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DEPARTMENT OF PHYSICS AND ASTROPHYSICS

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Dear Carl:

This is in reply to your letters of 24 and 29 September. I hope it can also serve as replies to Thornton Page's letter of 23 September and Phil Morrison's postcard of 29 September. I will try to clarify my position and hope to escape from further correspondence on this subject.

I thought a great deal about the idea of a AAAS UFO symposium last year when it was proposed for Dallas, and again this year after seeing in Science for 15 August that it is proposed again for the 1969 Boston meeting. I remain convinced that under the given circumstances such a symposium will not contribute to better popular understanding of science, or of scientific method (though it could produce wider public knowledge of the less praiseworthy traits of some scientists). Therefore I think it ought not to be held. In any case, I have made a personal decision not to take part, based on the view just stated, and on the fact that my findings are fully available in the published report, and that participation would bring me into contact with individuals in whose integrity I lack confidence.

Therefore, as I wrote to Walter Roberts on 5 September, please "include me out."

This letter could end here. But I do have respect for the good intentions and integrity of those to whom copies are being sent, so I will try to be more explicit. In doing so, I hope no one will say that I am "emotionally involved", unless they mean by this having a sense of weariness and boredom, and regret for time wasted and for direct experience of dealing with persons of unbalanced perspective.

I did not want to do the UFO study but was talked into it in August 1966 by staff of the U.S. Air Force Office of Scientific Research, largely on the basis of appeals to duty to do a needed public service, which sounded like the appeals being made now for me to take part in the proposed symposium. So I became committed and immediately received the commiserations of good friends for the fix I was in, and large numbers of job applications from persons who were plainly quite kooky about UFOs.

Contrary to numerous accusations, I did not approach the study with a bias against the hypothesis (ETH) that some UFOs come from extraterrestrial places

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as visitors from other civilizations. The only "bias" of which I am aware, was against accepting conclusions without valid supporting evidence. I would love to discover visitors from other worlds, but I would hate to announce such a discovery without adequate data to back it up! Nor do I indulge in vague and misleading claims that ETH is the "most probable hypothesis" to explain UFO sightings. Nor am I impressed by the number of the reports: if they are unsound or deceptions, they do not become true by being numerous.

I want to narrow the focus to ETH because that is what gives the subject its great popular interest. Much needs to be done to improve our understanding of atmospheric optics and radar propagation anomalies, but let us not kid ourselves that the public is responding to that need. We need more serious research on these subjects, as we do on the psychology of distorted perception, cognition, self-deception and chicanery, but understanding of these subjects will not be advanced by running around interviewing people who report seeing strange lights in the sky. I am sure of this with regard to physical phenomena. Several psychiatrists with whom I have talked feel that such UFO study is no help to understanding the psychological problems involved.

Very soon after starting the near impossibility of obtaining objective UFO data -- our own photographs, spectroscopic data, even our own visual experience -- became vividly clear. Observer network schemes would be inordinately expensive with no promise of success. Experience with checking on stories of untrained and excitable observers showed what little value attaches to interviewing the sighters. And there is nothing else to do. My quick recognition of this fact does not show prejudice. It merely shows that I am capable of learning what is pretty obvious. Nevertheless we worked hard, putting teams in the field to learn what we could, especially because the very heterogeneity of the cases makes it hard to be sure of any firm conclusion. Hynek hopes that there is a signal in all that noise. There certainly is a lot of noise. I see no evidence that a signal is there, but I do not say for sure that there is none. I do say, emphatically, that I do not feel that there is likely to be one, and I feel certain that if there is, it will not be found by any of the methods of UFO study I have heard suggested so far. The burden of proof is on the believers to come up with a practicable scheme for finding the signal if it is there.

I am reminded of Project Ozma, when we listened for signals but sent none, because to send would exceed our budget, apparently unmindful of the possibility that those other presumed civilizations might not be sending because they have budget difficulties too!

During the spring of 1967, James McDonald was campaigning around the country giving UFO speeches to many audiences. A fair sample is the 28 page, single-spaced document prepared for presentation in Washington on 22 April 1967 to the convention of the American Society of Newspaper Editors, which is entitled "UFOs: Greatest Scientific Problem of Our Time?" Toward the end he declares, "There is, in my opinion, no sensible alternative to the utterly shocking hypothesis that the UFOs are extraterrestrial probes from somewhere else."

Of course, he has a right to his opinion. But also I have a right to my opinion of his opinion, especially after the amount of study I have devoted to the subject, including discussions with him and hours-long presentations by him of his views to the staff of our group, myself included. (He complained to a

staff member that I had dozed off three times during his talk, but the staff member told him he should be pleased that I woke up twice.) Although extremely energetic and diligent at it, he has done nothing, so far as I can make out, but reinterview persons who tell marvellous stories about what they say they have seen. He went to Australia, Tasmania and New Zealand in the summer of 1967 at US Navy (ONR) expense, ostensibly for another purpose, but found time to interview some 80 UFO sighters in three weeks, and in addition, to make an Australian broadcast vigorously attacking our government for not doing more UFO study.

During the summer of 1967 I was troubled to realize that one of my staff, David Saunders, was showing a strong tendency toward uncritical acceptance of unverifiable features of the reports. He was diligent in advocacy of ETH. In violation of staff policy that no speeches were to be made without my approval, he arranged to talk in September to the American Psychological Association in Washington on the psychology of how we should start to prepare the American public for acceptance of the great finding of visitors from elsewhere. At my insistence he withdrew from that program, but he advocated strongly that a large part of the effort of the project be diverted to study of this problem, implying that it was already a foregone conclusion that we would have an announcement of the actuality of ETH to make. I demurred on the ground that I saw no sign after nearly a year of work that such would be the outcome, and in addition that molding public opinion was not part of our job. I think that to him this was a rather clever ploy to attempt to get the project committed to making such an announcement.

On the subject of uncritical acceptance of uncertain data, at which flying saucer buffs are talented, I have among my souvenirs a mimeographed invitation to a meeting of the Denver UFO Society which says, "Some of you have had sightings! Come and tell us about them. We will not test you for credibility."

Some time in the fall of 1967, a staff secretary (who was allowed to resign in February, 1968, after admitting that she was a regular user of LSD, and who was also a great believer in ETH), discovered in the files of Robert J. Low a foolishly indiscreet memo which he had written during August 1966, without my knowledge until much later. She made unauthorized copies and showed it around to the staff, Saunders included, but not to me. Saunders had been a member of NICAP even before the study started, and was acting as our liaison man with Donald Keyhoe, the director of NICAP. Much later I learned that Keyhoe had a copy of that memo, but can only surmise how he got it. Apparently many copies were circulated among buffs, chain letter style. By this time the buffs realized that we were not likely to come out with what they call a "positive" report, so they had already started to plan to discredit the study as much as possible.

About 1 February 1968, McDonald wrote a long letter to Low, sharply criticizing the way the study was being conducted. In the course of it he discussed the Low memo. This revealed to me for the first time (a) the existence of that memo, (b) that it had been taken from Low's files and copies circulated without his knowledge or permission, and (c) that McDonald was among those in possession of a copy.

The next day Saunders and Norman Levine admitted having given a copy to McDonald and were discharged. Aside from the specific deed, the incident proved

conclusively that they were not loyal to the University in the conduct of the study.

By telephone I requested McDonald to return his copy of the memo, but he refused. I appealed to his institute director, Dr. Richard Kassander, and to the president of the University of Arizona for help, but they were unable to get McDonald to return it. He argued, probably correctly as I now realize, that by then so many copies were in existence that his having one could make very little difference. Since the memo contained no scientific data of any kind, having been written before the study was undertaken, and therefore contained nothing of use for scientific study, it could only be useful for controversy. The incident shows the level at which McDonald operates in zealous support of his "utterly shocking hypothesis".

A few days later McDonald had his attorney write to me threatening a libel suit, alleging that I had said things about him that I had not said. At that time I decided to have as little as possible to do with McDonald in the future.

As you all know, this memo was exploited several months later by Look magazine in a scandal story by John Fuller. Coordinated with this, Keyhoe staged a major press conference in Washington attacking us, in which he discussed the strategy of how best to discredit our work. At the same time I was harassed by the news staff of Science which insisted on sending Boffey out to smell around Boulder to maximize the scandal. This was done despite personal appeals to Abelson, Wolfle, and Roberts not to allow Science to be used for scandal mongering. But freedom of the press was preserved. I sent in my resignation as a member of AAAS, of which I was president in 1953.

Locally Saunders and Levine had summonses served on me for libel suits, which were never pressed, but a good deal of time and energy had to be devoted to this brush fire. With a depleted staff, I set to work to complete the report, being fortunate in having obtained the services of Dan Gillmor, a distinguished New York science editor, who dropped other work for this task. He also resigned from the AAAS in indignation at the way Science had joined with Look and NICAP.

The report was delivered to the Air Force on 15 November, reviewed favorably by a distinguished panel of the National Academy of Sciences, released by the Air Force in January and made widely available as a paperback by Bantam Books. Then came the denunciatory shrieks of the few UFO believers. McDonald says he has studied the report with great care. A friend says that he has done me the honor of saying that the report is having a "devastating" effect on the prospects for the major federal support for further UFO study that he advocates. In one speech he said the cost of the UFO study that he wants should "dwarf" that of the space program.

During the spring of 1969 McDonald has made many speeches passionately denouncing the study, as he has a right to do, but I also have the right to disagree with his judgment. In one of his speeches, circulated to a personal mailing list, he says, "I urge all of you to secure copies of the Condon Report and to study it carefully." I agree, but like some theater advertisements, this is taken out of context! I thoroughly disagree with what he says before and after.

Flying saucer enthusiasts tend to regard no one as properly qualified on the subject of UFOs except those that agree with them. For example in a letter dated 19 March 1969 Hynek says: "I have been shocked to learn, for instance, that Markowitz has been asked to be a Sigma Xi lecturer this year on 'UFO Mania'. Sigma Xi in the past has always had lecturers who have an established proficiency in the field they are discussing." Although I think Hynek may have that attitude toward any speakers you might find who do not believe wholeheartedly in ETH, or in a large federal subsidy for further UFO study, he or McDonald may not openly say so. To get an idea of McDonald's concept of balance, look at the Symposium on Unidentified Flying Objects, held by a subcommittee of the House Committee on Science and Astronautics, on 29 July 1968, chaired by J. Edward Roush, a now defeated Congressman from Indiana, which McDonald largely planned. In this case I suspect that they are willing to trade presentation of a little dissent from their views for the enormous advantage to them of being allowed to spread their thoroughly unscientific views at a big occasion like an annual meeting of the AAAS.

I am sure that you and Page mean well, but this enterprise is a mistake. I hope that you will not push it, but that if you insist, that the AAAS Board will say no even at this late date.

That is all I have to say. I hope not to have to devote any more of my time to this silly nonsense. If people will only read it, I am sure that our report will stand on its own merits.

Sincerely,

E. U. Condon

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