spoke of his anxiety over the lack

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of trained personnel at Boulder, his work on the *NYT Magazine* article, all of his talks, and his ever-stronger conviction regarding the ETH. He said he found the psychologists much more interested than the astronomers, and said the former seemed quite opposed to any "exotic psychological explanations or hallucinatory phenomena." [\[111\]](#)

From a letter McDonald sent to Jim Hughes it would appear that he wanted ONR support for his UFO work. He told Hughes that after all the time he put in, the conclusions he reached, and the many years the ONR supported his research, that he felt some sort of colloquium or closed briefing at the ONR seemed warranted.

Furthermore, he said he knew through Keyhoe, of a backlog of ONR Navy UFO reports, which under the circumstances, it appeared logical for him (McDonald) to begin to examine. He emphasized to Hughes that the issue "is soon going to blow wide open." He wanted Hughes' opinion on what strategy might be used to focus the attention of the ONR on the problem and voiced the fear that his treading on the toes of the Air Force might prejudice his position in any ONR study of the phenomenon. [\[112\]](#)

Hall told McDonald shortly thereafter of his talk with Tom Ratchford from AFOSR, the go-between for the Air Force with the Colorado project. Hall conveyed his apprehension that there were too many psychologists on the project and not enough physical scientists, a worry of McDonald's, to which Ratchford replied that CU administered the project, but complete cooperation existed with scientists at ESSA and NCAR. The Air Force did it that way, he explained, to avoid the red-tape of contracting with other government agencies and to facilitate communication since NCAR, ESSA and CU were all located in Boulder. In terms of outcomes Hall said Ratchford was frank. He stated that "if Condon came up with a

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positive reading and the NAS Review Committee found no fault with Condon's methodology, a worldwide investigation of some type would ensue." Ratchford stressed, according to Hall, that in its review the NAS Committee would not concentrate on content, but on methodology. [\[113\]](#)

Although he found these comments interesting McDonald's connections in the scientific community left him feeling ambivalent. For instance, the Director of the Atmospheric Research Institute at the UA, Richard Kassandra, was also chairman of the University Corporation for Atmospheric Research (UCAR) Board of Directors, and he told McDonald that the Board recommended that Walter Roberts, Director of NCAR, not get too involved with Colorado because NCAR was already over-committed. This meant that although Ratchford claimed NCAR

would make a significant contribution to the CU study, McDonald realized that such an outcome was unlikely.

In his previous letter Hall spoke of a semi-official talk with Dr. Robert Mood, Deputy Director for Research and Development, Advance Systems and Technology, Douglas Aircraft, initiated by Wood on the subject of writing a paper on the aerodynamic characteristics of a disc-shaped object. Now McDonald seized upon this to do a bit of ironic speculation on the cover-up versus foul-up hypotheses. He suggested that perhaps a foul-up existed at the Air Force level all along and now the stir which he and others made had awakened the slumbering giant who, seeing advantages at the international level in discovering the nature of the UFO propulsion system was now feeling out Douglas Aircraft -- hence the visit of Wood. Assuming this, McDonald went on, it is possible the Air Force might put into action a real cover-up on the UFO issue in order to obtain an advantage over the Russians and others. McDonald remained

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unconvinced, however, for he went on to say, "But is something along such lines entirely out of the question? It makes me wish we had some real leverage on the international aspects of the UFO problem."

Lastly, he told Hall that the joint psychology-sociology talk went well in Phoenix. T.H. Hoult, head of the Sociology Department, received jeers for his negativism, while Bachrach, head of the Psychology Department, essentially supported his (McDonald's) position. [\[114\]](#)

But McDonald did not get rid of Hoult that easily, for Hoult wrote him a letter to explain his position in some detail. Or perhaps it would be more correct to say that he attempted to explain McDonald's position to McDonald. He said he felt McDonald started with the most complicated of hypotheses -- the ETH -- and did not adequately examine the more mundane. He argued that McDonald needed to be a spokesman for the weak and far out, UFOs just being the latest manifestation of this behavior pattern. Moreover, he considered McDonald's defense of this position verging on fanaticism, the kind found when observing a religious phenomenon. Hoult predicted McDonald's interest in, and the saucer craze itself, would soon wane and McDonald would wonder why he ever became involved. [\[115\]](#)

So, even from Hoult, a friend, McDonald received some stiff criticism. Hoult did not look at the data, rather as he put it, "My speculation is in line with historical events and with well-established theory." [\[116\]](#) He used those tools which were at his disposal in an attempt to discredit McDonald in the only way that he knew. He used a particular world view based upon the discounting of various bizarre phenomena in the history of science, and to him, rightly or

wrongly, it seemed that UFOs fit into the same category. Actual investigation of

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sighting data apparently seemed irrelevant to him. He found a convenient niche in his world view for UFOs and there they would stay.

On December 23 McDonald received a cordial response from Gerard Kuiper indicating that he could address the Lunar Planetary Laboratory staff. Kuiper admitted that as an astronomer, and assuming Earthlike organisms, the probability of the ETH being correct appeared very small and so he remained skeptical. However, he said that, "I will revise my opinion without any hesitation if I am convinced that there is novel information that can really be trusted." [\[117\]](#)

SUMMARY AND CONCLUSIONS

This ends McDonald's first year of active participation in the UFO controversy. It should be clear from the above that he attempted to gain "access" (to use Truman's term [\[118\]](#)) to decision-makers in the scientific, governmental and military communities in order to shift a paradigm. His behaviors were blatantly political and his personal politics of science lends itself to discussion in traditional political terms.

He needed to gain access to obtain legitimacy for the study of the UFO phenomenon. He saw successful access to the scientific elites as a means of convincing government to fund UFO research. Successful access to governmental elites, on the other hand, could produce the same outcome, but through forcing research on the scientific community in the name of the national interest. In the case of the military elites, if they could be convinced of the importance of the UFO problem, assuming they didn't already know, then McDonald believed they would focus their scientific talent on solving the puzzle to stay ahead of the Russians.

We can refer to the above as components of McDonald's personal political strategy. But if his strategy was to gain access to scientific,

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governmental and military decision-makers to obtain legitimacy for UFO studies, what were his political tactics? That is, how did he attempt to implement his strategy? In retrospect it would appear that his tactics can be most readily discussed in terms of those used directly to influence members of the decision-making communities mentioned above and those used indirectly to accomplish the same ends. In the case of the former his overtures were essentially

straightforward, while the latter consisted of his efforts to create a favorable climate of opinion for UFO studies by obtaining publicity for the phenomenon and by attempts to buttress and/or maintain his own credibility. How did he go about this in each of the above instances?

THE SCIENTIFIC COMMUNITY

With respect to the scientific community he began his campaign in a subdued manner in March of 1966 with overtures to Tom Malone for a one-man summer study through the Committee on the Atmospheric Sciences of the NAS. When he became aware that the Air Force intended to fund a university UFO study to be reviewed by the NAS, he retracted his plan.

At the University of Arizona McDonald applied for an NASA Institutional Grant of \$1300 through the university's Space Sciences Committee. He realized that funding was a political question, and subsequently contacted Gerard Kuiper and Aden Meinel, who sat on the committee, to convince them of the significance of the UFO problem.

A month later he tried to impress the committee with the significance of his research and in particular his conclusion, after visiting Wright-Patterson Air Force Base, that the ETH was correct. He asked Gerard Kuiper if he might present his findings at a committee meeting. It is not clear if he was permitted to do so, but it is apparent that

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he felt such a tactic would have a salutary affect. Three weeks later McDonald received the \$1300 NASA grant through the committee.

It was as a result of the WPAFB trips that McDonald became irritated with Hynek for not speaking out in his role as the Air Force consultant, or that failing, dramatically calling a press conference in the early 1950s to resign. We will never know why McDonald adopted the tactic of attacking Hynek for his past timidity. But it would appear that the behavior is best explained as an effort to impugn Hynek's Air Force consulting work and thereby call into question eighteen years of Air Force UFO pronouncements. For while the Air Force used many consultants over the years, only Hynek had eighteen years of experience and filled the role of the watchdog of Blue Book for the scientific community. Another possible explanation for McDonald's behavior is jealousy. He considered Hynek remiss in his duty to the scientific community, but at the time the subject was about to blow wide open Hynek managed a letter to *Science*, *Discovery* and *Saturday Evening Post* articles and a preface to Vallee's book. McDonald believed Hynek intended these actions to make him appear to be something he was not.

However, attacking Hynek proved a poor tactic for McDonald to employ because it forced a

wedge between himself and Hynek at a time when they were the foremost academics in the field. Their bickering rather than cooperation, and the resultant squabbling among their respective followers wasted considerable time and energy which could have been more profitably spent in a joint effort to legitimate UFO studies.

In August McDonald began speaking out on campus (see Appendix A for a list of his speaking engagements from 1966-69). From the tone of his correspondence it is clear that he wanted to increase his academic

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constituency through this tactic. There is no doubt that he intended to get as much mileage as he could out of the fact that he, as "a respected atmospheric physicist" had given UFOs a long hard look and had found a significant scientific problem. He had credentials, was aware of their importance, and used them wherever he considered it efficacious.

After many months of interacting with Ratchford and Price at AFOSR, seemingly to no avail, McDonald developed a deep concern over the events taking place on the Colorado Project. When he didn't receive answers to his letters to Condon, he finally telephoned Boulder, essentially forcing Condon to extend him an invitation to brief the project staff.

Neutralization of individuals who dismissed UFO data as explainable in other than extraterrestrial terms also occupied some of McDonald's time. After hearing of Klass' plasma explanation he wrote him a condescending response and enclosed a letter to the editor for *Aviation Week and Space Technology Magazine* which he no doubt intended to rectify, what he considered, the misimpressions created by Klass' article.

Donald Menzel of Harvard had written two books on UFOs, both of which treated the subject as nonsense. He was, and is, considered the foremost academic proponent of that position. Cognizant of this, and convinced that Menzel's explanations were qualitatively seductive but quantitatively untenable, McDonald decided to hire a part-time worker to plot radiosonde data which he believed would invalidate many atmospheric refraction and inversion explanations which Menzel had proffered for past cases.

When McDonald heard of Gerard Kuiper's negative remarks on UFOs at a local American Bar Association meeting, he quickly jotted him a note

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to determine if the newspaper quotes were correct and if Kuiper knew more about the phenomenon than the last time they had spoken. He pursued the matter until he obtained an invitation from Kuiper to speak on UFOs to the Lunar Planetary Laboratory staff.

However, McDonald did not direct all of his tactics toward resolving the UFO issue. Proceeding under the assumption that it "would break wide open," he contemplated some vindictive actions. Scholarly texts on life in the universe irritated him because of the manner in which they slighted the UFO phenomenon. Therefore, he obtained appropriate quotes from these volumes for inclusion in the papers he intended to pen after the "big breakthrough." In these papers he would make the authors of the above-mentioned scholarly texts eat their own words.

THE GOVERNMENT

He began his government-oriented tactics in March 1966 with a letter to Representative Morris Udall. He wanted to work through Gerry Ford, who had called for hearings on UFOs, but did not know if he could trust Ford to be circumspect. Therefore, he asked Udall to forward his proposal only if Udall believed Ford could be depended upon not to leak the request for a quiet three-man investigation.

Because of his conviction that under ideal circumstances UFOs were a scientific and not a military matter McDonald wanted the problem transferred from the Air Force to NASA. He spoke with Gerard Kuiper, who he felt was influential at NASA, and asked if he should wait for Kuiper to intercede for him or go ahead with his NASA plans without Kuiper's aid.

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When McDonald finally had the opportunity to speak at NASA he encountered Joe Fletcher, a representative of the Rand Corporation. Fletcher "sat in" while McDonald emphasized the budgetary significance of the UFO question to NASA personnel. The talk, McDonald felt, planted the seeds which would bear fruit at some future date. The NASA people knew of his talks to the Air Force and through the Rand representative the Air Force would learn of the colloquium at NASA. It pleased McDonald that this might cause some interagency rivalry.

When Kuiper proved to be an adversary instead of an ally McDonald attempted to enlist the support of Al Eggars, an aerodynamacist at NASA. Eggars also proved of negligible help, but this did not lessen McDonald's belief that UFOs belonged under the aegis of NASA. When he decided to "come out" in October he told Hall that he intended to begin a letter-writing campaign to assorted Congressmen and wanted Hall to put the full weight of the NICAP membership behind it. The thrust of the endeavor would be to urge Congressional Hearings to remove UFOs

from the domain of Air Force responsibility and place them under the wing of NASA.

THE MILITARY

In 1966, however, McDonald spent most of his energy on the military. After he had become aware of the Air Force plans for a university study, but before it was clear what the study would consist of, he asked Brian O'Brien, who sat on the Air Force Scientific Advisory Board, to raise the idea of a small summer study at the Air Force Systems Command.

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As early as April McDonald informed Hughes at the ONR of his UFO activities and in June the ONR paid for his first trip to WPAFB. It was on that trip that McDonald concluded that the ETH was correct and afterward asked Hughes for, but did not receive, direct funding for his UFO work.

McDonald made three trips to WPAFB and uncovered what he considered a wealth of material and unsurpassed Air Force investigatory incompetence. He tried to bully Major Quintanilla into changing case classifications and gave advice to Dr. Cacciopo, who was ostensibly responsible for Blue Book, on reorganizing the project. At that time he still believed that the Air Force wanted to solve the UFO mystery and not salve the public psyche. Proceeding with that mistaken assumption he attempted to aid Colonel DeGoes' three-man Blue Book review team through extensive discussions, both verbal and written, of the problems on the project.

Concomitantly he was concerned about the possibility of the Air Force summer study and the ensuing full-scale university team examination of the phenomenon. With respect to the former, he found that O'Brien, just as Kuiper, proved an adversary and not an ally. He set out to neutralize O'Brien by educating him about UFOs in the hope that O'Brien, through the AFSAB, would not scuttle his chances for the summer study. In the case of the university team approach, he offered his services to Ratchford and Price at the AFOSR to sell the UFO project concept to any prospects for the principal investigator position and proffered his six months of experience in any capacity in which he could serve. This, of course, was long after it was patent that he would not be asked to head the investigation himself.

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THE PUBLIC

By September, however, McDonald's disillusionment with the Air Force made him decide to "go public" with his ETH findings even though the university team approach was in the offing. It is also possible that Hynek gave him impetus in this direction by "coming out" in *Science* in August. McDonald's pre-"coming-out" letters to Colonel DeGoes, Ratchford and Cacciopo suggest that he thought that the threat of going public, especially with a position highly critical of the past research of the Air Force, would coerce the Air Force into reevaluating Blue Book. But such was not the case.

He went through with his "coming out" in which his remarks were highly critical of the Air Force and received limited press coverage. This began a two-pronged offensive which lasted until publication of the Condon Report in January 1969. On the one hand he was critical of the Air Force handling of the UFO question, but on the other he tried to initially help Ratchford and Price organize the university study and as it proceeded he attempted to apprise them of its progress, continued to offer his services, and generally worked to keep the project moving in what he considered "the right" direction even though his meddling proved disruptive.

Meanwhile, he became more knowledgeable about UFOs and felt that his research on what he considered the most important scientific problem of the twentieth century deserved funding. Having had ONR contracts for eight years he wanted ONR to fund his UFO work and with that in mind began setting the stage in December through Hughes for an ONR colloquium on his UFO findings.

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The tactic of publicity-seeking, with the hope of swaying public opinion, proved a delicate matter for McDonald. He knew that it could work, as witnessed by the House Armed Services Committee Hearings of April 1966. Moreover, pressuring Congress, the Air Force and the scientific community into taking action appealed to him, but he realized that while he needed publicity, to be branded a publicity-seeker would lose him the support of those people who could help him the most. Consequently, he proceeded with caution.

As early as August, however, he approached *Look* and *U.S. News and World Report* about writing UFO articles. Apparently *U.S. News* lacked interest, while *Look* was interested, but not, they said, until the furor subsided over several *Saturday Review* UFO articles. Eventually McDonald did prepare a piece for the *NYT Magazine* which was never used and participated in a two-hour UFO special for WNEW-TV in New York City.

In September the planning began for his "coming out." Hall, who did most of the organizing anticipated ample press coverage. McDonald used two talks in Tucson as trial balloons prior to his October 19 American Meteorological Society talk in Washington, D.C. He learned in Tucson to be leery of the press for its blatant misquoting. He also found that he had to modify his presentation of hypotheses because his audiences sensed his true convictions regarding the ETH. He did not want to frighten away potential converts, particularly scientists, and so he adopted the circumlocution that the ETH was "the least unsatisfactory hypothesis" to explain UFO data.

Although he had initially been interested in radio and TV interviews, a press conference, and private talks with individual reporters, his Tucson experience sobered him. The events surrounding

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the actual Washington coming out are not clear, but it appears that the Air Force stole some of his thunder by announcing its university project and the appointment of Ed Condon as its head just a few days before McDonald spoke. This, no doubt, decreased the impact of his remarks and made him appear an unofficial late-comer to the UFO problem.

McDonald was always aware of the fact that his campaign was one of persuasion; that he did not have the kind of intersubjectively verifiable evidence that invariable would win the day if only given the opportunity. Therefore, he tried in various ways to guard his credibility in what seemed an incredible area of research.

For instance, in the Spring of 1966 it took months before he had enough confidence in Hall and NICAP to expose his true interest in UFOs. Moreover, during this period, although he corresponded with Hall, he did not inform him of his NAS overtures.

The Tucson press coverage, as mentioned above, frightened him. He didn't mind going out on a limb, but he desired it to be one of his own making. Consequently, he told Hall that he only wanted good press people covering his Washington coming out so that his position would not be misrepresented. To try to ensure this he adopted the "least unsatisfactory hypothesis" phrase.

Even though he initially wrote Klass in a condescending tone, McDonald had second thoughts when he realized the influence Klass could have in Washington., D.C., through his editorial position with *Aviation Week and Space Technology Magazine*. Although he did not wish to reply to Klass' letters he decided that it was incumbent upon him to do so in order to nip any rumors in the bud which he suspected might circulate regarding his inability to respond to Klass' arguments.

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Lastly, when the National Lecture Bureau contacted McDonald to tape a UFO segment with Frank Edwards, a UFO writer and general raconteur, he declined. He did not want to take the chance of Edwards side-tracking the discussion away from the scientific merits of the UFO problem and concomitantly associating him (McDonald) with that style of argumentation. One thing he felt he had learned about such situations was to always keep the discussion directed toward the scientific aspects of the problem and the best ways to reach scientists. He believed this enhanced one's credibility and left a good impression with respect to the seriousness of the subject.

SUMMARY OF TACTICS

Such were the strategy and tactics employed in McDonald's personal politics of science in 1966. In his pursuit of legitimacy and a subsequent paradigm shift he concentrated his energies on what Almond and Coleman have called interest articulation. ^[119] He tried to make demands on elite decision-makers in the governmental, scientific and military arenas to obtain the scarce resources to carry on the research he believed necessary to resolve the UFO problem. When the elites failed to respond as he had hoped he took his case to the public, probably thinking that public opinion might influence decision-makers in a manner which he could not. In going about this McDonald employed the following tactics.

Academia

1. Spoke on campuses to increase his academic constituency.
2. Attacked Menzel, Kuiper and Klass to neutralize their arguments among scientists.

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3. Planned a post-"breakthrough" attack on exobiology writers who slighted UFOs.
4. Worked through the CAS of the NAS to get a summer study funded.
5. Spoke with Kuiper and Meinel to insure the success of his NAS Institutional Grant proposal.
6. Attacked Hynek's past Air Force UFO work to show how poor a foundation the Air Force UFO pronouncements rested upon.
7. Tried to provide guidance to the Condon Project.

Government

1. Attempted through Udall and Ford to get a "quiet study" funded.
2. Tried to get Gerard Kuiper to intercede for him at NASA.
3. Spoke at NASA in an attempt to convince NASA staff that they should take on the responsibility for the UFO problem.
4. Informed NASA scientists of his Air Force lobbying hoping that this might start an interagency rivalry.
5. Advised Al Eggers at NASA on how to proceed on the UFO question.

Military

1. Asked O'Brien of the AFSAB to help him get a small summer study funded.
2. Kept Hughes informed of his UFO work for purposes of obtaining future ONR support.
3. Offered suggestions for the reorganization of Blue Book.
4. Offered his services to Price and Ratchford at the AFOSR to sell the UFO project.
5. Attempted through Hughes to organize an ONR colloquium on his UFO work.

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Public

1. Had a trial balloon "coming out" in Tucson.
2. Had a "coming out" in Washington, D.C.
3. Tried to get an article in a national magazine.
4. Tried to get a letter to the editor in *Aviation Week and Space Technology Magazine*.
5. Took part in a TV special on UFOs.
6. Developed the "least unsatisfactory hypothesis" circumlocution to avoid alienating potential supporters,
7. Avoided civic club speaking engagements to foster his credibility.
8. Developed his ties, which were never official, with NICAP slowly for fear of associating with a disreputable organization.
9. Avoided APRO because he thought occupant reports were unacceptable evidence and APRO investigations were not thorough.

This chapter examined McDonald's behavior in detail and, I believe, made the case for the proposition that the scientific process, at least in this instance, was a political process. Therefore, there is no need to continue the presentation in such a micro-analytic fashion. In the approximately four years which followed the strategy remained essentially the same while McDonald continually refined the tactics as he became more accustomed to the difficulties of engaging in borderland science activity. The remaining chapters focus on four important events in McDonald's campaign.

The first concerns his response to, and interactions with, Ed Condon and the staff of what became known as the Condon Project. This provides an opportunity to observe McDonald's tactics at a time when it appeared that the future of UFO research hung in the balance.

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The second event involves McDonald's successful attempt, to obtain UFO hearings within the House Committee on Science and Astronautics in the summer of 1968. As far as he could tell things were not going well at Boulder and so it seemed wise to take the case for UFOs to Congress where he hoped interest could be stimulated in a thorough investigation of the phenomenon. The battle with Phil Klass over McDonald's use of ONR atmospheric physics research funds for UFO studies makes up the third event. It furnishes the occasion to examine the tactics which may be resorted to if straightforward funding of a research area is not forthcoming for political reasons. It also suggests something about the extremists one must be prepared to confront if one decides to do borderland research and become known as an extremist oneself. Lastly, preparations for the scientific establishment in convention is the fourth event. In 1969 the American Association for the Advancement of Science held a UFO Symposium at its annual meeting in Boston. Although McDonald did not play a role in the organization of this event it is what he nevertheless wanted; a hearing before the scientific community. Following the planning of this event will enable us to observe the trials and tribulations of the prime movers, view, for the first time, the tactics of those elder statesmen of science who opposed UFO studies, and determine what purpose the AAAS Symposium organizers had in presenting the program.

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FOOTNOTES

1. Hall to McDonald, April 21, 1960, McDonald Letters. Hereafter referred to as M.L. - [Back To Text](#)
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13. McDonald to Coleman, April 16, 1966, cc. Tom Malone, M.L. - [Back To Text](#)
14. McDonald to Coleman, April 16, 1966, cc. Tom Malone, M.L. - [Back To Text](#)
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16. McDonald to Hughes, April 4, 1966, M.L. - [Back To Text](#)
17. McDonald to O'Brien, May 14, 1966, M.L. - [Back To Text](#)
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41. See Herbert Strentz, *Press Coverage of UFOs 1947-1970*, a dissertation submitted to the Department of Journalism at Northwestern University, 1971. - [Back To Text](#)
42. Hynek to the Lorenzens, July 26, 1966, APRO Files. - [Back To Text](#)
43. Uman to McDonald, August 4, 1966, M.L. - [Back To Text](#)
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45. A personal conversation with Fred Beckman of the University of Chicago's Argon Cancer Research Institute and close associate of Hynek. Later denied in a letter from Hynek to the author. - [Back To Text](#)
46. Hynek to the Lorenzens, August 8, 1966, APRO Files. - [Back To Text](#)
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48. Memo from Low to Archer & Manning, August 9, 1966, M.L. - [Back To Text](#)
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50. McDonald to Hall, August 25, 1966, H.L. - [Back To Text](#)
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57. McDonald to Hall, October 1, 1966, H.L. - [Back To Text](#)
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59. McDonald to Ratchford, Octobers, 1966, M.L. - [Back To Text](#)
60. McDonald to Malone, October 3, 1966, M.L. - [Back To Text](#)
61. I don't believe this is true but some behind-the-scenes activity did take place prior to publication. - [Back To Text](#)
62. Hall to McDonald, October 5, 1966, M.L. - [Back To Text](#)
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III. THE CONDON STUDY

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CONTEXT

Each of the four chapters which follow overlap one another chronologically. To some extent the partitioning of McDonald's life during this period reduces the intensity of the impact which the simultaneous recounting of these events would otherwise have. However, for purposes of clarity and analysis this approach seems better than endeavoring to relate the events concurrently. During this time McDonald did many of the same kinds of things which he did in the previous chapter, but the central issue became the Condon Study. The study should be viewed in the context which was developed in chapters one and two.

For a decade, and in some instances longer, the UFO groups desired a full-scale scientific investigation of the UFO phenomenon. Then in early 1966 as a result of numerous reported sightings, public outcry, the quiet behind the scenes work of J. Allen Hynek, the NICAP lobby effort, the recommendations of the Ad Hoc UFO Committee of the Air Force Scientific Advisory Board and possibly other pressures the Air Force agreed to fund such a study by independent university contractors.

However, the project proved a mixed blessing because while it provided the long-awaited scientific analysis, on the other hand the funding came from the Air Force. No one knew what the implications of such funding were, if any, or what strings might be attached, but they (meaning those seriously concerned with the UFO problem) were circumspect regarding the Air Force involvement. Although impossible to demonstrate that a conspiracy existed to hide the

facts about UFOs, even those who would argue no such plot existed were hesitant about Air Force backing

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of the proposed research. For most UFO watchers saw the Condon Study as an opportunity either to legitimate UFO studies in the halls of academe if the Condon team reported that the phenomenon warranted further study or, to the contrary, set the field of Ufology back ten years if the final report concluded the phenomenon were nonsense.

The make-up of the research project staff and the duration of the Air Force contract for the study added to this concern. The Air Force feared that anyone already involved with the UFO problem whether pro or con would bring these predispositions to the project and make any resulting conclusions suspect. Consequently, unlike the motivation for doing most other research -- namely, interest on the part of the investigators -- the staff of the Condon Study, by Air Force stipulation, was chosen for its lack of prior interest. This, combined with the year-and-one-half contract, tended to frighten those UFO researchers familiar with the problem because they considered the contract period too short a time to become apprised of the complexity of the UFO situation and also conduct meaningful analysis.

With these sorts of considerations in mind we can better understand McDonald's actions in 1967, 1968 and 1969. We have already seen his distress begin to develop in 1966. First he thought he might be the principal investigator. When that possibility waned he volunteered his services to "sell" the none too popular project to any lukewarm scientists who might be candidates for the job of principal investigator. When the Air Force failed to take advantage of his offer this not only annoyed him, but also made him wonder about Air Force intentions. Once Edward Condon's name appeared as principal investigator and the University of Colorado received designation as the research site

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McDonald again offered his services in an advisory capacity. When Condon was slow to invite him to Boulder he chafed at the bit and, as we saw, eventually obtained an invitation by sheer perseverance. What he found at CU did not please him and he concluded that the project had to be closely monitored. It should be remembered that by the end of 1966 he had lived with the conclusion that UFOs were a form of extraterrestrial visitation for some five or six months and consequently believed the issue to be the most important problem facing twentieth century science. He felt action had to be taken immediately and believed himself more qualified than any

other academic to pursue the problem. It is at this point that we begin the story in 1967.

HISTORY OF EVENTS

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[The Low Memorandum: Confrontation](#)

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Early Concerns

Condon was always considered a good after-dinner speaker; he knew how to tell a funny anecdote. With the UFO contract under his belt the number of invitations he received increased. He, being the type of person who enjoyed such occasions, did not refuse. It would be a number of these speeches which would cause him difficulties within the UFO world.

As early as January Hall wrote McDonald that he, and therefore NICAP, worried about Condon's public statements. He indicated that if Condon continued in his ways that NICAP might have to withdraw support from the project or possibly ask for a new project or project leader. ^[1]

Hall's concern was for an article in the *Elmira Star Gazette* of January 26, 1967 which quoted Condon as saying in his Corning Glass Works talk:

It is my inclination right now to recommend that the government get out of this business. My attitude right now is that there is nothing to it, but I'm not supposed to reach a conclusion for another year.

It might seem strange that Hall could make the above threat of severing relations with CU, but it must be kept in mind that Condon planned to use NICAP data and was already in the process of establishing a nationwide UFO reporting network using NICAP members. In addition, the people in NICAP were the most vociferous about UFOs. If NICAP withdrew support and the Condon Study results were negative, NICAP members, many feared, would not accept the findings. Condon reacted by writing a letter to Donald Keyhoe claiming that the press had distorted his remarks. ^[2]

This turn of events markedly affected McDonald. He decided that the time had come to put his case before the National Academy of Science (NAS). In a letter to Philip Seitz, President of the NAS, he did just that. He explained his previous "quiet study" strategy with the Committee on Atmospheric Sciences and his more recent lobbying efforts which failed to bear fruit. Then he indicated that the researchers at CU were not aware of the dimensions of the problem and called CU a good beginning but too small a project. Finally he turned to the responsibility of the NAS to keep the public informed on UFO matters. He said he wanted to brief Seitz because the subject would be of overriding public concern in a few months. He argued that the interrelationship between science and the public was of great importance and that the NAS should be prepared to answer the questions on UFOs which would shortly be asked. ^[3]

At this juncture it appears that McDonald hoped to arrange a briefing with Seitz by convincing him that the NAS might be caught

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napping on UFOs and come under considerable public criticism. One thing McDonald probably did not know was that Seitz was a former student of Condon, respected him, and believed he could do the job in Boulder adequately.

While providing a briefing at CU Hall wrote back to Keyhoe regarding Condon's lack of interest in McDonald's offer of aid. Hall claimed that it was hinted that McDonald's tilt with the Air Force over ringing Tucson with missile sites made him persona non grata with the Air Force and consequently Condon didn't want to cause himself problems by asking McDonald for advice. ^[4]

Approximately a week later McDonald wrote Condon making another consulting offer and pointing out that he had ten months of experience to draw on. ^[5] The same day he dropped a note to Bob Low, the project administrator, stating that he would put himself at Low's disposal the three days Low would be in Tucson to meet with the Lorenzens of APRO. ^[6]

On January 22 Seitz wrote a terse reply to McDonald in which he said that the NAS had an interest in his remarks and took the matter seriously. Seitz further hoped McDonald kept in close contact with Condon. [\[7\]](#)

After Low's visit to Tucson he wrote McDonald a positive letter in which he expressed his desire to arrange an opportunity for McDonald to speak in Boulder. Low mentioned his attempt to get Norman Levine, an electrical engineer who had just finished his Ph.D. at the University of Arizona, and William Hartmann, a UA astronomer, on the project. With regard to putting further UA people on the payroll Low said he knew everyone from the UA was independent but the public didn't and the impression left with the public had to be considered. [\[8\]](#)

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Apparently at this point McDonald wanted to confront Condon publicly. In a letter to Neubold Noyes, in charge of the UFO Panel discussion to be presented at the American Society of Newspaper Editors convention, McDonald said he had Donald Menzel of Harvard committed to appear, but couldn't get Condon because Low opposed the idea. [\[9\]](#)

One way or another McDonald obtained an appointment to see Seitz at the NAS on April 19. Two weeks prior to that he wrote Seitz to firm up the engagement. While so doing he included an extensive list of his latest interest articulation efforts, no doubt to impress Seitz with the names of the various governmental, scientific and military groups he had addressed and to insure that Seitz realized how important the UA atmospheric physicist considered the problem. [\[10\]](#)

In a note received several weeks thereafter Low began by asking McDonald how he would spend a large UFO grant if he hypothetically had one. Then he touched upon the issue which brought him to write, namely a public statement by McDonald that Condon did not spend much time on the project. Low assured McDonald that what he had told him in Tucson was that Condon did not spend much time with the sighting files, but nevertheless he did contribute one-half of his time to the project. [\[11\]](#)

His continued concern with winning over Brian O'Brien, Chairman of the AFSAB, led McDonald to forward O'Brien copies of his latest UFO talks. In passing he mentioned that CU just didn't have enough scientific talent to get the job done. [\[12\]](#)

In early August McDonald went up to Boulder to brief the project staff on his recent Australian trip during which he interviewed more than eighty UFO sighting witnesses. He returned from Boulder with a jaundiced view according to a missive sent to Mary Lou Armstrong, Condon's

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administrative secretary and project worker. How a balanced report could be done as long as Condon refused to personally do interview work escaped him, although Roy Craig, a physical chemist from CU, impressed him. ^[13] Here we see the schism which would appear much more evident in the future. McDonald could talk and write openly to Armstrong and a few others, although Condon and Low, the leaders, remained at a distance. This distance apparently resulted from a number of the staff becoming convinced the UFO phenomenon was a significant scientific problem, possibly of extraterrestrial origin, while it seemed to these same individuals that Condon and Low did not take the matter seriously.

The disquietude evinced by McDonald to Armstrong he also passed along to Hall. He said he tried to talk Condon into interview work, but to no avail. In fact, Condon fell asleep twice during the briefing. When informed of the \$50,000 contract to be given to Stanford Research Institute to prepare a paper on anomalous propagation phenomena in radar sightings McDonald said he suggested a \$3.00 paperback on the subject. He concluded by saying both Armstrong and Wadsworth, the latter being a graduate student working on the project, were upset with Low. ^[14]

In a letter to Hughes McDonald again spoke of his pessimism regarding CU. He said that Ben Herman, a consultant to the project from the UA, "tells me they put on a show for me, and that after I left everything lapsed back into its normal apathy." ^[15]

However, McDonald must have received encouragement upon reading a note from Armstrong a few days later. She told him there was still hope, that Dave Saunders, a CU psychometrician on the staff, would take a strong positive stance if dissension arose in the Spring as she expected it would. She felt that his data-based computer runs gave him

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the power to make or break the study. ^[16] Ten days later Saunders distributed an alternative outline to the one developed by Low for the final report. It was, in effect, a dissenting viewpoint. It remained to be seen how many of the staff would agree to it.

Although McDonald might have been heartened by Armstrong's remarks he felt the situation had deteriorated to the point that the NAS had to establish a panel to escalate scientific concern over UFOs. He told Seitz this and also laid out in general terms the panel's tasks while hurling a few barbs at CU. Finally, he argued it was the responsibility of the NAS to confront the problem and so he asked for an appointment to discuss the matter. ^[17]

Reaction to Condon Statements

In mid-September Condon gave another of his UFO talks. This one was for an Atomic Spectroscopy Symposium of the National Bureau of Standards (NBS) in Gaithersburg, Maryland. A friend of McDonald's, William Bickel, attended and he wrote a memo to McDonald expressing his disgust with Condon's funny, entertaining stories about contactees, hoaxes, and research problems confronted because of ridicule, when Condon had an opportunity to stimulate the interest of the best atomic physicists and spectroscopists in the country. [\[18\]](#)

In a letter to Low discussing UFO cases McDonald raised the issue of the NBS talk and mentioned his concern over the matter. Low did not respond. [\[19\]](#) Donald Keyhoe at NICAP received a copy of the Bickel memo from McDonald and felt it had to be followed up. McDonald suggested to Keyhoe that he indicate that he had the memo to strengthen his bargaining position with Low. McDonald said he would wait until October 4 to see if Keyhoe obtained an answer. If he did not then McDonald would write Condon himself. [\[20\]](#)

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However, before this could be settled McDonald received word of another Condon slap at the UFO phenomenon. This time while speaking with the press on September 27 he indicated disillusionment with the UFO business, almost wished he could drop it, and had praise for a *Science* article on UFOs by William Markowitz, a physicist at Marquette University, who attempted to demonstrate the impossibility of interplanetary travel given the physical laws as we know them. Lastly, Condon said he agreed with Markowitz that twenty-first century physics would laugh at us for investigating UFOs. [\[21\]](#)

As a result of the above statements made by Condon a concerned William Hartmann, a UA astronomer doing UFO photo analysis for the CU project, called McDonald. He tried to explain that Condon's isolation from the project permitted him to concentrate on crackpot cases while the remainder of the staff pursued better data. He said he (Hartmann) talked the staff out of a September 28 press release refuting Condon's September 27 remarks and as far as Hartmann could tell a blow-up would probably be avoided, as would a minority report. It was likely that a compromise would be reached and even if a dissenting report were written only Condon's voice would be heard.

Hartmann believed that Condon's statements subtly sealed the lips of the project staff who could not contradict him and found this reinforced by Bob Low's comments that the staff would now be forced to go along with Condon. At this time McDonald was about to write Condon, a strategy Hartmann seconded, and in fact urged McDonald to emphasize in his letter the need for

Condon to keep in close touch with his staff. [\[22\]](#)

This increased McDonald's concern for the future of UFO studies. His worst fears were apparently being realized. To determine if this

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were true he augmented his efforts to gather information about the inside workings of the UFO project and the authenticity of various public statements attributable to Condon.

Considered a fighter by all who knew him, Condon responded to the commotion over his public utterances not by conciliation, but by further inflammatory remarks. On October 16 the *New York Times* quoted him as saying he was inclined to think UFO studies should be discontinued unless someone came up with a new idea on how to approach the problem. [\[23\]](#)

Meanwhile, McDonald got together with Norman Levine and David Saunders who were on the project. They discussed Condon's NBS speech, the untenability of the Air Force conspiracy hypothesis and the possibility of Air Force interference in the project if the research appeared to be coming to a positive conclusion. [\[24\]](#)

Levine reacted to some of McDonald's questions in a note in which he said that: [\[25\]](#)

1. He couldn't find a copy of Condon's letter to the editor.
2. Condon is 50% on the project.
3. Wadsworth's statement on the third university man is not available.
4. Low's statement on Alamosa (a case) is enclosed.
5. Low spent one day at Loch Ness.
6. He had not checked on the Rocky Mountain News Condon quote.

Point five was important to McDonald because Low spent project money to attend an astronomical conference in Europe, but did not contact any European UFO researchers. Low's stop at Loch Ness annoyed him further because it seemed to put UFOs and the Loch Ness Monster in

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the same category of phenomena. This was something McDonald did not accept.

By this time the disquietude at NICAP had reached a new high. Keyhoe wrote two letters, one each to Low and Condon, demanding answers to numerous questions related to the credibility of Condon and the research project. He outlined the various ups and downs which NICAP had had with Condon, including the severing of relations two months earlier as a result of Condon's public statements. Throughout Keyhoe showed concern that Condon would coopt NICAP into appearing to endorse by association a negative, perniciously concocted final report. [\[26\]](#)

The end of November found McDonald thinking beyond the CU study. Based upon a letter to Saunders we can conclude that they, along with Norm Levine, had already begun discussing the future of UFO studies. McDonald asserted that all federal research agencies were waiting for the outcome at Boulder before committing themselves. He felt a NASA size effort was in order, but if CU were negative then a different strategy would be needed. He believed a professional organization that focused on the study of UFOs was necessary to raise money in the form of dues to support a journal and research. Such an organization would be made up of academics and be research-oriented. He wanted to discuss this with Saunders and Levine the next time they met. [\[27\]](#)

Condon replied to Keyhoe, as did Low, assuring Keyhoe of the integrity and independence of the project. Condon would not respond to most of the questions because he said the answers would give NICAP advance information about project conclusions. He asserted that all of his public statements were unbiased. He also provided assurances that if

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dissenting opinions existed when the research concluded that they would appear in the report. [\[28\]](#)

However, McDonald remained unconvinced of Condon's good intentions. On December 11 he wrote Aime Michel, considered the foremost European UFO researcher, to inform him that Condon's staff did not share Condon's attitudes and "our hope lies with them." [\[29\]](#)

However, by the 28th in another letter to Michel, McDonald indicated that he was "disappointed and disillusioned with Condon." Evidently more so than previously for he said, "some confrontation is going to have to be effected. This is difficult to engineer. A number of us are working on that problem and thinking about it as carefully as we can." [\[30\]](#)

The Low Memorandum: Confrontation

1968 brought with it the confrontation which McDonald desired. He played the major role in this episode which turned on the infamous "Low memo" mentioned in the previous chapter.

Although intended as one facet of a multi-pronged offense launched by McDonald against Condon in an attempt to change the direction of the project, it turned out that the "Trick Memo," as it came to be called, became the central topic of discussion for several months. This began with a letter from McDonald to Low in late January in which he detailed his criticisms.

> He wrote to Low because of a phone conversation they had engaged in a few days earlier in which Low became upset when McDonald directed some harsh comments toward the administration of the project. He began the letter by saying he wanted to outline his main concerns about the project and that he saw no reason for Low to become upset. He argued that criticism at that time was important because it could influence the outcome of the research, and even though an unusual step to take prior

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to the completion of a study he nonetheless felt justified in doing so because the future of UFO research hung in the balance.

McDonald's major points revolved around Condon's negative press statements, his preoccupation with crackpots, his refusal to examine witnesses in good cases, the limited amount of communication between Condon and his staff and the lack of investigation of obfuscation cases (cases in which McDonald felt the Air Force had prohibited or inhibited investigation).

In addition, he discussed the Low memo in straightforward terms suggesting that its presence in the "open files" meant it no doubt could easily be explained. He also brought up what he called project communication problems, the waste he considered the \$50,000 Stanford Research Institute radar study contract, the inappropriateness of Aldora Lee's opinion poll 15 months after the project began, and the scientific unproductiveness of bringing Air Force intelligence officers up to Boulder for a briefing when the project staff were about to begin writing the final report.

[\[31\]](#)

A letter from McDonald to Hall indicates that they viewed the Low memo as very important, and quite possibly their major weapon, from the beginning. As early as January 31 McDonald told Hall that after a talk with Gordon Lore, NICAP Assistant Director (Hall had left NICAP), and Donald Keyhoe that they resolved to get the memo on the record at the NAS and at the Air Force Office of Scientific Research. Saunders apparently concurred in this tactic and McDonald commented that an NAS staff member agreed that this was the best way in which to accomplish his objectives at NAS. [\[32\]](#)

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By February 9 McDonald had not heard from Condon regarding the critical letter of January 31 to Low and, concerned that Condon might not rise to the bait, penned him a note. McDonald indicated that via CU administrative channels he knew that Condon had been apprised of the letter to Low. He told Condon that he intended to send copies to the NAS as a first step in making his case; a more detailed statement would follow. [\[33\]](#)

The same day McDonald sent off a statement to the NAS. He submitted the 1/31/68 letter to Low along with the Low memo, and his United Aircraft Research Labs paper presented January 26, 1968. He considered these a preamble to the more detailed criticisms of the "lack of scientific open-mindedness and lack of scientific vigor in project administration" which would follow and stated that they were in keeping with the concerns which he expressed to NAS staffers Coleman and Sievers on 4/20/67, Coleman on 9/29/67 and Coleman again on 1/30/68. [\[34\]](#)

The repercussions of McDonald's 1/31/68 letter to Low were much greater than anticipated. On February 8 Condon called Saunders and Levine to his office, accused them of stealing the memo from private files and fired them from the project for "incompetence." McDonald wrote Saunders and Levine expressing his distress that Condon had such small regard for the scientific matters at stake. He also regretted that Saunders and Levine had "to pay so high a price for getting on with a proper scientific investigation." [\[35\]](#)

It seems that McDonald had second thoughts about the interpretation which the NAS staffers might put on his forwarding of the Low memo and so he wrote a note to qualify his actions. He explained that only

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project members and those with a scientific concern for the project had copies. He said the NAS received one because of its responsibility in the area. [\[36\]](#)

He also evinced a concern that Keyhoe of NICAP, known for his publicity exploits, might over-react to the situation. He explained to Keyhoe that Saunders and Levine would not go to the press and intended to respond solely at the NAS level until it was certain what the NAS would do. Consequently there was no need for NICAP to get involved in the behind-the-scenes activities.

In the same letter McDonald conveyed a bit of the thinking underlying his own involvement. He felt that Condon used the incompetence excuse for the firings to insure that reporters would not look for the memo. He indicated that Condon called Dr. Harvill, president of the UA, to tell

Richard Kassander, McDonald's superior, that he (Condon) would soon contact Kassander demanding the return of the memo. However, McDonald had briefed Kassander on the situation and so Kassander told Condon to call McDonald if he wanted the memo. ^[37]

In troubled times McDonald usually got off a note to Jim Hughes his project monitor to keep him informed; this was no exception. He told Hughes of the events which occurred and passed along copies of the memo and NAS correspondence which he wanted Hughes to keep confidential. He said he forwarded the material to keep Hughes apprised in light of the day when he would apply to the ONR for funding to do UFO work. He told Hughes that any discussion of these matters should go to his home address because he didn't want such explosive material channeled through his department. ^[38]

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A cordial letter which must have infuriated McDonald arrived from Seitz. He contended that Condon was a distinguished scientist who took his work seriously; press and grapevine accounts no doubt were superficial. Seitz indicated that the project had to run its course, but he would discuss McDonald's suggestions with Condon at the next opportunity. ^[39]

Condon answered McDonald's letters with questions and accusations of his own. He asserted that the Low memo was stolen from private files because it bore a date prior to the commencement of the project. Referring to his talk with Kassander he said he had done so because he thought the memo had been misrepresented to McDonald and once McDonald understood that he would return it. Now that he realized that McDonald would not return it he considered such conduct gravely unethical.

Furthermore, Condon said the review request of the project which McDonald made to Coleman at the NAS puzzled him. He wanted to know if McDonald desired a review prior to the completion of the report, or a review of the situation as a whole. Even though McDonald had adopted an adversary position with respect to the project in Condon's eyes, he closed by asking McDonald's cooperation on several procedural matters concerned with UFO Investigation. ^[40]

In a newsy note to Hughes, McDonald forwarded the Seitz and Condon retorts with the comment that the "NAS will not pick up the hot potato." He asked Hughes to keep the letters confidential and hoped they would bear on getting him some support in the future. Regardless of the cold shoulder given him by the NAS he indicated to Hughes that he intended to write up and forward the statement of concern already begun. ^[41]

By this time Saunders and Levine had started legal action against Condon for the use of

"incompetence" as the reason for their firings.

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Mrs. Saunders wrote McDonald thanking him for the support he gave the fired pair in a press release which appeared in the *Denver Post*. She was worried because Condon allegedly told Saunders, "for this (releasing the memo) you should be professionally destroyed." [\[42\]](#)

The Low Memorandum: Aftermath

McDonald again wrote Aime Michel to ask him to keep European ufologists abreast of the CU situation. He told Michel that "behind the scenes" efforts to force changes in the project were in progress. He said that although Hynek's participation had been hoped for, he nevertheless dropped out. The loss of Saunders and Levine, according to McDonald, seemed a high short-term loss but would be worth it in the long run. Incompetence not conspiracy was the villain, along with Condon's prestige, which proved a formidable weapon, as attested to by the lack of NAS action. [\[43\]](#)

It is difficult to assess what McDonald thought would be gained from publicly attacking Condon in UFO circles, but this is what he did and advocated. In a note to Charles Bowen, editor of the British *Flying Saucer Review*, he asked that Bowen speak out against Condon's project management if he felt this were possible in good conscience. [\[44\]](#)

Through his inside channels McDonald learned more about happenings at CU and passed this along to Hughes. Apparently a staff meeting took place at which Condon received considerable criticism. It appeared that he saw for the first time that the project had gotten into Low's hands and that Low was incapable of running it. [\[45\]](#)

McDonald was more informative in writing Hall the same day. He said that Roy Craig would be the primary investigator with the departure of Saunders and Levine and Craig might leave according to rumor if Low were not fired. Moreover, while Condon told Craig he could not go as

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far as the ETH, he did say that Craig could write report conclusions in which UFOs were considered a serious problem warranting further attention. McDonald suggested caution, however, since people on the project claimed that Condon could not be trusted.

On the legal front McDonald planned to get a lawyer to force Condon to retract various charges,

in particular, that McDonald had coerced Saunders and Levine into stealing the memo. Saunders and Levine were also proceeding with legal steps as mentioned previously. [\[46\]](#)

About the same time Mary Lou Armstrong, Condon's administrative secretary and full participant in the project, proffered her resignation. In a ten-page letter she argued that Low was the cause of the project's problems. She enumerated nine points which portrayed Low in a negative light and suggested that if Condon had directed the project the conflictual situation would have been avoided. [\[47\]](#) This letter was cause for rejoicing on the part of McDonald and others of his persuasion because it seemed that this placed more pressure on Condon to take a further look at the conclusions his project would endorse. For at this time a set developed by Low already circulated among the staff.

The gravity of Armstrong's actions are best revealed by the letter through which Condon replied. He apparently spoke with her on the phone and then wanted to reaffirm the confidentiality of the resignation. He said he would consider it both unethical and a grave offense to reveal its contents which he suspected people would tempt her to do. [\[48\]](#)

On February 28 William Messing, McDonald's attorney, wrote to Condon. He informed Condon that unless he retracted the accusations made to Kassander, Harvill and the NAS in ten days Messing would have to take all necessary steps to protect McDonald's legal remedies. [\[49\]](#)

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On still another front McDonald, Saunders, Levine and Armstrong took action of their own. They met with John Fuller, who previously wrote two UFO books, and now planned to pen an expose of the CU project focusing on the Low memo. He spoke with Condon, Low and Manning (the latter being the recipient of the memo) but received "no comments." McDonald told Hall he expected the article to appear in *Look* in four to six weeks. He also argued that although the NAS had taken no action, that once the article came out it would have to, if only behind the scenes. [\[50\]](#) In writing the article McDonald assured Fuller that he could count on the cooperation of William Bickel who wrote the note to McDonald describing Condon's NBS talk. [\[51\]](#)

Condon said in his February 15 note to McDonald that he had not studied McDonald's January 31 letter to Low, so in his response McDonald asked Condon to do so. In addition, he spoke to Condon's other points, the most important being, in McDonald's eyes, the use of "stolen" and "coerced" in his 2/15 letter and the intimation that McDonald had not attempted to cooperate with CU.

With regard to the former McDonald said he did not participate in any coercion or theft. He demanded a retraction which would go to himself, Kassander and Seitz. He said he wanted to

hear from Condon on those charges. Concerning the charge of non-cooperation McDonald detailed his distress with the project going back to an 8/1/67 conversation with Condon. Moreover, he argued that this statement on the part of Condon revealed how little he knew about his own project, whereupon McDonald proceeded to list his efforts to cooperate with Low and Condon from as early as 10/16/66. [\[52\]](#)

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After his response to Condon, McDonald sent an enraged note in answer to Low's missive of 2/2/68 in which Low asked for case material in a way which McDonald interpreted as feigning naivete concerning McDonald's past offers of aid. Probably McDonald suspected this to be an effort on Low's part to develop "files protection" regarding the relationship of the project to McDonald just in case an investigation of the project were ever made.

McDonald asserted that the letter had a hollow ring to it; that it was written in a style suggesting Low had just heard about his (McDonald's) interest in UFOs. He said he found the events at Boulder shocking and was still waiting for a retort to his critical letter of 1/31/68. [\[53\]](#)

In his fifth letter of the day McDonald sent off a dissatisfied reply to Seitz at the NAS. He was sorry the concern shown by Seitz was so slight and paraphrasing Seitz said, "I think it will be scientifically undesirable to let the Condon study run its course." He was upset that the material he had sent Seitz (the 1/31/68 letter to Low and the Low memo) had not convinced people at the NAS that Condon and Low had a preset bias which the Air Force overlooked. In the selection of the NAS Review Panel he hoped this would be considered. [\[54\]](#)

Condon wrote McDonald to the effect that he rejected McDonald's notion that the memo was taken from "open files." That it was done surreptitiously was proof enough for Condon that the perpetrators were engaged in wrong-doing. He believed McDonald's conduct was unethical and explained that, because the issue had become a legal matter, he had been advised to terminate correspondence by his attorney. [\[55\]](#)

McDonald, after mentioning that he wanted to be included in the indemnification insurance he understood the *Look* lawyers were considering

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for contributors to the Low memo article, told John Fuller he was glad the *Look* editors cleared the manuscript. He said, "I feel sure that its impact on scientific attitudes (as well as public attitudes) concerning the handling of the UFO problem will have a most salutary effect." [\[56\]](#)

However, he believed the article needed several sentences indicating Harriet Hunter (a project staff member) confronted Low with the memo in October 1967. This would thwart, he thought, Condon's claim that it was done behind his and Low's backs. ^[57]

Meanwhile McDonald decided to postpone any legal action against Condon for maligning his good name. He told Hall he felt it would be a tactical error to pursue legal instead of scientific channels in a dispute which was essentially scientific. ^[58]

When the *Look* article finally came out McDonald found it a bit short at 5,000 words, but he believed the salient points were put into the open record. He sent fifteen copies off to various foreign UFO groups. ^[59]

On April 30, a day prior to publication, Keyhoe managed to complicate the already confused situation by calling a NICAP press conference "to blow it all open." What annoyed Keyhoe was the fact that *Look* did not intend to give NICAP a box comment in the article, and that Fuller and the *Look* editors refused to see him in order to avoid a confrontation on the issue. At the press conference Keyhoe broke relations with the Condon Study, accused Condon of a conspiracy (according to the press) and released copies of the memo to the press, which angered Saunders, Saunders' lawyer and Fuller. ^[60] Saunders' concern was primarily with his court case against Condon. He feared that what Keyhoe said might get back to Boulder and hurt his chances,

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particularly since Philip Klass had taped the press conference and reportedly took it to the AFOSR. ^[61]

Activity in Other Venues

At this point it would appear that the NAS did not react as McDonald expected and so he took his case to other quarters. It is unclear whether he approached *Science* or vice-versa. In fact, the situation is further confused by a letter to *Science* from one of its editors, Daniel Greenberg. ^[62] In it Greenberg responded to Lewis Branscomb's criticisms ^[63] of the article which finally did appear by saying that initially Condon approached *Science*, which already planned an article on CU, when he (Condon) thought he would have to counter the negative publicity from the *Look* article. When the negative publicity failed to materialize he refused to cooperate with *Science* and said he did not want a story done. At any rate in early June, McDonald wrote Philip Boffey, an editorial writer for *Science*, enclosing papers, background material, and his 3/5/68 letter to Condon. He urged Boffey to carry the matter as far as possible in the hope that *Science* might be able to rectify some of the neglect accorded the subject. Referring to his 3/5/68 letter he said,

"This whole thing is an unprecedented situation and only my strong conviction that we're dealing in the UFO problem with a matter of high scientific importance which Condon was going to casually reinter has led to my pursuing some of these courses." ^[64] Boffey eventually wrote an article critical of the Condon Project for *Science*. ^[65]

During this same period Congressman Roush of Indiana, who with McDonald engineered the House Science and Astronautics Committee

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hearings on UFOs in July of 1968, wrote Seitz at the NAS, probably at McDonald's request, making a few suggestions. He wanted the NAS to:

1. Examine the *Look* allegations and McDonald's objections to the Condon Study.
2. Examine other significant cases as recommended by Hynek.
3. Look into the dismissal of Saunders and Levine.

Roush said he would suspend a Government Accounting Office review of the project which he had initiated until the NAS finished its review. ^[66]

Another attempt at obtaining publicity, this time with *Industrial Review Magazine*, did not work out as well as the *Science* gambit. When the magazine first took an interest in CU -- with or without McDonald's prodding is unclear -- Condon who was on the editorial board of the magazine, tried to dissuade the board from the story. When he failed he resigned from the board. ^[67] However, the writer assigned to the story, an A. J. Cote, could not come around to seeing the Condon Study as McDonald did. In fact, he reached the point where he felt he had laid the groundwork to indict either the project or its critics, but he couldn't come down off the fence until he saw the final report. So, after writing an introductory article, he decided not to write the final piece. For Cote it was a question of whose accusations to believe and only the report could settle the matter. ^[68] It would appear that he did not write the final article even after the release of the report. This was a blow to McDonald after his expenditure of considerable time and energy in trying to convince Cote of the efficacy of his position.

Activity on the part of McDonald with respect to the CU study slackened during the summer months. This was due to his intense involvement in the July House Hearings mentioned above and their aftermath. Nevertheless, in October his concern for the outcome of

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the Boulder study remained high, as evidenced by a note from Phyllis O'Callaghan, Legislative Assistant to Representative Roush. Apparently McDonald wrote to her asking that she use the good offices of Roush to ferret out any information she could obtain about the secret NAS Panel which was to review the Condon Report. This was important to McDonald because he wanted to contact the panel members to give them the inside story on both the UFO project and the UFO phenomenon in general.

O'Callaghan reported that Coleman at the NAS told her the panel members were chosen by the President and members of the NAS Council and would remain secret. The review would not immediately be made public, but would be forwarded to the Air Force and released at the discretion of the Air Force. ^[69]

Intent upon informing the panel of what had taken place at CU McDonald pursued another tactic. He contacted Saunders, who by that time had written a book on his CU experience with Roger Harkins, a Boulder reporter. He persuaded Saunders and his publisher to send the galleys to the NAS. McDonald felt the book contained information not in the *Look* article which would have a salutary effect on the Academy scientists. ^[70] He undoubtedly took this action because of the above-mentioned letter to *Science* by Branscomb. Both he and Saunders felt that since Branscomb was a colleague of Condon at CU that the letter, which defended Condon and indicted McDonald, presaged a negative final report. ^[71]

Rebuttal Attempt: Working with NICAP

The Branscomb letter probably marked a turning point in tactics for McDonald. Up to that time there was always the feeling that the report might prove acceptable. However, three weeks after the letter appeared

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McDonald wrote to Hall to discuss good CU cases to begin reinvestigating for a rebuttal to the report. ^[72] McDonald did not want to be caught by surprise and as a result produce a rebuttal so far after the fact that the Condon Report would be forgotten and the rebuttal, therefore, without impact.

No doubt still looking toward the future possibility of funding from the ONR McDonald sent the Saunders galleys to Hughes along with his commentary on various parts of the book. He told Hughes that Saunders' choice of UFO cases did not impress him, but the administrative history of the project was well done and it heartened him to see it in the open record. ^[73] The galleys

eventually went to the NAS also, but only because the New American Library, Saunders' publisher, sent them. Saunders himself concluded that the galleys were not a scientific document and did not feel he could forward them.

Concern for the report further heightened when McDonald heard "a well-confirmed rumor" that Condon did not want the study to go to the NAS or the public unless he were covered for libel suits from witnesses. This made McDonald believe that Condon categorized many witnesses as cheats, frauds, unreliaables or psychotics. This possibility increased his desire to prepare the rebuttal and he told Hall to begin assembling a list of all CU cases if one didn't already exist. [\[75\]](#)

McDonald argued that Condon's desire for libel protection was a ploy to "bottle up" the report. However, he didn't think it would work because of the publicity received by CU in the *Look* and *Science* articles. He thought the Air Force would release the report even if it meant providing the libel protection. [\[76\]](#)

EDITOR'S NOTE: *The footnote numbering on this page skips from 73 to 75; there is no footnote 74.*

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He continued with plans to organize the rebuttal. He told Hall that they were not doing their homework; that the report would be out shortly, they would be asked for comment and be caught short if they did not hurry their preparation. The case review could best be conducted, he thought, by getting together with Saunders, Levine and Armstrong. To pursue post Condon UFO work with a formal organization, without a good critique of CU, McDonald considered sheer folly. [\[77\]](#)

Although going forward with these plans McDonald continued his efforts to get through to the NAS review panel. He offered Seitz his comments and critique of the Condon Study which he argued would be more helpful to the panel prior to its deliberations than after. [\[78\]](#)

In further rebuttal talk McDonald urged Hall to keep their efforts quiet. He argued that the rumor chain could work both ways and if the people at CU or the Air Force were to learn of the cases that were undergoing investigation they might have them reworked or excluded from the report. The next six to eight weeks seemed critical to him. [\[79\]](#)

In the interim Hall and his associates at NICAP worked up a plan for the rebuttal. It included the release of *UFOs: A New Look* along with a press release, the writing of a "white paper" on the CU project with a covering press release, the development of an analysis of the Condon Report

with an accompanying press release, and the systematic use of the media by NICAP Subcommittees and Affiliates to present local rebuttal throughout the nation. ^[80]

Rebuttal Attempt: Working Through NAS

When Seitz replied to McDonald it was to say that he thought there would be ample time for criticism after the report and review came out. He asserted that the panel was chosen to be as impartial as possible and would be given the Roush Hearings to peruse as well as McDonald's

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congressional statement. ^[81] This implied that everything which McDonald had to say could be read in those two documents. McDonald retorted that the hearings did not touch upon the Air Force handling of the UFO problem or the Condon Study. Therefore, access to the Hearings would not permit the committee to weigh those points he believed most salient. McDonald closed by directly asking Seitz for the names of the Review Panel members. ^[82]

In correspondence with Malone he said Seitz and the secrecy at the NAS made him uneasy. He thought the NAS was in the process of digging itself into a hole with Condon and so he asked Malone to provide any constructive suggestions that might occur to him. ^[83]

The interaction with the NAS continued with McDonald, not hearing from Seitz in six days, dashing off a quick note in a further attempt to get what he considered vital information to the Review Panel. He asked Seitz to give the Panel his 1/31/68 letter to Low as well as his 2/9/68 and 3/5/68 letters to Seitz expressing grave concern. McDonald claimed that he took every step since his 4/20/67 meeting with Seitz with misgivings, but he continued to believe the problem sufficiently important to justify his actions. He said that he wanted confirmation that the correspondence had been forwarded to the Review Panel. ^[84]

The same day Seitz wrote McDonald that any material he had for the Panel should be forwarded. However, the Panel had to remain anonymous to avoid intrusions into the members' lives. ^[85]

The entire situation looked ominous to McDonald after Lou Corbin told him that John Sievers at the Academy left the impression that all the panelists were Academy members. McDonald began attacking on another front. Having once successfully engineered House Hearings on UFOs he

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tried again. Jerry Pettis, a House member from California, publicly discussed the possibility after

the Redlands sightings in 1968. Now McDonald told Malone that he contacted Representative Udall asking him to urge Pettis to act. [\[86\]](#)

Release of the Colorado Report

Early January found McDonald eager to get at the soon-to-be released report. When he determined the actual date that the NAS would make it public he telegraphed Seitz, "Understand Condon Report available Thursday. Will fly Washington Wednesday and phone Thursday about studying copy at Academy or other source." [\[87\]](#) A week later he had been through the thousand-page document and was up in arms, for as he anticipated the conclusions were, from his perspective, negative. Condon stated: [\[88\]](#)

Our general conclusion is that nothing has come from the study of UFOs in the past 21 years that has been added to scientific knowledge. Careful consideration of the record as it is available to us leads us to conclude that further extensive study of UFO's probably cannot be justified in the expectation that science will be advanced thereby.

Properly conducted research, according to McDonald, would have justified an unfavorable outcome. However, McDonald viewed the research as prejudiced in approach, poorly conceived and improperly conducted; hence a rebuttal was in order. This took both verbal and written form, occupying him for most of 1969. He began by contacting Hynek at Northwestern to get him to speak out on the Report. In addition, he got Kuiper, Hartmann and others at the UA together in a colloquium for purposes of indicting the Boulder study.

The following week McDonald put in full-time rechecking the work done by Condon and his staff. He found numerous glaring errors epitomized by the statement, "the more I look the worse it gets." He

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admitted there were some bright spots, but they were not in the areas which made a difference. [\[89\]](#)

During this period plans went forward at NICAP for an extensive written rebuttal. Constant contact between the NICAP staff, particularly Hall, and McDonald took place. The latter intended to make a large written contribution as well as softening up selected government, military and academic audiences through speaking engagements. His speaking feats, prodigious since October 1966, took him to the podium seventy-eight times. In the first six months of 1969 he intensified these efforts by speaking out against the Condon Report at eighteen different

functions (see Appendix A for a list of his speaking engagements).

He pursued another tactic by attempting to find disaffected members of Condon's staff who would publicly denounce the Report. He asked William Hartmann, who did the photo analysis work for Condon, if he would make the same statements which he had made at an IAP colloquium, where he disagreed with the conclusions and recommendations of the Report, in a more public way in *Science*. He also wondered if other project members might join in since in a recent talk with Franklin Roach (who did the section on astronaut sightings) he found out that a number of the staff did not agree with the conclusions of the Report. ^[90]

Although Hartmann was not impressed by the substance of the UFO phenomenon, he did express concern that it did not receive fair treatment from journal editors. Nevertheless, he felt he could not speak out against Condon because Condon's chapters of summary, conclusions and recommendations were under his own name. Consequently, to criticize Condon publicly for using wording different from that used by

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his staff in other chapters to describe the research of the latter would be impossible. Hartmann further suggested that McDonald would be wise to take his case to *Science* rather than to the speaking circuit. ^[91]

With Hynek, Saunders and others McDonald did a Voice of America tape on the UFO problem and focused on the Condon Report. It disappointed him that Condon refused to participate. At this time he mentioned to Hughes undergoing the first major change in his position since June 1966, in that he believed the Condon Report was just one more example of the Air Force receiving bad advice from the scientific community. The Air Force did not foist a conspiracy on the public; the scientific community failed to advise the Air Force wisely and a twenty-year foul-up resulted. ^[92]

Continuing Rebuttal Activity

McDonald also recruited others to help in the rebuttal. He asked Hall if there was anything that needed doing which William Bickel, a professor of Physics at the UA, might work on? He said Bickel's area was plasma spectroscopy and he wanted to help. ^[93] By this time McDonald's ideas regarding the size of the rebuttal were grandiose. He spoke in terms of 300 pages, ^[94] while Hall considered 150-200 pages the optimum length. ^[95]

One might ask of what this rebuttal work consisted. To provide some idea of what took place the

following letter from McDonald to Ted Bloecher at NICAP is presented in full. [\[96\]](#)

Unfortunately, from McDonald's standpoint, after several months of effort both on his part and on NICAP's, administrative problems at NICAP forced the rebuttal to a halt. Nevertheless, the output from its preparation provided McDonald with a great deal of grist for his verbal and written critiques of the Condon Report.

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February 26, 1969

Mr. Ted Bloecher
N.I.C.A.P.
1536 Connecticut Avenue, N.W.
Washington, D.C. 20036

Dear Ted:

There are several points to briefly mention concerning cases that I have recently been checking.

1. I talked with Tom Nicholl, Leawood, Kansas, and was well-impressed with him. There were only a few details over and above his rather complete report that I got over the phone. Mainly, I feel that it can be treated as a fairly solid report and an interesting indication of Condon's casual handling of such cases. We made a tentative date to have lunch together in Kansas City when I am speaking to the AIAA Section there in late May.
2. Some weeks ago you sent me NICAP file material on the 3/25/66 sighting in Ann Arbor by Grenier and others. I have now talked with Grenier and with Kartlick who works at a University of Michigan lab (supported by NASA) but not correctly called a NASA Lab). Sounds like a strong case and I am getting further information from both of them on it. Dr. Espey, whose tergiversations with respect to swamp gas are an amusing facet of that case is no longer in Ann Arbor. Kartlick will try to locate him for me.
3. Finding the 3/25 incident more impressive than I would have guessed, I reviewed the short section in the Condon Report dealing with the March, 1966 wave in Michigan. The more I pondered Condon's casual rehash of the Air Force statement plus Bland's suggestion that maybe it was swamp gas after all, the more annoyed I became. There was the series of sightings which led directly to the creation of the Condon Committee and he passes it off with a page and one half most of it direct from Air Force press personnel.

So last Sunday I called Frank Mannor. Briefly, that has led me into an intensive recheck on the Dexter-Hillsdale sightings, and it has left me boiling mad at Allen Hynek. Quite by chance Allen, passing through Tucson, called me from the airport a few days ago and I

gave him some of my sentiments directly.

I have interviewed Mannor, Dexter police chief Taylor, Deputy Fitzpatrick, Hillsdale Civil Defense Director VanHorn, Sheriff Harvey, Sgt. Schneider, Hillsdale College Director of Public Affairs Ferguson, Deputy McFadden, and there are three or four more whom I am still hunting down including Mrs. Kelly Hearn of the Hillsdale sighting, Nolen Lee, and Robert Hunawill.

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I am digging into this because it is one more point of critique and rebuttal of the Condon Report which I now believe warrants strong emphasis. It also discloses the abysmal Condon-like prejudices that Hynek brought to that case and that characterize so much of his pre-1966 Bluebook work. I can't take time to even give you the highlights of what are now about 20 pages of telephone notes from this interviewing, except to say that it is one more incredible facet of the UFO problem.

If in the NICAP files you have anything from Mrs. Kelly Hearn, who I have yet to locate, I would very much appreciate copies. I see no point in asking you to copy what must be an extremely large file of material on the Michigan cases. However, if in going through it you should see any gems that you would guess that I have never studied, obviously I would be interested.

4. But there is one matter that I would like to ask you and Dick and Isabel to try to help me on, if possible. It's a detail in the 6/29/54 BOAC stratocruiser case. I just got a tape of the audio of the BBC-TV and I have listened carefully to the interview with Capt. James Howard. I have also gone over an article which was published in 1955 in *Stag* magazine, which was written by Howard "as told to" some writer. I have only one or two contemporary clippings on it, plus the other obvious references in the Evidence, etc.

An extremely important detail, that may serve to explode completely Thayer's mirage explanation concerns the height of the cloud tops below the stratocruiser. He was at 19,000 ft, and in the *Stag* article he states that the cloud tops were down around 5,000 ft. Do you know of any information that would tend to confirm cloud heights that low? I have calculated, from standard refraction theory, the horizon dip-angle for any of a number of assumed cloud deck altitudes. If the cloud tops lay anywhere below about 17,000 or 18,000 feet the dip-angle is so large that Thayer's suggestion of a superior mirage from an assumed overlying inversion layer is absurd. Can you help me at all on this?

If in going through the NICAP or the CSI files you locate anything in the way of any substantial press interviews or other articles in addition to the *Stag* article, I would greatly

appreciate getting copies.

I wrote to Captain Howard, c/o BOAC, several weeks ago, and then followed up with another letter about two weeks later. I want to get directly from him, if possible, information on those clouds. To date, no answer. I may write him again in about a week.

My California trip and other matters have gotten in the way of further writing of my RESA draft. I am getting back to it now.

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I can't take time and space to elaborate, but I have some very interesting information on the Kirtland case (page 141 in the Bantam edition), as well as the 5/13 Colorado Springs radar case. Also, Norm Levine has sent very relevant commentary on the Kincheloe case. I have so much material to discuss that my principal problem is how to boil it down.

Best regards,

James E. McDonald
Senior Physicist

JEM:mlt

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Nor did he give up on showing the Air Force the error of its ways. Probably his revised position with respect to the poor scientific advice provided in the past spurred him on. In the same time-frame as his attempts to critique the CU study he again knocked on the Air Force door. He informed William Price at AFOSR that he would be in Washington from June 9-11 for an NSF Advisory Panel Meeting on the Atmospheric Sciences and requested Price to arrange a colloquium on UFOs for interested AFOSR personnel. He told Price that after four months of checking he was very concerned about the Condon Report. ^[97] Price said the Air Force no longer had responsibility in the area, but McDonald could confer with the OAR and AFOSR people who handled the CU contract. ^[98]

Of paramount importance to McDonald was the anticipated Air Force action on the Condon recommendations. He spoke at the AFOSR giving a critique of the Condon Report and told Price that since the Condon recommendations did not rule out further studies if properly designed, that

he would submit a proposal for funding. ^[99] A Colonel Whitfield Martin wrote to McDonald some time later informing him that the AFOSR had no responsibility for UFOs and so he could not encourage the submission of a proposal. ^[100]

The CU Data Controversy

In his continuing effort to check cases in the Condon Report McDonald became involved in another imbroglio. One of the problems with the Condon Report, at least in the eyes of those interested in reinvestigation of cases, was the fact that witness names were not used and the geographic location of the sighting was given as a section, such as the Northwest United States. Condon claimed he adopted this approach so that witnesses would not be harassed after publication of the report. Some of the cases investigated were well enough known, however,

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that McDonald could determine from the above information alone who to contact. Yet, others were more obscure and it was to obtain information on these cases that he contacted Dr. Ralph Ellsworth.

He phoned Ellsworth, head of Norlin Library at CU, to check some cases in the Project files which Condon presented to the Western Historical Collection in Norlin. Ellsworth indicated McDonald needed Condon's permission to examine the material. McDonald thereupon wrote Condon asking to see the files in May and/or June. ^[101] Condon replied that McDonald could not consult the files because, "it is not our intention to make them publicly available in the near future." ^[102]

This undoubtedly brought McDonald to a slow boil. He had already obtained copies of the radar cases analyzed by Gordon Thayer in the Condon Report from Blue Book, but the witness names were excised. He argued to Condon that because the project had access to the names and because the copies of the Blue Book files held at Norlin were not a direct outgrowth of the project that he should be permitted access. ^[103] This argument did not persuade Condon. He reiterated that project files would not be opened in the near future, especially the Blue Book files. He asserted it would be inappropriate to do so since the Air Force kept witness names confidential. To get McDonald's hackles up he forwarded a copy of his American Philosophical Society paper on UFOs in which he placed UFOs in the category of occult nonsense. ^[104]

To this McDonald responded by going over Condon's head. He contacted Morris Udall who spoke with Secretary of the Air Force Seamans and Representative Moss who chaired the House Freedom of Information Subcommittee. He also forwarded Condon critiques of the Report and

told him that, "your Philadelphia and Irvine talks indicate you must have no

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real awareness of the weakness of the position you have developed" and "in giving the Academy such a Report, I believe you did science a direct disservice. That the Academy processes could lead to endorsement is disturbing." [\[105\]](#) In a terse note Condon thanked him for the enclosures and advised McDonald of the termination of the correspondence. [\[106\]](#) This is the beginning of the CU data controversy.

Even though this only brings us halfway through 1969 it is as far as McDonald's correspondence goes in that year with respect to the Condon Study, although it picks up again in mid-1970. This is largely due to his involvement in the SST debate, his preparation for the 1969 AAAS UFO Symposium and his battle with Philip Klass.

The CU data controversy is the last extended interaction of McDonald with the CU project other than his intermittent public talks, his *Icarus* review which put his criticism in a respected scientific journal [\[107\]](#) and his AAAS paper presented in Boston in December 1969. Therefore, this chapter will close by following his efforts to obtain access to the Condon Project data in 1970 and 1971 and omit mention of any other extant, but limited activities relating to the Condon Report.

In 1970 McDonald continued to develop his personal rebuttal to the Condon Report. Failing to secure satisfaction from Secretary of the Air Force Seamans and Congressman Moss, in June of 1970, after waiting a year, he made another request of Ellsworth at Norlin Library to access the Xeroxed Blue Book files which were part of the Condon Project material. [\[108\]](#) He considered this a propitious time to act because of Condon's recent retirement and he (McDonald) intended to travel to NCAR in Boulder for a meeting in mid-July. [\[109\]](#) John Brennan, Curator of the Western Historical Collection, replied for Ellsworth explaining that the

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files could not be made available because the Library continued to operate under the 4/22/69 instructions given by Condon. [\[110\]](#)

This attempt aborted as did a personal conversation with Brennan on July 6 while visiting NCAR on business; nevertheless, McDonald did not consider this the end of the matter. He wrote Ellsworth again using a slightly different approach. This time he pointed out that the Blue Book cases were archived at Maxwell Air Force Base in Alabama which meant they were available,

but he stated that he had no reason to be in that area of the country. However, he said he would be in Boulder in late August and asked Ellsworth to obtain Condon's permission to Xerox two to three dozen cases. [\[111\]](#)

In reference to this problem in a letter to Saunders, McDonald revealed the rationale behind his request. "I'm asking if I can see just the Blue Book Xeroxes alone. Since those items are not really research material generated by the Condon Project, but rather provided at no cost by a government agency, it would seem that my plea can't be brushed aside as unreasonable." [\[112\]](#)

Ellsworth responded that he asked Condon's permission and awaited a reply. [\[113\]](#) McDonald informed Ellsworth that he would be in Fort Collins for an American Meteorological Society meeting from August 24-28 and in Boulder during September. He enclosed a list of the cases he wanted to look at. [\[114\]](#) Several days later Ellsworth indicated to McDonald that Condon refused access on the grounds that a confidential relationship existed with the Air Force, but if the Air Force were to agree and the material could be sorted out, it would be alright. Ellsworth said that unfortunately the Library did not have the money or the staff to do the

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sorting and so he suggested McDonald travel to Maxwell Air Force Base in Alabama to acquire the material. [\[115\]](#)

We can obtain a feeling for Condon's position on this matter by looking at the rationale he presented to Ellsworth for the above response. He stated, "I believe that the material consists of a mixed bag that needs a lot of sorting. If there are some rough notes of the Colorado Project fieldwork in it, I would expect him (McDonald) to pounce on that and criticize our mode of handling it."

"But more formally, a refusal can be based on the fact that the Air Force cases were given to us in confidence with the understanding that names of persons involved in the cases would not be disclosed, so to let him see the files would be to violate our understanding." [\[116\]](#)

McDonald reacted to the Ellsworth letter by writing Colonel William Coleman at SAFOI in Washington to ask permission to Xerox the Blue Book material. [\[117\]](#) He forwarded a carbon to Ellsworth and told him that Saunders claimed the files were ordered in such a way as to minimize the staff time involved in sorting the material. [\[118\]](#) Coleman's answer was to the effect that McDonald could have access to the material if it could be worked out with the University of Colorado. [\[119\]](#) Ellsworth followed this up by saying that it was alright with him but McDonald still needed Condon's consent. [\[120\]](#)

Finally Condon personally entered the fray. He told McDonald that the project files were not a part of the Norlin collection, but were only in storage there until he could decide what to do with them. He went on to say, "On the basis of my previous experiences in dealing with you I have decided that you will not be given access to any of this material. As I read the fourth paragraph of Colonel Coleman's letter

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the names and addresses of witnesses are confidential. We have no machinery for administering this provision of their regulations." [\[121\]](#)

McDonald actually received the above letter from Condon on August 25. On August 24 while in Boulder he went to Norlin to determine if he could have access to the files. He was told that Condon removed them from the Library. He wrote Condon reiterating his position that he only wanted Blue Book Xeroxes, was already working with similar material from Maxwell and, according to Ellsworth's 7/24/70 letter, only needed Air Force permission and the cooperation of the CU staff to get the files. He said that since his prior correspondence made it clear that he had access to Maxwell material, and Coleman's letter only dealt with the disclosure of witness names in the scientific literature, and not with the examination of the files, he found Condon's refusal on the grounds that he had "no machinery for administering this provision of their regulations" difficult to understand.

He asserted that he still wanted the material and claimed Condon reversed himself on the conditions for access which he had previously laid down. McDonald wanted to know if Condon thought the files were his private property and, if so, if he considered it his right to destroy them? [\[122\]](#)

This closed the debate for 1970 because Condon never replied. There is only one more chapter to the story. It came in February 1971. Gordon Thayer, who did the radar case analyses for the Condon Report and later had some second thoughts on the UFO problem, agreed to write up the classic Lakenheath, England radar case for the journal *Aeronautics and Astronautics* of the American Institute of Aeronautics and Astronautics. He called McDonald for a copy of the Blue Book file, for by that time

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McDonald had made several trips to Maxwell, but McDonald told him to get it from Condon since he (Thayer) worked at ESSA in Boulder. Thayer replied that he asked Condon and was told

that the files were too bulky to store so he had destroyed them. ^[123] McDonald was furious, but to no avail. The attempt to guide the Condon Study and then to rebut its findings was over. McDonald lost, but what did he try to do, and how did he try to do it?

SUMMARY AND CONCLUSIONS

He operated within the framework established in the last chapter. With this strategy he intended to legitimate UFO research and in the process shift a paradigm. To accomplish this he tried to insure a favorable outcome to the Condon Study. For it was a well-recognized fact that the Boulder project would determine the future funding policies for the examination of UFO data in the short run and possibly for decades, McDonald knew that, because at all the federal agencies where he took his case they told him "to wait for Colorado." The answer seemed to be, then, to offer aid and guidance if necessary, through the completion of the research.

McDonald used several tactics beginning in 1967 and continuing through early 1971. In the beginning he offered Condon aid both to help and to monitor CU. When that failed he offered what he considered constructive criticism. Then he engineered a confrontation with Condon and concurrent with all three of the above tactics he took his case to higher authority in the form of the NAS. Finally, after all else failed McDonald went about the task of developing a rebuttal to the final report.

In March of 1967 McDonald offered to brief Condon and asked him to

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appear on the American Society of Newspaper Editors Panel with Menzel and Quintanilla, but Condon refused on both counts. A month later he publicly stated that Condon did not spend much time on the project and in August, after considerable persevering. Low asked him to brief the project staff. He came away convinced that the project was in trouble. Condon reinforced this feeling the following month when he gave his infamous NBS talk about which McDonald protested to Low.

Condon's NBS talk was apparently a turning point, for after that McDonald met with Saunders and Levine in early November and talked of engineering a confrontation. This did not materialize until two months later when McDonald sent his letter critical of the CU project administration and of the Low memo to Low. The letter caused Condon to fire Saunders and Levine which resulted in their combining with McDonald and Fuller to prepare the *Look* article exposing the inner workings of the project. This, McDonald hoped, might turn the project

around.

While Saunders and Levine began legal action against Condon so did McDonald. The latter felt that Condon owed him an apology for claiming that he forced Saunders and Levine to steal the Low memo. Eventually he discontinued the suit claiming that it was a poor tactic in a primarily scientific matter.

McDonald's appeal to the NAS took place at the same time as the above tactics. This was the result of his fear that things were not what they should have been in Boulder. Condon's February 1967 talk to the Corning Glass Ware Chapter of the [Sigma Chi Iota] Honorary Fraternity exacerbated these fears. This began McDonald's appeals to the NAS.

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He wrote Philip Seitz, president of the NAS, in March to explain that he wanted to brief Seitz on the UFO situation because he didn't want the NAS to be caught unprepared when the issue broke and the public began to ask questions. He obtained a hearing with Seitz in April, prior to which he forwarded some of his UFO work and a list of his speaking engagements to impress the NAS president with the degree of seriousness he imputed to the problem. McDonald's pleas proved of little avail, so he bided his time for several more months.

However, after his visit to Boulder in August he came away saddened by Condon's attitude and wrote Seitz again. This time he asked that the NAS set up a special review panel to look at UFO case material independent of the Condon Study. Again Seitz told him to wait for the results of Condon's research. That he could not do, for he saw what he believed were progressively deteriorating conditions at CU. So he engineered a confrontation over the Low memo and followed this with another letter to Seitz in February 1968. He enclosed his 1/31/68 letter to Low which started the skirmish at CU (getting Saunders and Levine fired), the Low memo, and earlier letters on the Condon Project sent to Sievers and Coleman, both NAS staffers. In McDonald's mind this constituted a preamble to the detailed criticism of CU which would follow.

At this stage of the NAS appeal he feared Keyhoe might bring the press into it. He wrote him not to do so, and indicated that it could be pursued quietly at the NAS level. McDonald expected action, but got none; he thought the study should be stopped, but Seitz said it should run its course. He assumed publication of the May *Look* article on the Low memo would finally force the NAS to do something, yet again nothing happened. As the months passed and the situation continued to look grave McDonald

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became sufficiently worried about the problem that in October he contacted Representative Roush's administrative assistant, Phyllis O'Callaghan to ask her to obtain the names of the NAS Panelists who would review the Condon Report so that he could contact them. Moreover, he talked Saunders' publisher into forwarding the galleys of Saunders' forthcoming book on the Colorado Project to the NAS. Then he followed this up a month later with an offer to Seitz of a critique of the Condon Project for the NAS Review Panel. This was at a time when he realized that he could not learn the names of the panel members.

Seitz told him that his testimony at the July 1968 House Hearings would be adequate to present his position to the Review Panel, but McDonald claimed it would be inadequate and again asked for the identities of the panelists. Before he received a reply he made a last effort to convey what he considered vital information to the panel. He asked Seitz in the last letter of his appeal to insure that the Panel obtained copies of the 1/31/68 letter to Low and his 2/9/68 and 3/5/68 letters to Seitz.

Such were the tactics when it appeared that the Condon Report might be reformed or exposed prior to its publication. By October 1968, however, it seemed highly probable to McDonald that the Colorado findings would be negative. Consequently, he began to prod Hall about rechecking CU cases in order to get out a good rebuttal. He felt that if it did not come out rapidly it would have a minimal impact. Moreover, a good critique of the Report was necessary if the academic UFO research organization which he contemplated were to flourish in a post-Condon environment. Since they were starting early McDonald told Hall to keep the rebuttal

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cases quiet. He wanted to avoid the possibility that the rumors would reach Boulder and result in the omission of the rebutted cases from the Report.

When the Condon Report appeared in January 1969 McDonald went through it in a week and began to contact academics to speak against it. He spoke to government, military and academic audiences (eighteen talks between January and June) to prepare them for the prospective rebuttal. Along with Hynek and Saunders he did a Voice of America tape critical of the Report and he attempted to recruit disaffected Project members to speak out against the research.

Finally, McDonald began to recheck sighting reports which led him to the battle with Condon over the Xeroxed Blue Book cases, which were probably just an excuse to get into the general project files. Before resolution of the issue occurred he appealed to Ellsworth, head of Norlin Library, Condon, Representative Morris Udall, Secretary of the Air Force Seaman's,

Representative Moss and Colonel Coleman at SAFOIS. Yet, despite his efforts Condon eventually won the day by destroying the material.

SUMMARY OF TACTICS

Aid to the Project

1. Numerous offers of briefings.
2. An actual briefing.
3. Forwarded his UFO papers.
4. Talked with Price and Ratchford.

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Exposure of Project

1. Attempted to get Condon to sit on the ASNE panel.
2. Claimed Condon did not spend much time on the Project.
3. Protested Condon's Corning Glass Works talk.
4. Protested Condon's NBS talk.
5. Engineered the confrontation over the Low memo.
6. Contributed to the *Look* expose article.
7. Provided Philip Boffey of *Science* material for an article.

The NAS

1. Appealed to Seitz at the NAS on several occasions
2. Told Keyhoe to keep the protest at the NAS level.
3. Had Saunders' publisher send his galleys to the NAS.
4. Offered the NAS a critique of the CU project.
5. Offered the NAS Review Panel correspondence critical of CU.
6. Attempted to obtain the names of the NAS Review Panel members.

The Rebuttal

1. Prodded Hall prior to publication of the Condon Report.
2. Advised secrecy with respect to rechecked cases.
3. Spoke against the Report 18 times between January and June 1969.
4. Criticized the Report in a VOA tape with Hynek and Saunders.
5. Rechecked cases.

6. Sought out disaffected project members to speak against the Report.
7. Appealed to Condon and others for access to the Blue Book Xeroxes.

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FOOTNOTES

1. Hall to McDonald, 1/31/67, McDonald Letters, hereafter M.L. - [Back To Text](#)
2. Condon to Keyhoe, 2/2/67, NICAP files. - [Back To Text](#)
3. McDonald to Seitz, 3/2/67, M.L. - [Back To Text](#)
4. Hall to Keyhoe, 3/2/67, NICAP files. - [Back To Text](#)
5. McDonald to Condon, 3/8/67, M.L. - [Back To Text](#)
6. McDonald to Low, 3/8/67, M.L. - [Back To Text](#)
7. Seitz to McDonald, 3/22/67, M.L. - [Back To Text](#)
8. Low to McDonald, 3/23/67, M.L. - [Back To Text](#)
9. McDonald to Noyes, 3/29/67, M.L. - [Back To Text](#)
10. McDonald to Seitz, 4/3/67, M.L. - [Back To Text](#)
11. Low to McDonald, 4/25/67, M.L. - [Back To Text](#)
12. McDonald to O'Brien, 5/4/67, M.L. - [Back To Text](#)
13. McDonald to Armstrong, 8/11/67, M.L. - [Back To Text](#)
14. McDonald to Hall, 8/15/67, Hall Letters, hereafter H.L. - [Back To Text](#)
15. McDonald to Hughes, 8/17/67, M.L. - [Back To Text](#)
16. Armstrong to McDonald, 8/17/67, M.L. - [Back To Text](#)
17. McDonald to Seitz, 8/23/67, M.L. - [Back To Text](#)
18. Bickel to McDonald, 9/20/67, M.L. - [Back To Text](#)
19. McDonald to Low, 9/22/67, M.L. - [Back To Text](#)
20. McDonald to Keyhoe, 10/2/67, M.L. - [Back To Text](#)
21. McDonald to Sagan, 10/10/67, M.L. - [Back To Text](#)
22. Hartmann to McDonald, 10/11/67, M.L. notes on a phone conversation. - [Back To Text](#)
23. *The New York Times*, 10/16/67, Section A, p. 10. - [Back To Text](#)
24. McDonald to Levine, 10/31/67, M.L. - [Back To Text](#)
25. Levine to McDonald, 11/1/67, M.L. - [Back To Text](#)
26. Keyhoe to Condon, 11/14/67, NICAP files. - [Back To Text](#)

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27. McDonald to Saunders, 11/27/67, M.L. - [Back To Text](#)
28. Condon to Keyhoe, 12/8/67, and Low to Keyhoe, 12/8/67, NICAP files. - [Back To Text](#)
29. McDonald to Michel, 12/11/67, M.L. - [Back To Text](#)
30. McDonald to Michel, 12/28/67, M.L. - [Back To Text](#)
31. McDonald to Low, 1/31/68, M.L. - [Back To Text](#)
32. McDonald to Hall, 1/31/68, H.L. - [Back To Text](#)

33. McDonald to Condon, 2/9/68, M.L. - [Back To Text](#)
34. McDonald to Seitz, 2/9/68, M.L. - [Back To Text](#)
35. McDonald to Saunders and Levine, 2/11/68, M.L. - [Back To Text](#)
36. McDonald to Coleman, 2/11/68, M.L. - [Back To Text](#)
37. McDonald to Keyhoe, 2/11/68, M.L. - [Back To Text](#)
38. McDonald to Hughes, 2/11/68, M.L. - [Back To Text](#)
39. Seitz to McDonald, 2/15/68, M.L. - [Back To Text](#)
40. Condon to McDonald, 2/15/68, M.L. - [Back To Text](#)
41. McDonald to Hughes, 2/18/68, M.L. - [Back To Text](#)
42. F. Saunders to McDonald, 2/18/68, M.L. - [Back To Text](#)
43. McDonald to Michel, 2/22/68, M.L. - [Back To Text](#)
44. McDonald to Bowen, 2/22/66, M.L. - [Back To Text](#)
45. McDonald to Hughes, 2/24/68, M.L. - [Back To Text](#)
46. McDonald to Hall, 2/24/68, H.L. - [Back To Text](#)
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48. Condon to Armstrong, 2/26/68, M.L. - [Back To Text](#)
49. William Messing to Condon, 2/28/68, M.L. - [Back To Text](#)
50. McDonald to Hall, 3/5/68, H.L. - [Back To Text](#)
51. McDonald to Fuller, 3/5/68, M.L. - [Back To Text](#)
52. McDonald to Condon, 3/5/68, M.L. - [Back To Text](#)
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54. McDonald to Seitz, 3/5/68, M.L. - [Back To Text](#)
 55. Condon to McDonald, 3/11/68, M.L. - [Back To Text](#)
 56. McDonald to Fuller, 3/15/68, M.L. - [Back To Text](#)
 57. McDonald to Fuller, 3/18/68, M.L. - [Back To Text](#)
 58. McDonald to Hall, 3/18/68, H.L. - [Back To Text](#)
 59. McDonald to Hall, 4/27/68, H.L. - [Back To Text](#)
 60. McDonald to Hall, 5/8/68, H.L. - [Back To Text](#)
 61. McDonald to Hall, 5/18/68, H.L. - [Back To Text](#)
 62. Letter to *Science*, "UFO Story: Is Propriety the Issue?" Daniel Greenberg, *Science*, 10/25/68, p. 410. - [Back To Text](#)
 63. Letter to *Science*, "Irrational Public Debates," Lewis Branscomb, *Science*, 9/27/68, p. 1297. - [Back To Text](#)
 64. McDonald to Boffey, 6/10/68, M.L. - [Back To Text](#)
 65. Philip Boffey, "UFO Project: Trouble on the Ground," *Science*, 7/26/68, pp. 339-42. - [Back To Text](#)
 66. Roush to Seitz, 6/20/68, M.L. - [Back To Text](#)
 67. McDonald to Saunders, 6/25/68, M.L. - [Back To Text](#)
 68. Cote to McDonald, 8/3/68, M.L. - [Back To Text](#)
 69. O'Callaghan to McDonald, 10/17/68, M.L. - [Back To Text](#)
 70. McDonald to Hughes, 10/21/68, M.L. - [Back To Text](#)

71. McDonald to O'Callaghan, 10/21/68, M.L. - [Back To Text](#)
72. McDonald to Hall, 10/21/68, H.L. - [Back To Text](#)
73. McDonald to Hughes, 10/28/68, M.L. - [Back To Text](#)
74. McDonald to Hughes, 11/18/68, M.L. (**EDITOR'S NOTE:** This document not cited in text.) - [Back To Text](#)
75. McDonald to Hall, 11/20/68, H.L. - [Back To Text](#)
76. McDonald to Saunders, 11/20/68, M.L. - [Back To Text](#)
77. McDonald to Hall, 11/26/68, M.L. - [Back To Text](#)
78. McDonald to Seitz, 11/29/68, M.L. - [Back To Text](#)
79. McDonald to Hall, 12/1/68, H.L. - [Back To Text](#)

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80. Hall to McDonald, 12/2/68, M.L. - [Back To Text](#)
81. Seitz to McDonald, 12/4/68, M.L. - [Back To Text](#)
82. McDonald to Seitz, 12/10/68, M.L. - [Back To Text](#)
83. McDonald to Malone, 12/9/68, M.L. - [Back To Text](#)
84. McDonald to Seitz, 12/16/68, M.L. - [Back To Text](#)
85. Seitz to McDonald, 12/16/68, M.L. - [Back To Text](#)
86. McDonald to Malone, 12/20/68, M.L. - [Back To Text](#)
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93. McDonald to Hall, 2/18/69, H.L. - [Back To Text](#)
94. McDonald to Hall, 3/9/69, H.L. - [Back To Text](#)
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IV. THE ROUSH HEARINGS

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INTRODUCTION

For approximately ten years prior to McDonald's arrival on the UFO scene there were attempts to get Congressional Hearings on the UFO problem in the hope that Congress could coerce the Air Force out of its unconcerned posture toward the phenomenon. The most persevering force behind

this effort was NICAP under the leadership of Donald Keyhoe.

As a result Speaker McCormack's House Subcommittee on Atmospheric Phenomena, a part of the House Select Committee on Astronautics and Space Exploration, held closed hearings in 1958. Also there were several aborted efforts in the early 1960s, most notably on the part of Representative Joseph Karth of the House Science and Astronautics Committee. The only public hearing occurred before Mendel Rivers House Armed Services Committee in April 1966. However, from the standpoint of those who wanted hearings, it proved unproductive because only the Air Force position was heard.

As we have seen, by early 1967 McDonald was uneasy about the situation in Boulder. Moreover, he favored a many pronged assault on the UFO problem to avoid putting all his eggs in one basket. It was at this time, for instance, that he began his approaches to the NAS and initiated a campaign to influence Representative J. Edward Roush of Indiana who sat on the House Committee on Science and Astronautics.

HISTORY OF EVENTS

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Initial Contacts

For reasons which are not clear, but are probably related to some federally funded program at the UA, Congressman Roush went to Tucson in late February 1967. Somehow McDonald contacted him and they engaged in

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a lengthy session on UFOs. Apparently Roush already had an interest in the matter because McDonald commended him in a note for his "fresh examination of the problem of the UFOs."

McDonald went on to criticize the Air Force and forwarded a critical statement on Blue Book which he recently gave to the *Tucson Daily Citizen*. He further offered his assistance to Roush and his staff along with a list of UFO references. Since he would be in Washington to address the ONR and possibly NSF and the NAS on the UFO problem April 17-19 he suggested to Roush that they get together.

Lastly, McDonald stated that his ten months of intensive investigation had convinced him that the UFO issue was a problem of highest scientific importance. He urged Roush to push for Congressional Hearings which would listen to more than just the official position. ^[1]

While in Tucson, Roush also met with Gerard Kuiper, an internationally known member of the UA's Department of Astronomy, and asked him for his opinions on UFOs. Kuiper sent him a letter deprecating the subject and forwarded a carbon to McDonald. ^[2] To this McDonald replied with a note to Kuiper. He told Kuiper that his letter to Roush put him in the camp of those scientists who dismissed the problem without studying it. He suggested that if Kuiper were to apply himself to the matter he would change his mind. He further stated that Kuiper's aid in obtaining his NASA intramural grant and his call for study of the UFO issue were helpful, but did not balance out the kind of statement made to Roush which, backed by the full weight of Kuiper's prestige, ^[3] could prove damaging.

A few days later McDonald again wrote Roush reminding him of the upcoming Washington trip and reiterating the desire for hearings through

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the House Committee on Science and Astronautics. In this communication McDonald presented a schedule with which he no doubt intended to impress Roush. While in Washington he planned to speak at the ONR, the Naval Research Laboratory (NRL), and the American Society of Newspaper Editors convention. He had a tentative appointment with the president of the NAS and had not yet heard from the AFOSR and NSF where he also wanted to speak. In addition, he intended to go over to NASA on an informal basis, and on his way back to Tucson would talk at the Los Alamos Scientific Laboratory and see Lloyd Berkner, a member of the 1953 CIA-convened Robertson Panel on UFOs, at the Southwest Center for Advanced Studies in Dallas. In closing he included Roush in this impressive itinerary by asking if he might stop in and see him. ^[4]

For the remainder of 1967 there is very little interaction, at least as reflected in McDonald's correspondence, between himself and Roush. In June Dr. Phyllis O'Callaghan, Roush's administrative assistant, told McDonald that Roush was out of the country, but interested in

McDonald's work on UFOs and in particular the outcome of his UN talk. She closed by saying, "We remain very interested." ^[5]

McDonald replied almost two months later because O'Callaghan's letter arrived while he was on his ONR sponsored Australia trip which will be discussed in the next chapter. He enclosed his statement to the UN Outer Space Affairs Group which he sent to all members of the UN Committee on the Peaceful Uses of Outer Space and to sixty or seventy scientific attaches at embassies in Washington. Lastly, he outlined his efforts to convince various NASA bureaucrats of the seriousness of the UFO problem and again asked that Roush open hearings. ^[6]

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In October Representative Wyman submitted a resolution to the Committee on Science and Astronautics calling for hearings on the UFO question. ^[7] McDonald wrote Roush endorsing the resolution and urging Roush to bring it before the Committee. ^[8] Roush answered by saying that he too favored the hearings, but it seemed a poor time. He felt the best witnesses would be those scientists on the Colorado Project, yet he did not think it appropriate to call on them before their research reached completion. ^[9]

Therefore, it appears that McDonald made contact in 1967, but was unable to do much more than get in the good graces of Roush. Nevertheless, this proved very important in 1968 as the situation in Boulder, in McDonald's opinion, worsened and the need, again in McDonald's opinion, for Congressional Hearings prior to the termination of the Project increased.

As previously indicated in the chapter on the Condon Study, the fur flew in early 1968. McDonald forced the confrontation at Boulder which resulted in the termination of Saunders and Levine, lawsuits, *Look* and *Science* articles, and the resignation of Mary Lou Armstrong.

By the end of April, however, it seemed to McDonald that it was time to concentrate his attention on Roush. He told Hall that he now agreed with him that the only real hope lay with Congress. He intended to send Roush a copy of the *Look* article and planned to see him in Washington the following week. ^[10] The same day he wrote Roush enclosing the *Look* expose and a copy of his 1/31/68 letter to Low which ignited the confrontation. He told Roush that it no longer seemed reasonable to "wait for Colorado," a by-word in Washington in 1966 and 1967 whenever the subject of UFOs arose. Congressional Hearings were necessary as soon

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as possible. He claimed that for "those of us who knew its shortcomings waiting for Colorado

was never adequate for the *Look* article represented only a partial disclosure of the scientific shortcomings of the project. He concluded that a Congressional inquiry could serve to put the matter "in a context amenable to scientific progress." [\[11\]](#)

Roush Speaks Out

Three days later Roush took to the floor of the House to ask for Hearings. He criticized the Air Force handling of the problem and also cited the *Look* article entitled the "Half Million Dollar Trick," which referred to the phrasing of the Low memo and the \$500,000 budget of the Condon Study. Roush argued that the article raised questions of scientific profundity and objectivity. He said that as a result of his talks to, and correspondence with, most serious UFO researchers he proposed inviting them to Hearings conducted by the House Committee on Science and Astronautics. [\[12\]](#)

The following day Roush took the floor again. He explained that if the *Look* article were incorrect it needed correcting and if it were correct it raised questions about the University of Colorado's approach to the federal contract process. To get some answers to these questions he said he wrote Secretary of the Air Force Seamans asking for comments and also the Comptroller General requesting an investigation of the use of public money at CU. Roush asserted that he was an agnostic with respect to UFOs, but wanted to get to the bottom of the riddle through the use of the scientific process. [\[13\]](#)

This brought the pot to a boil again both for NICAP and McDonald. Don Berliner, an NICAP staffer, wrote Julian Hennessey, a subcommittee chairman in England, that McDonald's talks with several members of the

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Science and Astronautics Committee indicated they were very upset over CU. He said, "We are sending over (to the Hill) gobs of material." [\[14\]](#)

McDonald confirmed this in a note to Malone in which he said he spent May 3 talking with Roush's staff, William Ryan of New York, and D'Addario's assistant Tom E. J. Keena. He said he pushed for an investigation going back to 1947 with Roush and asked Malone to write D'Addario urging the same approach. He mentioned that he spoke with the assistants to Congressman Brotzman from the Boulder area who will ask the president of CU for a full accounting. [\[15\]](#)

Although he recognized it was a poor year (election year) for trying to get a Congressional inquiry off the ground McDonald felt that the attempt should be made. He asked Hall to put the full weight of NICAP's 10,500-person membership behind a letter-writing campaign directed at Congressmen Roush, Ryan, D'Addario and Karth urging hearings. [\[16\]](#)

McDonald considered the remarks made by Roush on the House floor sufficiently important that he sent copies of them to "concerned people and scientific agencies inside and outside of the United States." He assured Roush that he would write to all of the members of Congress with whom he had spoken and had already asked Morris Udall of his own district to do whatever he could. He felt the best thing he could do was begin informal discussions with members of the Committee on Science and Astronautics and he wanted to know if the Subcommittee on Science, Research and Development would be the appropriate place to hold the hearings. If so, he would get colleagues from around the country to write the subcommittee letters of encouragement. [\[17\]](#)

Dick Olsen, an aide to Udall suggested to McDonald that he prepare a letter which Udall might show other Congressmen when button-holing

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them on the UFO question. After writing it McDonald told Udall he purposely composed it in a low-key fashion because he knew from experience that this was the best way to present the subject. In closing he said that he doubted anything short of a Congressional Hearing could get the issue out into the open. He had given up on his colleagues in the scientific community. [\[18\]](#)

Preparing for Hearings

Several days later O'Callaghan began giving McDonald instructions. She told him to write Congressman George P. Miller, Chairman of the Science and Astronautics Committee with a carbon to D'Addario. She said that the other scientists who McDonald mentioned were to do the same. Apparently Roush recommended hearings to Miller before, but Miller proved uninterested. Now interest existed, but he feared jurisdictional problems with the House Armed Services Committee. O'Callaghan felt the letter-writing campaign might make the difference. [\[19\]](#)

She wrote again to tell McDonald that D'Addario's Science and Astronautics Subcommittee would be the body to hold the hearing. Somehow McDonald had failed to write Miller and she told him to do so, with the objective of impressing Miller with the urgency of the matter and McDonald's own professional competence and seriousness. She emphasized that McDonald needed to persist without becoming a nuisance and in this regard recommended that he personally visit Miller and D'Addario when in Washington. McDonald could feel free to use

Roush's office to make appointments and get organized. ^[20] In another note the same day O'Callaghan told McDonald that she assured two men from the Government Accounting Office that he could provide them with quotes from Condon's speeches illustrating his position in the early stages of the project. ^[21]

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When McDonald eventually got around to the letter to Miller, he wrote that as a scientist with two years of experience in the UFO field he wanted to support Roush's plea for hearings. The *Look* article suggested that it was no longer wise to "wait for Colorado" as the NAS, NASA, NSF, et al., argued. Furthermore, he told Miller that the UFO matter was essentially a scientific question and as such belonged under his committee and not the House Armed Services Committee. He said he intended to visit Miller while in Washington and enclosed several of his UFO talks. ^[22]

In an effort to convince Miller that there existed a widespread interest within the scientific community in the UFO problem McDonald forwarded:

1. two articles from the May 1968 *American Engineer* which stressed the cross section of scientifically trained persons investigating UFOs,
2. an abstract of an article from the *Bulletin of the American Meteorological Society* in which three General Electric scientists expressed their concern for the status of UFO studies and the public responsibility of the scientific community to establish the identity of the objects,
3. a letter copied from the February 1968 *Astronautics and Aeronautics* written by Stanton Friedman representing a group of concerned Pittsburgh area scientists.

In addition, he mentioned Robert Baker's UFO article in the *Journal of the Astronautical Sciences* and assured Miller that there would be no shortage of participants if hearings were held. ^[23] McDonald followed the letter with a very similar one to D'Addario who replied with a note saying that he would contact Tom Malone for advice as McDonald suggested. ^[24] This in turn was followed by a letter to Miller from Levine in which he claimed that some UFO reports were of great scientific but not military interest. He

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said the Air Force did a poor job analyzing sightings, the *Look* article raised doubts about the objectivity of the Condon Study and consequently he believed congressional hearings were in order. ^[25]

From the last chapter we know that McDonald wanted ONR funding for those aspects of his UFO work which he believed related to his atmospheric physics research and, if possible, also for pure UFO studies. Roush, it would appear, attempted to do McDonald a favor by writing the ONR. He used Hynek for leverage in the discussion by prefacing his remarks with the statement that Hynek wrote to him to express concern over the significance of unexplained UFO reports. It would seem more than coincidence that Roush asked Admiral T. B. Owen, Chief of the ONR, what, in light of the lack of scientific concern for UFOs, ONR had done to research the problem? And, in anticipation of future hearings what research did ONR support on astronomical and meteorological hypotheses which might explain UFO observations? ^[26] The latter was, in fact, what McDonald often claimed he did.

Owen referred the inquiry to James Hughes, McDonald's contract monitor, who explained that the ONR did not sponsor a program to investigate UFOs, but did look at the problem in terms of how it might affect aerial or surface reconnaissance. He went on to say that the ONR asked McDonald to look at extreme refraction and electrical phenomena invoked to explain some sighting reports, but the research remained in progress. ^[27]

Congressman Miller received a late letter of encouragement from Roger N. Shepherd, noted psychometrician from Stanford. Shepherd said that he wanted to add his voice to those of his colleagues calling for hearings. In the past he had awaited the CU report but the seemingly

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reliable reports of prejudice and gaps in the study had made him change his mind. ^[28]

At this point Roush wrote Seitz at the NAS asking him to look into:

1. the *Look* allegations and McDonald's objections to the Condon Study,
2. significant cases recommended by Hynek, and
3. the dismissal of Saunders and Levine. Roush stated that he would suspend his GAO review of the project until the completion of the NAS Review. ^[29]

As it turned out Morris Udall eventually got McDonald an appointment to see Miller. On June 3 McDonald spoke to the Capitol Hill Burro Club made up of Democratic staff assistants. He so impressed Udall's staff that they convinced Udall to do a local Tucson TV spot on UFOs with McDonald. Then on June 19 Udall obtained an appointment for McDonald with Miller who gave tentative agreement for hearings. As McDonald put it to Saunders, "It went so easy a reversal would not surprise me." Remembering how Representative Karth withdrew his support for hearings in 1961 when Keyhoe made a premature announcement McDonald told Saunders to keep it quiet until Roush or Miller made a formal statement.

Through Roush, McDonald found that the GAO staff of lawyers and CPAs assessing the technical aspects of the CD study wanted to turn the problem over to the NAS. McDonald told Roush that he was of mixed emotions on the matter. The NAS was better equipped to do the scientific job but if it came to a fight in the Fall the financial data would be good to have. He suggested to Roush that GAO do the latter for purposes of Roush's own evaluation and leave the former for the NAS. [\[30\]](#)

The Symposium

On June 28 O'Callaghan phoned McDonald to ask him for his thoughts on a UFO seminar. Apparently McDonald discussed this idea with Roush as a precursor to actual hearings thinking that the issue could be

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privately sanitized, while the event itself could serve to put the NAS on notice. McDonald provided O'Callaghan with the following list of possible participants.

- Dr J. Allen Hynek, Northwestern, Astronomy (Alternate, Dr. T. Page, NASA Manned Spacecraft Center, Houston, Texas).
- Dr Robert Hall, University of Illinois, Sociology (Alternate, Dr. Roger N. Shepherd, Stanford, Psychology, Dr. R. Leo Sprinkle, University of Wyoming, Psychology).
- Dr. R.M.L. Baker, Jr., UCLA, Astronautical Engineering (Alternate, Dr. Frank Salisbury, Utah State University, Plant Physiology).
- Dr. Carl Sagan, Cornell, Astronomy (Alternate, Dr. Robert Nathan, Jet Propulsion Laboratory, California Institute of Technology).
- Dr. Robert M. Wood, Deputy Director for R. and D., Douglas Missile and Space Systems Division, Douglas Aircraft Company, Santa Monica, California (Alternate, Dr. Gary Henderson, Senior Research Scientist, Space Sciences, General Dynamics, Fort Worth Division).

He then discussed the pros and cons of each man and recommended that the publicity should be handled by Robert C. Cowen of the *Christian Science Monitor* who would "push the whole problem along more constructively and more energetically than would Sullivan of the *New York Times*." [\[31\]](#)

Two weeks later McDonald received an invitation from Roush's office to appear at a July 29 Symposium on UFOs to be held under the auspices of the House Committee on Science and Astronautics. The participants were not to touch upon the past Air Force investigatory efforts or the situation at Boulder. "Our emphasis is to understand the technical and scientific facts

surrounding these phenomena as the facts are understood by competent researchers in the field. Your presentation should reflect these views." [\[32\]](#)

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A week later Bob Wood of Douglas Aircraft canceled his appearance at the symposium. To illustrate the degree to which McDonald orchestrated the hearing he told O'Callaghan that Jim Harder of Berkeley's Engineering Department would be an effective substitute, wrote Harder telling him about it and prepared the wording of the invitation he wanted O'Callaghan to send. He noted that Congressman Miller represented the Berkeley area and hoped, therefore, that Harder's Berkeley affiliation might be able to sway him. [\[33\]](#) Further orchestration evidence is furnished in a note from McDonald to O'Callaghan in which he stated that he had completed the bulk of telephoning on Roush's credit card to Hall, Baker, Wood, Sagan, Hynek and Harder. Moreover, because the participants did not have much time to prepare their papers he feared they had possibly neglected recommendations for future Congressional action. Therefore, he felt that Roush should take it upon himself to prepare a summary of the talks and in so doing infer from them the kinds of actions the Committee might see fit to take in the future. [\[34\]](#)

The Symposium (which it was called rather than a hearing) took place on July 29 as scheduled. The speakers were McDonald, Hynek, Hall, Baker, Harder and Sagan. McDonald expressed satisfaction with the proceedings in general, although he found Hynek's remarks said very little and Sagan, by contributing his *Encyclopedia Americana* statement, provided a negative note. [\[35\]](#) Nevertheless, McDonald managed to obtain a Congressional Hearing on UFOs which presented a pro-UFO bias, an unprecedented and unreplicated feat in the history of the UFO controversy.

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SUMMARY AND CONCLUSIONS

The strategy which McDonald adopted with respect to the Roush Hearings is now a familiar one. He intended the Hearings as a tactic to obtain legitimacy for the study of UFO data. He wanted hearings from as early as September 1966, but he would not take action until the Colorado confrontation of February 1968, with the concomitant firings of Saunders and Levine, convinced him of the low probability of a positive Condon Report. Once he reached this conclusion he turned most of his energy toward the House Science and Astronautics Committee as a means of laying bare the substance of the phenomenon to Congressmen who could eventually conduct an investigation of their own which, he hoped, would catapult UFOs from obscurity to national

prominence.

Throughout this episode in the politics of science McDonald depended on a number of different tactics. First he convinced Roush of the efficacy of the Hearing, then he presented his case to other influential Congressman, along the way he used various Congressmen for his own ends, and finally he orchestrated the symposium itself.

Between March 1967 and April 1968 McDonald made several attempts to convince Roush that UFO Hearings were in order. He used his usual discussion of past investigative experiences, the significance of the problem and his credentials in an initial letter to Roush. In a second letter he tried to counteract the negative influence of a communication from Gerard Kuiper to Roush. In a third note, while setting up a Washington meeting, he conveniently scheduled Roush amongst numerous scientific agencies and luminaries he intended to visit, to impress the Congressmen with the importance of the problem. In late 1967 McDonald endorsed Congressman Wyman's resolution for Hearings and when

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they did not materialize waited for the Boulder confrontation to develop before taking further action. When it did, in April 1968, he forwarded Roush the *Look* article and his 1/31/68 letter to Low which ignited the confrontation. Three days later Roush asked for UFO Hearings on the House floor.

This did not end the lobbying effort, however, for McDonald needed to convince other Congressmen of the efficacy of the Hearings. He spoke with Congressman William Ryan of New York and D'Addario's assistant in early May. To increase the pressure he asked Hall to get the NICAP membership to start writing to the House Science and Astronautics Committee, particularly Roush, Ryan, D'Addario and Karth. Then at the insistence of Roush's assistant O'Callaghan, McDonald wrote the committee chairman George Miller supporting Roush's plea for hearings and enlisted other scientists to do likewise. Moreover, he enclosed several articles intended to give Miller the impression that widespread interest in UFOs existed within the scientific community. Lastly, he spoke to the Burro Club, a group made up of assistants to Democratic Congressmen, in a further attempt to spread the UFO word in Congress.

He also urged other Congressmen to aid him. He encouraged Congressman Brotzman from the Boulder district, who already asked for an accounting from the CU president with respect to the UFO project, to continue his efforts. Morris Udall, McDonald's representative, also agreed to help. For this purpose McDonald wrote him a low-key letter on the UFO problem which Udall could show to any Congressman whom he could button-hole. Finally, Udall obtained McDonald

an appointment to see Chairman Miller, who then agreed to hold the Hearing.

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In several instances McDonald's guidance in the planning of the Hearing is evident. He provided O'Callaghan with the names of the speakers and alternates. He also proposed that Robert Cowen of the *Christian Science Monitor* be used for publicity. When Robert Wood of Douglas Aircraft could not participate McDonald first recommended Jim Harder, and then invited him to take part, arguing that it could prove useful in swaying Miller to have the testimony of one of his constituents. And a week prior to the Hearing, when McDonald realized that no provisions existed for future more intensive investigations, he wrote O'Callaghan explaining how Roush could summarize the papers in his closing remarks which McDonald thought should point toward further Hearings.

SUMMARY OF TACTICS

Persuasion of Roush

1. Emphasized his academic credentials.
2. Emphasized his investigative work.
3. Indicated the significance of the problem.
4. Scheduled Roush among scientific luminaries.
5. Countered Kuiper's remarks.
6. Endorsed Wyman's resolution for Hearings.
7. Forwarded the Look article and Low letter to Roush.

Persuasion of Others

1. Spoke with Congressman Ryan and D'Addario's assistant.
2. Got NICAP members to write Roush, Ryan, D'Addario and Karth.
3. Wrote to Miller supporting Roush.
4. Had other scientists write Miller.

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5. Enclosed articles to Miller leaving the impression of widespread scientific interest in UFOs.

6. Spoke to the Burro Club.

Manipulation of Others

1. Urged Congressman Brotzman of Colorado to look into the situation at CU.
2. Used Udall as a mean's of access to Miller.
3. Drafted a letter on UFO Hearings for Udall to use in button-holing Congressmen.

Symposium Orchestration

1. Provided the speakers' names.
2. Suggested how the publicity could be handled.
3. Acted as an intermediary for Roush with the speakers.
4. Provided a substitute speaker.
5. Suggested to O'Callaghan the form a summary of the papers should take.

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FOOTNOTES

1. McDonald to Roush, 3/3/67, McDonald Letters, hereafter M.L. - [Back To Text](#)
2. Kuiper to Roush, 3/20/67, M.L. - [Back To Text](#)
3. McDonald to Kuiper, 3/30/67, M.L., cc. Roush, Udall, M.L. - [Back To Text](#)
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5. O'Callaghan to McDonald, 6/27/67, M.L. - [Back To Text](#)
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17. McDonald to Roush, 5/13/68, M.L. - [Back To Text](#)
18. McDonald to Udall, 5/14/68, M.L. - [Back To Text](#)
19. O'Callaghan to McDonald, 5/16/68, M.L. - [Back To Text](#)
20. O'Callaghan to McDonald, 5/20/68, M.L. - [Back To Text](#)
21. O'Callaghan to McDonald, 5/20/68, M.L. - [Back To Text](#)
22. McDonald to Miller, 5/23/68, M.L. - [Back To Text](#)
23. McDonald to Miller, 5/27/68, M.L. - [Back To Text](#)

24. McDonald to D'Addario, 5/28/68, M.L. - [Back To Text](#)
25. Levine to Miller, 5/29/68, M.L. - [Back To Text](#)

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26. Roush to Owen, 6/6/68, M.L. - [Back To Text](#)
27. Hughes to Roush, 6/12/68, M.L. - [Back To Text](#)
28. Shepherd to Miller, 6/17/68, M.L. - [Back To Text](#)
29. Roush to Seitz, 6/20/68, M.L. - [Back To Text](#)
30. McDonald to Saunders, 6/25/68, M.L. - [Back To Text](#)
31. McDonald to O'Callaghan, 6/28/68, M.L. - [Back To Text](#)
32. Roush to McDonald, 7/10/68, M.L. - [Back To Text](#)
33. McDonald to O'Callaghan, 7/16/68, M.L. - [Back To Text](#)
34. McDonald to O'Callaghan, 7/17/68, M.L. - [Back To Text](#)
35. McDonald to Hall, 9/5/68, H.L. - [Back To Text](#)

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V. THE OFFICE OF NAVAL RESEARCH CONTROVERSY

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BACKGROUND

The genesis of the ONR controversy is difficult to pinpoint. On the one hand we have Philip Klass' explanation that as a concerned taxpayer he wondered why the ONR used tax money to support something as nonsensical as UFO research. On the other hand we have McDonald's interpretation of events which portrays him as a victim of writer's pique on Klass' part. They held competing theories of the UFO phenomenon, according to McDonald, and when Klass, untrained in atmospheric physics, could not defend himself against McDonald's rigorous attacks he turned to an indirect counterattack.

The explication which follows is not an attempt to settle the matter one way or the other. Rather it is an opportunity to observe various ramifications of McDonald's borderland science undertaking. It is my belief that both McDonald and Klass are examples of scientific extremists. By this I mean that McDonald enjoyed nothing more than being an iconoclast and upsetting a well entrenched apple cart. Klass, to the contrary it would appear, took great pleasure in upholding the tried and true, and ridiculing those who deviated from the straight and narrow through his use of sarcasm and appeals to authority.

When these two met, head-on so to speak, there had to be a battle. McDonald viewed the UFO phenomenon as the most important scientific problem of the twentieth century. He believed in 1966 and 1967 that the time was ripe to "break it wide open." Klass, as well as other critics, he viewed as impediments to the crusade who needed to be dealt with. This he began to do in Klass' case in October of 1966 with

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several critical letters following the August 22, 1966 *Aviation Week and Space Technology* article on UFOs in which Klass proposed his ball lightning hypothesis [\[1\]](#)

Although Klass entertained the ball lightning hypothesis as early as March 1966, [\[2\]](#) and possibly earlier, it would appear that the first time he interjected himself into the McDonald-NICAP milieu was in August of that year when he interviewed Hall. [\[3\]](#) Plans for a book were probably already stirring in Klass' mind, so both he and McDonald were beginning to have an increasingly larger stake in defending their respective positions. In the ensuing months they exchanged a number of letters, but once Klass' second article appeared in October, from McDonald's perspective, the lines were drawn. He subsequently addressed himself to the ball lightning hypothesis in all his talks and arranged to teach a course on ball lightning at the UA in order to keep abreast of the phenomenon. Klass, for his part, managed to attend several of McDonald's talks in the Washington area and ask what he considered embarrassing questions related to ball lightning.

HISTORY OF EVENTS

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The Principal Issue

We can pick up the story in mid-1967 where the principal issue began to surface: did McDonald appropriate funds from his ONR contract for atmospheric physics research to finance his UFO work? It is difficult to determine what triggered Klass' pursuit of this matter, but it would appear that McDonald's ONR sponsored trip to Australia in late June, 1967 put him on the track.

Some time in early June McDonald wrote a memo to Bob Low at CU to inform him of the upcoming trip. Unbeknownst to McDonald this memo

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found its way to Klass' file labeled "Affair McDonald" and annotated "very interesting!" ^[4] This undoubtedly marks the beginning of what would eventually develop into eighteen months of on and off trauma for Klass, McDonald, Hughes and various bureaucrats at the ONR.

Prior to the trip McDonald told Hughes that he planned to visit several UFO groups, speak to a number of academic seminars and interview as many witnesses to UFO sightings as he could while in Australia. ^[5] On his return he said that:

The UFO situation in the Australia, New Zealand, Tasmania area is essentially the same as in the United States. I found the same types of UFO phenomena, the same predominance of discs and cigar shaped objects, the same type of carstopping incidents, and so on. There were many good cases, and before I left I had checked almost all of the "classic" Australian UFO episodes.

He commented that several dispatches which emanated from Australia based on various radio and TV appearances he made were distorted. He referred to the fact that someone quoted him to the effect that the ONR sent him to Australia to look into the UFO situation. To McDonald's regret Klass would hear of these quotes and they would provide him with more evidence for his claims of financial malfeasance.

McDonald also discussed the renewal of his ONR contract for 1968. This is quite relevant because he said: ^[6]

On that point, let me ask you if it isn't possible now for ONR to "make me an honest man" with respect to my UFO research. In view of the somewhat altered climate of opinion about UFOs, can't we bring my work out in the open and make it an explicit part of my next year's work? I would particularly like to use ONR funds to publish some scientific reports here on certain aspects of my findings. I keep thinking about the strictures of publishing in the journals, space limitations always being so severe. I think it would be very profitable to begin putting out, for the limited distribution that our Institute Scientific Reports typically enjoys, some detailed discussions of the parts of the problem

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on which I've been working so intensively. I'd like to hear from you as to whether it will be O.K. for me to cite such objectives in the contract proposal that I shall have to be submitting very shortly. I have run out of my \$1,300 of local money many weeks ago, and am now operating (primarily on telephone tolls and travel) on my ONR funds. It looks like the contract funds will go to a flat zero by the end of the contract period, incidentally.

At this point Hughes and possibly others at the ONR knew of McDonald's use of contract funds to do his UFO work. It would seem that there is no question but that it did occur. The question which does arise, however, is whether this was a *sub rosa* effort on the part of the Navy to keep abreast of UFO happenings and get in on the ground floor of the "big break through," solely an attempt by Hughes to keep the ONR competitive with the Air Force, or a favor from Hughes to McDonald for the advancement of science. Although these questions are beyond the purview of this study, the knowledge that McDonald used his ONR funds for UFO research is pertinent background to the strategies and tactics employed by McDonald and Hughes in their skirmish with Klass.

Klass Raises the Issue

Klass took the offensive in December by writing Russ Greenbaum, the public information officer at the ONR. He provided six quotes from McDonald in the period September, 1966 through September, 1967 (from McDonald's papers or tapes of his lectures) in which McDonald asserted he spent full time pursuing the UFO question. Klass asked: [\[7\]](#)

Since McDonald also carries on a teaching schedule, or so he told me on April 22, 1967, and has repeatedly made statements that he is spending essentially full time on the UFO problem, WHEN DOES HE FIND TIME TO WORK ON THE \$38,000 OFFICE OF NAVAL RESEARCH CONTRACT TO STUDY CLOUD PHYSICS? Was McDonald's heavy commitment to UFO

research known to ONR in November 1966, at the time the \$38,000 grant was made?

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Notes from a phone conversation between Klass and Greenbaum indicate that McDonald gave the ONR some answers pertaining to his media exposure in Australia and to what he called confusion on their (the media's) part as to his purpose for making the trip. Greenbaum told Klass his memo (Klass') went to Hughes who was "quite shook." Furthermore, Greenbaum told Hughes that the whole matter would have to go before the Research Director. He informed Klass that Hughes asked the UA for an accounting of McDonald's contract, but that Hughes had changed his story to the effect that there existed a connection between McDonald's trip and UFOs. Furthermore, Hughes would prepare a paper to demonstrate the connection. ^[8]

Shortly thereafter Richard Kassander, Director of the Institute of Atmospheric Physics at the UA, wrote to Hughes explaining what McDonald did with his time and his grant money. Klass apparently received a Xerox from Greenbaum.

The letter emphasized that McDonald put in a 60 or 70 hour week and consequently he could easily claim that he worked full time on UFOs while still having time to fulfill his UA and ONR obligations. Kassander listed the various pieces of research either McDonald or graduate students advised by him completed or collaborated on and related this work to the ONR contract. He further argued that the ONR contract covered work in Atmospheric Physics, not Cloud Physics, and said:

I believe it has always been clearly understood that irrespective of one's interest in UFOs, per se, a great deal of information is to be gained on atmospheric optics, radar propagation, and atmospheric electricity from a careful study of reported UFO sightings.

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Lastly, Kassander outlined what McDonald did while in Australia focusing on his visits to Radiophysics Division CSIRO, Sydney, Division of Meteorological Physics, Melbourne and Camden Labs of the Upper Atmospheric Laboratory of CSIRO in Sydney. He mentioned both McDonald's UFO talks and the witness interviewing he did, relating the latter to optical and electrical processes. ^[9]

However, McDonald did not satisfy Klass that easily. The latter spoke with Greenbaum on December 19, 1967 about the above letter and the next day offered Greenbaum additional aid in pinning down McDonald. He provided copies of McDonald's October 19, 1966 AMS talk in

which McDonald claimed that atmospheric phenomena such as rare electrical effects, clouds and plasma effects did not explain UFOs. Klass claimed, in other words, that McDonald's own words refuted the claims of Kassander that his atmospheric physics work related to UFOs. Klass concluded: [\[10\]](#)

Let me emphasize again that I do not question McDonald's right to believe what he will about UFOs, nor do I suggest that his views in any way diminish his skills as a cloud physicist. What I do question is whether ONR is really funding UFO research under the guise of cloud physics, either knowingly or unknowingly.

Some time between December 19 and January 5, 1968 Klass learned from Greenbaum that ONR would not formally terminate McDonald's contract, but would let it expire. The ONR pursued this course rather than have it appear to be questioning the word of the UA or challenging academic freedom. Klass told Low in a "Strictly Personal" letter that this satisfied him. He also explained the history of the dispute with ONR to that point. [\[11\]](#)

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Much to Klass' surprise he received word from Greenbaum a week later that both the UA and the ONR felt satisfied that McDonald fulfilled his contract obligations. True, he used some of his UFO work to shed light on atmospheric physics problems, but it also related to contract objectives and in such situations ONR policy prohibited the direction of the research of any scientist under contract. [\[12\]](#)

To this turn of events Klass responded by reiterating his argument to Greenbaum concerning McDonald's previous position that atmospheric phenomena did not account for UFO sightings. He facetiously suggested that McDonald might have changed his mind and then asked if McDonald directed all his ONR funded UFO work to acquiring atmospheric physics knowledge. Quoting from the UA letter Greenbaum provided him Klass wanted to know if the "interviews with a number of witnesses of unusual atmospheric phenomena having possible bearing on optical and electrical processes" conducted by McDonald in Australia were the same UFO reports he cited in his briefing of the Condon Project staff? [\[13\]](#)

Although the storm blew over as far as McDonald could tell, he informed Hall that the grant of \$1,000 Hall spoke of giving him "...would be of considerable tactical-political advantage...." He alluded briefly to the recent harassments by Klass. [\[14\]](#) Evidence of the fact that he did not appreciate the gravity of his situation is found in a letter to Hughes which focused on the dire situation at CU (Saunders and Levine had just been terminated) and in which McDonald urged Hughes to get him funding for his UFO work because the contract continuation only provided

phone money. [\[15\]](#) After he received the above mentioned \$1,000 McDonald told Hall it would be helpful to

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Dick Kassander when asked by state legislators how McDonald could spend full time on UFOs without any funding. He said it could also be used to throw Klass, "whose nibbling hasn't ceased, I understand," off the trail. [\[16\]](#)

McDonald responds

Klass published a book on the UFO phenomenon in March of 1968. [\[17\]](#) McDonald told Hughes that Klass took hard looks at some cases which appeared to be plasmas, but said he (McDonald) didn't consider them good UFO sightings. Nevertheless, he asserted that something might be learned about atmospheric electricity by funding research in the area. [\[18\]](#) McDonald commented on Klass' book, which contained a ten page chapter solely on his interactions with Klass, because in a paper he recently presented at the Canadian Aeronautics and Space Institute (CASI) Astronautics Symposium he devoted 18 single spaced pages to critiquing Klass' position. [\[19\]](#)

In his January 7, 1968 letter to Greenbaum Klass indicated that he wanted details of McDonald's Australian UFO interviews. Greenbaum worded his reply deftly. He said that the ONR only concerned itself with anomalous refraction phenomena which could cause UFO reports, not UFOs per se. He advised Klass to contact the UA or McDonald for sighting details. [\[20\]](#)

An indication that McDonald took his tiff with Klass seriously is shown by the zeal with which he attacked the above CASI critique. He mentioned to Hall that he intended to add to what he presented in Canada and would multigraph it thanks to Hall's grant. [\[21\]](#) He also said that he would send it out to those interested in the UFO phenomenon and hoped NICAP would forward it to various editors and publishers. [\[22\]](#)

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McDonald sent it to Robert Ross at the CASI with the request that he publish a revised version. [\[23\]](#)

McDonald carried his attack even further because he couldn't believe some of Klass' citations which were the result of letters from, or conversations with, noted authorities in atmospheric electricity. To ascertain the authenticity of these citations he wrote to the individuals whom Klass cited. For instance, he found that Marx Brook, a professor of physics at the New Mexico

Institute of Mining and Technology, felt that Klass misunderstood him, ^[24] while James Powell at Brookhaven and Martin Uman, a friend, who worked for Westinghouse Research Laboratories failed to reply. ^[25] Whether they agreed with Klass, wanted to avoid getting drawn into the fray, or both, is unclear.

The JEM White Papers

At approximately this time Klass initiated a new tactic. He sent to individuals active in the UFO field what he called "JEM White Papers." Between May 14, 1968 and August 21, 1968 he produced eleven of these one page statements directed at McDonald. In a number of these White Papers he claimed he caught McDonald changing his position without acknowledging it and indirectly accused him of lying. The papers were partly substantive, that is they addressed the issues, but primarily procedural, i.e., attempts to catch McDonald in a semantic trap or inconsistency. In several of these papers Klass discussed the UFO problem in a sarcastic manner and in two instances he presented his side of the ONR controversy. The White Papers usually began with a pithy quote meant to convey the flavor of what followed. For example, "Half The Truth Is Often A Great Lie," Ben Franklin; ^[26] "There Are

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Certain Persons For Who Pure Truth Is Poison," Andre Maurois; ^[27] and "One Falsehood Treads On The Heels Of Another," Terence. ^[28] McDonald claimed Klass could not address the scientific issues involved and so he resorted to this and the ONR expose. Klass argued that McDonald distorted the facts and lied when it suited him to discredit the plasma explanation of UFOs. But because he (Klass) did not have the access to forums which McDonald did, and because McDonald refused to debate him, Klass claimed there existed no other means but the White Papers to correct the false impressions McDonald created.

One incident related to the White Papers is worth mentioning. Apparently Klass put John Fuller on his list of recipients. After he received the first two White Papers Fuller wrote Klass to say he regretted Klass' need to stoop to smears to counter McDonald. He said he felt it was one thing to disagree with a man's theories, but quite another to attack him personally. ^[29]

Fuller forwarded copies of the White Papers and a letter of concern to *Aviation Week* publisher Bob Martin who said he would talk to Klass about the matter. ^[30] Klass replied to Fuller stating that it pleased him that Fuller did not disagree with the accuracy of the JEMs. He regretted Fuller's interpretation of their purpose, claiming he only wanted Fuller to have the opportunity to avoid being tarred with the same brush of events which was already in motion (ONR). As for Bob Martin, it pleased him that Klass continued in the *Aviation Week* tradition of "The Truth,

Even When It Hurts." [\[31\]](#) Fuller kept McDonald apprised of these events and he responded by forwarding two copies of

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his CASI paper with the recommendation that Fuller send one to Bob Martin so that Martin could see "the real source of Phil's concern." [\[32\]](#)

Klass Queries ONR

After three months of inaction on the ONR front Klass returned to the offensive in late June. He no doubt believed that Kassandra's letter of December 16 outflanked him, but that McDonald could not back up the claims Kassandra made for the great benefits accruing from the study of UFO reports. Referring to Kassandra's letter he wrote Greenbaum to say that he wanted the reports which Kassandra's letter implied existed. They were:

1. a report based on McDonald's interviews in Australia "with a number of witnesses of unusual atmospheric phenomena having possible bearing on optical and electrical processes."
2. a report on McDonald's efforts indicating "a great deal of information is to be gained on atmospheric optics, radar propagation, and atmospheric electricity from a careful study of UFO sightings."
3. a report on a ball lightning review carried out with ONR funds.

If they were not available Klass wanted to know when they would be and if and when they would be published in scholarly journals. [\[33\]](#)

Greenbaum replied that the reports were not available, but the work would appear in journals, although no publication dates were available because article backlogs often delayed publication. [\[34\]](#) This did not satisfy Klass. He shot back a letter in which he asked if it were ONR policy to dispense funds and not require a yearly report? Furthermore, he wanted to know if McDonald submitted papers -- and, if so, their titles and to what journals? He argued that if McDonald's hypothesis were incorrect that should have been reported to his

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contract monitor and no papers should be anticipated, but if the hypothesis were correct he would expect ONR to publish the results quickly or possibly call a special conference. Klass closed by saying he thought the ONR appeared indifferent to the outcomes of spending taxpayers' money

and therefore an explanation was in order. ^[35] His correspondence does not indicate that Greenbaum responded, nor does it contain a response to Klass' further escalation of the confrontation which occurred four days later.

This was the time of the Roush Hearings (July 29, 1968) out of which came UPI Dispatch 125, among other things. The dispatch quoted McDonald to the effect that "he had spent the last two years studying UFOs under a grant from the United States Office of Naval Research, spending several months in Australia, New Zealand and Tasmania tracking down sightings." Klass forwarded the quote to Greenbaum and asked for a comment. ^[36] McDonald claimed he never said what the dispatch attributed to him and felt Klass, who he asserted huddled with the press at the Hearing, probably took a hand in it. ^[37] Klass argued the claim was absurd. ^[38] In retrospect it is a bit hard to accept the UPI Dispatch given the problems McDonald already experienced with Klass and the ONR and his sensitivity to the situation as a result of Klass' intermittent attacks for almost eight months. This will remain one of the unanswered questions of the controversy.

The next move on Klass' part, or so McDonald believed, took about a month to unfold. McDonald told Hall that Jack Anderson called to discuss the CU situation, but in the course of the conversation asked if anyone were out to discredit him. When McDonald brought up Klass,

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Anderson made it clear, without naming names, that Klass complained to him about McDonald mispending ONR funds. ^[39] A week later Anderson's Merry-Go-Round column contained a piece on the forthcoming Condon Report in which McDonald received mention as a critic who himself received criticism for the misuse of ONR funds to finance his UFO work. ^[40]

Klass realized that Greenbaum at the ONR could not, or would not, help him. To take advantage of the situation, therefore, he forwarded the Anderson article to Dr. Robert A. Frosch, Assistant Secretary of the Navy for Research and Development. Klass told him the allegations of misuse of Navy funds were true, abetted by Hughes and that he could document the charges. ^[41]

Word of this reached McDonald through a call from Hughes on the seventeenth of September and subsequently McDonald sent Frosch a telegram in an attempt to refute the allegations. He said Anderson's comments were contrary to what he (McDonald) told him in the interview. He asserted that local research support existed for his work and explained how the overlap between atmospheric physics and UFOs made the use of Navy funds justifiable. The Australian trip involved a look at anomalous refraction cases, cloud physics, weather modification and other tasks for the project monitor. ^[42]

DOD Involvement

But Klass also informed another member of the DOD family of the problem. Finn Larsen, Deputy Director of Defense Research and Engineering, wrote Klass to say, "Thank you for calling to my attention the ONR sponsorship of Jim McDonald at the UA. I am asking the Navy to let me know precisely what the situation involves." [\[43\]](#)

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The orders to review McDonald's work filtered down to Hughes who asked McDonald for a statement of his recent research accomplishments. McDonald said Hughes believed the request was not in response to Klass, but rather to a climate change at the ONR resulting from numerous contracts which showed no output for over a year. Hughes told McDonald which papers Kassander should cite as well as recommending that mention be made of McDonald's work on the Advisory Panel Project Stormfury sponsored by the Navy and ESSA, and his work on the NAS weather modification panel. According to McDonald Hughes wanted to use his file to make the case that "output can't always be deduced from reports and reprints in (the) short run." [\[44\]](#) This would imply that either Hughes' superiors did not keep him fully informed of the McDonald investigation or Hughes found himself in a position which prohibited him from revealing the true nature of his actions to McDonald.

McDonald responded directly to Robert Frosch concerning the Anderson allegations in an eight page letter which elaborated on his previous telegram. He laid out the dispute with Klass as he perceived it. Essentially it was a question of Klass' inability to counter McDonald's scientific arguments that led him (Klass) to make personal attacks. McDonald discussed the Anderson column as an example of this and outlined an argument which exposed (in his view) its errors. He portrayed Klass as a disgruntled author whose scientific arguments were specious and which, if reviewed by ONR scientists familiar with atmospheric electricity, would reveal the cause of Klass' pique -- namely McDonald's criticisms of an untenable position.

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He went on to discuss how some UFO research related to atmospheric physics and defended his use of ONR funds to do this work. He cited considerable research which resulted from his UFO studies that he considered good atmospheric physics and would eventually be published, but that he put off because of the urgency of the UFO problem. He expended much effort on clearing Hughes of any charges of collusion to mispend ONR monies, citing Hughes' continual refusal to

directly fund UFO research even though McDonald persistently requested him to do so. McDonald claimed that this was the reason that led him to permit his ONR contract to expire after ten years. He felt he needed full time support for his UFO work and needed to look elsewhere for it. [\[45\]](#)

Klass received a copy of the above statement and quickly replied to it. Not to be outdone by McDonald he wrote a ten page single spaced retort. He argued that McDonald did not address the basic issues which in Klass' view were:

1. "...was the primary motivation of McDonald's ONR funded UFO investigations intended to serve Navy objectives or his own?"
2. "Was the relationship between the ONR contract monitor (Hughes) and the contractor (McDonald) a traditionally proper one?"

He endeavored to show how little credence McDonald put in an atmospheric physics explanation of UFOs by quoting from five of McDonald's public statements between October 19, 1966 and March 26, 1968 in which McDonald stated directly or indirectly that few UFO reports could be explained as freak atmospheric phenomena. Klass further claimed it improper for the ONR to fund McDonald's review of the ball

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lightning literature, essentially to rebut Klass, when it was not McDonald's field and the ONR already funded two UA professors in that area.

Then Klass went on to describe the entire Australian trip affair viz McDonald, Hughes, Greenbaum, Kassander, the UA and himself. He emphasized the lack of publication on the part of McDonald, given he developed a new methodology, i.e., studying UFO reports to gain atmospheric physics knowledge, and intimated that this was a surprise in view of McDonald's past record of publication and the significance of his new approach. Klass also argued that it seemed quite unusual that McDonald did not credit the ONR for support of his UFO work; a standard procedure which the funding agency usually demands.

He further implied that Hughes played a role in his exclusion from a symposium on ball lightning at the Fourth International Conference on Atmospheric Electricity in May, 1968. This he surmised was because of his disagreement with McDonald and the ONR. And lastly he questioned the manner in which the ONR funded McDonald's Australian trip, and asked how the ONR prorated it if McDonald spent some of his time on ONR business and some on UFO work. Klass concluded by saying that he wanted the direct confrontation which McDonald suggested

and would be glad to appear in Frosch's office any time. [\[46\]](#)

Having also forwarded a copy of the Anderson article to Admiral Thomas Moorer, Chief of Naval Operations, Klass received a reply the day after he wrote Frosch. Moorer indicated that Anderson referred to McDonald's NASA grant in his column not the ONR contract. [\[47\]](#) To this Klass pointed out (with copies to Larsen and Frosch) that the

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evidence for the charges appeared in the Frosch letter, a copy of which must have crossed Moorer's letter to him (Klass) in the mail, and he hoped Moorer would reconsider his conclusion after reading it. [\[48\]](#)

McDonald never responded to Frosch regarding Klass' September 30 letter. He did write Hughes to explain his position with respect to parts of it and said he believed Klass began with incorrect assumptions concerning the validity of the Anderson article and UPI Dispatch 125 and from there built castles in the air. [\[49\]](#)

However, Klass did not give up. He forwarded Finn Larsen some of his speculation on what might have happened in the case of John Fuller's *Look* article. He argued that McDonald was behind it, something which Fuller never made clear, and that the entire affair caused disruption of the CU project. Klass pointed out that Saunders and Levine could have looked to Condon, top university scientists, the Air Force or DOD for redress of the memo grievance, but instead sent copies to McDonald and Keyhoe. He said he did not know how much of the matter the Navy paid for through the ONR, but it appeared that the Navy financed interference in an Air Force program. Because this seemed tangential to the real issue of McDonald's ONR contract Klass told Larsen that he wouldn't send copies to Frosch and Moorer, but Larsen could feel free to do so. [\[50\]](#)

It seems that Klass kept Major Hector Quintanilla, then in charge of the Blue Book program, informed about the ONR affair. Quintanilla wrote him thanking Klass for copies of all the correspondence and, among other things, he mentioned that the subson research McDonald told Finn Larsen resulted from work on a UFO photo actually occurred

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after McDonald's contract expired. Quintanilla said he knew this because McDonald obtained the UFO photo through the Blue Book office. [\[51\]](#) Not being one to miss such an opportunity Klass wrote Larsen pointing this out. He said that either McDonald erred in his contention that he did

the subsum work for ONR or generously pursued this research after the expiration of the contract. [\[52\]](#)

Contract Audit

By this time Frosch decided a full scale audit of McDonald's ONR account at the UA might quiet Klass down. It should be remembered that *Aviation Week* is a highly respected trade publication in government circles. What must have constantly been on the minds of the ONR brass involved was the possibility that Klass might run an expose story and cause the ONR unnecessary problems on Capitol Hill. This did not prove difficult for them to keep in mind because Klass used *Aviation Week* stationery in all of his correspondence with the Navy, although he averred that he wrote only as a concerned taxpayer.

McDonald tried to avoid the audit. He sent Hughes a rough draft of the account expenditures and offered the alternative of a smoothed out version by Kassander. He said he believed it provided enough evidence to show Klass in the wrong. Along with this communication he forwarded copies of the Fuller, Klass, Martin correspondence to keep Hughes abreast of that aspect of the Klass squabble and told Hughes that Phyllis O'Callaghan of Roush's office intended to check into the reporter who wrote the infamous UPI Dispatch 125. [\[53\]](#)

Even though Frosch requested the audit he informed Klass that after a month reviewing the case he saw no substance to the charges. However, he felt an independent audit would clarify matters for all concerned. [\[54\]](#)

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The tentative conclusion disappointed Klass but it heartened him to see that the audit would go ahead. He asked Frosch if the auditor would receive a copy of his September 30 letter to Frosch and in turn requested: [\[55\]](#)

1. a work statement defining the scope of McDonald's activities under the ONR grant
2. an ONR policy statement on the freedoms and constraints imposed on an investigator relative to his work statement
3. confirmation or denial ... "that Greenbaum told me that the Director had, upon investigating matters brought to his attention, delivered an oral reprimand to Hughes as a result of the McDonald affair?"

In a second letter the same day Klass forwarded a September 28, 1966 letter from McDonald [\[56\]](#) in which the latter explained his initial efforts to make headway on the UFO problem and in Klass' estimation incriminated himself on the question of funding. Klass interpreted the letter for

Frosch and asked that it be shown to the auditor. ^[57] Frosch agreed to provide the "independent auditor" with Klass' information. Moreover, he furnished Klass with McDonald's work statement which said:

The contractor shall conduct theoretical, laboratory and field studies in cloud dynamics and the physics of cloud and precipitation processes. He shall also study other meteorological processes that are in any way related to cloud dynamics; additional investigation on the extreme refraction and visibility phenomena of the atmosphere, and the kind of visual or radar impressions they produce.

With regard to Klass' request for contract constraints Frosch quoted a guide for submission of research proposals to the ONR which stated:

It is an objective of the ONR to use flexible contract and grant procedures which are best adapted to the effective accomplishments of sponsored research programs.

Finally Frosch said that Greenbaum denied making the statement with respect to the Hughes reprimand over the McDonald affair. ^[58]

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The audit went about as well as McDonald could have hoped. He told Hughes that the auditor considered Klass "a ways off-base." He only wanted to know whether McDonald pursued UFOs clandestinely with respect to the Navy and what agency claimed legal responsibility for UFOs, i.e., did the Navy infringe on Air Force prerogatives through McDonald's work? The auditor showed no interest in McDonald's telephone interviewing or the atmospheric physics of the UFO problem and said the inquiry should have been stopped earlier. McDonald found the draft of his report quite acceptable. ^[59]

Final Exchanges

Needless to say the note from Frosch of November 18 in which Frosch cited McDonald's work statement and said that Greenbaum denied the oral reprimand of Hughes infuriated Klass. Although he waited until January of 1969 to forward his response, what Klass wrote plays a role in understanding what is to follow and deserves mention now. Klass claimed the ONR rewrote history because he taped the phone conversation of December 4, 1967 in which Greenbaum read McDonald's work statement verbatim and it differed from Frosch's version. He asked Frosch to get the original and discover whether McDonald or Hughes proposed the changes. He enclosed memos on the phone conversations, prepared to get the letter notarized and said he would submit

to a polygraph test on the matter. [\[60\]](#)

The next day Klass spoke with a Dr. Raney of Frosch's office whom he told about the Greenbaum tape and suggested handling it cautiously or it could cost Greenbaum his job. Raney told Klass it would have to wait a week until the budget preparation period ended. [\[61\]](#) In a second conversation the same day Klass told Raney he would hold his

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November 21 letter to Frosch while they tried to determine who changed the contract. Raney responded that it would be difficult because it could be done verbally between the contractor and contract monitor, but he would try. [\[62\]](#)

Although telephonic communication between Klass and others in the controversy may have occurred in December, 1968, it would appear that a letter from Frosch dated January 9, 1969 provided the first written word Klass received after his talks with Raney. The letter informed Klass of the auditor's conclusions which were:

1. McDonald acted within the terms and conditions of his contract in his pursuit of information and investigations of UFOs
2. the ONR was aware of the time McDonald spent on UFOs
3. there was no law or regulation prohibiting Navy research on UFOs
4. McDonald used sound judgment in the distribution of expenses and provided adequate answers to the ONR questions

Frosch told Klass that he understood how Klass could misinterpret McDonald's public statements even though no impropriety occurred. He explained that McDonald's interests shifted over the years to a low priority area (UFOs) and as a result the ONR closed out his contract as of the beginning of fiscal year 1969. [\[63\]](#)

This did not satisfy Klass. He indicated to Frosch that he previously wanted to get together to discuss the unsent letter of November 21 and to listen to the Greenbaum recording. But because Frosch showed no interest Klass enclosed the letter "for the record." He said he would drop the matter if Frosch would tell him the date of revision of McDonald's work statement and what percentage of McDonald's

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air fare to Australia and per diem the ONR paid? Klass then changed his tactics and asked if he

could quote from Frosch's January 9, 1969, letter in future articles. ^[64] This was a new turn because in the past Klass always said he did not intend to write up the ONR matter for *Aviation Week* because, as McDonald's protagonist in the UFO controversy, people could get the wrong idea.

Klass also apparently contacted Finn Larsen to ask for a comment on the McDonald matter because Larsen wrote him "I cannot comment on the disposition of the matter but it does seem fairly clear that Dr. McDonald is unlikely to have continuing contracts in the field. ^[65]

Frosch responded to Klass' letter of January 13, 1969, and the November 21, 1968, enclosure, which Klass notarized, by saying he was "sorry that you should have construed a lack of confirmation of your earlier statements as an attack on your veracity." He answered Klass' questions by indicating the contract change took place on July 15, 1966, that Klass should ask McDonald about the money spent over and above the \$1,500 the ONR paid out for Navy business in Australia, and told Klass that any material addressed to him was his personal property which he could use as he saw fit. ^[66]

This ended the ONR controversy which solved little and pained many. In McDonald's parting shot to Hughes he summed it up this way. ^[67]

It seems to come to about this. I criticize deservedly a branch of the DOD (the Air Force) and lose my ONR support. An ONR scientist unjustifiably criticizes me (by attacking McDonald's UFO position in a nationally distributed ONR publication) and then brushes my objections aside (McDonald could not get retraction or satisfaction from the ONR at any level). And in the middle of the sequence the Navy puts me to no little awkwardness and some embarrassment by sending an auditor to investigate charges made by a journalist whose position is little more than writer's pique.

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SUMMARY AND CONCLUSIONS

Unlike the previous chapters which presented McDonald on the offensive, this one depicted him in a defensive posture. It would appear that a strategy evolved which McDonald used to protect his reputation. What were the tactics he used to accomplish this?

He fought two battles with Klass simultaneously. On the one hand, he responded to demands made by the ONR on a procedural level to counter Klass' allegations concerning the misuse of funds. On the other hand, he struck back at Klass through intensive attacks on Klass' plasma hypotheses. This no doubt seemed the only means of countering what McDonald viewed as

Klass' unorthodox manner of disputation.

When Klass first took his case to the ONR in December, 1967 the ONR demanded an accounting of McDonald's time from Richard Kassander. Kassander's favorable response to Hughes appears to have been written in part or toto by McDonald because some sections were composed in the first person.

With Klass, as well as the Arizona State Legislature in mind, it pleased McDonald to receive the \$1,000 for UFO research from Hall. He saw it as providing a tactical advantage, since it enabled him to open a UFO grant account at the UA which he could cite should he or Kassander be queried as to the source of his funding. Klass, he felt, might well be thrown off the trail by such a tactic.

By February of 1968 Klass realized, contrary to Greenbaum's assurances, that the ONR renewed McDonald's contract and so he began another assault. McDonald developed an eighteen page critique of Klass' position in his CASI paper as a response. He also tried to

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justify open funding of his UFO research at the ONR on the grounds that, as Klass pointed out in his book, more could be learned about plasma phenomena by studying UFO reports.

To check on Klass' arguments with respect to atmospheric electricity McDonald wrote Brook, Powell and Uman. When he heard about the JEM White Papers through John Fuller he suggested Fuller forward a copy of the CASI paper to Bob Martin at *Aviation Week* so he would have the opportunity to see why Klass distributed the JEMs.

As the argument grew more heated in September, 1968 McDonald denied the validity of UPI Dispatch 125 and the Anderson column claiming that they misquoted him. In addition, at approximately that point, he began to emphasize the good atmospheric physics research which resulted from his UFO studies. When Hughes indicated that an audit of McDonald's research account at the UA would take place he tried to avoid it by sending Hughes rough figures on his expenditures which he said Kassander could improve on later.

It should be obvious that Klass and McDonald employed two different styles. McDonald fought in the traditional scientific style of critiquing Klass' theory in papers and talks. It is true that he did this with a vengeance, which characterized everything he did, but nevertheless he remained within the confines of acceptable academic disputation. Klass, however, went beyond the pale. This could be, as Klass asserted, because McDonald refused to appear with him in debate, and because he could not find forums for his views. Regardless, he struck back in an unusual fashion

(the JEMs) and in an administrative manner (the ONR contract). But lest we be too quick to condemn, it

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should be remembered that McDonald's tactics viz the Condon Study were very similar. We might, in fact, conclude that extremists have a tendency to use extreme tactics.

It isn't clear whether McDonald bootlegged his UFO research, as Klass would argue, whether he received *sub rosa* ONR encouragement to pursue it, or whether Jim Hughes was the sole person giving impetus to the work. But that is really not at issue here. What is important is the lengths to which McDonald, Hughes, and/or the ONR went so that McDonald could engage a borderland phenomenon. Moreover, once McDonald took the steps, steps which could only be characterized as bending the protocols associated with normal scientific endeavors, McDonald had to be prepared to face the consequences of his actions. The consequences consisted of the non-renewal of his ONR contract, which he, always being in good form, said he decided not to renew because he wanted straight forward full time funding for his UFO work, something which the ONR could not provide.

SUMMARY OF TACTICS

To avoid exposure

1. wrote Kassandra's first response to Greenbaum
2. opened a grant account at the UA
3. tried to justify open UFO funding as good plasma research
4. denied UPI Dispatch 125 and the Anderson column
5. emphasized the good atmospheric physics coming from the study of UFO reports
6. submitted rough expenditure figures to Hughes to avoid an audit

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To fight back

1. Included an eighteen page critique of Klass' book in his CASI paper
2. tried to get the CASI paper published
3. wrote Brook, Powell and Uman to check on Klass' statement

4. encouraged Fuller to send JEMs to Bob Martin of *Aviation Week*

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2. Klass to Hynek, 3/28/66, Hynek Letters. - [Back to Text](#)
3. Hall to McDonald, 9/8/66, McDonald Letters, hereafter M.L. - [Back to Text](#)
4. McDonald to Low, early June, 1967, Klass Letters, hereafter K.L. - [Back to Text](#)
5. McDonald to Hughes, 6/12/67, M.L. - [Back to Text](#)
6. McDonald to Hughes, 8/17/67, M.L. - [Back to Text](#)
7. Klass to Greenbaum, 12/4/67, K.L. - [Back to Text](#)
8. Greenbaum to Klass, 12/6/67, notes on a phone conversation, K.L. - [Back to Text](#)
9. Kassander to Hughes, 12/16/67, M.L. - [Back to Text](#)
10. Klass to Greenbaum, 12/20/67, K.L. - [Back to Text](#)
11. Klass to Low, 1/5/68, K.L. - [Back to Text](#)
12. Greenbaum to Klass, 1/12/68, K.L. - [Back to Text](#)
13. Klass to Greenbaum, 1/17/68, K.L. - [Back to Text](#)
14. McDonald to Hall, 1/31/68, Hall Letters, hereafter H.L. - [Back to Text](#)
15. McDonald to Hughes, 2/11/68, M.L. - [Back to Text](#)
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21. McDonald to Hall, 3/18/68, H.L. - [Back to Text](#)
22. McDonald to Hall, 3/25/68, H.L. - [Back to Text](#)
23. McDonald to Ross, 4/2/68, M.L. - [Back to Text](#)
24. McDonald to Uman. 4/26/68, M.L. - [Back to Text](#)

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25. McDonald to Uman, 4/15/68, McDonald to Uman, 5/17/68, M.L. - [Back to Text](#)
 26. Klass, Philip J., JEM 1, 5/14/68, K.L. - [Back to Text](#)
 27. Klass, Philip J., JEM 2, 5/15/68, K.L. - [Back to Text](#)
 28. Klass, Philip J., JEM 5, 6/11/68, K.L. - [Back to Text](#)
 29. Fuller to Klass, 5/29/68, M.L. - [Back to Text](#)
 30. Martin to Fuller, 6/13/68, M.L. - [Back to Text](#)
 31. Klass to Fuller, 6/15/68, M.L. - [Back to Text](#)

32. McDonald to Fuller, 6/25/68, M.L. - [Back to Text](#)
33. Klass to Greenbaum, 6/26/68, K.L. - [Back to Text](#)
34. Greenbaum to Klass, 7/24/68, K.L. - [Back to Text](#)
35. Klass to Greenbaum, 7/26/68, K.L. - [Back to Text](#)
36. Klass to Greenbaum, 7/30/68, K.L. - [Back to Text](#)
37. M.L. date unknown. - [Back to Text](#)
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39. McDonald to Hall, 9/7/68, M.L. - [Back to Text](#)
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44. McDonald to Kassander, 9/20/68, cc. Klass, Kassander, M.L. - [Back to Text](#)
45. McDonald to Frosch, 9/25/68, M.L. - [Back to Text](#)
46. Klass to Frosch, 9/30/68, cc. Admiral Thomas Moorser, McDonald, Kassander, Larsen, K.L. - [Back to Text](#)
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51. Quintanilla to Klass, 10/11/68, K.L. - [Back to Text](#)
52. Klass to Larsen, 10/15/68, K.L. - [Back to Text](#)
53. McDonald to Hughes, 10/17/68, M.L. - [Back to Text](#)
54. Frosch to Klass, 10/22/68, K.L. - [Back to Text](#)
55. Klass to Frosch, 10/23/68, cc. Larsen, K.L. - [Back to Text](#)
56. McDonald to Klass, 9/28/66, cc. Larsen, K.L. - [Back to Text](#)
57. Klass to Frosch, 10/23/68, K.L. - [Back to Text](#)
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61. Raney to Klass, 11/22/68, notes on telephone conversation in K.L. - [Back to Text](#)
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64. Klass to Frosch, 1/13/69, K.L. - [Back to Text](#)
65. Larsen to Klass, 1/22/69, K.L. - [Back to Text](#)
66. Frosch to Klass, 1/31/69, K.L. - [Back to Text](#)
67. McDonald to Hughes, 3/25/69, M.L. - [Back to Text](#)

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VI. THE 1969 AAAS UFO SYMPOSIUM

[Background](#)

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[Summary and Conclusions](#)

[Summary of Tactics](#)

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BACKGROUND

For many years those UFO researchers who considered the matter a significant scientific problem had long desired a hearing before the scientific community. In 1969 they viewed the AAAS forum as an opportunity to right the wrongs done by the publication of the Condon Report. This chapter is intended to illustrate the manner in which established science dealt with the UFO phenomenon through the 1969 AAAS UFO Symposium. We will see that it involved much more than getting the program organized and asking the speakers to take part. Moreover, we will find that there were three radically different perspectives on the symposium and its function, represented by those who organized it, others who opposed it, and a third group who welcomed the opportunity to legitimate the study of the UFO phenomenon.

The reason or reasons for the symposium are not readily identifiable. Various interest articulation efforts existed for years. Most notably APRO and NICAP and probably more importantly, from the perspective of the scientific community, the prodding begun by McDonald in April, 1966. However, the Condon Study in all likelihood proved a larger factor in creating a climate of opinion among scientists and the academic community in general which would not only tolerate, but by and large encourage, an examination of this bizarre phenomenon.

With this in mind, then, we can begin taking a look at the preparations for the symposium which was initially intended to take place at the 1968 AAAS Meeting in Dallas, Texas. As the story unfolds the arguments of the symposium proponents and opponents should become

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clear. This will provide an occasion to observe the manner in which, at least in the case of the UFO problem, established science came to grips with a borderland science subject. In particular, it will enable us to examine the personal political strategies and tactics of those individuals who

came to verbal blows over the efficacy of the UFO symposium concept.

The issue began to surface in April, 1968 when Dr. Thornton Page, then of Wesleyan University's Department of Astronomy, and co-organizer of the symposium with Dr. Carl Sagan of Harvard, wrote Dr. Dael Wolfe of the AAAS to explain his reasons for wanting the AAAS to hold a UFO symposium at the 1968 meeting. He argued that:

1. the subject was interdisciplinary
2. it was of interest to the public
3. most scientists sneer at it but are not well informed
4. those scientists who have studied the problem disagree in their conclusions
5. the Condon Report will deserve discussion
6. the subject of UFOs would serve to inform the public on current knowledge in astronomy, atmospheric physics and space biology

Apparently Page already did some spade work on the project because he said Menzel of Harvard, author of two UFO books, and Condon refused to appear because of the kook aspect of the subject. Nevertheless, he felt that Hynek from Northwestern's Astronomy Department, Sagan from Harvard's Astronomy Department, Markowitz of Marquette's Physics Department, McDonald from the University of Arizona's Institute of Atmospheric Physics, Drake of Cornell's Center of Radiophysics and Space Research, McCrosky of NCAR, Klass from

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Aviation Week and Space Technology and someone from the Air Defense Command would make for an interesting discussion. [\[1\]](#)

A few weeks later Page wrote Hugh Odishaw at the AAAS again to explain the plans for the symposium and to request that the AAAS, which would review the Condon Report, release it prior to the AAAS meeting and provide the symposium participants with copies two weeks in advance. (Since it was the NAS and not the AAAS that would review the Condon Report the above statement indicates Page's lack of familiarity with the UFO situation.) At this point Page provided his first recognition of the extreme views held by some on the UFO issue. He said: [\[2\]](#)

Although our proposed list of speakers includes competent scientists acquainted with the UFO problem, there are several others. One extreme case is Donald Menzel (author of two books on flying saucers) who opposes any two-sided discussion. The other extreme is Keyhoe, Ruppelt and other Ufologists supercritical of the Air Force. We plan proper scientific discussion without

regressing to either extreme.

This is the last communication concerned with the planning of the symposium in 1968. The issue remained dormant for the summer except for those involved in trying to abort the event. The principal parties to that attempt were Menzel, Markowitz and Klass. In the summer of 1968 they tried to compose a joint letter to *Science* which went through six or seven drafts. All three opposed the symposium, but they could not agree on the wording of their statement. Finally, they decided to write individual letters, but then never sent them. ^[3] The same men would oppose Page and Sagan in 1969.

Because of the work of the above trio, and possibly behind the scenes activities on the part of others, Sagan wrote McDonald that the

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symposium would be postponed until 1969. Sagan gave two reasons. He said that the CU Report would not be out in time for the symposium participants to discuss it in 1968 and that individuals in some quarters became quite emotional about the holding of the symposium so a postponement would both provide time for the report to come out and for the air to clear. ^[4]

HISTORY OF EVENTS

[Initial Planning](#)

[Condon's Opposition](#)

[Additional Obstacles](#)

[Forging Ahead](#)

[Symposium and Aftermath](#)

Initial Planning

By early 1969, however, the obstructors knew that the symposium plans were again in the offing. Menzel, apparently asked by Page to give his views on the symposium arrangements, asserted that UFOs had nothing to do with outer space, and anyone who invoked the ETH to explain them

was a crackpot or crank who should not be permitted on the panel. He said he would not appear with any of them because they did not publish scientific analyses of the problem; this included McDonald who Menzel considered unscientific. If McDonald or any other "of his ilk" participated Menzel made it clear that he would not and, in fact, would oppose the symposium.

With respect to who should speak and on what, Menzel also held views. He thought Hynek unpredictable but alright to relate his Air Force experiences, while Major Hector Quintanilla could provide the Air Force position. Menzel himself would explain what prompted UFO observations, Brian O'Brien could speak to the physiological optics of sightings. Marcel Minnaert for the meteorological optics, William Markowitz on reported landings, Philip Klass on plasma phenomena and a psychologist for the psychological aspects of sighting reports.

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Menzel believed that the AAAS should begin where the NAS Review of the Condon Report left off -- namely, with a responsible position. He explained to Page that if the symposium included the crackpots they would dominate the proceeding and contribute ignorance rather than knowledge. Therefore, he urged their exclusion and suggested that if Page feared their criticism for holding a one-sided symposium that he cancel the program. He said that he talked to Condon and that even he would participate if Page did not invite the crackpots.

Menzel claimed in his closing remarks that future science would not be synonymous with magic and that some things in physics, which he apparently considered mutually exclusive of the UFO phenomenon, such as special relativity, some form of general relativity, the laws of conservation of mass and energy, the second law of thermodynamics and the impossibility of perpetual motion would be part of physics for a 1,000 or even 10,000 years. He said: [\[5\]](#)

I am fully aware that several of the believers, including some of those who contributed to the Roush Hearings, accused me of being old fashioned and reactionary. But I think it is significant that none of these individuals has made any substantial contribution to science in his own right.

Page enclosed a copy of Menzel's letter when he wrote Sagan, Philip Morrison and Walter Orr Roberts. The former two (Page and Sagan) were the major planners of the symposium and the latter two (Morrison and Roberts) minor planners. Morrison was a member of the Massachusetts Institute of Technology Department of Physics, while Roberts was at NCAR in Boulder and President of the AAAS. This letter indicates Page already did some plotting to insure an interesting panel. He said Condon might take part and Markowitz showed signs of

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softening. He proposed three panels; **The UFO Phenomenon; The Possibility of ETI;** and **Science, The Public and UFOs.** "Then we can tell Menzel and Condon that McDonald is not in the symposium on the UFO phenomenon. A bit devilish, but it might work." ^[6] Page considered Condon's participation important because the Condon Report came out in January of 1969 and members of the academic community considered it the only scientific contribution to the UFO debate. But the Report did have detractors, principally McDonald and Hynek, and the symposium would be an appropriate place to rehash some of the issues. Given the magnitude of the CU effort and the atmosphere in 1969 viz UFOs, Condon's absence would be a genuine loss to the symposium organizers.

Three months passed before Page dropped Sagan a postcard concerning the symposium. In it he said he spoke to Walter Berl at the AAAS who indicated Condon might regain his sense of humor in time to take part in the Boston proceedings. ^[7]

Sagan contributed more on Condon's participation a week later. He stated they talked at a conference on "Science and the Future" and Condon remained on the fence about his participation. Sagan argued that avoiding a confrontation between McDonald and Condon seemed the logical approach. This, he thought, could best be accomplished by having them appear on separate days and assigning Condon the task of discussing the role of UFO literature in science education. ^[8]

The planning committee was hardly packed with "believers" as some critics would eventually argue. This can be seen in the remarks of Morrison to Page after apparently talking to Hynek, George Price-Williams, a psychologist at Rice, Page and McDonald.

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In referring to the symposium he said, "the scheme seems to be that the strange nature of UFOs is to be assumed unless disproven." He showed displeasure with that approach and averred that neither Hynek nor McDonald showed him any truly anomalous cases. Moreover, after reading the Condon Report he believed that enough work had been done in the area. He said he would speak at the symposium not on UFOs, but on the nature of physical evidence and would do it in the spirit of public education. ^[9] Page responded that if Sagan and Roberts agreed then Morrison would speak last on the nature of physical evidence. He assured Morrison that by the time he reached the podium the ETH arguments would be discredited. ^[10]

Roberts also showed concern about linking ETH discussions with the UFO presentations. He told Page that such a juxtaposition could convey the wrong idea. Turning to another subject he

considered it unfair to have Low confront McDonald on atmospheric explanations of UFO observations and prevailed on the group to invite Franklin Roach, a noted astronomer with a specialty in air glow, who analyzed the astronaut sightings in the Condon Report. Roberts also made the first mention of the motivation for holding the symposium when he said: [\[11\]](#)

As you know, I am in tune with Carl Sagan's notion of using things like the UFOs to gain the attention of the public, and then using these as ways of providing knowledge about the specific subject matter, as well as science generally.

In August the speakers and topics for the symposium in December remained tentative. Page informed Sagan that Morrison approved of Condon, but not Menzel, and continued to oppose any discussion of the ETH although he felt a critique of the news media's coverage of UFOs seemed in order. Page thought the latter as tangential as the former,

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but told Morrison to draft a letter to Walter Sullivan, the Science Editor of the *New York Times* if he wanted someone to cover the news media. [\[12\]](#) Sagan spoke to Morrison about the ETH and found Morrison willing to permit him to discuss the ETH provided that he gave equal time to other "equally poorly based" hypotheses; to that Sagan agreed. [\[13\]](#)

Condon's Opposition

At that point the plans for the coming symposium became public knowledge through an announcement in the August 15 *Science*. This spurred a new onslaught apparently spearheaded by Condon. He wrote Roberts that the AAAS Board should carefully consider its action which he deemed inappropriate. Condon said he wrote to squash the rumor that he would participate. Contrary to that rumor no one asked him and he would not take part if asked. Moreover, he would not attend any portion of the annual AAAS function if Sagan and Page held the UFO Symposium.

In a further attempt to sway the Board he sent each member a copy of the Condon Report. He told them that its distribution was poor and consequently they probably did not obtain a copy. He also enclosed a *Rocky Mountain News* article on some of the people he thought would take part in the symposium and ridiculed their intellectual caliber, while lamenting the fact that they had Ph.D.s, were probably AAAS members and could not be refused. He pointed out that none of the "great" scientists who showed enthusiasm for UFOs developed a research proposal in the area and suggested that Roberts read *Error and Eccentricity in Human Belief* by Joseph Jastrow to understand "kooky beliefs" on a par with UFOs.

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Condon did not oppose a fair discussion of controversial ideas but he considered it impossible.

"The UFO buffs are a slippery lot, and do a great deal by insinuating, so it is usually useless to try and find out what they are really contending. Some never had any critical faculty, some are suffering severely from progressive degeneration of whatever critical faculty they ever had."

He believed that by holding the symposium "the ignorant will be misled. The intelligent will think the AAAS is crazy."

He concluded by suggesting that the AAAS was pro-UFO. *Science* displeased him by sending a scandal monger (Boffey) out to Boulder in 1968 to write about the project. Then *Science* did not review the finished report, and finally a two day symposium. He asked, "why this foolish behavior? Have UFO kooks infiltrated 1515 Massachusetts Avenue?" [\[14\]](#)

Condon's letter preceded one from Hudson Hoagland, a Psychologist/Physiologist from Harvard, who agreed with Condon that no self-respecting scientist should dignify the meeting with an appearance. He likened the UFO issue to the hysteria surrounding mediums after World War I. He had exposed Margery the medium in a 1925 *Atlantic Monthly* article and then scientists who held a belief in her attacked him. [\[15\]](#)

A note from C. D. Shane, an Emeritus Astronomer at Lick Observatory who served on the NAS Review Panel for the Condon Report followed. He agreed with Condon, saying reputable scientists would not take the time to refute all the wild claims made by UFO buffs. He saw the symposium as equivalent to one on astrology or Velikovsky's views. [\[16\]](#)

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However, Page did not give up on getting Condon to speak. He felt Condon became upset because word leaked out about his appearance before he received his invitation. Page told Roberts that in the future all the names would remain secret until receipt of the acceptances. If, in fact, Condon refused then perhaps Roach and/or Low could talk about the Colorado Project. [\[17\]](#)

Page wanted Major Quintanilla, the Air Force officer in charge of Blue Book, to appear. He asked Hynek to intercede with Quintanilla's commander to obtain permission. Page cautioned Hynek to keep all the prospective speakers names quiet because he feared some, such as Condon, would not accept if they knew who else might take part. [\[18\]](#) Apparently the political problems inherent in this endeavor were new to Page for he said in a note to Sagan, "In an attempt to

improve on my political naivete I cut up the copies of our tentative program for Price-Williams, Hynek and Sullivan, warning them that changes may be made if some proposed speakers do not accept." In other words, Page sent out the programs to prospective speakers, but excised the names of those issued invitations. In passing he mentioned that Roberts continued to cajole Condon in the direction of an acceptance. [\[19\]](#)

Roberts wrote to Condon to invite him to participate on behalf of Page, Sagan and Morrison. He pointed out that many school teachers would be present in the audience (Condon feared the insidious effects of permitting children to devote science class hours to UFOs in school) which would be an ideal time to discuss the dangers of introducing crackpot ideas in the curriculum. The planners tried to be careful with Condon, as shown by the following: [\[20\]](#)

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Your participation has been planned in such a way that we do not think there will be any likelihood of vitriolic exchanges on the platform. And we have carefully chosen chairmen who should be able to keep things, throughout the symposium, generally in control, even if there are some extreme UFO cranks in the audience.

The same day Page set about correcting his earlier faux pax with Condon, no doubt having previously coordinated his efforts with Roberts. He explained his error in permitting the AAAS to send out a tentative speakers list in June and he regretted that it happened. He told Condon: [\[21\]](#)

Our hope is to correct some of the public misconceptions your study uncovered, and to show the public that scientists can discuss controversial topics rationally, and with a sense of humor.... It is true that we also agreed to invite one scientist whose speculations we do not individually agree with but our purpose is clear: if we don't exhibit the other side, how can we correct the misconceptions listed in the Condon Report?

But these efforts did not prevail, in fact it would appear that they infuriated Condon. He wrote a refusal and some choice remarks to Page, the AAAS Board and "most invitees." He said he did not know about the symposium in June, but he wished that he did so that he could have started opposition to it earlier. He hoped that time still remained to cancel it because the press coverage would focus on the sensational and irresponsible with a resulting disservice to the AAAS on the part of Page and Sagan. Condon enclosed copies of the Hoagland and Shane letters to underscore the fact "that I am not alone in this matter." [\[22\]](#)

Nor did Condon stop at refusing to take part; he also attempted to get others to decline. R. E. McCrosky of Harvard's Astrophysical Observatory informed Sagan that he could not accept the

invitation and

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felt it best to phrase his decline in terms of the Condon-Shane-Hoagland correspondence. He stated that he did not want harm to come to the AAAS or for mass resignations to occur. Moreover, he viewed the UFO issue as one of the least important problems facing science. Consequently he urged that the symposium be called off. [\[23\]](#)

Page decided to counter Condon's campaign in order to shore up the ground on which the symposium stood. He wrote Wolfle at the AAAS on the topic of Condon's circulation of the letters opposing the symposium. Page asked for the support of the AAAS, arguing that his motives were much the same as Condon's for doing the Condon Report, that Shane's fears of focusing on the sensational seemed unwarranted, and that Hoagland's disquietude over supporting the charlatans and the deluded would prove unjustified. Page claimed the facts which emerged from the Condon Report indicated:

1. a large fraction of the public was poorly informed
2. an emotional public reaction to press coverage
3. most, but not all, reports were explainable in terms of physical phenomena

Therefore, he felt that the symposium would prove to be an educational effort of great significance. [\[24\]](#)

Sagan did not give up on Condon. He asked him to reconsider his decision in light of the fact that the people who appeared with him would be sober, responsible and critical. Sagan also referred to Condon's 9/5/69 comments stating that no one on the arrangements committee leaked the fact that he accepted, the people cited in the *Rocky Mountain News* article were never considered for the symposium,

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and in fact, proposals for UFO research did go to NSF because he reviewed one. [\[25\]](#)

Brian O'Brien, the retired physicist who chaired the AFSAB Panel on UFOs in 1966, also declined. He claimed unfamiliarity with the subject would make it impossible for him to add anything to the discussion. Condon reached him either verbally or by letter, however, for he said that if Sagan took the proper precautions with the press that the problems anticipated by Condon could be avoided. [\[26\]](#)

Additional Obstacles

But Condon was not the only one that Page and Sagan had to contend with. Donald Menzel had not been heard from for a while, probably because of his visit to Europe. On his return Roberts wrote Page that he intended to protest. He didn't want to give a platform to people he labeled as crackpots and worse. Because McDonald excluded him from the Roush Hearings he still fumed and claimed that McDonald did not abide by the standard rules of evidence. He indicated that he would not appear in the same symposium as McDonald; that his refusal was final. In addition, he wanted to know about the formal channels for protest because he intended to do everything he could to stop the symposium. This worried Roberts because he could see that the possibility of mass withdrawals from the program was a reality, one which would spoil the balance of the panels. [\[27\]](#)

Page was indeed having trouble finding speakers. He believed that Condon talked McCrosky, who was to speak on the Prairie Network of Sky Cameras, out of appearing and then the Air Force informed him that it would not provide speakers, meaning Quintanilla. [\[28\]](#) Finally, David Atlas of the University of Chicago, an authority on anomalous

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propagation phenomena, i.e., unusual atmospheric conditions resulting in radar returns, explained that he had to conduct experiments during the period that the AAAS Meetings would be held and consequently could not take part. Atlas said, however, that the Condon letters did not bear on his declination, in fact, he considered the symposium a good idea. [\[29\]](#)

Sagan continued his efforts to win converts and bolster the status of the symposium enterprise. He wrote a letter to Roberts, Page, Morrison, Condon, and the AAAS Board, which he no doubt meant to influence the latter two. He said that in criticizing the UFO endeavor some thought that astrology and Velikovsky might be next. He wanted to agree with the reasoning, but not with the implied absurdity of the conclusion. Sagan pointed to a drift in the universities away from the sciences and toward borderland areas. He believed symposia such as the one planned for the UFO phenomenon provided a way to confront such claims with the scientific method. He went on to argue that an obligation existed for the AAAS and other similar organizations to address subjects which already interested the public. In the UFO case men with good credentials disagreed, making it the duty of the scientific community to keep the lines of communication open.

At that point he tried to social pressure Condon and others by citing a 1969 statement which emanated from a conference on "Science and the Future" sponsored by the BAAS and the AAAS

which accorded with his above comments. Edward Condon, Ian Cox, Steven Dedijer, Dame Kathleen Lonsdale, Robert Morrison and Sagan signed it. [\[30\]](#)

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This approach did not win the day with Condon. He responded to Sagan with a five page single spaced letter to explain why he would not participate and to detail his past involvement. The letter carried the following statement:

CONFIDENTIAL: Intended to be read only by those to whom
copies were sent. Distribution list at end.

He began by saying that the symposium would not contribute to a popular understanding of science or the scientific method. He said, to the contrary, it might show some of the less praiseworthy traits of the scientists on the panels. Condon hoped the AAAS Board would cancel the proceedings. His presence was out of the question because "participation would bring me into contact with individuals in whose integrity I lack confidence." He then continued for three and one half pages outlining his experience with the UFO phenomenon and those associated with it. The implication was clear; It constituted nonsense and he desired no further entanglements. The following received copies: [\[31\]](#)

Thornton Page

Philip Morrison

Walter Roberts

The AAAS Board

Philip Handler and the NAS Review Panel

Philip Seitz, President of the NAS

Daniel Gillmor, Editor of the Condon Report

Walter Sullivan of the *New York Times*

William Price, AFOSR

J. R. Smiley, President of the University of Texas at El Paso

Fred Thieve, President of CU

Harold Brown, President of the California Institute of Technology

William Markowitz, Marquette University

Richard Kassander, University of Arizona

Hector Quintanilla, Blue Book

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Others who have a need to know in connection with possible future efforts to get support for UFO studies.

Spiro Agnew, Chairman, National Aeronautics and Space Council

Lee DuBridg, Office of Science and Technology, Executive Branch

Bob Seamans, Secretary of the Air Force

John H. Chaffee, Secretary of the Navy

Tom O. Paine, NASA

William D. McElroy, NSF

Bob M. White, ESSA

Senator Clinton Anderson, Chairman of the Senate Committee on Astronautical and Space Sciences

Representative George Miller, Chairman of the House Committee on Science and Astronautics

While Condon reiterated his refusal Menzel began to capitulate, but with conditions. He told Page that he would take part with regrets but, "The condition is that I be allotted a time equal to the sum of the time allotted to Hynek and McDonald or one hour, whichever is greatest." In his note he indicated that Page verbally accepted the arrangement in a phone conversation. [\[32\]](#)

In addition, he wrote to Roberts to say that although his misgivings still remained, and there

existed strong pressures not to participate, he would, nevertheless, take part for several reasons. First of all he felt he would like to stop the symposium, but he could see that such an effort would fail. Secondly, the program, according to Menzel, did not have balance and since he was the only one with extensive experience studying Air Force Cases he believed a responsibility existed to the AAAS to make his conclusions known. It upset him and he thought it unfair to be required to appear with five scientists who testified at the Roush Hearings, particularly when the only places they published were sensational magazines, press releases and newspapers. He advised Roberts that the participants should be

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prohibited from releasing their own press material; he felt all press releases should go through the AAAS. [\[33\]](#)

The following day Menzel wrote Roberts again. He reiterated his desire to see the affair cancelled, but said that he would appear. He would do so because of his experience in the area. Moreover, he intended to counter those who wanted to use the AAAS to get Federal funds for UFO studies. Therefore, he did not want his participation interpreted as endorsement but as a means of opposing those who desired to use the AAAS for their own ends. [\[34\]](#)

Forging Ahead

After receiving Condon's letter of 10/6/69 Page became upset, if we can judge from a response he penned but never sent for fear of rousing Condon again. He said he sympathized with the treatment Condon experienced at the hands of extremists and hoped Condon could understand his resentment of the treatment the AAAS Special UFO Committee now received from another group of extremists. He asserted they would carry on, however, just as Condon's group did. Page claimed the logic of how the AAAS could be damaged by a nine hour symposium, when Condon's team worked fifteen months on the subject escaped him. He went on to say that if Condon opposed the authoritarian methods utilized against Galileo that he should stop his appeals to authority. Page intended to enclose 21 stamps so that Condon could send copies of the letter and his response to the people on his distribution list. [\[35\]](#)

As if Sagan and Page didn't have enough problems, Hynek made a complaint from the other camp. The experience and competence of several of the proposed speakers concerned him. He asked if Menzel, Morrison or Condon (apparently he did not know Condon declined) ever investigated cases? He continued:

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Yet they will speak most learnedly of the phenomenon. Better I should address a medical association on the function of the liver and its diseases, about which I would have as much information as Morrison about UFOs.

He said that if Menzel claimed he examined all the Air Force files he would be prepared to state that he (Menzel) had not. Hynek suggested that Page would do better to include George Kocher from The Rand Corporation, William Powers from Northwestern's Astronomy Department or Joachim Kuettner from ESSA. [\[36\]](#)

Page told Hynek that Powers would add little, but he would propose Kocher. He said Hynek could ask Menzel at the symposium about his outlined cases, "but he will get sore (which we don't want) if you claim he is ignorant." Page assured Hynek that Morrison would not talk about cases, but rather the nature of scientific inquiry. [\[37\]](#)

The claim that Menzel examined all the Air Force files apparently raised Hynek's hackles for the above letters were not the end of it. No doubt because Hynek considered himself the authority on Air Force cases and knew Menzel couldn't have examined some 10,000 or so reports in the few days he spent at Blue Book in 1962 or 1963. He told Sagan: [\[38\]](#)

Thornton Page wrote me saying that he hoped whatever I had to say would not make Menzel "sore" and I had replied that if Menzel got "sore" that was his problem, but that I would not countenance his stating that he had examined all the Air Force files.

In response to Page's comments Hynek stated that he did not intend to make personal remarks except in self defense. He claimed that he would come to the symposium to report on and not to sell UFOs; moreover he would not attack Condon directly. [\[39\]](#)

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Page forwarded a summary of his talk to Menzel and asked Menzel to prepare one as per the guidelines of the symposium. Page included a bibliography of UFO literature which Menzel criticized as inadequate. Menzel argued that two lists labeled "crackpot" and "scientific" were in order. Only Tacker, Sullivan, Cantril, Klass, Condon, Markowitz and by implication Menzel belonged on the scientific list. [\[40\]](#) While on the topic he pointed out that neither McDonald or Hynek published anything that could be viewed as a scientific approach to the problem.

Regarding a summary of his presentation Menzel hesitated. He said he didn't know of the requirement and might not prepare it because he didn't want the content of his paper known to McDonald and Hynek. But if they forwarded theirs, then he would write up his since he saw

himself as providing balance to their position. He told Page that he continued to favor cancellation. [\[41\]](#)

Menzel was far from a happy man at this point for he would take part under duress and as he saw it his protests went unheeded. He wrote Roberts to this effect, saying that Roberts' letter of 10/10/69 almost made him decide to withdraw for he claimed that Roberts misconstrued his letters of 10/7/69 and 10/8/69. He wanted Roberts to convey to the AAAS Board his reasons for wanting the symposium cancelled, not his reasons for agreeing to appear. Noting that Condon, Quintanilla, Atlas and O'Brien did not intend to submit papers and that six participants from the Roush Hearings were, he plaintively asked why he just didn't withdraw and leave the proceedings to the believers. [\[42\]](#)

Roberts worded his reply in conciliatory terms. He apologized for the ambiguity in his 10/10/69 letter claiming that he felt he

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understood Menzel's reasons for wanting the symposium cancelled and for taking part and he informed the AAAS Board of both. He told Menzel he would help in any manner he could to make participation easier. [\[43\]](#)

Although Page did not send his letter of 10/8/69 to Condon, judging from Condon's 11/7/69 letter to him Page probably relayed his concern either as an aside in a note asking Condon to look over an article on UFOs which Page wrote for the *Encyclopedia Britannica*, or possibly by enclosing the 10/8/69 letter even though it bears the "not sent" heading in Sagan's files. Condon asserted that he did not send copies of his 10/6/69 letter to individuals in government in the hope that they might in some way cancel the symposium, but rather to warn them about future attempts to get UFO research grants. Condon made the further claim that holding the UFO proceedings had nothing to do with the "preservation of freedom of scientific inquiry, for a specious presentation of both sides will merely confuse the public all the more by giving AAAS sponsorship to scientific chicanery." [\[44\]](#)

Another consideration began to take on importance in Sagan's mind -- namely Menzel's health. At 68 years of age Menzel did not have a good heart; Sagan feared, after reading the 10/30/69 letter to Roberts, that Menzel's "heated approach" might cause him to have a heart attack in the session that Sagan chaired. He asked Roberts "do you think there is any way we can cool him down?" [\[45\]](#)

Based upon a letter from Menzel to Page it would seem that Page wrote him on 11/6/69 to advise Menzel not to get upset over the symposium because it could harm his health. Menzel agreed to

that, but said it only angered him that he agreed to take part when the list of

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knowledgeable people such as Condon, McCrosky, Quintanilla and O'Brien kept dwindling, while Sagan, Drake and Morrison were not knowledgeable about the subject, leaving him to do battle with the "believers" from the Roush Symposium.

Finally, he wanted to know if it were true that Sagan and Page did not invite Klass because McDonald refused to appear with him? [\[46\]](#)

Hynek also wrote to Sagan of his concern for Menzel's health problem and said he didn't want him to have a heart attack. His previous remarks, he stated, regarding Menzel's examination of Air Force cases in retrospect seemed a bit edgy; he would correct Menzel if necessary, but wanted a friendly discussion. [\[47\]](#)

Klass probably complained to Menzel when Sagan and Page did not ask him to speak, which prompted the above remark to Page on McDonald's refusal to appear if Klass spoke. At about that time Klass apparently volunteered his services through Roberts because Page wrote the former to explain that it was too late. He told Klass that since he (Klass) refused in 1968 that the committee did not bother to contact him. [\[48\]](#)

Actually the reason Page gave - tardiness -- is debatable because on the same day he wrote Klass he sent out a last minute invitation to George Kocher of Rand. [\[49\]](#)

Klass responded by recounting his earlier interactions with Page and Sagan for the purpose, according to him, of informing the AAAS Board of the pre-symposium history. His remarks put Page's refusal in a slightly different light. He said he wrote Roberts about the poor balance on the panels thinking the planners weren't aware of it, and Roberts replied that it seemed difficult to get critics to speak. Klass

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labeled six of the proposed panelists moderate to strong proponents of the ETH. He told Page that based on his 1968 organizing experience Page knew critics would decline, which was all the more reason to ask Klass. Contrary to Page's remarks he said he did not refuse to appear at Dallas, in fact was eager, but Sagan offered only 5-10 minutes on the program and Page offered 10-12 minutes, so naturally he refused since McDonald and Hynek would receive 45-50 minutes. Nevertheless, he wrote to Sagan in the summer of 1969 and in September to Page indicating he

would speak, but did not receive a reply. In closing Klass made it clear that he believed that the AAAS would survive the symposium but, "it pains me to find that the AAAS will lend its prestige to pumping new life into this pseudo-scientific fantasy." [\[50\]](#)

Page told Menzel that as he viewed it, Klass withdrew the previous summer and now wanted "to get in on the act," however it was too late. With respect to Menzel's concern about balance, Page assured him that McDonald was in a minority and would be opposed by Franklin Roach, an astronomer, Lester Grinspoon, a psychiatrist; Robert Baker, an aeronautical engineer; Kenneth Hardy, a meteorologist; and Philip Morrison, a physicist. He assured Menzel that McDonald's late summary resulted from his defensive stance. [\[51\]](#)

Meanwhile Sagan took Menzel's advice and wrote McDonald to explain that the AAAS would put out one press release based on the summaries of the individual papers, rather than letting each participant issue his own statement. [\[52\]](#) This was in direct response to McDonald's tactic of meeting with the press whenever he could and distributing his UFO papers.

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In his reply to Klass Page treated him lightly by saying that he (Page) was sorry Klass was so emotional in his effort to dispel historical misconceptions. He said that a psychologist would speak to that phenomenon in the symposium. As for Klass' claim that six panelists were pro-ETH, Page asserted just the opposite was true. He apologized for the 10 minute offer and for failing to answer Klass' September letter before telling him not to add any more to the AAAS correspondence load for, "we want to keep our sense of perspective and humor for December 26-27." [\[53\]](#)

The Klass reply spoke to the composition of the symposium panels and the intentions of Page. With respect to the former Klass stated the panelists Page considered anti-UFO, with the exception of Menzel, were pro or neutral, while definitely inexperienced. On the other hand, McDonald, Hynek and Hall, he argued, were familiar with case material which could not be countered by the above mentioned inexperienced speakers. He further claimed that based on a letter written to Walter Sullivan of the *New York Times*, Page was obviously pro-ETH and wanted the symposium to focus on that hypothesis. [\[54\]](#) That proved to be the last of the Page-Klass exchange.

Symposium and Aftermath

This ended the efforts to both organize and stop the symposium. Of significance to some was the seemingly deftly timed announcement on December 19, just a week prior to the symposium, that

the Air Force decided to follow the recommendation of the Condon Report by closing its Blue Book project. Regardless, the symposium took place December 26 and 27 in the midst of a heavy Boston snow which caused McDonald to miss the first day. The event did not prove exciting or the demise of the AAAS.

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Walter Roberts read Menzel's paper on doctor's orders for fear that Menzel might suffer a heart attack at the podium. Before it was over sixteen scientists participated in an orderly fashion.

It seems that McDonald did not know of Condon's 10/6/69 letter with Spiro Agnew as one of its recipients. He became aware of the matter in Boston and interpreted Condon's behavior, as did Page, as an appeal to Agnew to stop the proceedings. McDonald gave a statement to UPI explaining that he would protest Condon's actions through the appropriate AAAS channels. [\[55\]](#)

An issue of some import which arose as a result of the termination of Blue Book became the preservation of the 12,000 or so case reports the Air Force filed at WPAFB. A number of individuals feared that the Air Force might destroy 22 years worth of data. Consequently, twelve of the sixteen panelists along with Roberts composed a document sent by Page to Secretary of the Air Force Seamans which made four requests:

1. insure that all the material, both classified and unclassified, be preserved without alteration or loss,
2. declassify promptly all documents filed by the Aerial Phenomena Section of the WPAFB which are classified by virtue of AFR 200-17 and AFR 80-17,
3. make all the unclassified documents available to qualified scientific investigators at a more suitable location than the USAF Archives (we recommend a major university in the Midwest), and
4. order an annual review of the remaining classified documents in the present file to determine when they can be declassified without alteration in accordance with current USAF security procedure.

Walter Roberts, Franklin Roach, William Hartmann, Lester Grinspoon, Robert Hall, Philip Morrison, Douglas Price-Williams, J. Allen Hynek,

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James McDonald, Carl Sagan, Walter Sullivan, George Kocher and Thornton Page signed the request. [\[56\]](#)

Menzel wrote Page that he wanted to store the material, but he thought it should be impounded for ten years to keep it from the "buffs" and to allow time to restore a sense of balance to the subject. He said he thought qualified people were entitled access to the material, on the authorization of the Secretary of the Air Force or the president of the NAS, but he didn't know how to define qualified. Basically, he said, he wanted to keep McDonald, Hynek and UFO groups such as APRO and NICAP from having access to the data. ^[57]

As a final determination for the Blue Book file problem the Air Force sent them to its library archives at the Air War College, Maxwell Air Force Base, Alabama. The results of the symposium were limited. The papers were largely conservative with the exception of Hynek and McDonald. Sagan and Page eventually published them in a book which is considered an authoritative academic reference work on UFOs in the sense that it presents the gamut of scientific opinion as of 1969. ^[58] The repercussions of the symposium for the AAAS and the scientific community were nil. There were no mass resignations of scientists from the AAAS nor did the public interpret the AAAS Symposium as endorsement of UFOs by that august scientific body and as a result turn out in throngs to demand a Congressional investigation or a NASA type agency to study the problem. In short, the UFO situation that existed before the symposium remained much the same afterward, and the publication of the Sagan-Page volume of papers in 1973 seemingly did not effect the number of academics involved with the issue.

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SUMMARY AND CONCLUSIONS

In the previous chapters we only looked at McDonald's strategy and tactics, yet in each instance there also existed opposing individuals and groups with their own strategies and tactics. In this chapter the opportunity arose to examine the "other side." In this instance, the other side did not so much oppose McDonald as it opposed Sagan and Page for their naive attempt to organize the AAAS UFO Symposium. Nevertheless, it is an excellent occasion to observe the manner in which this opposition took form and the way in which it functioned; for it proceeded in a similar fashion against McDonald on other issues.

Menzel and Condon saw Sagan and Page as sympathetic to the ETH, but were more concerned, I think, that they were dupes of the flying saucer conspiracy. Therefore, they believed, as elder statesmen of science, that it was their duty to do all in their power to stop the symposium plans which would tarnish that revered body and lend support to the notion that UFOs represented a legitimate topic of scientific inquiry.

On the other hand, Sagan and Page conceived of Condon and Menzel as extremists, who

exaggerated the harm which could come from the symposium and failed to see the good which could be derived. The good they defined as confronting a bizarre belief system with the scientific method and witnessing the demise of the former. This they believed would redound to the long term benefit of science as an ongoing process.

The strategy for Sagan and Page consisted of doing what appeared necessary to hold the symposium. To the contrary, the strategy of

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Menzel and Condon comprised doing whatever was necessary to stop it. Let's look at the tactics each employed to implement their respective strategies.

The obstructionist tactics of Condon and Menzel differed. Condon never intended to take part and he didn't. Menzel didn't want to, but in the end could not resist the challenge. Consequently Condon took steps to actually stop the symposium, while Menzel basically needed to be dragged to the altar.

Menzel actually began his obstructionist tactics in the summer of 1968 in an attempt to stop the originally conceived Dallas symposium. He, Klass and Markowitz tried to draft a letter of protest to *Science* which in the end they never sent. Then in January of 1969 he urged Page to exclude crackpots, meaning scientists like McDonald, and said he would not appear with them. He suggested cancellation if a one-sided program were not feasible and held out the possibility that Condon would appear in such a one-sided event.

When he returned from Europe in September he told Roberts he would not appear, and intended to stop the symposium; he said he intended to use formal AAAS channels of protest. In October, however, he relented, assuming he would get speaking time equal to that of McDonald and Hynek combined. He indicated to Roberts that he would participate to offset the five scientists who spoke at the Roush Hearings and to see to it that they didn't use the AAAS to obtain funding for UFO studies. However, he remained of the opinion that cancellation was the best thing for the AAAS. In late October he made his last attempt at being

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obstreperous by refusing to forward Page a summary of his paper until he received summaries from McDonald and Hynek.

The battle with Condon occurred during September. He began by telling Roberts he would not

attend any AAAS functions if the symposium were held and ridiculed the AAAS for its pro-UFO position. Then he began an effort to get various elder statesmen of science to oppose the function and others to cancel their appearances. In the former group Hoagland of Harvard and Shane of the Lick Observatory appealed to the AAAS with Condon. To make his case Condon distributed copies of their AAAS letters of protest, and his own, to various prominent scientists and to all the prospective symposium participants. Condon argued that the event would do a disservice to the AAAS and only provide a platform for legitimating future efforts of UFO buffs to obtain Federal money for UFO studies.

As a result of this campaign McCrosky of Harvard cancelled his presentation for the good of the AAAS. O'Brien of the AFSAB also declined as did Atlas of the University of Chicago, although they claimed it was not due to Condon's lobbying. And finally, the Air Force refused to provide speakers, who, Page hoped, would present the official position.

When it seemed that the symposium would be held anyway Condon made a last appeal to Sagan in which he reiterated his early arguments and encapsulated the history of his own involvement with UFOs. As a last minute effort he sent carbons to everyone in the scientific and governmental communities who he thought had "a need to know," particularly with respect to future funding policies in the area.

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The tactics of Sagan, Page and to a lesser extent Roberts focused on obtaining speakers for the symposium. This involved humoring Menzel and Condon, countering Menzel and Condon, and trying to appease Menzel and Condon. They did this because Menzel and Condon were the most important members of the anti-UFO camp. Without their presence it was felt the symposium would lack balance and fail to obtain support from the AAAS Board.

Because of the opposition to the symposium in 1968 Page expected similar objections in 1969. As early as January he told Sagan that the best way to avoid a confrontation would be to have three panels to insure that McDonald would not appear with Menzel and Condon. In April the situation became so tense that Sagan suggested to Page that Condon and McDonald should be scheduled on separate days. In September after Condon became infuriated over the rumor that he would take part, when in fact he remained uninvited, Page told both Roberts and Hynek to keep the panelists names quiet until they formally accepted. To insure this Page sent out tentative programs to potential speakers with the panelists names excised.

When Condon said he would not speak several efforts were made during September to persuade him to do so. Roberts, knowing Condon's opposition to the allocation of school time for pseudoscience, told him that many school teachers would attend the symposium which would

provide a good opportunity to educate them. Page used a different approach. He told Condon they wanted to hold the symposium to correct the public misconceptions which the Colorado study unearthed. Sagan also asked Condon to reconsider and in the same letter explained his

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rationale for the benefit of the AAAS Board. He claimed it was necessary to counter borderland science notions with the scientific method because the physical sciences lost too many good students to the study of the bizarre.

Sagan, Page and Roberts used a similar strategy when Menzel returned from Europe to find the symposium planning in full swing. Throughout November Roberts, Page and Sagan worked on him. Roberts told him everything possible would be done to make his appearance easier. Because Page feared Menzel might have a heart attack he told him not to get upset over the planning. Sagan evinced the same concern, but feared the attack would occur during the symposium. He asked Roberts how they might cool Menzel off. When Menzel charged the panel with pro-ETH sympathies Page explained that six or so members would oppose McDonald. Finally, Sagan took Menzel's advice with respect to publicity and informed McDonald that the AAAS, rather than the participants, would arrange for it.

SUMMARY OF TACTICS

Organizers

Sagan

1. suggested scheduling McDonald on different days than Menzel and Condon
2. told Condon that the symposium was an opportunity to counter a borderland science notion with the scientific method
3. took Menzel's advice to have the AAAS handle all publicity

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Page

1. suggested putting McDonald on different panels than Menzel and Condon
2. urged Hynek and Roberts to keep the panelists names a secret
3. excised panelists names from tentative programs sent to prospective speakers
4. told Condon that the symposium would correct public misconceptions about UFOs

unearthed by the Condon Report

Roberts

1. told Condon that many school teachers would attend the symposium
2. assured Menzel that everything would be done to make his appearance easier

Obstructionists

Menzel

1. drafted 1968 protest letter to *Science*
2. said he would not appear with crackpots
3. suggested cancellation if a one-sided program were not possible
4. said Condon might appear in a one-sided program
5. told Roberts he would protest through formal AAAS channels
6. asked for speaking time equivalent to McDonald and Hynek combined or he would not appear

Condon

1. threatened not to attend any AAAS functions if the symposium were held
2. ridiculed the AAAS Board for the proposed symposium
3. got Hoagland and Shane to appeal to the AAAS Board
4. sent his letter along with Shane and Hoagland's protests to prospective symposium participants and elder statesmen of science

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FOOTNOTES

1. Page to Wolfle, 4/8/68, cc. W.G. Berl, Hynek, McDonald, Sagan, Whipple, Hynek Letters. - [Back To Text](#)
2. Page to Odishaw, 4/25/68, Sagan Letters, hereafter S.L. - [Back To Text](#)
3. This correspondence is available in the personal papers of Donald Menzel at the American Philosophical Society Library in Philadelphia. Although I read through the material I did not take notes because I assumed that I had Xeroxing privileges. A month after leaving Philadelphia under the impression that the Library would forward the material I had designated for copying I was informed that Menzel had refused me the Xeroxing privilege. The remarks which appear in the text are based on my recollections. - [Back To Text](#)
4. Sagan to McDonald, 9/16/68, McDonald Letters. - [Back To Text](#)
5. Menzel to Page, 1/27/69, S.L. - [Back To Text](#)

6. Page to Sagan, 1/30/69, S.L. - [Back To Text](#)
7. Page to Sagan, 4/18/69, S.L. - [Back To Text](#)
8. Sagan to Page, 4/25/69, S.L. - [Back To Text](#)
9. Morrison to Page, 6/2/69, S.L. - [Back To Text](#)
10. Page to Morrison, 6/6/69, S.L. - [Back To Text](#)
11. Roberts to Page, 6/11/69, S.L. It is probably safe to say that the same approach was used by Sagan at the 1974 AAAS Symposium devoted to the ideas of Emmanuel Velikovsky. - [Back To Text](#)
12. Page to Sagan, 8/10/69, S.L. - [Back To Text](#)
13. Sagan to Page, 8/28/69, cc. Roberts and Morrison, S.L. - [Back To Text](#)
14. Condon to Roberts, 9/5/69, cc. the AAAS Board, McDonald Letters. - [Back To Text](#)
15. Hoagland to Roberts, 9/8/69, cc. Condon, Wolfle, Berl at the AAAS, S.L. - [Back To Text](#)
16. Shane to Roberts, 9/12/69, cc. Condon, S.L. - [Back To Text](#)
17. Page to Roberts, 9/13/69, S.L. - [Back To Text](#)
18. Page to Hynek, 9/13/69, S.L. - [Back To Text](#)
19. Page to Sagan, 9/13/69, S.L. - [Back To Text](#)

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20. Roberts to Condon, 9/15/69, cc. Page, Morrison, Sagan and Wolfle, S.L. - [Back To Text](#)
 21. Page to Condon, 9/15/69, S.L. - [Back To Text](#)
 22. Condon to Page, 9/19/69, S.L. - [Back To Text](#)
 23. McCrosky to Sagan, 9/23/69, cc. Condon, Morrison, Page and Roberts, S.L. - [Back To Text](#)
 24. Page to Wolfle, 9/23/69, cc. Roberts, Morrison, Sagan and Hynek, S.L. - [Back To Text](#)
 25. Sagan to Condon, 9/24/69, cc. Page, Morrison and Roberts, S.L. - [Back To Text](#)
 26. O'Brien to Sagan, 9/26/69, cc. Page, S.L. - [Back To Text](#)
 27. Roberts to Page, 9/28/69, cc. Sagan, Morrison, Wolfle, and Bentley Glass, S.L. - [Back To Text](#)
 28. Page to Morrison, Roberts and Sagan, 9/29/69, S.L. - [Back To Text](#)
 29. Atlas to Sagan, 9/29/69, cc. Condon, Roberts and Page, S.L. - [Back To Text](#)
 30. Sagan to Roberts, Page, Morrison, Condon and the AAAS Board, 9/29/69, S.L. - [Back To Text](#)
 31. Condon to Sagan, 10/6/69, S.L. - [Back To Text](#)
 32. Menzel to Page, 10/6/69, S.L. - [Back To Text](#)
 33. Menzel to Roberts, 10/7/69, cc. Page, Sagan, Morrison and Wolfle, S.L. - [Back To Text](#)
 34. Menzel to Roberts, 10/8/69, cc. AAAS Board and a blind carbon to Sagan, S.L. - [Back To Text](#)
 35. Page to Condon, 10/8/69, S.L. never sent. - [Back To Text](#)
 36. Hynek to Page, 10/11/69, S.L. - [Back To Text](#)
 37. Page to Hynek, 10/15/69, S.L. - [Back To Text](#)
 38. Hynek to Sagan, 10/27/69, S.L. - [Back To Text](#)
 39. Hynek to Page, 10/29/69, S.L. - [Back To Text](#)
 40. Colonel Lawrence Tacker was in charge of Blue Book and wrote *Flying Saucers and the United States Air Force*, Princeton, N.J., Van Nostrand, 1960. Walter Sullivan, the *NYT*'s social editor*, wrote the introduction to the Condon Report. Handley Cantril, a social psychologist, wrote *The Invasion From Mars: A Study in the Psychology of Panic*, Princeton University Press, 1940. A study of the Orson Wells broadcast of 1939. The work of Klass and Condon has been cited, while

William Markowitz wrote "The Physics and Metaphysics of Unidentified Flying Objects," *Science*. September 15, 1967, pp. 1274-79. - [Back To Text](#)

*Editor's Note: Sullivan was actually the science editor of the *Times*; he was appointed to that position in 1964.

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41. Menzel to Page, 10/29/69, S.L. - [Back To Text](#)
42. Menzel to Roberts, 10/30/69, cc. Condon, Markowitz, Klass, Shane, Page, Sagan, Wolfle, Boyd, S.L. - [Back To Text](#)
43. Roberts to Menzel, 11/3769, S.L. - [Back To Text](#)
44. Condon to Page, 11/7/69, S.L. - [Back To Text](#)
45. Sagan to Roberts, 11/12/69, S.L. - [Back To Text](#)
46. Menzel to Page, 11/17/69, S.L. - [Back To Text](#)
47. Hynek to Sagan, 11/18/69, S.L. - [Back To Text](#)
48. Page to Klass, 11/18/69, S.L. - [Back To Text](#)
49. Page to Kocher, 11/18/69, S.L. - [Back To Text](#)
50. Klass to Page, 11/22/69, cc. Athelstan Spilhaus, Roberts, Morrison, Sagan, Menzel, Condon, Markowitz and Quintanilla, S.L. - [Back To Text](#)
51. Page to Menzel, 11/22/69, S.L. - [Back To Text](#)
52. Sagan to McDonald, 11/24/69, McDonald Letters. - [Back To Text](#)
53. Page to Klass, 11/24/69, cc. Spilhaus, Roberts, Morrison, Sagan, Menzel, Condon, Markowitz, Quintanilla, S.L. - [Back To Text](#)
54. Klass to Page, 12/5/69, cc. Spilhaus, Roberts, Morrison, Sagan, Menzel, Condon, Markowitz, Quintanilla and Baker, S.L. - [Back To Text](#)
55. McDonald to UPI, 12/28/69, McDonald Letters. - [Back To Text](#)
56. Page to Seamans, 12/29/69, S.L. - [Back To Text](#)
57. Menzel to Page, 1/9/70, S.L. - [Back To Text](#)
58. Carl Sagan and Thornton Page (eds.) *UFOs: A Scientific Debate*, Ithaca, New York, Cornell University Press, 1973. - [Back To Text](#)

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VII. CONCLUSIONS

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Before I suggest what the McDonald experience implies it might be well to mention the limitations inherent in an attempt to generalize from this case study. First of all we must keep in mind that it is just that -- a case study. Consequently, the possibility that it is an aberrant example of the scientific process is ever present. In addition, while it may not represent normal, scientific activity one could argue that it is typical of borderland science. Yet even that position is assailable on the grounds that we are still dealing with a single case.

One means of coping with this question, at least on a tentative basis, is to treat it in a "what if?" fashion. That is, the case study material exists and we would like to draw conclusions from it even though such an undertaking is laden with risks. Therefore, we can make an assumption for heuristic purposes; what if the McDonald case study is representative of the borderland scientific process? From there it is possible to proceed with the discussion while being fully aware of its limitations.

In the first chapter I claimed that the scientific process is, at least in the case of a potentially anomalous phenomenon, a political process. The basic hypothesis was that in order to function within the scientific community, i.e., to do science, the scientist must do more than apply the scientific method to his subject matter. It is necessary for him to engage in other activities which are not associated with our traditional conceptions of the scientific endeavor. It was further argued that such behaviors are political and in conjunction with the scientific method compose what I defined as the scientific process.

This conception views the scientist not as a passive seeker after truth in the groves of academe, but rather as an active advocate doing whatever he perceives as necessary to foster his research. In chapter two the case for this argument received documentation through a detailed look at McDonald's first year of active involvement in the UFO controversy.

Chapters three, four, five and six were not essential to demonstrating the hypothesis, but were useful in presenting further insight into borderland scientific activity. Moreover, they helped me to develop my ideas with respect to the borderland science endeavor and by implication its counterpart normal science. What follows, then, is a speculative discussion of both borderland and normal science activity with the emphasis on the former. It is based upon data from the McDonald case study, the research of other social scientists and intuitive feelings about the nature of the scientific enterprise which developed as a result of undertaking this research.

TYPES OF SCIENTISTS

Let us begin by assuming that not all scientists are equally political. For purposes of discussion they can be differentiated on the basis of the amount of political behavior they engage in, the issues they study, and the political tactics they use. This will enable us to talk about different types of scientists, issues, and tactics. Although this conceptual breakdown is lacking in precise operational determinants, it nonetheless is useful in taking an initial look at the phenomenon I am calling the personal politics of science.

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It is assumed that all practicing scientists are political and that the apolitical scientist is a myth. This does not mean that all scientists are as political as McDonald, but it does imply that each in his own way initiates behaviors which are not part of the scientific method and yet are intended to further the scientists' research activities. If we are to accept the apolitical scientist concept we must believe that scientists exist who do not consider the social implications of their research and do nothing to foster their own professional interests except their work -- trusting solely in the community of scholars to reward them on the basis of merit. Because this entire line of reasoning appears counterintuitive there is no further discussion of such hypothetical individuals here.

TYPE I

However, three different types of scientists are suggested. The first type engages in average amounts of political behavior. That is, he is the normal scientist who does not attempt to wheel and deal in his discipline or pursue revolutionary breakthroughs. [\[1\]](#) He does his research on

normal issues and where necessary employs normal political tactics to achieve his ends.

TYPE II

The second type of scientist takes part in above-average amounts of political behavior. He is one of the prolific members of his discipline and/or a scientific statesman. The former requires that he always has a book or an article "in press" and the latter that he sits on and organizes associational panels in his discipline and functions on the editorial boards of journals. In either case he is constantly tending

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to his own upwardly mobile interests within the scientific community. This individual gravitates toward "fashionable" topics of research that exist on the periphery of paradigms but which do not threaten the assumptions of the paradigms themselves. In so doing he utilizes considerably more in the way of normal political tactics to achieve his ends than our Type I scientist.

Within this category there is a subgroup which because of my value orientations I will call the "reactionary extremists." They are successful Type II scientists who take it upon themselves to use extreme tactics to do battle with Type III scientists over potentially revolutionary issues.

TYPE III

The Type III scientist, "the progressive extremist," unable to obtain satisfaction through labor in the vineyards of "normal science," is attracted to potentially revolutionary research areas. He focuses an enormous amount of political behavior on these topics and does not hesitate to bring extreme tactics into play. For the sake of a breakthrough he will venture to the borderlands of science in the hope of returning with a new view of reality.

The scientists of both polar persuasions, then, share several characteristics which seem aberrant and justify the label of extremist. Both the "progressive" and the "reactionary" are attracted to borderland areas of research. The former as an active iconoclast and the latter as an upholder of authority. Each in his own way exhibits traits which Rokeach has called dogmatic.^[2] Lastly, both groups are willing to substitute political tactics for the process of verification.

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It should be clear that this is a first tentative attempt at mapping part of the personal politics of science domain. As a result the rigor which is desirable in such an endeavor is not always

present. Nevertheless, some of the terms are amenable to definition now and others may yield to more precise delimitation in the future.

ISSUES

Issues are either normal, fashionable normal, or revolutionary. By "normal" I mean that in Kuhn's sense it is just another puzzle in whatever discipline it occurs. It isn't considered "exciting" as perhaps it once was and consequently popular magazines do not feature articles about the men who work in the area. It is respectable, but it is the sort of thing that most scientists are engaged in. On the other hand, "fashionable normal" issues are those which are studied by the big names of science. They usually require vast funding, exist on the periphery of paradigms, and because they seem to promise the opening of new vistas of understanding they exude excitement. Most academics are at least vaguely aware the research is underway and even the man on the street may know of its existence through the coverage provided by the news media. The revolutionary issue differs from the above issues in two ways. First, it has no status as a priority problem in the scientific community, and, second, and more importantly, it is neither directly within nor on the periphery of an extant paradigm, and therefore not verifiable in traditional terms. As a result very few scientists are attracted to such problems.

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TACTICS

The tactics of political persuasion are either normal or extreme. Normal tactics are any and all behaviors used by scientists to further their research ends as long as they are not brought into play as a substitute for verification. Extreme tactics, to the contrary, are used for purposes of political persuasion either by progressive or reactionary extremists. Such tactics serve as surrogates for verification in borderland science areas where mutually acceptable criteria of verifiability are lacking, i.e., where the contending groups each adhere to their own mutually exclusive criteria of acceptance. The exhibited behaviors, then, can be identical in the normal or revolutionary context. What determines if they are normal or extreme tactics is whether or not they are intended as surrogates for the verification process.

A word of caution is in order at this juncture because it is tempting to conceive of an extreme tactic, especially when comparing tactics, in terms of the degree to which it appears to violate the canons of science. For instance, which is more extreme, fraud which is the product of falsifying results, or interfering in a fellow scientist's research project? The answer is that this is not a question which is addressed by my usage of extreme. An extreme tactic is merely a tactic employed by extremists of either caste to circumvent the accepted verification process in a

potentially revolutionary context. Therefore, both "fraud" and "interference" could be normal or extreme tactics depending on the intent and the context.

It would seem that empirical inquiry into what scientists do would lead to more or less precise indicators of average and above-average amounts of political behavior, however, at this time the work has not

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been done. Moreover, the scientific types alluded to are by no means always pure. It may be that a given scientist's type varies according to the issue, or that in some cases as a scientist advances in age and security he evolves from a Type I to a Type III or vice-versa. Those kinds of questions await further research. What should be obvious, however, is that McDonald was a Type II who became a Type III when he began to pursue the UFO phenomenon in 1966. At the same time he took his first cautious steps into the area known as borderland science.

BORDERLAND SCIENCE

Up to this point the notion of borderland science has been used rather loosely. The term is meant to connote the practice of both science and pseudoscience in areas that are so nebulous that the distinction between the two undertakings is blurred. The practitioners of borderland endeavors consider themselves members of the avant garde in whatever field they work. They believe that they are doing good science which is ignored and/or that they are unable to properly engage the phenomenon of their choice because of various socio/political/economic reasons which put that particular domain of investigation beyond the pale.

To exist beyond the pale for our purposes means that such areas do not fall neatly into an extant paradigm. In fact, their assumptions may violate knowledge claims which are revered in accepted disciplines. As a result borderland science subjects are generally not taken seriously by the vast majority of the scientific community. If they were, they could not longer be considered borderland. At any rate, such subjects fall into disrepute and among the elder statesmen of science are labeled pseudoscience.

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The problem is now becoming apparent as we see that both intellectual extremes are able to take advantage of the situation. For instance, anyone who pursues a borderland interest and experiences a poor reception by his peers is able to claim that this is the plight of all men who are before their time. Using this argument pleas of harassment, along with demands on available

resources such as funding, journal space, and peer recognition are possible. Conversely, there are individuals within science who point to their favorite examples of pseudoscience and argue that all borderland activity is of a similar nature. They then proceed to argue either directly or indirectly that such issues, therefore, do not deserve a hearing before the scientific community. Of course, not all scientists who pursue "so called" bizarre interests are on the verge of a revolutionary breakthrough, nor are all of them engaged in worthless research. The question is how to discriminate between worthwhile and worthless work.

If the controversial issue rested squarely within an accepted paradigm then the dispute would be readily justiciable, for agreed upon criteria of verification would exist among all those concerned. However, one faction always claims that a particular matter is anomalous, i.e., falling outside of extant paradigms, while the other asserts that the issue is readily explainable in terms of one paradigm or another. This makes a mutually acceptable solution impossible.

The disagreement, then, results from the contending parties approaching the problem with differing assumptions about the data. As long as this remains true they are not able to obtain the same conclusions, but, rather, dispute assumptions and largely talk by one another. This politicizes the verification process because it is no

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longer a question of intersubjectively verifiable criteria, i.e., scientific persuasion, to determine the acceptance or rejection of the hypothesis, but a question of political persuasion. How many scientists, which funding agencies, etc., can be convinced that research on the problem should or should not proceed?

Of course, while this describes the problem it does not go far enough. For the demand on the part of the paradigm defenders that the paradigm shatters meet the formers' criteria of verification is a convenient means of disposing of the problem but is in fact a straw man. This is true because practicing scientists are well aware of the fact that the degree of rigor demanded for evidence claims varies both within and between established disciplines and yet work goes on. This points up what is the *sine qua non* for an ongoing research area; it is political power herein defined as a constituency of well-funded scientists sharing the same world view.

THE McDONALD EXPERIENCE

McDonald did research, but recognized that what was possible with his limited funds was not adequate for a convincing demonstration of the anomalous nature of UFO data to his scientific peers, i.e., meeting the verifiability criteria of the old paradigms. Therefore, he began a political campaign to shift the conventional paradigms to make room for the legitimate study of the UFO

phenomenon with specific reference to the extraterrestrial hypothesis.

BORDERLAND ENTRY

What, if we assume the McDonald experience is representative, are we able to say about the personal politics of science of the borderland science situation? It would appear that entry into the field is a slow

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process for seasoned scientists. Because they serve their graduate student apprenticeships learning how to do non-revolutionary research few scientists, possibly none, are really out to shift paradigms until some critical event in their lives. In McDonald's case a UFO observation in the Arizona desert in the early 1950s probably planted the seed of discontent. Yet, even first-hand experience did not result in immediate pursuit of the subject. Rather than such a bold course he embarked on a low-key investigatory effort in the Tucson area which lasted some ten years. When he interacted with NICAP in those early years he demanded and obtained assurances that his name would not be used. Finally, in 1966 when he decided to launch a one-man study through the NAS, Congress or the AFOSR he wanted to do it quietly. It was only after he visited Wright-Patterson Air Force Base and reached his ETH conclusions that the phenomenon seemed to develop a dynamic of its own which, with each passing month, drew him further toward the center of the controversy.

On the basis of this information we can infer that while most scientists never venture into the borderland, those who do so, do it gradually. They received training and pursued careers in which the byword is caution. Therefore, when confronted with the potentially anomalous, phenomena which according to accepted world views shouldn't exist, it is only to be expected that even those scientists considered reckless in judgment by their colleagues appear conservative to the layman.

There are also a number of socio-political reasons for this circumspection. It is not true of all borderland subjects, but is true in the case of the UFO phenomenon, that no one is academically trained to come to grips with the multi-faceted data. Consequently, to attempt

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to do so is an extremely time-consuming interdisciplinary task which discourages most investigators.

In addition, scientists are accustomed to research funding. Yet, borderland topics are not usually within recognized areas of university research so it is difficult to imagine where to submit a proposal. But should submission of an application occur, as a result of the disrepute in which the scientific community holds the subject, it has a minimal chance of success. This is because it is not the sort of research which a funding agency desires to justify to its board of directors or endowing body as an example of its philanthropy. Or in the instance of a federal agency, proposals are usually refereed by outstanding scientists who have their own conceptions of real world priorities which don't include the peripheral topics of borderland science. So this leaves the scientist the choice of absorbing the research expenses himself, siphoning funds from another grant as McDonald did, or abandoning the field. It is not surprising that most opt for the latter alternative.

The question of career advancement also faces every scientist. In academic institutions this means a scientist must concern himself with tenure and promotion. However, tenure and promotion generally depend on a mixture of publication and social acceptance in one's department. Unfortunately, there is seldom a respected journal which will publish borderland material and since such areas are often considered pseudoscience to one's colleagues, social acceptance may suffer (some of McDonald's peers worried that his work would hurt the reputation of the UA) by their pursuit. Consequently, there is no incentive to focus on borderland research within the work situation. In fact, the work situation rewards those who adhere to the tried and true, not those who

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deviate from it. This leaves several possibilities. The scientist can undertake the research "out of pocket" as a side interest, putting career-oriented work first. He can give it first priority with the likely loss of his job or at least non-promotion. Alternatively, as in McDonald's case, the scientist can put the work aside until he obtains his full professorship and an impressive file of publications. Or, finally, and not surprisingly under the circumstances, abandonment of the work may occur. Therefore, for most scientists avoiding the borderland is probably less of a conscious choice than a course of action necessitated by the career constraints of the academic environment.

It would seem, therefore, that socio-political problems associated with career advancement and the funding of bizarre research tend to insure that few scientists entertain thoughts of unorthodox work and still fewer act upon them. However, once a scientist does act, what sort of concerns become paramount for him? Of first importance is credibility.

CREDIBILITY

Borderland research topics carry with them a stigma. This often takes the form of a cultist fringe which is associated with them in one way or another and may include "fast buck artists" who know a bizarre subject, if presented properly, has considerable appeal to the gullible. The serious scientist who concludes that within the noise of such a phenomenon there also exists a signal must in some manner dissociate himself from the unsavory aspects of the subject.

For instance, beginning in the early 1950s the UFO issue acquired religious and space-brother overtones fostered by various individuals who claimed contact with extraterrestrials. This became an important

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factor in keeping the problem relegated to the status of a matter worthy of ridicule but not serious research. McDonald recognized this as an issue which could reduce his effectiveness and took a number of measures to increase his credibility.

Initially he didn't want his name used even by NICAP, which by UFO organization standards is conservative. Because APRO accepted occupant reports, while at the beginning he did not, McDonald avoided APRO even though APRO headquarters was in Tucson. Since he viewed the Air Force Blue Book project as responsible for much of the disrepute associated with UFOs he set out to refute its conclusions. After accomplishing this, to his own satisfaction, he publicly attacked the Air Force in his talks. Even though he became in great demand as a speaker, however, he would only address groups in which he felt some further scientific light might be thrown on the subject or scientific converts made. In other words he did not want to be known as "the after-dinner UFO speaker" for the Kiwanis, Rotary and other civic organizations. In his presentations, which were largely to scientists and engineers, he used carefully selected language to avoid appearing sensational. This is epitomized by his reference to the ETH as "the least unsatisfactory hypothesis." In the same presentations he threw barbs at those elements who he considered part of the cultist fringe. Moreover, his recognition of the interdisciplinary aspects of the UFO problem compelled him to get the views of specialists in as many social, physical and life science departments as were willing to hear him speak. This approach saved him a great deal of research and increased his credibility. It allowed him to make statements such as "Dr. X in psychology, an authority on mass hysteria, told me only last month that UFO reports could not be explained

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by the mass hysteria hypothesis." Lastly, as McDonald became more deeply involved in the UFO controversy he began to refute the work of Donald Menzel, Philip Klass and eventually the Condon Report.

Through this activity McDonald intended to increase both his credibility and the credibility of UFOs as a legitimate topic of scientific inquiry. He avoided the sensational wherever possible in order not to frighten the scientists he wanted to convert, yet to some degree he found himself trapped in a paradox. For the more mundane the problem became due to his use of various circumlocutions designed to increase the scientific palatability of the subject, the weaker the case became for researching it. On the other hand, if McDonald's version of the truth were told both his credibility and that of the issue would suffer, while the number of conversions would decrease. We can assume, then, that establishing credibility is a primary concern of the borderland science practitioner. Probably the concern varies directly with the degree of bizarreness associated with the research area. Although one's credibility in the practice of normal science is virtually taken for granted, when scientific matters become a question of political persuasion (outside of the traditional framework of mutually acceptable verification criteria) credibility must be carefully nurtured and augmented whenever possible. ^[3]

GAINING ACCESS

Also of concern is gaining access to those decision-makers who can legitimate the study of the borderland area in question by channeling research funds to it. Therefore, interest articulation efforts are necessary to convince the decision-makers that funding should take place.

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This approach is essential because borderland subjects are not respectable areas of study and as a result none of the traditional avenues for the acquisition of funds are open.

Since it is a question of political persuasion all means which would appear to positively contribute to the goal of legitimacy may come into play. Actual tactics would seem to be a function of the temerity of the researcher. In the case of McDonald he approached leaders in the governmental, military and scientific communities. Moreover, he eventually staged a "coming out," began a speaking campaign which took him the length and breadth of the country, meddled in the Colorado Project finally resulting in his masterminding of the *Look* expose article, appealed to the NAS and proved instrumental in organizing the Roush Hearings.

THE SCHIZOID SITUATION

This illustrates the schizoid character of borderland science behavior. For it is obvious that McDonald considered the UFO question a matter of political persuasion; of converting as many individuals as possible and hoping that the weight of numbers would win the day. Yet it seems he never quite understood, or could accept, that for most disinterested decision-makers conversion would only take place if they could envision some benefit to themselves greater than the costs which might accrue. So, on the one hand, to build a constituency he took his case to the scientific community and the public through speaking engagements, informal communications and various forms of publicity, and on the other hand he besieged decision-makers, even going so far as to present his concern with the Condon Study to the NAS. In viewing these

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behaviors it would appear that they are open to two interpretations. Conceivably McDonald wanted to campaign on as many fronts as possible, thinking that it would increase his probability of success. Alternatively, he believed (beneath his cynical demeanor) that reason would win out among the rational elder statesmen of science, government and the military. Admittedly both interpretations could be correct. McDonald, the seasoned scientist, considered the strategy of attacking on many fronts a good one. But despite his political sophistication he also maintained a streak of idealism, and undoubtedly this idealism made him, in the last analysis, hold out the hope that individuals trained in the rites of rational discourse would draw similar conclusions to his own. In fact, it may be that those scientists who work in borderland areas (Type III) tend to be those individuals who have deeply internalized the rational ideal, while Type II scientists have not done so and consequently remain aloof from research which could tarnish their reputations and/or careers. ^[4] The Type III scientist, then, may believe strongly in scientific ideals, but nevertheless engage in considerable political activity. The difference in this instance between the Type II and Type III is that the former is more pragmatic and realistic than the latter. While the former weighs each research decision in terms of its costs and benefits to his career, the latter tends to engage an interesting issue regardless of the personal repercussions it might have.

There is another reason for the development of what I want to call the schizoid situation. It is the extreme frustration which results from the study of a borderland research area. The borderland scientist is frequently under a state of siege. He is often attacking or being attacked, winning a skirmish now and then, but never winning the battle.

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Consequently, when the prognosis is grim appeals to otherwise hostile appearing sources of authority should come as no surprise.

This occurred in McDonald's case when he appealed to the NAS to examine what he considered the incompetence of the Colorado Project. The situation appeared acute for a number of reasons. Those familiar with the problem viewed the Condon Study as the critical experiment with respect to the UFO phenomenon. McDonald felt that twentieth century scientific priorities would undergo a complete reshuffling if a favorable outcome resulted. Moreover, if this occurred it would provide an exciting new area of well-funded research for him. But if the Colorado results proved negative then the field of UFO studies would be set back decades and surely not experience a resurgence prior to the termination of his productive years of scholarship. To further exacerbate matters the whole UFO controversy existed within a shroud of unproved, but potentially real national security problems, the implications of which were unknown, yet subject to widespread speculation with regard to the Condon Study. It is not surprising, then, that after numerous attempts to aid, guide and cajole Condon, McDonald went directly to the NAS where he somehow thought that redress could take place.

THE CRITICAL EXPERIMENT

This episode in the controversy also underlines the importance of a critical experiment in a borderland area. In addition, it illustrates the unusual treatment which a borderland phenomenon may receive. For once the critical experiment takes on formidable dimensions with far ranging implications, it is monitored by extremists of many colors, thus promoting the development of a circus-like atmosphere. Furthermore, it

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is possible that the experiment can, as in the Colorado case, carry with it the implication that it will remove a nuisance problem from someone's shoulders. In this instance from the purview of the Air Force, which imposed an unusual restriction on the research. Because the Air Force instinctively recognized the outcome would largely be a question of the predispositions of the project staff, it stipulated in the contract that scientists previously associated with the topic could not take part in the study, surely a most unusual manner in which to conduct an analysis. This tactic virtually insured that the staff would consider UFOs a nonsense problem. In all likelihood this is an atypical example of the critical experiment due to the Air Force involvement. However, it does indicate the degree to which violation of the accepted canons of science may occur if the borderland area in question is of sufficient import. Of course, violation in this instance proved a

two-edged sword. For while the Air Force made certain that it would obtain conclusions compatible with its public position, McDonald busied himself meddling in the Colorado Project hoping to thwart an outcome unfavorable to his public position.

THE EXTREMIST CONFRONTATION

The ONR controversy exemplifies another aspect of borderland science; the confrontation of two intellectual extremists. If it were possible to array the intellectual predilections of scientists in a frequency distribution I think it would be safe to conjecture that one or two percent, the Type III scientists, would fall in one tail of the distribution. As indicated previously they gravitate toward the study of the unusual and it is for that reason that I label them the "progressive extremists."

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However, there are also extremists in the other tail of the distribution. They expend considerable time and energy attacking Type III scientists. These "reactionary extremists" consist of a small number of Type II scientists who become incensed over borderland issues. The reactionary extremists assert that they wish to protect both the public and the scientific community from pseudo-science and its kook practitioners. To make their point they cite what they postulate to be typical examples of belief systems which were intellectual fads of the past, i.e., elves, black magic, astrology, witches, etc., and claim that the borderland subjects which they indict fall into the same category. For example, astronomer Donald Menzel states: [\[5\]](#)

The nature myths of the ancient Greeks gave way to beliefs in demons, evil spirits, the devil incarnate, witches, wizards, ogres, ghouls, harpies, fairies, fire drakes, werewolves, goblins, specters, wills-o'-the-wisp, ghosts, banshees, nymphs, elves, mermaids, leprechauns, minotaurs, centaurs, satyrs, cyclops, unicorns, and chimeras, to mention just a few. The belief in the existence of such creatures was by no means evanescent. History is full of serious claims that human beings have seen or encountered such things.

While physicist William Markowitz concludes: [\[6\]](#)

We have been reminded (by Hynek) that 21st-century science will look back on us. This is true. We, ourselves, look back on eras when people believed in the existence of centaurs, mermaids, and fire-breathing dragons. I am afraid that 21st-century science will contemplate with wonder the fact that, in an age of science such as ours, the United States Air Force was required to sponsor repeated studies of UFOs.

And physicist Edward Condon claims: [\[7\]](#)

Flying saucers and astrology are not the only pseudo-sciences which have a considerable following amongst us. There used to be spiritualism; there continues to be extrasensory perception, and psychokinesis, and a host of others Perhaps we need a National Magic Agency to make a large and expensive study of all these matters, including the future scientific study of UFOs, if any.

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These individuals are the defenders of orthodoxy as much as the Type III scientists are the defenders of the unorthodox.

Although he is not a professional scientist we must give Philip Klass his due in this area. The ONR controversy illustrates what can occur when the reactionary extremist confronts the progressive extremist. It is the type of altercation that occurs in the borderland because emotions run high among the combatants. I would surmise that this argumentation becomes so emotionally charged because there are no mutually acceptable criteria of verifiability to discuss. Both the reactionary and the progressive, therefore, experience great frustration as a result of their respective inabilities to convince the other of his obviously incorrect belief. The outcome is a no-holds-barred struggle in which the personal politics of science, not the scientific method, is paramount.

THE RESPONSE OF ESTABLISHED SCIENCE

In all borderland areas the conviction of practitioners is that they have evidence for their beliefs. If only organized science would examine the data, which it won't, they argue that they would obtain vindication. ^[8] The preparations for the 1969 AAAS UFO Symposium provide an excellent opportunity to observe the treatment accorded a borderland belief by organized science. It is probably not unreasonable to assume other bizarre beliefs would receive similar treatment as witnessed by the 1974 Velikovsky AAAS Symposium which Carl Sagan also planned.

The UFO Symposium illuminates two kinds of resistance to a new paradigm in science -- the direct and the indirect. Menzel and Condon represent the former, while Sagan and Page represent the latter. Menzel and Condon are examples of successful Type II scientists who are

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reactionary extremists on some issues and make no attempt to veil their position. To the contrary, Sagan and Page are prototypes of successful Type II scientists who are reactionary extremists on

some issues but who disguise their intentions by publicly acting in the name of the rational model of science. In this instance providing a platform for the discussion of unorthodox views.

Both of the above twosomes wanted to put an end to what they considered a modern myth, but their approaches differed. Menzel and Condon, although considered major parties to the UFO debate in 1969, wanted to stop the Symposium and went about it in a direct fashion by letter-writing campaigns and word-of-mouth. They exhibited no desire to thrash out the issues in open debate, in fact they felt that such an airing could only lend an aura of AAAS legitimacy to the UFO phenomenon.

On the other hand, Sagan and Page did not worry about legitimating UFO studies through the Symposium for they had a very different purpose in mind. In good liberal form they claimed they wanted to present the pros and cons of a current topic of public interest. Actually they intended to expose what they considered an untenable belief system to hard-nosed scientific method and by so doing destroy one of the pseudoscientific subjects they believed jaded the attitudes of college students toward the physical sciences.

Condon and Menzel, therefore, did not want to expose the subject to the light of day, while Sagan and Page wanted to expose it, but with the intent of shattering the assumptions underlying it. As it turned out neither the skeptics nor the believers won the day at the Symposium. As usual, no agreement as to what constituted acceptable evidence claims existed amongst all the participants. Consequently, those who found

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UFOs a significant scientific problem were not convinced to the contrary and those who viewed the matter as another pseudo-science craze went home with their beliefs intact. Such, of course, proved a disheartening outcome for McDonald because it was in just the type of forum provided by the AAAS that he hoped the tide of scientific thought on UFOs would turn.

THE REVOLUTIONARY ANALOGY

There is something which emerges from this look at McDonald's attempt to obtain a paradigm shift. It is the temptation to develop an analogy. Arguing by analogy is always dangerous because of the amount of conceptual slippage which is possible. No two situations are ever alike with the subsequent result that an analogy often produces a seductive, but nevertheless misleading portrayal of the data. With this in mind I am going to proceed by drawing out an analogy which appears to furnish an interesting and hopefully useful means of looking at the strategy and tactics of the borderland science endeavor.

Therefore let me begin by saying that borderland science is an intensely political activity in which one group of scientists claims, on the basis of faith, that the extant view of scientific reality is adequate, while another smaller group asserts, also on the basis of faith, that some problems are not being solved because of the accepted world view and therefore it is time for a change. What is meant by this is that both groups use above-average amounts of political behavior to seek their respective ends. Each brings extreme tactics into play as surrogates for the verification process and both endorse mutually exclusive sets of criteria for adherence to their belief that the other is correct.

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The extant view of reality is considered adequate by the opponents of the new paradigm because it answers the questions which interest them and, through the years, provided the context within which they obtained the perquisites of the scientific profession; perquisites which might be redistributed if a new world view came to the fore. On the other hand, the extant view of reality is considered inadequate by the proponents of the new paradigm because it does not furnish a useful context for solving the problems which interest them and consequently denies them the rewards of the scientific profession. What ensues is, for all intents and purposes, a political campaign by the proponents of the paradigm which is not in power to bring about a revolution that will overthrow the paradigm which is, and possibly its proponents as well. Kuhn suggested just such an analogy, as pointed out in chapter one, however, he does not seek to flesh it out in explicitly political terms. ^[9] Although he draws the parallel between political institutions which are not capable of reform and paradigms which do not have the capacity for solving new problems, he fails to see that the analogy is capable of further extension for purposes of providing insight into the context within which a paradigm is actually shifted. Kuhn further recognizes that the process is one of conversion, in which persuasion plays the most important role, but he does not label it a political process or political persuasion.

How then might we extend the analogy? First the basic argument should be presented. It was mentioned above that just as political institutions may prove incapable of reform from within and consequently be overthrown from without, so too can paradigms undergo a similar experience; each in its own way because it cannot solve the problems

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with which it is presented. In both instances the term revolution is an appropriate term to describe the transition which occurs when the legitimate process for change is circumvented. In

the case of the political institution power is passed on through an overthrow of the government and not via constitutional means. In the case of a paradigm, it is replaced by converting potential adherents on the basis of a non-institutionalized act of faith rather than through an intersubjectively verifiable "act of faith demonstration." In other words, it is not a question of scientific method, but one of conviction.

THE INCUMBENT PARADIGM

Granted that the above is correct how might the McDonald experience help in throwing light on the conversion-resistance process which I want to call a revolutionary political process? Let us examine the advantages of the incumbent paradigm in light of what is known about the advantages of incumbent political leaders.

A political leader carries the mantle of legitimacy as a result of coming to power through the legally prescribed procedures of the state. This legitimacy is one of the reasons he commands the allegiance of his constituents. Although, as Kuhn points out, paradigms always come to power by way of revolution, they nevertheless acquire an air of legitimacy as textbooks are rewritten to suggest an orderly ascension to power and as those scientists who know the true story become fewer and fewer with the passing of time. Consequently, paradigms too command the same sort of allegiance as a political regime. This allegiance is enhanced as a political regime meets increasingly more of the demands of its constituents or at least in one way or another leaves the

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constituents with that impression. So too does allegiance to a paradigm grow as the number of explanations of heretofore insoluble problems increases.

Such an outcome should not be surprising. Just as politicians who owe their careers to a regime set the tone of the political issues which the party faithful confront, the elder statesmen of science, who made their names within the old paradigm, set the intellectual style for a particular era and by implication give support to the legitimate problems of the day. This is done in several ways. The equivalent of political largess in the scientific community is grant money. The elder statesmen have considerable control over who is funded through the refereeing of grant proposals and because their own work constitutes an indicator by example of what is priority research. ^[10] So, in the same way as lower-level politicians must fall into line in order to keep funds from the central government flowing back to their constituencies, lower-level scientists must do acceptable research if they want to obtain funding. Moreover, in the name of the dominant paradigms the elder statesmen of science control the editorial boards of scholarly journals. Consequently, it is very difficult to publish a study which attacks a dominant paradigm,

assuming the research is possible, without starting one's own journal. The phenomenon is similar to the immediate access the incumbent administration has to the news media while the rebels can only obtain coverage if they rape, pillage and loot.

REVOLUTIONARY TACTICS

This brings us to the tactics which the rebel scientist must employ to bring about his paradigm shift in the face of overwhelming odds.

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Granted, the case would be more impressive if McDonald succeeded. But that is not of great importance, for whether he was wrong or championed an idea whose time had not come, he nevertheless had experience in the personal politics of science. Therefore, regardless of the validity of the ends, we are probably safe in assuming the means are quite representative of those which would be employed by any Type III scientist in a similar situation.

Before enumerating specific tactics several distinctions are in order. Previously I mentioned that tactics are either normal or extreme. It should further be pointed out that both normal and extreme tactics are found in the revolutionary situation, but we would not expect extreme tactics in the normal science context. In other words, just because a revolutionary condition exists does not mean that only extreme tactics come into play, but it does mean that normal science situations only evoke normal tactics. The crucial thing to understand is at what point in both the normal and revolutionary scientific process that political tactics occur. In normal science political persuasion ends at the point that validation begins. Various tactics are used to further the scientist's interests, but only until it is time to validate. At that juncture the agreed-upon criteria of scientific persuasion in a given discipline are called up, and it is on those criteria that the scientist's work rises or falls. In the revolutionary instance, however, political persuasion occurs throughout the scientific process. This is because there are no agreed-upon criteria of verification between the opponents and proponents of the new paradigm. As a result the Type III scientist not only attempts to convince his audience of his findings, but also of his world view. It is in the latter instance where extreme tactics come into play.

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With the above in mind let's examine the McDonald case with an eye to his tactics. We can again begin to flesh out the revolutionary analogy presented by Kuhn. While McDonald did his research and satisfied his own demands for proof of extraterrestrial visitation he knew that his

evidence claims were not sufficient to convert the scientific community. So what did he do? He set out to make his case in person with as many leaders of the scientific, governmental and military communities as would listen to him. To do this he stumped the country to make conversions and get endorsements. If he could convert elite scientists he thought they might pry loose key grant money and/or force the government to look more closely at the problem. Congress might also lobby for a study or, as many hoped, conduct an investigation of the past efforts of the Air Force. In the case of the military, if they were actually in the dark and not conducting secret UFO research, conversions could possibly result in the mobilization of their well-funded research arms.

McDonald used the colloquium as a form of political rally. Between October 1966 and May 1969 he spoke on 96 separate occasions. In each instance he attacked some aspect of the conventional wisdom on UFOs in an attempt to foment discontent. To do this he undertook to show how prevailing paradigms did not explain UFO data and in the process tried to discredit the Air Force because of its staunch support for the incumbent world view. Therefore, the direction his efforts took was one intended to make the incumbent paradigms appear inadequate to the task of explaining the phenomenon. This is very reminiscent of the manner in which political revolutionaries try to convince the populace that the ruling regime is not solving the problems of the people.

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McDonald was not above extreme tactics, of which we have examined two. Rather than permit the Condon Project to run amuck he blatantly interfered with its ongoing research in order to influence the verification process. This even extended to the development of his own intelligence network within the project staff and the newsstand exposure of what he considered mismanagement of the research. The other extreme constellation of tactics were associated with the Roush Hearings. Instead of taking part as a mere invited participant, he organized the proceedings. In lieu of a balanced presentation he packed the symposium with advocates of his own position. Moreover, in laying the groundwork for the Hearing he orchestrated his own write-in appeal to key Congressmen. Although in the long run these measures failed, at the time McDonald implemented them it was with the intent of making a case through political techniques which could not be won in the methodological arena of science.

While much of his activity might be called interest articulation McDonald also did considerable image manipulation. For example, just as the political activist often avoids confronting the real issue in favor of something less extreme and more palatable to his audience, so too did McDonald shy away from the question of extraterrestrial visitation. Initially he didn't mention it, then he began to use the circumlocution "the least unsatisfactory hypothesis," but still evaded the

problem of craft occupants, and eventually he came to the point of publicly discussing reports of humanoid UFO occupants.

With his credibility in mind he also refused to do a National Lecture Bureau taping with Frank Edwards whose raconteur image left him uneasy. In addition, after his Tucson press experience he tried to

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insure through Dick Hall that only first-class press people would cover his October 1966 "coming out" in Washington.

These examples show McDonald's sensitivity to the fact that both what he said and how he appeared effected the number of converts which he would make in the scientific, governmental and military communities. He knew that most scientists were conservative and that the governmental and military types functioned within bureaucratic structures which encouraged conformist rather than innovative behavior. Consequently, he didn't want to take the chance of frightening away any of these individuals by appearing too strident in his remarks, as the result of inaccurate publicity, or due to his association with people having suspect reputations.

McDONALD AS REVOLUTIONARY

As a revolutionary McDonald would have to be labeled a failure since the revolution never took place. However, he acted at a time which, in retrospect, seemed ripe. The Air Force sponsored Condon Project gave an air of respectability to UFO studies which had never before, or since, been realized. Whether he carefully planned his entrance into the controversy to coincide with the new image of the phenomenon is unclear, but doubtful. What is clear is that the outcome of the Condon Study proved the turning point in McDonald's campaign. If Condon's conclusions had been positive UFO research, with McDonald at its forefront, would have experienced a vigorous period of growth. As it turned out, however, Condon's conclusions were negative and the weight of his prestige along with the seemingly official imprimatur of science proved more than McDonald could overcome.

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Analogies are seldom perfect when moving from one realm of discourse to another. However, in this instance I believe the degree of isomorphism is sufficiently great to justify the endeavor. Although this fleshing out of the revolutionary analogy is incomplete, it nevertheless indicates

the direction which I think fruitful thinking should take with respect to the problem of understanding paradigm shifts. For it would appear that as long as the scientific process at the level of the individual scientist is viewed as somehow transcending politics that we will never grasp, even in shadow form, the manner in which knowledge grows.

REVOLUTIONARY SCIENCE

What happens in the potentially revolutionary situation, then, is that both extreme groups have no choice but to abandon scientific method for purposes of making their respective cases. They rely exclusively on extreme tactics to obtain conversions, although they each claim that they are providing a scientific demonstration of their position. Actual scientific method can only come into play once the set of assumptions underlying the paradigm in question is accepted by all concerned, or at least goes publicly unchallenged by the elder statesmen of science. For instance, if the UFO phenomenon were considered a legitimate area of research then an entire literature resulting from the use of scientific method could spring up around UFO types, landing cases, the geographical distribution of reports, etc. But as long as the validity of the phenomenon itself is in question, the funding necessary for the above undertakings will not be forthcoming.

Probably what is important in such a situation is not the number of converts which is made, but the amount of political influence they wield.

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For the latter will determine if the area receives funding. And funding is in all likelihood the best indicator of legitimacy. This is because once a paradigm is well heeled and has its own journals within which to publish it is less open to attack even if critics exist who disagree with its underlying assumptions.

No doubt this is because legitimacy decreases the efficacy of such an attack to almost nil. Prior to obtaining legitimacy the paradigm shatterers need to respond to criticism because the burden of proof is essentially on their shoulders. They want the scarce resources of society to pursue their work and cannot afford to ignore detractors whose arguments might derail the revolution. Once established, however, a dialogue becomes unimportant to the victors who can ignore attack from without and in this manner extinguish such behavior on the part of those with whom they do not share assumptions.

Of course, this begins the cycle of knowledge growth all over again. Were the UFO phenomenon legitimated, for instance, and a successful research tradition developed around the subject-area over let us say a period of 25 years, then the former paradigm shatterers and their world view

would become an entrenched paradigm. The revolutionaries of today would in many instances become the reactionaries of tomorrow as "young upstarts," questioned the assumptions of the UFO paradigm by claiming that it constrained them in their desire to study a potentially anomalous phenomenon. The fact that this process has essentially escaped the inquiring eye of the political scientist suggests something about both science and political science. The former as an institution and ongoing method of acquiring knowledge has over the past 300 years attained a status very similar to that which was once reserved for the Catholic church.

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As a result of its unquestionable success and the lack of a history of science based upon first-hand experience, the chroniclers of scientific activity have tended to take the word of scientists themselves for how science is done. This, along with the writings of numerous twentieth century philosophers of science, has all but removed the human element from science and substituted for it a formalism focused on scientific method to the exclusion of the attendant social processes which I have called political. [\[11\]](#)

I think political scientists have by and large acquiesced to this for several reasons. Foremost is the fact that the personal politics of science is not within the traditionally accepted scope of the discipline. For instance, it often will have nothing to do with government. Consequently, since it could be argued that the prevailing world view in political science considers a governmental orientation (however broadly defined) as requisite to doing political science it is not surprising that very few political scientists want to engage in an endeavor which could be construed by their peers as something other than political science. In addition, the past fifteen years has seen an attempt within the discipline to make it more rigorous, to emphasize the "science" rather than the "political" in its name. While the merits of this undertaking are not at issue here it should be obvious that it is unlikely that individuals engaged in an attempt to inject a traditionally soft subject with a hard epistemology would find the personal politics of science an appealing area of inquiry. This is true because an acceptance of the personal politics of science tends to reduce the polarity between the traditional and contemporary modes of political

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inquiry. In other words, it acts to undermine rather than support strong arguments in favor of change in the direction of rigor.

However, it would appear that the battle over the degree of rigor appropriate to the study of political phenomena are over. In retrospect we can see that victories were few and far between

with the participants in the imbroglio, while they have mellowed a bit over the years, essentially clinging to their initial positions. Nevertheless, now that a period of peaceful coexistence pervades the discipline, reducing the demands for orthodoxy, the climate may be more suitable to the study of heretofore unacceptable subjects. Ideally within this context the study of the personal politics of science will have an opportunity to flourish.

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FOOTNOTES

1. This chapter uses the normal and revolutionary science concepts as defined by Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd edition, Chicago: The University of Chicago Press, 1970, pp. 23-43, 92-110. - [Back To Text](#)
2. Milton Rokeach, *The Open and Closed Mind*, New York: Basic Books, 1960. - [Back To Text](#)
3. R.A. McConnell, "ESP and Credibility in Science," *American Psychologist*, 1969, 24, pp. 531-38. - [Back To Text](#)
4. See Alfred DeGrazia, "The Scientific Reception System and Dr. Velikovsky," *The American Behavioral Scientist*, September, 1963, pp. 45-49, for a discussion of the rational model of science. - [Back To Text](#)
5. Donald Menzel, "UFOs: The Modern Myth," in Carl Sagan and Thornton Page (eds.), *UFOs: A Scientific Debate*, Ithaca, Cornell University Press, 1972, pp. 123-24. - [Back To Text](#)
6. William Markowitz, "The Physics and Metaphysics of Unidentified Flying Objects," *Science*. September 15, 1967, p. 1279. - [Back To Text](#)
7. Edward U. Condon, "UFOs I have Loved and Lost," *Proceedings of the American Philosophical Society*. Vol. 113, No. 6, 1969, p. 427. - [Back To Text](#)
8. See J. Allen Hynek, *The UFO Experience: A Scientific Inquiry*, Chicago: Henry Regnery Company, 1972, for a subdued but elegant plea to the scientific community. - [Back To Text](#)
9. Op. cit., pp. 92-93. - [Back To Text](#)
10. This hypothesis would seem to be intuitively obvious. It says something about the politics of science of social scientists that it apparently has not been researched. - [Back To Text](#)
11. See F.S. Kessel, "Philosophy of Science as Proclaimed and Science as Practiced," *American Psychologist*, 1969, 24, pp. 999-1005, for a discussion of the latter problem. - [Back To Text](#)

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APPENDIX A

The following is a list of groups before whom lectures, colloquia, and discussions were presented by J.E. McDonald in the period October 1966 through early 1969, in connection with continuing studies of observations of anomalous aerial phenomena. (For brevity, only the group addressed and location are listed. Title and emphasis varied considerably in the various discussions.)

DATE	GROUP
October 5, 1966	Colloquium, Department of Physics, University of Arizona
October 6	Colloquium, Department of Meteorology, University of Arizona
October 10	Aerospace-Mechanical Engineering Seminar, University of Arizona
October 19	Washington, D.C., Chapter, American Meteorological Society
October 26	Colloquium, Department of Psychology, University of Arizona
November 1	Staff, Steward Observatory, University of Arizona
November 2	Tucson Amateur Astronomers Association, Steward Observatory
November 9	Planetary Atmospheres Seminar, Kitt Peak National Observatory, Tucson
November 15	Seattle Chapter, American Meteorological Society, Department of Meteorology, University of Washington
November 16	Conference, Department of Psychology, University of Washington, Seattle
November 17	Staff, Department of Geophysics and Astronomy, Rand Corporation, Santa Monica, California
December 6	Tucson Chapter, National Pilots Association

December 8 Seminar, Department of Electrical Engineering, University of Arizona

December 14 Joint colloquium, Arizona State University Psychology and Sociology Honoraries, Tempe, Arizona

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DATE	GROUP
March 8, 1967	The University Club, Pioneer Hotel, Tucson
April 17	Scientific Staff, National Science Foundation, Washington, D.C.
April 17	Staff, U.S. Air Force Office of Information, SAFOI, Pentagon, Washington, D.C.
April 18	Staff, Office of Naval Research, Navy Department, Washington, D.C.
April 19	Staff, Naval Research Laboratory, Anacostia, Maryland
April 20	Staff, U.S. Naval Observatory, Washington, D.C.
April 20	Staff, Advanced Research Projects Agency, Department of Defense, Pentagon
April 20	Staff, Naval Ordnance Station, Indian Head, Maryland
April 21	Staff, Office of Naval Research, Navy Department, Washington, D.C. (overflow from talk of 4/18)
April 22	American Society of Newspaper Editors, Washington, D.C., annual meeting

April 24	Physics Division, Los Alamos Scientific Laboratory
April 29	Arizona Academy of Sciences, Tucson, UFO Symposium
May 8	Arizona State University, Interdisciplinary Seminar
May 16	International Students Club, University of Arizona
May 19	American Society of Civil Engineers, banquet meeting, Tucson
June 7	Staff, Goddard Institute for Space Sciences, New York, New York
June 7	Outer Space Affairs Group, U.N. Building, New York, New York
June 9	Staff, Federal Aviation Agency, Washington, D.C.
June 21	Staff, Rand Corporation, Santa Monica, California

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DATE	GROUP
June 27	Staff, Radiophysics Division, CSIRO, Sydney, Australia
June 29	Institute of Physics and Department of Meteorology, University of Melbourne, Melbourne, Australia
June 29	Staff, Division of Meteorological Physics, CSIRO, Aspendale, Victoria, Australia
August 2	Staff, Sandia Corporation, Albuquerque, New Mexico
August 28	American Institute of Aeronautics and Astronautics, Seattle,

Washington

- September 24 Kent State University, Kent, Ohio, public lecture
- September 25 Interdepartmental Physics Colloquium, Westinghouse Research
Laboratories, Pittsburgh, Pennsylvania
- September 26 Boston Chapter, American Meteorological Society, Boston,
Massachusetts
- September 27 Staff, Air Force Cambridge Research Laboratories, Bedford,
Massachusetts
- October 8 Sunday Evening Forum, University of Arizona, public Lecture
- October 12 Tucson Chapter, Reserve Officers Association, Davis-Monthan Officers
Club
- October 23 Colorado Springs Chapter, American Meteorological Society, Colorado
College, Colorado Springs
- October 24 Department of Psychology, Harvard University, Cambridge,
Massachusetts
- October 26 Pennsylvania State University, University Park, Pennsylvania, public
lecture
- October 27 University of Omaha, lecture to staff and students
- October 28 Omaha-Offutt Chapter, American Meteorological Society, Omaha,
Nebraska
- November 3 Arizona Science Teachers Association, U.A. campus meeting

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DATE**GROUP**

November 28	Newman Forum, University of New Mexico, Albuquerque, public lecture
January 26, 1968	Staff, United Aircraft Research Laboratories, E. Hartford
January 29	Naval Ordnance Laboratory, Silver Spring, Maryland, Applied Physics Department
January 30	Baltimore Chapter, American Institute of Aeronautics and Astronautics, Towson, Maryland
February 8	Sigma Delta Chi Journalism Fraternity, Tucson
March 12	Canadian Aerospace Institute, UFO Symposium, Montreal
March 13	Institute for Aerospace Studies, University of Toronto, staff conference
March 26	American Institute for Aeronautics and Astronautics, Flight Test, Simulation and Support Conference, Los Angeles
March 26	Santa Ana, California Chapter, American Institute for Aeronautics and Astronautics
April 6	9428th Air Reserve Squadron, Davis-Monthan AFB, Tucson
April 22	Interdisciplinary Seminar, Arizona State University Tempe
May 29	Alabama Section of AIAA, Huntsville, Alabama
May 31	Chicago Chapter of American Meteorological Society, Chicago, Illinois
June 3	Burro Club, Washington, D.C.
July 24	NSF Secondary Science Training Institute, University of Arizona, Tucson, Arizona

August 7	Boeing Management Association, Boeing Aircraft Company, Seattle, Washington
August 8	Aerospace Education Workshop, Seattle, Washington
August 8	Pacific Science Center, Seattle, Washington
September 26	Chicago Chapter of AIAA, Chicago, Illinois

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APPENDIX A (Continued)

DATE	GROUP
October 10	Arnold Air Society, University of Arizona
October 19	Arizona Section of AIAA, Phoenix, Arizona
November 3	Pima County Sheriffs Aero Squadron, Tucson
November 14	Central Oklahoma Section of AIAA, Oklahoma City, Oklahoma
November 15	Omaha Section of AIAA, Omaha, Nebraska
December 13	Aviation Aerospace Writers Association, Dallas, Texas
December 18	Cape Kennedy Section of AIAA, Cape Kennedy, Florida
December 19	Miami Chapter of AMS, Miami, Florida
January 14, 1969	Institute of Atmospheric Physics, University of Arizona
January 30	Medical Students Colloquium, College of Medicine, University of Arizona

February 12	Dupont Chapter of RESA, Wilmington, Delaware
February 13	Symposium on Meteorological Observations and Instrumentation, AMS, Washington, D.C.
February 18	Pacific Missile Range Chapter of AIAA, Pt. Mugu N.A.S., California
February 18	Antelope Valley Chapter of AIAA, Lancaster, California

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APPENDIX A (Continued)

The following are talks scheduled in the March-May 1969 period.

DATE	GROUP
March 19, 1969	Utah Chapter of AIAA and Salt Lake City Chapter of AMS, Salt Lake City, Utah
March 26	San Diego Chapter of AIAA, San Diego, California
March 28	Southwest Texas Chapter of AIAA, San Antonio, Texas
April 11	Vandenberg Chapter of AIAA, Vandenberg AFB, California
April 21	Department of Physics, Iowa State University, Ames, Iowa
May 6	Arizona Branch, Reading Reform Foundation, Phoenix, Arizona
May 7	N.I.C.A.P. lecture, Washington, D.C.
May 8	Mansfield State College, Mansfield, Pennsylvania
May 22	Tulsa Chapter of AIAA, Tulsa, Oklahoma

May 23 Kansas City Chapter of AIAA, Kansas City, Missouri

May 26 Edison Foundation, Counselors Conference, Macalester College, St. Paul, Minnesota

May 28 Sacramento Chapter of AIAA, Sacramento, California

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ACRONYMS

AAAS American Association for the Advancement of Science

AFOSR Air Force Office of Scientific Research

AFSAB Air Force Scientific Advisory Board

AMS American Meteorological Society

APRO Aerial Phenomena Research Organization

ASNE American Society of Newspaper Editors

CAS Committee on the Atmospheric Sciences

CASI Canadian Aeronautics and Space Institute

CU University of Colorado

DOD Department of Defense

ESSA Environmental Sciences Service Administration

FTD Foreign Technology Division

IEEE	Institute of Electrical and Electronic Engineers
NAS	National Academy of Science
NASA	National Aeronautics and Space Administration
NBS	National Bureau of Standards
NCAR	National Council for Atmospheric Research
NICAP	National Investigations Committee on Aerial Phenomena
NOL	Naval Ordnance Laboratory
NRL	Naval Research Laboratory
ONR	Office of Naval Research
SAFOI	Secretary of the Air Force Office of Information
UA	University of Arizona
WPAFB	Wright-Patterson Air Force Base

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IMPORTANT ACTORS

Mary Lou Armstrong:	Condon's administrative assistant who resigned in protest.
Ted Bloecher:	A NICAP staffer who aided McDonald.
John Fuller:	Author of several UFO books and the <i>Look</i> article on the "Low memo."

Russ Greenbaum: The public information officer at the Office of Naval Research.

Richard Hall: Assistant director of NICAP from 1960-69.

James Hughes: McDonald's ONR project monitor.

Allen Hynek: Scientific consultant to Project Blue Book for twenty years and chairman of the Department of Astronomy at Northwestern University.

Richard Kassander: Chairman of the Institute of Atmospheric Physics at the University of Arizona.

Donald Keyhoe: Author of four UFO books and director of NICAP from 1956 until 1969.

Philip Klass: Avionics editor for *Aviation Week and Space Technology Magazine* and author of a book on UFOs.

Gerard Kuiper: Director of the Lunar Planetary Laboratory at the University of Arizona.

Norman Levine: A professor of Electrical Engineering fired from the Condon Project.

Jim and Coral Lorenzen: Founders of the Aerial Phenomena Research Organization.

Robert Low: Project administrator for the Condon Study at the University of Colorado.

Tom Malone: Chairman of the Committee on the Atmospheric Sciences of the National Academy of Science.

Donald Menzel: Former director of the Harvard Observatory and author of two books on UFOs.

Phyllis O'Callaghan: Administrative assistant to Representative Edward J. Roush of Indiana.

IMPORTANT ACTORS (Continued)

Thornton Page:	A professor of astronomy at Wesleyan University and co-organizer of the 1969 AAAS UFO symposium.
William Price:	In charge of the contract for the Condon Study at the Air Force Office of Scientific Research.
Major Hector Quintanilla:	The Air Force Officer in charge of Project Blue Book.
Thomas Ratchford:	With William Price in charge of the contract for the Condon Study for the Air Force Office of Scientific Research.
Walter Orr Roberts:	Director of the National Council for Atmospheric Research and president of the American Association for the Advancement of Science.
Edward J. Roush:	The member of the House Committee on Science and Astronautics responsible for the 1968 UFO hearings.
Carl Sagan:	Professor of astronomy at Harvard and co-organizer of the 1969 AAAS UFO symposium.
David Saunders:	A professor of psychology at the University of Colorado who was fired from the Condon Project.
Philip Seitz:	President of the National Academy of Sciences.

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