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THE UNIVERSITY OF COLORADO UFO PROJECT: THE “SCIENTIFIC STUDY OF UFOs”

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ABSTRACT: Because of its unique place in UFO studies, the 1967-68 Air Force-sponsored “Scientific Study of UFOs” is claimed by many academics to have been a definitive statement about the UFO phenomenon. This article examines the origins, personnel, methodological debates, activities, problems, and results of the project. The key questions of intellectual prejudice and the coherence of results to conclusions are raised. The answers indicate a chapter in the history of science more subjective and embarrassing than scientific.

THE ORIGINS OF THE COLORADO PROJECT

The United States Air Force sponsored a “Scientific Study of UFOs” (the Colorado Project) in the late 1960s primarily to relieve itself of the burden of UFO investigations. The outcome of an event often reflects its beginnings, and this is in part true of the Colorado Project.

The primary duties of the Air Force include:

- a. Monitoring United States air space to determine if any violations constitute a threat to the nation’s security. This is its *Intelligence* function.
- b. Researching atmospheric phenomena and aerodynamic principles to obtain knowledge useful in weapons development or defense. This is its *Research and Development* function.
- c. Maintaining a high level of public confidence that the American people are well protected by their honorable public servants, the military. Although this is partly an intelligence issue, it is essentially its *Public Relations and Public Information* function.

When UFO reports erupted in 1947, any one of these functions could have been involved. All of them point to the military’s only real concern—national security. To make the present safe and the future secure, what should the Air Force do about UFOs? In 1947 the opinion was firmly held that UFOs were primarily an intelligence problem. These things looked potentially threatening, yet no one knew much about them. They were also a public-information problem, but this needed to be closely

tied to the intelligence issue. Therefore, the problem fell to the Directorate of Intelligence in the Pentagon, which soon created a clearinghouse for information in the technical intelligence command post at Wright-Patterson Air Force Base. Thus was born what became Project Blue Book.

From the very beginning there were deep divisions of opinion in the Air Force about how to handle UFOs. Some officers thought that a significant number of reports might involve foreign terrestrial technology and thereby constituted a conventional intelligence-related threat. Others thought that the reports were explainable by ignorance, stupidity, lies, hoaxes, or even our own technology, and that UFOs were purely a public-relations problem. Still others thought that something new and exciting was going on, naturally or extraterrestrially. Some saw this as a potential threat, others as a scientific opportunity.

These factions debated one another continually and sometimes intensely. Arguments took place and decisions were made at very high levels (Swords, 1996). At the Intelligence Division of the Air Materiel Command at Wright-Patterson, the arguments mimicked those in the Pentagon. As early as 1950 the scientific advisor attached to T-2 intelligence, A. Francis Arcier, was pushing for separating the UFO project from the intelligence community (Arcier, 1960).

Although director of intelligence Gen. Charles P. Cabell saw the project as important, and his successor, Gen. John A. Samford, essentially agreed, by the beginning of 1953 the CIA and many important Air Force officers had decided that UFOs were not a direct threat to national security, but posed an indirect threat through their psychological effect on the American public. Thus UFOs were no longer perceived as an intelligence problem, but as a public-information problem (Jacobs, 1975).

Through the middle and late 1950s the sentiment was growing within the leadership at Wright-Patterson to rid itself of this burden. Although the personnel directly allocated to Blue Book was minimal, Air Force expenses accrued every time there was a field investigation. Because the project had basically become a public-relations function rather than an in-depth examination of a mystery (and not at all a national security activity), it was an unsatisfactory situation for all concerned. Added to the grief associated with administering this project were the constant pressures and attacks from the civilian UFO organization, the National Investigations Committee on Aerial Phenomena (NICAP), and its energetic leader, Maj. Donald E. Keyhoe (USMC, ret.). Keyhoe and NICAP were dedicated to the viewpoints that UFOs were probably extraterrestrial spacecraft and that the Air Force was botching the investigation and/or covering up the data. The two constant targets were the Pentagon and Project Blue Book. Strapped with a burden that was largely considered fictitious, useless, expensive, and irritating, it took no genius to decide that it would be a blessing to discard the UFO investigation.

But other factors worked against the simple dissolution of Project Blue Book. Many in the Air Force felt that the public perception of UFOs demanded that the Air Force appear conspicuously on the job. Having a project allowed the Air Force to claim that its own team had authoritatively solved a seemingly disturbing report.

Closing the project would add fuel to NICAP's continued charge of cover-up. Also, within the project itself, not everyone was convinced that the work should simply be given up (Friend, 1959).

The main force behind this latter view was the sometimes enigmatic figure of Dr. J. Allen Hynek, civilian scientific advisor to the Air Force since 1949. Hynek regularly tried to conduct better investigations, gather more useful data, and obtain permission to speak to other scientists and experts. He even secretly joined and met with civilian UFO groups (Civilian Saucer Intelligence of New York, 1956; Bloecher, 1978). Hynek had done about all that he could in the 1950s and early 1960s to expand the Air Force's concern about the *scientific* interest inherent in the UFO phenomenon. For the most part, his concerns fell flat. Project Blue Book members, however, came to believe that there *was* reason to pursue the science in this, and were reluctant to recommend a simple shutdown.

So in 1959, under many pressures within and without, Blue Book staff made a series of recommendations about the project's future. Hynek's concerns reflected their conclusions. They named four options: (1) transfer Blue Book to the Air Research and Development Command; (2) transfer Blue Book to the Office of Information Services; (3) close up shop; or (4) maintain the status quo (Jacobs, 1975, pp. 169–70).

Number one was the preferred option—a scientific solution. Number two was next, with Hynek continuing as science advisor, and with other scientific analyses allowable. Number three did not seem politically feasible. Hynek supported the transfer option because he knew that the UFO problem would never receive adequate scientific attention while stuck in an intelligence/public-relations function.

The first option was attempted. The commander of the Air Research and Development Command, Lt. Gen. Bernard Schriever, was not to be fooled. He instructed his vice commander, Maj. Gen. James Ferguson, to refuse politely and firmly. A special plea from Hynek to Air Research and Development Command fell on deaf ears. Wright-Patterson intelligence then tried to persuade the Office of Information Services to administer Blue Book. On no account were they willing to absorb this thankless source of irritation. Wright-Patterson was stuck with it and did not like it. This reflects attitudes dating as far back as 1955 when Air Force chief of staff Gen. Thomas D. White suggested that it might be a good idea to turn over all investigation and analysis to an outside contractor, such as Rand or Battelle (U.S. Air Force, 1959).

In 1962 Wright-Patterson once again tried to transfer the program. An opportunity to brief the office of the Secretary of the Air Force led to discussions of shifting the UFO investigation to NASA, the Smithsonian, the National Science Foundation, the Brookings Institution, or some other research organization that could be monitored by the Air Force Office of Scientific Research. Lt. Col. Robert Friend, then Project Blue Book chief, wrote the following prescient words:

The UFO program [could be] contracted to some private organization, such as Brookings, which will make positive statements regarding the

program and the Air Force's handling of it in the past and make recommendations regarding its future, i.e. disband the program completely or handle it as outlined in A or B above [transferred to some scientific agency such as NASA, etc.]. [Friend, 1962; Jacobs, 1975, pp. 185-86]

This is the message that the University of Colorado received four years later. In 1962, as in 1959 and 1960, no one would touch the project. Wright-Patterson continued grimly, now with a new project leader, Maj. Hector Quintanilla.

Unlike the broad and somewhat open-minded Friend, Quintanilla knew that he had a tough job to do, but a simple one—data collection, no analysis, and consistently negative commentary on the mysteriousness of UFOs. This neglect of real investigation disturbed Hynek, and ultimately created a public-relations crisis. In 1965, Hynek took an opportunity to interest the Pentagon in the idea of involving the National Academy of Sciences with UFO analysis. In a letter to Col. John Spaulding, he suggested a working panel of academics, involving themselves over a period of several months. There would be physical scientists as well as social scientists. Their discoveries should help the Air Force solve both its scientific and its social problems. Hynek, of course, volunteered his own services (Hynek, 1965; Jacobs, 1975, pp. 197-98).

By the end of 1965 the Air Force decided to detail the problems that UFOs were causing Project Blue Book to a select group of its Scientific Advisory Board (the O'Brien Committee). It was only a one-day assessment and it met in February 1966. The committee recommended a strengthening of the investigation by contracting with a central university (in alliance with several other institutions) to do in-depth research on about one hundred sightings per year. Information and other cooperative links to Blue Book would be maintained. The university's work would be as public as possible and would provide regular briefings to any interested member of Congress (Steiner, 1966a). Hynek's ideas seemed to be getting somewhere. Then he played his final, fateful role in this process.

Quintanilla's technique of quickly explaining away UFO cases was a public-relations accident waiting to happen, and in late March 1966 it exploded. This was at the time of the Michigan UFO flap. Quintanilla pushed Hynek to issue a debunking press statement about what this group of sightings really was, before people got too excited. Hynek made his biggest public blunder at that time: the suggestion, without sufficient investigation or reflection, that the UFOs were merely swamp gas. Quintanilla was happy to have such a ridiculously prosaic "explanation," and Hynek did not protect himself with enough qualifying language. The result went far beyond Hynek's, Quintanilla's, or the Air Force's expectations.

People were outraged. They called their congressmen. Congressmen became outraged in front of microphones and in print. Worldwide publicity ensued. U.S. Representative Gerald Ford called for an apology to his constituents and an investigation of the Air Force's UFO procedures. Hynek became a laughing stock. NICAP made full use of the opportunity to blast the Air Force along its usual lines of cover-up and

disservice to the public. Dr. James McDonald, a University of Arizona physicist who was to become the leading scientific proponent of the theory that UFOs were extra-terrestrial vehicles, took courage and began his intense UFO research that would lead to his explosive entry into the spotlight by fall 1966. Hynek, too, finally turned the corner. Feeling betrayed after all his loyal work, he said: "This is the last time that I try to pull a chestnut out of the fire for the Air Force."

From that point onward, his relationship with Quintanilla and the project was formal and chilly at best. In addition, the wheels were finally moving to "buy" a university to study UFOs and to get rid of the UFO project. Never has a cloud of swamp gas exerted so much motive force.

WHY COLORADO?

Within a week of the public furor over the swamp-gas explanation, Hynek, Quintanilla, and Secretary of the Air Force Harold D. Brown were called to testify on these matters by the House Armed Services Committee. The O'Brien Committee recommendations were brought up. Hynek strongly supported them. Brown suggested that he was considering a university study. The Committee took this as a good suggestion and indicated that they would look forward to his arranging for a solution to this problematic situation that Blue Book had created (Jacobs 1975, pp. 204-206).

If there were any doubts about searching for and funding a university UFO study before the hearing, there were none afterwards. Secretary Brown immediately instructed the Air Force chief of staff to search for a university. It fell to the Air Force's Directorate of Science and Technology at the Pentagon to recruit a university and monitor its work. Lt. Col. Robert Hippler got the assignment. He selected another panel of experts to help. They suggested that Dr. H. Guyford Stever, head of the Air Force Scientific Advisory Board, president of Carnegie Institute of Technology, formerly of the Massachusetts Institute of Technology, and later to become Gerald Ford's science advisor, be brought in. Stever, though not quite the equivalent of Vannevar Bush in the 1940s, was as much a governmental science insider as one could get (Steiner, 1966b; Hippler, 1966).

The panel suggested the University of Dayton, but Stever apparently shot higher: MIT, Harvard, California, North Carolina, the typical locations of many previous secret governmental science projects. Fearing that any contact with the concept of UFOs would damage their reputation, every school refused, despite the bait of a contract for several hundred thousand dollars. UFOs were anathema to academia. Even Hynek's Northwestern University had balked at a separate project merely to code the Blue Book database on computer cards (Cooper, 1966). Col. Hippler, even with Stever's influence, could not get a university to agree to house the contract.

The other office of the Air Force which would be involved, as the so-called buyer of the project, was the Office of Scientific Research. This office would provide the funding and a project officer to help monitor and facilitate matters. The officer was

Dr. J. Thomas Ratchford and he was soon stuck with the recruitment problem. He started with a longtime Air Force contractor, the National Center for Atmospheric Research (NCAR) and its leader, Dr. Walter Orr Roberts. Roberts had benefited from Air Force funding since the late 1940s when NCAR was still the High Altitude Observatory and Roberts a Ph.D. fresh from the classrooms of Donald Menzel at Harvard. Unlike his famous mentor, however, Roberts had a mildly sympathetic closet interest in UFOs for years.

But he still was unwilling to head the project himself. What he would do, however, was to try to interest NCAR's sister institution, the University of Colorado, by promising NCAR's personnel and moral support. His longtime executive assistant, Robert J. Low, was now employed in the university's administration. He would make an effective salesman and organizer. And Low had become friends with the perfect head scientist, Dr. Edward U. Condon, even to the point of considering writing his biography. At the end of July, Ratchford personally applied the final pressures to Condon to get him to agree (Condon, 1966a). By the first of August, Low was already at work lining up support from NCAR and the Environmental Science Services Administration (ESSA) and making sales pitches to the University of Colorado administration. It was not all clear sailing. To pull it off, Low found it necessary to write the notorious Low memorandum on August 9, of which we shall hear more later.

Ratchford and his immediate superior, Dr. William T. Price, visited the campus to talk to faculty the next day. Enough interest was shown, enough support marshaled, and enough fears calmed, that by mid-August the University of Colorado seemed ready to take the plunge. For the next 30 days, university politics and faculty recruitment were still in evidence, but Condon knew that the proposal would now go forward at least a week and a half before it was presented to the Board of Regents. In the meantime, the Air Force transferred UFOs (and Project Blue Book) to the Air Research and Development Command (as Wright-Patterson had requested nearly a decade earlier). The National Academy of Sciences agreed to assess the quality of the final report. As an aside to the difficulties in getting anyone to accept the project, Ratchford thought it prudent to lie to the university in the following manner: He told them that, other than their sister institution NCAR, Colorado was the Air Force's first choice. Mentioning the refusals by MIT, Harvard, California, North Carolina, and Dayton was impolitic.

The Air Force contract was for \$313,000 (Brittin, Condon & Manning, 1966). It was a very peculiar scientific grant. Normally a government grant goes to the scientist who initiated it, or who is at least vitally interested and experienced in the field and knows exactly what to do. This grant went to a scientist who was pushed into it, had little interest and apparently no experience, and, despite his brilliance, had no concept of how to conduct the investigation. Because the UFO phenomenon is so complex and multidimensional, this short-term "backwards grant" was doomed to failure before it was even signed. It did not take the project personnel long to realize this, as we shall see. The Air Force had surely realized it by this time. So why did it sponsor such a study? It simply had other goals.

The recruitment of personnel went easier than expected, because the 1966 UFO wave had piqued a lot of interest. It is somewhat difficult to identify everyone who was considered a project member. This is because of the disorganized nature of what the project became (with persons in and out of Boulder, coming and vanishing, and consultants of widely varying degrees of involvement), plus the well-publicized controversy that took place after a year or so of work. Still, we can make an attempt of defining who was who.

Edward U. Condon was, of course, chief scientist. He was a great physicist, a patriot, and a hell-raiser. He was district head at Los Alamos during the Manhattan Project, director of the National Bureau of Standards in the late 1940s, and a target of Richard Nixon's bogus accusations of pro-Communist security risks in that same time period. In 1966, Condon was nearing the end of his scientific career and was much honored. A strong man physically and personally, he was proud of his tendency to ruffle feathers and do it his own way. He was also a hilarious joker. At this stage of his life, UFOs seemed an amusing diversion, just as long as he did not personally have to spend too much time on them (*Denver Post*, 1966; Spruch, 1969).

The person who ensured the latter was Robert J. Low. He had been Walter Roberts's administrator at the High Altitude Observatory for years, and was responsible for keeping the place functioning while Roberts went on his countless fund-raising trips. Low was the perfectly loyal right-hand man. He would do the job that Condon did not want to do, and yet be strictly answerable to the grand old man. It was Low's availability as much as anything else that allowed Condon to agree to the UFO project.

Other major researchers were officially listed in the early documents: Dr. Stuart W. Cook of the Colorado psychology department, who helped get things going, then backed out but stayed in touch; Dr. William Scott, a psychologist, who never was a factor and left; Dr. Michael M. Wertheimer, a psychologist who contributed one very important intellectual analysis and little else and might with justice not be called a primary member; Dr. David R. Saunders, a psychologist and a major player in all ways; and Dr. Franklin E. Roach, an atmospheric physicist from ESSA, an important element, but a major loss when he left after the summer of 1967.

There was quite an uproar about the preponderance of psychologists. People suspected that a study of abnormal persons rather than abnormal aerial phenomena was about to take place. This was somewhat alleviated by the additions of Dr. Roy Craig, a physical chemist from Colorado, Dr. Norman E. Levine, an electrical engineer from the University of Arizona, and Dr. William K. Hartmann, a planetary astronomer also from Arizona. Although Hartmann stayed in Arizona (Levine came to Boulder), his level of communication and integration qualify him fully as a member of the research team. Three members of the support staff also had lengthy, in-depth involvements: psychology graduate students James E. Wadsworth and Dan Culberson, and Condon's administrative assistant, whom he loaned to the project, Mary Lou Armstrong.

There were a great many other individuals who dipped in and out of the project's

business, and it is difficult to know where to draw a line in the spectrum of commitment. Some of the more important and involved of these deserve to be mentioned. Frederick Ayer II, a Colorado physicist, conducted a few case investigations and wrote a chapter in the final report. Although Condon does not cite him, Frederick J. Hooven of Ford Motors did a lot of consulting for the project, even in Boulder. Gordon D. Thayer, a physicist and radar expert at ESSA, stepped into the gap created by the firing of Levine and conducted analyses on the radar cases. Lastly, Dr. Gerald M. Rothberg of the Stevens Institute of Technology spent an entire summer doing field investigations for the project in Pennsylvania. Many other persons are mentioned in the final report and in project documents. Most of these seem to have been bystanders or distant, disconnected experts from whom the project was purchasing highly focused studies to enlarge the report.

This difficulty in maintaining a stable project staff is further evidence of the oddness and disorganization of this particular investigation. The following list, however, constitutes the essential *dramatis personae*:

A. Primary staff

Dr. Edward U. Condon, physicist
 Mr. Robert J. Low, administrator
 Dr. David R. Saunders, psychologist
 Dr. Norman E. Levine, electrical engineer
 Dr. Roy Craig, physical chemist
 Dr. William K. Hartmann, astronomer
 Dr. Franklin E. Roach, physicist
 Mrs. Mary Lou Armstrong, administrative assistant
 Mr. James E. Wadsworth, graduate assistant
 Mr. Dan Culberson, graduate assistant

B. Secondary staff

Dr. Stuart W. Cook, psychologist
 Dr. Michael M. Wertheimer, psychologist
 Mr. Frederick J. Hooven, engineer
 Dr. Gerald M. Rothberg, physicist
 Mr. Gordon D. Thayer, physicist
 Mr. Frederick Ayer II, physicist

Others were occasionally called on for special expertise—Courtney Peterson, Herbert J. Strentz, Martin D. Altschuler, William S. Blumen, John B. Ahrens, and Joseph H. Rush. Contributing to the report from the periphery were Aldora Lee, Paul R. Julian, Mark W. Rhine, and Samuel Rosenberg. Harriet Hunter helped edit the final report, but had no input into the quality of the investigations. Many others, including Hynek, William Powers, Raymond Fowler, June Larson, George Kocher, Peter Van Arsdale, helped the project significantly with the field investigations. Lastly,

Herbert E. Roth, who had organized the Volunteer Flight Officers Network for reporting satellite decays, cooperated with the project.

WHAT TO DO? THE PROBLEM OF METHODOLOGY

After the "backwards contract" was signed and the initial staff assembled, seven academics sat in Condon's office staring at one another and wondering what to do. These first seven individuals were: Condon, Low, Roach, and the four psychologists (Cook, Scott, Saunders, and Wertheimer).

They met five times between October 14 and 31, 1966, before the contract began officially (Colorado Project, 1966a-e). Cook took an early leading role, trying to organize a coherent discussion. The only thing they really agreed upon was that they needed a lot of help. From early November through December a number of helpful briefings took place with Hynek, UFO researcher Jacques Vallee, the officers at Wright-Patterson, Keyhoe, Richard Hall, and several others (Colorado Project, 1966f-h). During these first three months, the atmosphere in the committee meetings changed from a lackadaisical verbal jousting between intellectuals trying to impress one another (Wadsworth, 1967) to an awareness that they had a real problem on their hands. There was very little agreement on whether anything could really be done, let alone how to get on with it. And *some* decision had to be made.

A major part of the difficulty was that the seven of them were there for widely different reasons. Condon felt that he was doing the Air Force a favor and did not want to be there at all. Low had promised Condon and the University and probably Walter Orr Roberts that he would administer the project full time; but he also seems to have been genuinely intrigued by the UFO mystery. Although they all had slightly different slants on the subject, three of the psychologists (Cook, Scott, and Wertheimer) were not really interested in UFOs. They were there to use the spectacular potential of UFO reports to assess the psychology of the witnesses. Their views on how to spend the Air Force's money were not overly central to the interests of ufologists or even the Air Force. Saunders and Roach wanted to study UFOs—mysterious cases, old and new, individually in depth and statistically in bulk. The group even had problems communicating with one another. The psychologists tended to see the problems one way, Saunders and Roach another. Low was torn between taking the Saunders and Roach approach because of his interest in the subject and conforming with Condon's overriding negative attitude about what they should be doing.

The briefings did not help undo this division, but they did push a decision nearer. The Hynek-Vallee briefing especially impressed Saunders with Vallee's views on the potential of data processing and statistics (Colorado Project, 1966f). The Keyhoe-Hall briefing confirmed Roach's and Saunders' opinions on the value of investigating earlier UFO cases (Colorado Project, 1966h). Low seemed to feel the same. Even Condon reckoned that Hall was a smart individual (and later recommended him to the *Encyclopaedia Britannica* to write a UFO article for its yearbook) (Condon,

1967b). The Air Force briefing was largely spent with Col. Hippler bad-mouthing the value of looking at old cases. This, in an odd way, reinforced the psychologists' view that the proper technique was to set up experiments to test witness perception and accuracy (Colorado Project, 1966g).

In early December 1966 two important attempts were made to influence the project's choice of methodology. A December 7 memorandum from Saunders to the rest of the seven outlined his "framework for the analysis of UFO data" (Saunders, 1966). It was a scheme to place all UFO data into a massive database for use in computerized statistical analysis. It emphasized Vallee's approach and was based upon the Hynek, Keyhoe, and Hall vision of the evidential value of old cases. Saunders assumed new cases would go into the database as well as old, but what he perhaps did not see was that his suggestion would turn the whole Colorado Project into a feeder mechanism for his computer and his correlations. Also, he was swimming against another fairly strong tide. His concept depended upon a profusion of cases, including many cases not investigated by the project itself. Almost everyone else wanted to emphasize the team's own fieldwork at least as much as the older classics.

The second attempt occurred at almost the same time in December 1966. It was a bombshell. Wertheimer wrote down some philosophical and pragmatic meditations about the problem. In the first half of his critique, he discussed the many ways in which a witnessed event may become degraded as the experience passes through the human sensory apparatus and cognitive system. Here he was speaking from his background as a cognitive psychologist and expert in perception. His point was that there is little assurance that UFO reports necessarily reflect an accurate depiction of the stimuli that prompt them. This was the pragmatic half of his argument (Wertheimer, 1966, pp. 9-11).

The philosophical element involved the extraterrestrial hypothesis for UFOs. Saunders, Roach, Low, and even Condon wanted to attempt to make some statement in the final report in support or opposition to the extraterrestrial hypothesis. Wertheimer objected that since one could honestly remark neither positively nor negatively about it, why should it be addressed at all? His reasoning went as follows:

- a. Regarding the negative position (UFOs are not extraterrestrial spacecraft): A negative of this type cannot be proven. If one case is solved or a thousand cases are solved, all cases have not been solved, nor can they be. There is always the possibility that any one of the cases in doubt or unexamined is an alien spacecraft.
- b. Regarding the positive position, barring an extremely unlikely circumstance (like a landing with overt contact): The positive cannot be proven either. Any case or set of cases resisting identification is simply unknown. He invented the name "framasands," a nonsense word, to label things that cannot be identified. Whenever a group discussion began about the extraterrestrials during the next month or so, he would deflect it (often in a way that was offensive to Saunders personally) with

interjections about framasands and how nothing else could be said about the matter.

In terms of pure, positivist logic it was difficult to argue with what began to be called the Wertheimer Hypothesis. Condon said that he was "staggered" by it. Without having thought about UFOs much, Condon still felt that he could make some comment about the extraterrestrial hypothesis in the end. (All indications are that he was sure that this would be a strongly negative one.) Saunders said that he also was bothered by Wertheimer's logic. For him, though, it was more of an irritant that was difficult to get around. As group discussions stagnated, Wertheimer resorted to calling Saunders a "quasi-believer." This was a thinly veiled insult, as the term "believer" had acquired a strong pejorative connotation among the academics. Saunders put up with this for awhile (Saunders & Harkins, 1969).

By the end of December, the seven had been meeting for three months with no agreement on how to proceed. Worse than that, the Wertheimer Hypothesis had made some doubt that there was any proper procedure, at least on certain important questions. Already there was friction: between Saunders and Wertheimer, between Roach and Low (for some undiplomatic remarks Low had made about Roach's skills), and between Scott and everyone in general (he could not understand why people were not fired up in support of some extremely narrow and almost irrelevant testing he wanted to do). Meetings in general were tense and not very productive. James Wadsworth, the graduate student, acted as a candid fly on the wall:

Most of the project heads have duties in their departments and are only part-time on this. Thank God that is the case as most of them contribute generously with the axe and have little positive to offer, much less enthusiasm. I feel like each general meeting sets the whole project back. You wouldn't believe the chicken-shit security-nitched academic egotism that goes on.

It's as though the first concern of the group is to protect themselves from getting tainted by the quasi-scientific animal known as UFO. By the time they have succeeded at this, their value as open-minded scientists has suffered greatly. They are too busy maintaining a role to let loose what little creativity they have. [Wadsworth, 1967]

The new year opened for Bob Low with a renewed sense of urgency. The Air Force (Hippler, Ratchford, and several scientists) had scheduled a visit on January 12 to find out how things were going. For Low it must have been a nightmare. His boss was "staggered" and needed to know what to say. Low furnished him with some poorly defined works in progress to talk about and then suggested that he discuss the Wertheimer Hypothesis (and the problems associated with it) at length (Low, 1967a).

At the briefing Condon was definitely in charge. He spoke or interrupted twenty

times, more than twice as much as anyone else. The staunchest advocates of case investigation (as opposed to the mere psychological testing of witnesses), Saunders and Roach, each spoke briefly only twice. Even Wertheimer and Cook doubled that. Condon opened with Low's prepared survey, and handed the ball to Wertheimer to explain his hypothesis. Roach finally objected but Condon cut him off. Cook, Wertheimer, and Condon then began a disconnected argument for concentrating the research on UFO witnesses. Ratchford, from the Office of Scientific Research, seemed to like this, but Col. Hippler said no—twice. He did not want Colorado (and the Air Force) receiving any more criticism for not taking the reports seriously and looking at the reporters as if something was wrong with them (Colorado Project, 1967a).

Saunders spoke about his database idea, which, as usual, fell flat. Ratchford briefly referred to it later in the context that finding correlations between old ladies and certain reports might tell you something. He consistently suggested ideas to focus upon that were not central to the UFO problem. He was enthusiastic about research on atmospheric conditions, plasmas and ball lightning, anomalies of the human mind—anything but the core of the enigmatic UFO cases.

The only two positive suggestions coming from the Air Force were made by Sacramento Peak Observatory director Jack Evans, a former student and colleague of Walter Orr Roberts. Evans remarked that a small amount of instrumented, dependable data would be worth tons of unreliable data. Could not a dedicated instrumentation program be set up? This sparked a pessimistic exchange between Condon and Roach that was quickly dropped. Later, following some typical "just the facts" responses by Col. Quintanilla of Project Blue Book, the high reliability of UFO reports by pilots was admitted. Evans immediately jumped in with the opinion that this, then, is what was needed: Concentrate on known classes of reliable observers. Surprisingly Hippler, perhaps letting down his guard at the very end of the meeting, agreed and then added something odd. He said that when pilots in the Pentagon talked to him about UFOs, many said that they saw unidentifiable aerial phenomena all the time, and that they were getting used to it. Unaccountably, no one followed up on this intriguing comment.

The project's main objective in the briefing was to get some help with their problems and advice on methodology. Very little was useful, however. Low, naturally more worried about organizational problems than anyone else, finally interrupted with a frank question: Since the study could not solve the UFO mystery, what was the project's role? Ratchford and Hippler were casual and vague in their response. We want you to just give it a try, they said. You do not have to make any final recommendations that you do not feel strongly about. Condon said that if there was a residue of mysterious cases, but there was no threat to national security, then he would recommend just ignoring the residue. William S. Blumen, a Colorado geophysicist, reminded him that ignoring residues is exactly what science should not do. Ratchford and Hippler each briefly mentioned how much it cost to study this subject. At the end of the meeting one can almost see Condon shrugging his shoulders in the transcript and saying the correct approach was still a mystery and the

project was way behind on its deadlines.

In its odd fashion this meeting ended up shaking several apples out of the tree. Project members were acutely aware that decisions had to be made, and that they had better state their individual cases soon. Also, a private message of major significance between Hippler and Condon and Low resulted (of which we shall hear more later). At least six documents were generated and many private conversations took place in the next week.

In a January 19 memorandum Low made a strong attempt to characterize this disparate group as a team, while laying out a three-pronged investigation with scientific, political, and educational objectives (Low, 1967b). Since this was allegedly only a scientific study, this may have caught some individuals by surprise. Basically, Low's team would conduct a variety of scientific activities, especially field investigations of new cases and assessments of promising hypotheses like decaying comets and ball lightning. The educational objective was to produce a manual and a handbook that could be used to educate the public about UFOs and, most importantly, the many wonders of the natural world which are misidentified. Roach, a physicist of the old school, wanted to do this because he believed that the primary duty of the scientist was public education. The political objective was more intriguing, in more senses than one. This involved any recommendations for further study at the close of the project. Low suggested that this was his and Condon's area of concern. (Low and Condon had just been told what this recommendation was to be, as we shall see.) Low then added two further critical topics to this political area: the alleged Air Force conspiracy to withhold data and a study of the classic UFO sightings in case they were "asked about [them] in a hypothetical Congressional hearing." One wonders if Low had thought much about how this division of labor and authority would be received, especially by Saunders and Roach, but he did end by saying that this was just a "talking piece" (Low, 1967b).

Roach and Saunders made their feelings known immediately. Roach insisted that the primary problem was the unexplained residue of sightings, especially reports in the Blue Book, NICAP, and APRO files (Roach, 1967). They should be brought to Boulder, cataloged, computerized by Saunders. Saunders also fought for the priority of his statistical database. Plus he had finally had it with Wertheimer. After a lively meeting on methodology on January 20, Saunders blasted Wertheimer's idealistic sophistry as a preposterous blueprint for practical action. He pointed out that all scientific endeavors were subject to the same positivist criticism, and that if taken to its ultimate conclusion, no scientific exploration should be bothered with at all. He also strongly hinted that he resented Wertheimer's potshots, especially his using the term "quasi-believer" (Saunders, 1967a).

After the responses to the Air Force meeting were in, Low found himself with a different team. Scott had long gone. Cook had just been in it to help with organizational matters and was essentially gone. Wertheimer went back to full-time teaching. Roach was leaving for duties in Hawaii and offered to do what he could from there. Condon was rapidly retreating into virtual isolation, punctuated by outbreaks

of negativism and press gaffes. Low and Saunders were staring at each other.

Actually this situation might have worked rather well if Condon had not had such an understandable influence on Low, and Low and Saunders, despite severe personality differences, could have gotten to understand one another. The less case-oriented staff members were gone, and left behind were two valuable graduate students, Wadsworth and Culberson. Armstrong was a fine administrative assistant. Ayer, Blumen, and Rush seemed willing to help open-mindedly where they could. Hartmann wrote to volunteer his services from Arizona. Craig and Levine joined the group as active primary staff. Had they been able to keep Roach, this would have made an excellent group to attack the core of the UFO mystery. As it was, it was not bad. The only real absolute negatives were Condon and the far-too-short time period.

The period of time between late January and late April 1967 is a bit of a mystery. People seriously began to organize the field investigation teams and the communications lines to feed them cases. Low worked very hard travelling, getting briefings and information, lining up help of all kinds, and thinking about the final report. During this time or shortly thereafter Saunders began to think of the project as in reality his own and Low's (Condon, 1968d). Several important briefings took place: CIA picture analyst Art Lundahl, the Aerial Phenomena Research Organization (APRO), Donald Menzel, and Ford Motor engineers Frederick J. Hooven and David Moyer. Hall returned to help with the selection of old cases.

Condon's activities were a lot less helpful. He made an incredible gaffe during a talk at Corning, New York, and did not even seem to understand why. He became amused by a psychic's prediction that a UFO would land at Bonneville Flats and sent Wadsworth to "investigate," while offering the governor of Utah and Donald Menzel rides in the saucer when it landed. He began studying any weird person or claim accreting to the edges of the phenomenon. And he accepted James Moseley's invitation to attend a public UFO symposium in New York composed almost solely of UFO buffs and "kooks," as Condon called them.

By the end of June 1967 it appeared that many in the project had lost all hope for Condon, but that may not have included everyone. Armstrong seemed to shift the blame onto Low, while Craig never lost confidence in the Condon (Craig, 1995). Low, for his part, seemed to be having fun and doing a good job when he was not having to clean up a Condon mess. He also felt that he had seen enough of the nascent project functions to lay out the framework for the rest of the study in an April 21 position paper, which was quite reasonable and even excellent in many ways. Saunders wrote in *UFOs? Yes!* that he took real alarm at it, but one wonders whether that was his actual feeling in April 1967 or hindsight in mid-to-late 1968 (Saunders & Harkins, 1969). Either way, Low's paper outlined, with minor modifications, the direction that would be taken in the final report. Its outline and that of the final report are nearly identical (Low, 1967d).

The two cornerstones of Low's position paper were the acquisition of data from UFO reports and a set of contracted reviews on special topics (optical mirages, ra-

dar, plasma, etc.). Low wrote mostly about field investigations of new UFO cases and this may have been in part what alarmed Saunders, who wanted to emphasize the puzzling UFO classics. This seeming emphasis on *new* cases was probably a result of incomplete writing, though, as in the full plan it is obvious that old cases, the toughest ones, were supposed to play an important role. Low also did not highlight the statistical treatment of data in his comments, but he did list it as a special section in his outline. Again, there may have been no problem except one of communication. Low knew perfectly well that old cases and a statistical treatment were to figure prominently in the report, and was taking them as givens. Saunders, however, can be forgiven for some understandable paranoia. Who would not be, with the way Condon was acting?

The contracted reviews on special topics have been widely viewed in the UFO community as everything from a waste of money to irrelevant filler or debunking. Perhaps all of these criticisms seemed appropriate after the fact, but I believe that Low's decision to include them was sound at the time. Low was convinced that the evaluation of many classic UFO cases often came down to unresolvable disagreements on matters of science between equally distinguished scientists. Harvard astronomer Donald Menzel was almost always on one side of the disagreement and lately, James McDonald often was on the other. Someone had to be wrong, but who? Low's solution was to get outside experts to write review papers on several of these contested areas, without asking them to comment on specific UFO cases. Instead, they would provide relevant, authoritative information from which the Colorado team could apply principles to the puzzling cases, new and old.

The idea was, in theory, rational and practical, given the project's short duration. The outside experts would not have to dirty their hands with UFOs, but just provide information. The Colorado Project would not have to deal with their biases and would make their own judgments on UFO cases. But because the project fell apart due to the infamous Low memorandum episode and the project members did not work diligently enough on the case book of classic puzzlers (for which all sides should take blame), these studies came about with no data to apply them to and no one to do the work. Thus they sit in the final report as apparent wastes of money, pages, and insight.

Low included several other research directions in his position paper, some good, some marginal. He wanted a special section on photographic cases, and fortunately he found an enthusiastic young astronomer, Hartmann, to pursue them energetically. Low also wanted sections on alleged artifacts, UFO history, the UFO phenomenon outside the United States, the alleged government cover-up, and the problems of human perception. Most of this makes perfect sense.

Low divided the project's research question into three segments. The broadest question was, "Are there really sightings that cannot be explained?" He felt that the Project *must* give a robust answer to this question. He discussed epistemological and philosophical problems for a while, but then *came down strongly on the side that many reports must be accurate*. Further, he suggested that it is reasonable to con-

clude that many of these reports will *not* be explainable. Therefore, the team could almost have predicted in April 1967 that the report would admit that there really are sightings (something external to the witness) that cannot be explained, and that, therefore, there *is* a "UFO problem," in his words.

The second question embedded within this larger one then came into play: "Are any of these [external stimuli] solid objects?" And the third question was: "Are any of these objects extraterrestrial spaceships?" Low repeated how difficult it seemed to be to obtain final answers to these questions, and suggested that the team needed to discuss their criteria for answering them. He seemed to believe that there might be sufficient evidence to state the probability that UFOs are solid objects, but the extraterrestrial-spacecraft question was tougher. Still, he was not opposed to trying: "One can certainly [at that point] say that they are either extraterrestrial spaceships or they are terrestrial phenomena of an as yet unknown source and description."

Low was hinting here that the direction of the project seemed headed toward an almost certain recommendation in favor of ongoing UFO research, whether extraterrestrials were involved or not.

Low also laid the Wertheimer Hypothesis to rest with a quote from Richard Feynman: "It is scientific only to say what is more likely and what is less likely, and not proving all the time the possible and impossible." Therefore, although the team was unlikely to *prove* the extraterrestrial origin of UFOs (barring a captured spaceship), they might be willing to use words like "probably," "perhaps," or "no evidence." He wrote that despite a lot of people trying to back him off of the extraterrestrial question, he did not want to duck it.

Saunders, perhaps because he was experiencing the project's activities and attitudes at first hand, reacted to this differently. He viewed the glass as half empty (it was very unlikely that the project would demonstrate the extraterrestrial hypothesis) rather than half full (it was very likely to recommend future research on a genuine problem, and might even give a "probable" answer to the extraterrestrial hypothesis). Whatever difficulties Saunders saw at the time, he apparently did not voice them, and the project focused its activities around this framework: sharpening the field research and early warning systems, contracting topical reviews, entering statistical data, analyzing photos, and traveling to obtain special information. Unfortunately, one crucial element was overlooked: the case book study of older sightings.

THINGS FALL APART: THE SUMMER OF 1967

According to available documents it appears that the project moved forward smoothly between April and June of 1967. This seems to be mainly because Low was keeping a very light hand on the steering wheel, and people were following their own interests. However, there was concern about the efficacy of this procedure, and Courtney Peterson, a Colorado law professor, was hired to do two jobs: look over their shoulders and assess the relationship of their activities to their goals, and create a standardized format and procedure for entering cases into the case book. Oddly, he

never started the second task, but he did write an interesting thought-piece on the first, called "Methodology and Purposes of the Colorado UFO Project," June 23, 1967 (Peterson, 1967).

From his observations of the team's activities, he judged correctly that the basic points expressed in Low's April 21 position paper had indeed become the official methodology of the project. He agreed with Low's assessment of the three research questions. It was certain that some inexplicable UFO cases were accurately reported, and thus in some sense a UFO phenomenon existed. In fact, it was likely that Colorado's work would add to the inexplicability of the phenomenon by finding more cases, by reversing bad Blue Book diagnoses, and by agreeing with the diagnoses of unexplained classic cases. On the second and third questions (are they solid objects? are they extraterrestrial?), it was equally certain that conclusive evidence was unlikely (especially in the short term), and that the project would be forced to issue some statement regarding this inconclusiveness. In his view, a "yes" answer to question one followed by an "inconclusive" answer to questions two and three would not be "dispositive of public anxiety about the nature of UFOs."

Peterson also suggested that although these were the questions that scientists and the public would like to have answered, they were not really the questions that the project's sponsor wanted to answer. The Air Force wanted to know what should be done about further UFO study after the Colorado grant ended. Should it be continued or not? If yes, how, at what level of effort, and by whom? Peterson believed that the project was not proceeding with these questions in focus. He felt that it should begin with the premise that some UFO cases were inexplicable and spend its time addressing the question of ongoing research.

He presented a spectrum of recommendations from discontinuance to maximum effort, and then asked: Are your current activities heading toward a specific position on this scale, unintentionally, by default? Are serious evaluations of the alternatives not being made? He answered yes:

Simply because an organization as unstructured as this Project staff appears to be tends to develop its own internal structure and objectives, and to do so without much influence of conscious choice even when the final configuration is partly based on compromise.

Members of an organization cannot readily define their own responsibilities without direction.

Peterson discussed briefly whether the data generated by project activities were sufficient to make valid judgments about specific recommendations for continuance. He examined pros and cons of public anxiety, potential extraterrestrial hostility, and valuable scientific discoveries without attempting to attach any conclusions or values. He believed these were the three vital issues that must be addressed before continuance was decided. Peterson clearly felt that the current activities of the project would produce recommendations only by chance, as a by-product of their normal operations.

Peterson's analysis seemed to confirm the hypothesis that Low was exercising minimal control over day-to-day functions, Condon exercised no control at all, and team members were doing whatever they found enjoyable in whatever way they preferred. This seems perfectly in concert with Low's character away from Condon's influence. It is also consistent with the thinking of people who are having fun with some scientific exploration: Let's jump in, see what we can see, and make our conclusions about what naturally emerges. All that is fine for a simple ontological question (what are UFOs?), but it is not, perhaps, the best way to focus on whether such research should continue. The project's loose way of muddling along also explains how something as critical as the case book could fall through the cracks: No one chose it as their area of personal interest. Low should take a major share of the blame for this because of his management style.

About the same time as Peterson's memorandum was written, a significant change in Edward Condon became apparent. Until the end of June 1967, Condon had been relatively mellow, even rollicking and occasionally tolerant about this UFO business and the people who studied it. Even his negative media gaffes were delivered in a light, joking vein. Although he emphasized the ridiculous, he was at least enjoying himself. Sometime between the end of June and mid-July this changed. Exactly what happened is unknown. Condon metamorphosed from a Dr. Jekyll who could recommend Vallee's two books to Hooven (Dec. 27, 1966) (Condon, 1966b), recommend NICAP's *UFO Evidence* to the great physicist Merle Tuve (Feb. 9, 1967) (Condon, 1967a), and recommend Hynek and Hall to the *Encyclopaedia Britannica* to write UFO articles (April 28, 1967) (Condon, 1967b), into a Mr. Hyde who called Hynek and Hall derogatory names (Roberts, 1969). Condon's "fun with the odd-balls" public attitude extended right up to the last week in June when he attended Moseley's kook-laden Congress of Scientific Ufologists on June 22-23, 1967, in New York City. Then this mellow, jovial manner ceased.

The whole project staff had pleaded with him not to go, but he did anyway and created a circus atmosphere that generated much negative publicity about UFOs. When he returned he was apparently greeted by a disappointed and disapproving staff. This is reflected in the transcript of a meeting held on June 30 (Colorado Project, 1967b-c).

The meeting was attended by Condon, Saunders, Levine, Roach (back from Hawaii for a while), Peterson, Armstrong, and a visiting journalism graduate student, Herbert Strentz. Low was absent for unknown reasons, as was Craig. Condon was uncharacteristically without allies. The meeting concerned the case book which needed to be gotten off the ground. Condon had listened to Col. Hippler speak against using old cases and had consistently argued for that position. He did the same thing this time. He may have been surprised at how much Levine and Saunders stood up to him; in fact Saunders ran the meeting, and Levine did most of the arguing with Condon. It was a pure trialogue with very obvious sides. Saunders and Levine rolled over all of Condon's attempts to sink the case book. When the edited minutes were distributed, it was as if Condon had never been there. They read like a linear, me-

chanical layout of how the project was going to pursue the case book. Condon's name was listed first on the attendees list, but he had become irrelevant at this meeting.

When the team met again on July 6, it was to nominate the first set of cases for the case book (Colorado Project, 1967d). Condon and Low both attended. Condon refused to nominate a case, but Low happily nominated the Red Bluff, California, policemen's report of August 1960. This was not only one of the most puzzling cases on record but far outside Condon's desired parameters of limiting the age of cases to a year old at most. Condon must now have realized that even Low was not on the same wavelength. Discussion about the case book continued through July. Saunders was in the ascendant. Condon retreated. Low was planning a month-long European trip in August, and Saunders would be alone to run the show.

In the midst of these mutual disagreements (Condon's insistence on attending the New York conference vs. the staff ignoring his views), something else was changing in his mind. He saw belief in the UFO phenomenon as an indicator of serious mental damage and psychosis. He mentioned this in relation to certain witnesses, especially Betty and Barney Hill, during the June 30 meeting. He began to think that UFO stories might add to a person's mental problems and that the project should consult clinical psychologists about this. He asked Carl Sagan, who recommended that he consult Harvard psychologist Lester Grinspoon, which he did on July 24 (Condon, 1967c). This may have been the point when Condon decided that the UFO subject was dangerous to America's children. He expressed that opinion several times in the following months (Colorado Project, 1967e). An example of this was when he wrote that school children should be forbidden to read about UFOs and get credit for writing about them in school. When a columnist suggested that this was policing thought, Condon wrote on the clipping: "School children? You must be out of your mind!" (Pierce, 1969).

Condon's biases must have been apparent to anyone who was paying attention, but Low's willingness to go along with promising UFO research ideas provided a counterbalance. At this time, however, serious doubts were raised about Low's own objectivity. There were two problems. The first occurred during Low's extended trip to Europe in August 1967. He designed an elaborate itinerary that involved much project business, plus a visit to Loch Ness to satisfy a cryptozoological interest. This would have been tolerated by the others had it not been for a major blunder by Low. He scheduled his trip at a time when neither of the major figures in European ufology, Charles Bowen and Aimé Michel, would be available (Saunders & Harkins, 1969). This the project team considered outrageous. It seemed as if the Loch Ness monster was more important to Low than Bowen and Michel. Every other element in Low's itinerary should have been secondary to the meetings with Bowen and Michel, as they offered the greatest opportunity of the project learning something about European UFO research.

The second problem caused far more difficulties in the long run, even though the European trip seemed more drastic at the time. This was the discovery of the infa-

mous Low memorandum. It was written by Low in August 1966 as one of his first acts to convince a reluctant university administration to accept the UFO project. Low wrote an effective memo that accomplished this task. It was its controversial wording that led to trouble:

... 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 an almost 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 examination of the old question of the physical reality of the saucer, I think the scientific community would quickly get the message.

... 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. [Low, 1966]

Anyone who reads the Colorado Project documents in their entirety will get a different feeling about this memorandum than the two usual responses it evokes. UFO proponents see the memo as conclusive proof that from the beginning both the project and Low were irrevocably opposed to giving UFOs an open hearing. UFO skeptics read the memo as an innocuous missive that uses the word "trick" only in the colloquial sense of a clever way of doing something without any prejudice implied. Neither interpretation is correct. The tone of the memorandum argues against a benign use of the word "trick." Its purpose was to persuade frightened administrators that they could relax their fear that this project would smear the good name of the University of Colorado. Low loaded his language like a used-car salesman. He wanted the administration to read the memo exactly like ufologists later did, although with a lot less emotion. Low had a sales job to do. He slanted his language to make the sale and he sold it.

Despite the language, there is evidence that Low didn't really have the attitude that he expressed. This is where reading the rest of the project materials becomes important. Low seems to have been two people: the administrative automaton who did what he was told to do using whatever means necessary, while peacefully coexisting with everyone; and the real person, who occasionally shed his mask and enjoyed life, people, and the exploration of interesting things. Low's actions after the

August 1966 memo do not characterize an individual with an irrevocable negative bias towards UFOs. His January 1967 briefing to Condon, his April 1967 position paper, his willingness to let Saunders and the others pursue their own interests, his October 1967 talk at the California Institute of Technology—all show an administrator trying to solve a difficult mystery openly. But it is completely understandable that when the Low memorandum was discovered by Craig in July 1967 and passed around to the rest of the crew, they were shocked and concerned. They saw only its literal meaning, though it was meant for another audience. Even Condon acknowledged that the memo was inappropriate (Craig, 1995).

Rightly or wrongly, these two incidents seriously undermined staff confidence in Low. They also brought to a head concerns with Condon's rogue, off-center behaviors. (Low was Condon's gatekeeper, especially during the long stretches when Condon did not want to be bothered with any of the serious work.) And Low began to be blamed for the lack of organization in several areas of the project. This was a no-win situation for Low. Without strong, serious direction from the chief scientist, he was left to try to maintain order among a group of strong personalities, who considered themselves essentially his peers. Saunders, in fact, was justified in doing so, due to the amount of effort and direction which he had contributed. Low and Condon recognized this level of contribution by titling Saunders "co-principal investigator." Low dealt with this situation of being *de facto* leader with no real authority (he was neither a Ph.D. nor a scientist) by letting people run their project elements without much interference. Soft lack of leadership almost always ends in chaos, no matter how pleasant it may be. The chaos of the bumbling case book issue and the lack of direction were now being laid at his feet.

In this environment, Saunders's leadership was rising. But Saunders was an extremely taciturn individual, much preferring documents and databases to spoken debate. He was not a dynamic glad-hander like Low or a powerful ego like Condon. He lacked many of the natural expected characteristics of a leader, yet leader he was. Had Levine been a more senior man or Roach more committed and energetic, either would have been better in this role. Still Saunders did what he could, though he probably preferred seeking correlations with his computer.

Saunders had been meditating about the project's direction and its potential conclusions. He may have been stimulated by Peterson's analysis, but his concerns were different. He believed that there was an excellent chance that the final report would support the extraterrestrial hypothesis. At the same time, he felt that this would be a shock and he could see no evidence that Condon and Low were worried about that (Saunders & Harkins, 1969). Saunders wondered if the reason that Condon was unconcerned with a pro-extraterrestrial conclusion was that he had already decided not to have one. Because of Low's trip to Europe, a summit meeting on this matter had to be put off till September.

Just before Low left, University of Arizona atmospheric physicist James McDonald, the leading scientific exponent of the extraterrestrial hypothesis, returned from a research trip to Australia to brief the Colorado team. Condon slept through it

(McDonald, 1968a).

In August Low was away and Condon was in retreat. Saunders ran the project, but not effectively. The case book effort again lapsed. Saunders was concentrating on what he wanted to do, statistical analysis. Especially compelling to him was the "orthotenic" thesis of French researcher Aimé Michel (1958). Michel felt that he had discovered a pattern of straight lines in UFO landings during the French wave of 1954, which seemed to be an intelligent attempt to conduct a geographic survey. Verifying this pattern statistically would indicate intelligence and an extraterrestrial origin. This was exactly what Saunders wanted. He outlined the concept (subtly on paper, but prominently between the lines) in an "Alternative Final Report" presented to the team on August 27. This was to be the alternative to the outline in Low's position paper of April 21. It was broken down simply into standard academic research paper sections ("Statement of Hypotheses," "Experimental Procedures," "Results," etc.), but introduced the device of "Technical UFO Reports" (TUFORs) (Saunders, 1967b).

TUFORs were Saunders's solution to most of his current concerns. They were to be reports written by staff members (individually or together) on whatever aspect or case they had researched. The reports would be circulated among the staff as a whole to review, *but not veto*. They were the author's own work and would stand without censure. Appropriate disclaimers would be attached *and then the reports would be issued to the public*—long before any final report appeared. Later, the project team or its leadership would recapitulate the sections of the final report from these TUFORs as appropriate.

If anyone would have agreed to this plan, Saunders would have achieved peace of mind on two fronts. He was concerned that the ideas and findings of individual staff members would be ignored by Condon when it came time to issue the final report. And he was worried that the project was doing nothing to release information to the public or prepare them for whatever the final recommendations and conclusions would be. Unfortunately, there is no evidence that anyone paid attention to the idea, so another of Saunders's suggestions fell flat.

Low returned in September to a tense project with its normal mix of function and dysfunction. Field studies were flourishing and case book studies were not. Philip Klass's plasma-ball theories for UFOs were taking a beating by all the experts. Even Low wrote them off as irrelevant. A Marquette University physics professor named William Markowitz, in an article published in *Science*, argued that all extraterrestrial UFOs are a priori impossible (Markowitz, 1967a). Condon was impressed by these assertions and began a correspondence. In a later letter, Markowitz stated that the only reliable UFO report would have to be one made by a *well-known* scientist (Markowitz, 1967b)!

Saunders had his summit with Low and Condon for almost three hours on September 19. It was a private meeting and the reports of what went on differ. Many of the details are known, however. Saunders stated his view that if the project did not release any preliminary information, then it was either a colossal error (because he

expected the conclusion to be "probably extraterrestrial") or it was a signal that the report had already been decided in the negative. Not yet wishing to confront Condon with his suspicion that the negative scenario was preordained, Saunders suggested that they establish a plan for issuing carefully screened progress reports. This suggestion was emphatically rejected (Saunders & Harkins, 1969; Condon, 1968b; Low, 1968b).

The tone of the rest of the meeting is unclear. Saunders claimed that Condon did not understand what he was talking about nor what it was Saunders thought he should do about public expectation concerning the final report. Low said Condon did understand, but that Saunders was in an area which was none of his business. Condon himself claimed that he understood what Saunders was saying but did not agree with any of it. Perhaps this is irrelevant, but Saunders did not think so. The fiasco hinged upon the idea of evidence to support the extraterrestrial hypothesis, and what Condon would do if he found it. Condon said that if he had clear evidence pointing toward the reality of alien visitors, he would *not* announce it, but take it instead to the President's science advisor, Dr. Donald Hornig. In other words, Condon would allow only a negative conclusion in the final report. Condon had also previously told Craig that in such a circumstance he would call the Secretary of the Air Force (Craig, 1995).

Is there another resolution to this exchange? Practically speaking, it may well be that Condon had already fixed his mind on a negative conclusion. But this particular exchange with Saunders in Low's presence may have a slightly different interpretation. Clear evidence in support of the extraterrestrial hypothesis meant two vastly different things to these two men. Condon envisioned crashed disks, alien bodies, advanced technology, perhaps an unequivocally documented invasion of a high-security military facility. No set of puzzling cases, credible witnesses, films and photos would do. For Saunders, soft evidence was sufficient, especially when supported by his statistical work. Saunders saw the case analyses and statistics already pointing towards strong extraterrestrial probability, while Condon viewed them as weak at best. Low stood in the middle, just enough to see what both Condon and Saunders were talking about, but siding with his boss that progress reports were none of Saunders's business. Condon thought that Saunders had grudgingly accepted the decision on no advance releases, but that was just his taciturn manner. Saunders left the meeting convinced that Condon had made up his mind in the negative, and that any other suggestions would have to be made over his objections. He was correct, but possibly for the wrong reasons.

Further evidence of Condon's bias soon appeared. He had given an after-dinner talk at a symposium on atomic spectroscopy at the National Bureau of Standards just before the Saunders summit, and word leaked back to the project. Condon was back in his public mode of making the UFO field sound as ridiculous as possible by emphasizing the crackpot cases (McDonald, 1968a; Keyhoe, 1967). Several members of the audience were appalled and reported the latest Condon gaffes to McDonald, NICAP, and elsewhere. When Saunders brought the news and NICAP's reaction to

Low, Low responded with a groan of defeat (Saunders & Harkins, 1969). Low had been trying to hold together this mixture of personnel and philosophical differences, and had warded off all the Condon-created crises so far. Now Condon seemed finally to have done him in. At this moment Saunders was not overly sympathetic. The only solution—a statement by Condon that his views did not represent the project's and that project members were now free to express their own views—was again flatly rejected by Condon in a quick follow-up meeting. Condon then went out and gave another press interview praising Markowitz's article and criticizing NICAP's contributions to the project (McDonald, 1968a; Saunders & Harkins, 1969).

The next project meeting took place on September 27 without Condon but with Low. According to Saunders and Harkins (1969), there was an initial exchange about the problem where Low admitted that he *had* to support Condon publicly if he ever wanted to return to the administration. Low then left, and all day the rest of the team discussed the problems of Condon and Low, and what, if anything, to do about them. Allegedly, most of the staff were considering resigning en masse. Craig was, reportedly, the major holdout against this action, although in his memoirs he did not discuss the incident (Craig, 1995).

By October 1967 the major question was: Could anything worthwhile be salvaged? Condon did not seem to care much, although his attention was now turning to the structure of the final report. Low's amazing resilience seems to have allowed him to return to a sort of business-as-usual mode. Saunders and Levine continued their duties convinced that something else needed to be prepared as an antidote to how the project was proceeding. They began meeting with outside ufologists such as McDonald (McCarthy, 1975; Saunders, 1967c). The question of salvageability turns upon one's assessment of Low and whether he would ever stand up for what he felt was appropriate in the final report. Who can say whether he could have stood up to Condon, or at least finessed his extremism in some way, but on the issue of whether Low had a hopeful mind-set at this late stage, the evidence suggests that he did. In mid-October he gave a remarkable talk to the Jet Propulsion Lab at the California Institute of Technology (Low, 1967e). It was vintage Bob Low, affable, intelligent, lighthearted, joking about himself and the project, but insightful and serious as well. After discussing many of the outside problems they had faced, he loosened the audience up with whimsical remarks.

Discussing the project's naiveté: "Imagine yourself in November of 1966 and you suddenly have a contract . . . well, what in the hell do you do?"

On evaluating cases: "Nobody seems to understand what credibility is all about, and if you ask a psychologist it gets much worse."

On a witness' evaluation: "My husband and I knew that it was a UFO, because neither my husband nor I could tell what it was."

Throughout the talk, which was entirely upbeat, Low wove in interesting and revealing statements. He said that to investigate UFOs properly one must examine and analyze only the most puzzling cases. He said that many impressive people, including scientists, had seen unusual phenomena. He bemoaned the lack of scien-

tific data, but said that the payoff for getting it would be enormous. He worried about Congress not approving funds but felt that if the scientific community persevered beyond the Colorado Project it could get some better science done. He recounted the Betty and Barney Hill case respectfully and fairly. He showed a series of photos taken by Santa Ana traffic engineer Rex Heflin and said that he thought they might be real. Heflin was a solid character: "I would put him last on my list of persons who would perpetrate such a hoax." And regarding another famous photo: "I believe that McMinnville is even more impressive than this." He was very critical of Blue Book, saying that many so-called explained cases simply were only vaguely guessed at. Asked about two well-known debunkers, he hesitated, obviously torn as to what to say. Frankness won out. After a small verbal dance he admitted that he was not sure that Menzel's books constituted a serious study. As for Klass, the plasma speculator, he said: "I'm not sure that he has really contributed anything either." Returning to more pleasant topics, he showed a schoolboy's enjoyment in mentioning the Washington, D.C., radar cases of July 1952: "that one is my favorite . . . really very strange." He ended the session with a wide statement of praise for NICAP and the quality of their investigations.

CATAclysm

The state of the project entering the winter of 1967–68 was a group of individuals going forward with what they felt that they had to do, despite the emotional traumas that had beset them. There were at least three different activities: Some people operated almost as if nothing had happened (Craig, Hartmann, Wadsworth), Condon took personal charge of the planning of the final report with the assistance of Low and Hunter, and Saunders and Levine increased their contacts with outside ufologists (Keyhoe, Hall, McDonald, Hynes) to head off certain catastrophe. Saunders and Levine felt that something at least could be accomplished, if only in rebuttal.

A November 13 meeting between Condon, Low, and Hunter is informative (Colorado Project, 1967e). First, its purpose was to plan the structure and content of the final report, and Condon deliberately excluded all the senior staff. Second, Condon attacked the idea of the case book as well as the inclusion of classic cases in the report. Third, he insisted on including everything that the project had the slightest part in (even telephone interviews), no matter how ridiculous and insubstantial they might be. Then he unleashed his paranoias: He wanted personally to write a section on UFO literature, emphasizing that the authors were irresponsible liars: "The harm done via such 'intellectual pornography,' particularly among school children, is immeasurable." Low and Hunter apparently recognized the inappropriate character of this concept (and Condon's emotionalism) and tried to talk him into writing a separate article for a teacher's magazine instead.

The meeting ended with a rough list of subject chapters and authors, whose writing was to be completed by April 1. The significant points from this list were:

1. Condon reserved for himself the "Summary" (conclusions) and "Methodology" sections. In military contracts this is normal for the chief scientist, but in this case it was almost unethical, given his peculiar off-center involvement and subjectivity. Low was also earmarked for these sections, supposedly as an aid to Condon. However, he wrote nothing for them. Condon, as usual, got his way and even included his "immeasurable damages to school children" remarks in the Summary.
2. Condon assigned to his administrative assistant, Hunter, the other hot potatoes that he wanted to control—which cases were included, how they were written up, and whether there was evidence for a U.S. government conspiracy (something that should have obviously been assigned to Low)—suggesting that Condon no longer trusted Low to do things exactly as he wished.
3. Low was assigned five topics, but in the final report he wrote none. After about May 1968, Low was essentially a project nonentity, Craig had taken over most of the real project duties, and Low wrote occasionally about all the huge problems that had occurred.
4. Levine and Saunders were assigned three sections ("Plasmas" and "Radar Cases" for Levine, and "Statistics" for Saunders). Levine would also have to write up the cases that he investigated in the field. Saunders's role, given the centrality of his involvement, was shockingly small. Still, it illustrates the unfortunate concentration of his efforts on just the one thing that he wanted to do.

We can now compare this November 1967 plan to the final product that emerged by the end of summer 1968. Much of the list survived the intervening months, but many changes were made. In the final report, the following individuals were dropped: Saunders (fired), Levine (fired), Rush, Strentz, and Low. Rush's chapter was a minor one on instrumentation that he simply may not have gotten around to. Strentz was doing doctoral work on press coverage, and may also have been too busy. But the other three were major omissions. A list of the most involved senior staff members on this project at this time would include Craig, Hartmann, Saunders, Levine, and Low. To have a final report without the contributions of three of the five (and the two *most* central, Saunders and Low) illustrates the problems of the project and the final report (Gillmor, 1968).

Certain materials also did not show up in the final report: classic old cases, despite the fact that they were the cornerstone of Low's April 1967 position paper; press coverage (potentially controversial in supporting the idea that citizens do not get a clear picture of the phenomenon); conspiracy theory (definitely controversial); and power outages and UFOs (at least as a chapter; Low wrote a few pages on this which Craig inserted in his section).

Eight authors were added to the final report (Thayer, Roach, Mark W. Rhine, William Viezee, Vincent F. Lally, Blumen, Paul R. Julian, and R. V. Jones). The *subjects*

that were added, however, are indicative of what Condon was trying to accomplish. Despite the early insistence of Col. Hippler not to turn this study into a psychological report, Condon added two more chapters on psychology to the one already prepared by Wertheimer on perception. External reviews on optical mirages and balloons were added to the Stanford Research Institute review of radar and Altschuler's review of plasmas. All this would have been in accord with Low's April plan, but none of the reviews were applied to the case book of exceptional UFO reports, which never appeared. The presence of these reviews in the report, therefore, gives the impression of simple, though passive, debunking.

Roach, who had been an astronaut debriefer during Gemini, was asked to write a chapter on alleged astronaut sightings. Not much can be said about this except that the initial draft reader (Culberson) felt that the write-up was far too lengthy and padded with irrelevancies. He was told to leave all the padding in, because they wanted as much bulk as possible in the report (Culberson, 1968). Also, the former assistant director of intelligence for the British Air Ministry during and after World War II, R. V. Jones, had written a lengthy UFO debunking article in late 1967 or early 1968. Condon wanted very much to include it, but because it was not part of the project it was placed in an appendix.

It should be pointed out that although the "Statistics" chapter appears in both the November 1967 outline and the final report, a thorough study of UFO statistics by Saunders was hardly equivalent to the abstract intellectual commentary supplied by Paul Julian. Between November 1967 and final publication, the report lost significant amounts of its potential punch and added many debunking and negative elements. This is all the more unusual because the vast majority of the project's work was completed by November 1967.

What caused this major change of tone? A large part of that answer was "Jim McDonald." When the McDonald heard about the state of the Colorado Project, he was much agitated. He wrote to Michel that although Condon was a loser, the staff might be able to save the project (McCarthy, 1975). Still, it was important to plan ahead. He tried to get Congressman Roush of Indiana to hold hearings, but with the Colorado Project still going, there was not much general interest. He mentioned to colleagues and asserted in speeches that a "NASA-sized" effort was necessary.

At the same time, Condon was being quoted that "the whole business is crazy" (Michelmores, 1967). McDonald, uncharacteristically, bided his time for a while, and then, at the end of January 1968, blasted Low in a lengthy, heated phone call. He did not mention the notorious Low memorandum, which Levine had given him in mid-December, but he did allude to it in a follow-up letter (McDonald, 1968a). This was the first time that Low realized anyone outside the project knew about it.

Low went directly to Condon. Condon exploded. Saunders and Levine were called in on February 7 and they admitted giving McDonald a copy. Condon viewed this as theft, conspiracy, and unforgivable disloyalty. Saunders viewed the memo as a piece of project information in open files (it was), and relevant to the project's true constituents, the American public. Given Condon's absenteeism and Saunders's previ-

ous leadership in the project, he felt that he had as much right to make a decision on a thing like this as anyone. Condon fired them both the next day (Saunders & Harkins, 1969).

Following this cataclysmic event, a long sequence of social and scientific nightmares occurred.

McDonald wrote to Condon threatening him with an exposé (McCarthy, 1975). McDonald also wrote Frederick Seitz of the National Academy of Sciences complaining about the project and the memo (McCarthy, 1975). The project's administrative assistant, Mary Lou Armstrong, resigned in protest over Low's administrative misconduct (Armstrong, 1968). Condon ordered Armstrong to be silent about her views; she refused (McCarthy, 1975). Condon labeled Saunders and Levine "incompetent," but had to retract the statement (Condon, 1968c; McDonald, 1968c). Condon had earlier told Saunders that he deserved to be "ruined professionally," and he had to retract that comment as well. In the midst of this, Thornton Page wrote to Condon about organizing an American Association for the Advancement of Science symposium on UFOs (Sagan & Page, 1968); this threatened to turn into a showcase for Colorado's problems. Adding to the general disarray, an important project member was dismissed because of marijuana possession (Condon, 1968a; McDonald, 1968c).

McDonald began blasting the project in talks to professional societies across the nation (McCarthy, 1975; McDonald, 1969a–b), and a small stream of letters from academic scientists sympathetic to UFO reports began trickling into Condon's office (Colorado Project, 1968b). John Fuller and *Look* magazine published a sensational exposé of the project entitled "Flying Saucer Fiasco" (Fuller, 1968). Congressman Roush made a concerned speech on the House floor (McCarthy, 1975) and then began a General Accounting Office investigation of the project (Condon, 1968e). Several project members were quoted, negative to Condon, in the newspapers (Marvel, 1968). A libel suit was threatened by Saunders and Levine, and this was reported in the professional literature (Colorado Project, 1968a; *Scientific Research*, 1968).

Astronomer Frank Drake wrote to Frederick Seitz suggesting that the Colorado study be discredited (Drake, 1968). The university's deans and the president pressed Condon for answers to all this. *Science* magazine published a negative article about the project's problems (Boffey, 1968) and a nearly totally pro-UFO symposium was held before the House Committee on Science and Astronautics (U.S. Congress, 1968).

A general tide of dissatisfaction about Condon and the project was growing on all fronts (within ufology, among the general public, in the media, in Congress, and among some scientists).

Probably no scientist has had to face so many different types of hammer-blows as Condon faced after he fired Saunders and Levine. But it is difficult to feel too sorry for him. Condon did *not* act like a scientist on this job. In fact, he egregiously misbehaved. He lay back in his office fiddling with irrelevancies while the real work was done by others. He became emotional and paranoid about the subject, and allowed

that to enter into his actions and writing. He became unjustly autocratic and rejected the input of many of his senior and junior staff, who were far more involved. He deliberately and publicly made the subject of his half-million dollar grant appear ridiculous and beneath dignity, even though nearly his entire staff did not think so. He consistently opposed the cornerstone of his project administrator's plan (the case book), and, despite staff resistance, in the end blocked it entirely.

After the near disintegration of the project, his behaviors were no more admirable. He lost emotional control. He launched a series of name-callings and vindictive comments. Saunders and Levine were "incompetent" and deserved to be "ruined professionally." Hynek, whom he had recommended to write for the *Encyclopaedia Britannica*, was now a "kook." He used his new-found paranoia to construct an extraordinary excuse for all his troubles: Saunders and Levine had been plotting with NICAP all along to get him and ruin the project. He actually wrote this to Dean Manning in May 1968 (Condon, 1968f). Anyone reading the documents will realize how preposterous this invention is. Condon's own off-center and autocratic behavior predated every attempt by Saunders to inquire what was going on. Condon's mental state did not readily snap back to that of the jovial great scientist that his friends and colleagues remember. Two years later he was still considering blackballing Carl Sagan from membership in the prestigious D.C. Cosmos Club, because Sagan was too soft on UFOs. Hynek, he said, absolutely should be kept out (Condon, 1971).

BIAS

It comes as no surprise that a report written under the conditions described above was severely flawed. Actually, the parts of the report not overly influenced by Condon were reasonably objective. Hartmann's photoanalyses are perhaps the model for proper reserve and objectivity. Even here, though, Condon had some influence. Hartmann wished to make some reasonable points in his conclusions regarding poorly conducted Air Force investigations, and that the extraterrestrial hypothesis was at least consistent with the 2% or so puzzling unsolved cases; he also recommended further research. Condon wrote "Good God!" on the draft and crossed it all out (Hartmann, 1968).

The major offending area of the report was Condon's own summary (Gillmor, 1968). His conclusions and recommendations have little coherence with other sections of the report. Although containing statements discouraging school children from reading UFO books, most of the summary is toned down. Either he had a strong editor to inject some strategy into his phrasing of recommendations, or Condon himself had calmed down enough to realize that subtlety and cleverness was called for. So, while claiming that he did not wish to block persons from pursuing UFO research, he recommended that no funding or ongoing research be facilitated. Although knowing that many of the people who had attempted to look into UFOs believed strongly that the subject was a worthwhile one, he gave the impression that all

academics who look at UFOs decided that they are *not* worth researching. He stated that nothing involving UFOs has added anything to science, and is not likely to in the future, therefore there is no need for the government to sponsor research. He may have known that of the fifteen top staff members listed earlier in the article, at least twelve of them (Saunders, Levine, Hartmann, Roach, Armstrong, Wadsworth, Culberson, Cook, Hooven, Rothberg, Thayer, Ayer) definitely disagreed with him. It is probable that Low disagreed as well. If Condon *did not* know of their opposite views, he should have prior to publication or shortly thereafter, because they were all published externally or written in his own report or drafts.

Condon's summary gives one the impression that few if any UFO cases are in the slightest degree puzzling. Again, he wrote from his own bias and ignored even the evidence presented in his own report. It has been pointed out many times, including by knowledgeable, technical academics, how Condon's own report can be used as a *strong* case for UFOs as an important research area (Hynek, 1969; McDonald, 1969a; Sturrock, 1974). And one must remember that it was Condon himself who insisted on eliminating as many unsolved cases as he could from the report, because they were "old." This personal selectivity of data was certainly a peculiar interpretation of the scientific method.

Was this just a great old scientist with previously unsuspected prejudices and just the right domineering personality to roll over all persons, facts, and mores which would normally moderate such behavior? Perhaps, but there was something else going on here, too.

In January 1967, after the watershed Air Force briefing at which Low could not get Col. Hippler to tell him clearly what the project should be doing, Hippler responded (Hippler, 1967). The letter, written on his Pentagon office stationery, stated that these remarks were only his own opinions. Neither Condon nor Low believed that, as we shall see. His letter made the argument that Colorado should be able to come to an anti-extraterrestrial conclusion. Low rebutted that in his reply. But more importantly, Hippler stated how seriously the Air Force wanted to get rid of the UFO project at Wright-Patterson, or anywhere. He said that if they do not have enough time to make a "proper recommendation," an extension can be arranged. That would be far less costly than another ten years of Blue Book. Low thanked him for very clearly indicating what the Air Force wanted (Low, 1967c). Most astonishingly, Condon repeated the same ideas in a talk he gave two days after receiving Hippler's inside advice (*Elmira Star Gazette*, 1967). As far as the final report's conclusion that UFO reports would not require the Air Force (even in its scientific branches) to continue studying them, the fix was in by January 1967.

Simple logic says that a strong recommendation that the Air Force should get completely out of the UFO business would have had a difficult time coexisting with a conclusion that UFOs were still scientifically interesting, unless, that is, Colorado would recommend the Air Force out, but some other agency in. This was Low's solution to the problem. In October 1967 he was still hinting strongly that another organization should take this on. At Cal Tech he even spelled out N.A.S.A. to the

chuckling crowd. McDonald was simultaneously suggesting his "NASA-like budget" for UFO research. Others like Drake and Hartmann were suggesting continuing research somewhere in a government budget, perhaps at about \$1 million per year. Hynek and others were chiming in. In Condon's state of mind this was hardly welcome. In his copy of the House UFO Symposium proceedings, he underlined *every* instance of people talking about grants and funding, even in the few hundred dollar ranges. Equipment, when it was expensive, was also underlined. What explains this obsessive behavior? 1967 was the first of the so-called "doldrum years" of funding cuts for governmental big science. Congress had gone into one of its cyclical, parsimonious moods for science funding. Funding for anything would be tougher, yet here was a small furor gaining momentum, in Congress, toward the funding of something Condon considered dangerous.

Emotionalism, paranoia, don't-tread-on-me anger, orders from the Air Force, fear of failing research funds: How many sources of unscientific behavior do we need? The beginning of the Colorado project's final report, as written by its director, should stand as one of the worst cases of scientific bias documentable in recent history. The embarrassment does not stop there, though. Now that the deed was done, the establishment rallied around the honored old scientist. The National Academy of Sciences reviewed the study (as required in the contract) and wholly approved it (National Academy of Sciences, 1969). *Nature* happily reviewed the report as "A Sledgehammer for Nuts" (*Nature*, 1969). Famous Harvard astronomer Fred Whipple praised Condon for doing "a fine job" (Whipple, 1969). Smithsonian administrator and former CIA logistics man for the famous 1953 UFO-debunking Robertson Panel, Fred Durant, pronounced the report the "Gravestone for UFOs" (Durant, 1970). Famed MIT physicist Philip Morrison said that the report would stand forever as a monument to the scientific method (Hynek, 1970). The examples are legion. Almost certainly these admiring scientists had no idea what had been going on and generally did not know what they were talking about. But are scientists supposed to be making strong comments about things they know nothing about?

AFTERMATH

The Air Force got what it wanted: no more Project Blue Book. This was a great relief in many more ways than saving money. Without Blue Book, the UFO community had no focal point in the government to effectively query and keep interest up when things quieted. This especially affected NICAP which fed off governmental leaks, data, and cover-ups. NICAP, which was in a bit of decline anyway, rapidly shriveled and essentially died along with Blue Book. Public interest also fell off and another major UFO presence, APRO, began to fade. This latter was probably due to bad management (like NICAP, in fact) but without the Air Force, the UFO community had to carry all the weight. Even so, the years following the report were, paradoxically, a Golden Age of UFO research. Colorado had awakened many academics and intellectuals and they came, at least briefly, out of the closet with their interest.

It was a time which saw an AAAS symposium (Sagan & Page, 1972), serious interest within the American Institute for Aeronautics and Astronautics (AIAA, 1970), the emergence of Hynek as a civilian UFO research leader, the so-called "Invisible College" of academic researchers who gathered around him, and the establishment of the Center for UFO Studies (Jacobs, 1975). Also, at the grass-roots level, the Mutual UFO Network began its rise to become the largest UFO organization in the world. Ufology survived the Condon Report, even thrived for a while. This was because anyone who was interested in the subject could so easily see how incomplete and biased it was. But the report had one serious lasting negative impact on the academic community. It demonstrated to them that being sympathetic to UFOs was a very dangerous thing if one wished to flourish within the oft-closed corridors and minds of the Establishment.

SUMMARY

The Colorado Project was beset by many problems which destroyed its ability to produce a competent and objective study of the UFO phenomenon. It was led by a scientist with little interest in the subject who became remarkably biased toward it as the research year progressed. This prejudice, embellished by direct requests regarding procedure and conclusions from the Air Force, continued to affect even the materials and wording of the final report. The project's real administrator, Robert Low, was unable to distance himself in any effective way from Edward Condon's biases or the Air Force's demands. His own administrative failings created the central research problem of the uncompleted case book of unexplained UFO reports, upon which the entire methodology of the project was allegedly based. David Saunders, as co-principal investigator, was not free from blame for this critical oversight either. The insufficient time frame of the grant mitigated against a substantial contribution to knowledge in this difficult study area. Time, ignorance, prejudice, and mismanagement combined to produce a bulky and inflated document that upon examination reveals itself as a classic example of pseudoscience supported by the name of a famous member of science's inner establishment.

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On a final note: Edward Condon almost blocked the ability of researchers to study

the Project and to write articles investigating it. He blocked Jim McDonald from seeing the files even after the project was over. He blocked Paul McCarthy from seeing them as part of his Ph.D. work, and told him that he had personally destroyed them. He also blocked one of his own researchers, Gordon Thayer, in the same way. Fortunately, his emotionalism and unscientific behavior in these matters did not extend to the atrocity of actually destroying them (Condon, 1973). For that, at least, we thank him.

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