

NORTHWESTERN UNIVERSITY
EVANSTON, ILLINOIS 60201

DEPARTMENT OF ASTRONOMY

LINDHEIMER ASTRONOMICAL RESEARCH CENTER

15 April 1968

Dr. J. E. McDonald
Institute of Atmospheric Physics
University of Arizona
Tucson, Arizona

Dear Jim:

Allow me to congratulate you on the fine treatment of Klass's book on the plasma theory. I believe you have beautifully demolished his arguments (although one might say, "what arguments?" since they are so obviously the product of fuzzy thinking and poor physics) and I hope that you will have stemmed the tide of acceptance of Klass' by the great many who are incapable of looking closely at what he says.

I think we can now justifiably use Jacques Vallee's classic statement, "Klass dismissed" with good reason. I have just been informed of the forthcoming LOOK article* and have talked to both Saunders and John Fuller about it. It will be most interesting to see what the overall reactions to the article will be and I am awaiting the publication date.

Coming back to your excellent dissertation which you were kind enough to send me, I was particularly struck by the remarks concerning the apathy and slowness of acceptance of the UFO problem as a scientific problem by the elder statesmen of science. Perhaps you can now realize somewhat better than when we first met my situation years ago when I realized the futility (at that time) of bringing the problem out into the open. One must remember that at that time the data were by no means as impressive as they are now, if for no other reason than that of the great weight added by the persistence of the phenomenon over these past years, and particularly the large numbers of good reports in just the past two or three years, from this and other countries. If, therefore, you still find it difficult to gain a serious hearing with a far more numerically significant set of data, and a climate of opinion toward the UFO problem which today finds many scientists (anonymously) seriously

* It is being kept
confidential, of course.

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willing to admit that a problem exists, it is quite clear, to me at least, that for me to have waged a one-man crusade in the fifties, in the face of the overpowering dignity and weight of the Robertson Panel, would have been foolhardy, and would have resulted only in a scientific tar and feathering which would have totally and completely obliterated any future effectiveness. Maybe you still won't agree, but there are many colleagues of mine that do. I think only now has the climate become temperate enough to allow for a frontal attack. (You do not attack Russia in the dead of winter).

Sincerely yours,



J. Allen Hynek
Director

JAH:lp



THE UNIVERSITY OF ARIZONA

TUCSON, ARIZONA 85721

INSTITUTE OF ATMOSPHERIC PHYSICS

April 27, 1968

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ASTRONOMY DEPARTMENT
NORTHWESTERN UNIVERSITY

Dr. J. Allen Hynek, Director
Lindheimer Astronomical Research Center
Northwestern University
Evanston, Illinois 60201

Dear Allen:

Thanks for your letter of April 15, and its comments about my CASI talk. I shall probably write a more detailed critique of Klass's book, because there have been so many favorable reviews in various magazines and journals that he cannot be ignored. In many ways his arguments are even less reasonable than Menzel's, yet most of the reviews are quite laudatory.

I note that you are aware of the forthcoming "Look" article, which should be out by the time you receive this. We put a great deal of effort into all of that and I only hope that it serves to forestall the kind of superficially negative report that Condon was evidently planning to prepare. But just what final effect the "Look" article will actually have remains to be seen.

No, I am afraid, Allen, that I cannot agree with the remaining remarks of your letter. And my disagreement centers mainly upon the observation that the principal reason for the adverse climate of opinion about UFOs has always been the official Air Force handling of the problem. And since you have for so many years been the one scientific consultant to whom the Air Force has repeatedly turned for critique, I have to state quite frankly that I believe that you have been a major part of the very problem which you lament. When you emphasize the occurrence of a number of impressive reports here and abroad in the last year or two, you overlook a record of equally impressive cases running back to 1947. I have recently had an opportunity to go through some of the Grudge and Bluebook reports, and want to emphasize again what I said to you in your office in June, 1966, namely that I find it impossible to understand how you could have looked at such cases and come away with any other conclusion than that something of tremendous scientific importance lay within the UFO reports. Case after case back in those early years stands as evidence that the boat was missed already by 1950 and I don't think that either you or I can put the blame on the scientific community for its ignorance that there was a boat departing. With all of

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those statements that have emanated from Air Force sources, frequently with your name woven into the press releases to the effect that there was nothing to all the talk about UFOs, nothing of scientific interest, and nothing that wasn't receiving high caliber scientific attention, how could the scientific community, public, and Congress draw any conclusion other than the one that they did? You are the one scientist who was in a position to change all that.

Hence, I have to reiterate my June, 1966, rhetorical question: How could you possibly have missed the scientific significance implicit in all those cases in the late forties and early fifties? Every time I go over new facets of that early history I am reminded of all of these points that make me most unhappy with your negative contribution to the UFO problem. I certainly acknowledge that in the last year or two you have made positive contributions by your public statements, and I hope you will continue to make even stronger remarks in those directions. But the intimations, in your letter of April 15, that through the preceding 15 years you had no other course, I simply cannot except.

I would like to discuss all of this with you in much more detail sometime, face to face. I still wonder about your evaluation of many of those early cases. Even today more than a decade later it is possible to locate the principal witnesses, as I have done in a lot of them (and as you could have done 15 years ago far more easily and in an official capacity) and some are stunning. Ted Blocher's book on the 1947 cases tells the same story. There may well be some aspects of that early history that I do not yet appreciate, despite a lot of effort spent in trying to get maximum information about that period; but as I see it now, you are going to have a lot of trouble convincing anybody who really studies the record that scientific significance of the UFO problem remained obscure until about 1965. It was entirely clear before 1950 as I read the record!

I had a brief note and some material from Jacques a few days ago. It is good to realize that he may be keeping his hand in on the UFO problem, despite a rather silent period of recent months. I hope that Thornton Page succeeds in getting a AAAS UFO session in Texas in the fall. That could be a useful step in getting the UFO problem squarely before the scientific community.

Sincerely yours,


James E. McDonald

P.S. Could you send me Jacques' current mailing address? I threw away his envelope before realizing it was the only place he had given his present address.



THE UNIVERSITY OF ARIZONA
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C²
Bluebook
Eval.

INSTITUTE OF ATMOSPHERIC PHYSICS

July 11, 1968

Dr. J. Allen Hynek
Department of Astronomy
Northwestern University
Evanston, Illinois, 60201

Dear Allen:

The misconception that any atmospheric inversions of types known to meteorologists could conceivably cause total internal reflection is one of the most preposterous ideas that Bluebook has seized upon over the years. As I've said in several of the reprinted talks that I believe I've sent on to you, astronomers would scarcely be able to operate if atmospheric refractive-index discontinuities could impose the kind of disturbances that Menzel and Bluebook have intimated.

I enclose a copy of my Montreal CASI talk, although I think I sent you a copy earlier. On pages 16-18, you will find some quantitative discussion of that problem, which may be of reference-use to you.

I simply do not understand how Donald Menzel could, as a person reasonably familiar with the physics of light propagation, make the kind of assertions he has repeatedly made about "mirage effects". Mirage phenomena are limited to an extremely small angular departure from the horizontal in all light rays involved. The notion that miraging or "reflection" effects could seriously distort the image of anything like an airplane or a balloon and utterly mislead reasonably reliable observers at angular elevations exceeding a degree or so appears to be quite unlikely. As a matter of fact, most mirage phenomena are seen at angular elevations of only a few tens of minutes of arc departure from the horizontal.

For example, an inversion layer that is 10 meters deep in the vertical with a lapse rate of 0.2 degrees per meter, increasing upward, and which extends over a horizontal distance of 25 kilometers or more, is capable of so bending the incoming light rays that an object which lies on the observer's geometric horizon is imaged by the refracted rays incident on the observer's eyes as an apparent elevation of five minutes of arc above the horizontal.

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I'll probably cite some such figures as listed on 7/29, by the way

The amount of that refractive bending, 5 minutes in the above example, increases only as the square root of the product of lapse rate and "duct" thickness. Hence to get, say, 50 minutes of apparent refractive displacement, requires either an inversion 100 times deeper or having a temperature increase rate 100 times greater than cited above or any of an infinite number of other possible combinations whose product is boosted one-hundred-fold. Putting that latter point in a different sense, that carries useful meaning, one would require a "duct" across which the temperature difference in the vertical amounted to 200 degrees Centigrade (rather than 2 degrees Centigrade) to secure 50 minutes of arc. Inversions of that intensity are, as far as I know, entirely unknown within our atmosphere. And yet, you will note that, out of those latter figures, would come a miraging displacement of just under 1 degree. Menzel is utterly careless about his discussions of such matters and leaves his readers with the impression that one can be looking almost at the zenith and get total internal reflection out of atmospheric discontinuities. The Tombaugh sighting of 1949 is so interpreted by Menzel, and I have used that several times as an example of how far off base Menzel is.

Evidently in the March 6, 1968, incident near Amarillo that you wrote about, the witnesses also saw the unknown at some quite high angular elevation. The 3.5 degree Centigrade inversion (a very reasonable magnitude for a dawn inversion near the ground) is so far from being capable of doing what Bluebook would evidently like to claim that it is saddening rather than amusing.

I hope that the above comments, plus those contained in my CASI talk, will be of use to you. I intend to write a paper for one of the meteorological journals on this type of problem in the near future.

Evidently Bluebook wants to claim that the Amarillo temperature inversion distorted the image of the passing aircraft, as they went overhead. That's utter nonsense.

Commenting briefly on your opening sentence, I'd like to emphasize again how very much I would welcome the opportunity to sit down for a quite long discussion of the points of disagreement that you and I have over interpretation of the earlier history of the UFO problem. In a sense, the very matter that we are discussing in the above is an example of one of the things that continues to trouble me as I look back over the history. None of the optics involved is the least bit subtle or abstruse; and it is my feeling that Menzel and Bluebook ought to have been straightened out on this by you years ago, just as quite a number of other Bluebook misconceptions ought to have been eliminated years ago. In recent weeks, I've been going over Bluebook

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Reports Nos. 1-12 (the old monthly reports), in connection with the NICAP publication thereof. I think the hoped-for publication deadline was yesterday. These, and the old Grudge reports that I'm also in the process of going over currently, remind me once again of these points that I have made to you in the past. I don't take any special delight in remarking on such points, either here or in talking to scientific groups, but until it's been made clear that no one affiliated with the Bluebook effort was really digging into the scientific questions posed by this tremendous wealth of sightings, the scientific community will remain seriously misinformed. I think the scientific community is still misinformed, in that sense.

Now, if I am somehow misconstruing the real history of your involvement in Bluebook, or unfairly weighting the factors that I have built into my interpretation, I very much want to be straightened out on this. I mean that most sincerely, Allen. I had hoped, in that last trip to Chicago, that I would have had a spare day that you and I might have used to talk for the better fraction of a day about such matters, getting down to a lot of details and thrashing all of this out. I still want to do that. As I see it, you have not set the record straight on these points, and owe it both to science and to the years of efforts of groups like APRO and NICAP, etc., to set that record straight. Your position as the principal scientific consultant for the 18 or 19 past years of Air Force consideration of UFO matters was a position of scientific responsibility of a most unusual nature. I think that all of the points that I made very bluntly to you back in June of 1966, when we first met in your office, still seem to fit historical facts as I now see them. You evidently put a considerably different interpretation on that history, and I would heartily welcome a chance to hear you out at great length on all this.

Every time we've had a chance to discuss this, it has afforded far too little time to do more than open the discussion. That has been very unsatisfactory; I wish we could get together for something like a whole day on it in the near future. Is it possible that we could meet in Chicago the day after the coming Congressional session? I'm going to be tied up the day before, but could perhaps arrange to get into Chicago early on Tuesday and stay through the day, catching a plane to Tucson at the end of the day. Since your secretary mentioned that you are probably going to be interrupting vacation plans to get to Washington, I realize that this may be a very inopportune time to suggest. But I do want to emphasize that I look forward to a chance to discuss all this at very great length with you. If I am off-base, I want to be straightened out; I really am not happy about the continuing differences of opinion and interpretation that set our positions apart. The UFO problem is so important that it warrants our mutual efforts to take whatever steps are

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necessary to minimize the divergence of our viewpoints.

I want to say again how pleased I am that you are going to be able to make it to the July 29 session. One never knows about such things, but I am hoping that we'll have a salutary effect on the climate of thinking about UFO matters in Washington, by appearing before the Committee.

Best regards,



James E. McDonald

JEM/msr
Air Mail

July 26, 1971

Mrs. James McDonald
3461 East Third Street
Tucson, Arizona

Dear Mrs. McDonald:

It is rather difficult for me to write this letter. Jim and I were associated in the whole problem of UFOs (in which area we didn't always see eye to eye, as he possibly may have indicated to you), but his passing was a decided shock to me and to all his friends. The world has lost an astute scientist and a fearless one who never hesitated to speak out about what he thought was right. I am not adept at writing letters of condolence so let me simply say that I feel his loss personally and wish it all could have been otherwise.

Several of my scientific colleagues have suggested to me that I write to urge you to make sure that his papers and records not be lost. It has been further suggested that, in the UFO field, his writings be brought together in one volume and published in the interests of science and the history of science. The record of his trail-blazing should not be lost.

Whether I am the one to suggest this, or undertake it, is another matter. The important thing is that the record of his investigations, and his writings, be kept together and not parceled out, a little bit here a little bit there.

It may be that you have already made arrangements in this direction and if so, my colleagues and I will be happy to learn of this. In any event, they should not be made the private property of any one individual or organization without the understanding that his researches will be available to responsible scientists.

I am enclosing a copy of a recent letter of mine to PHYSICS TODAY which is self-explanatory.

Northwestern University has an observatory near Las Cruces, New Mexico, and I have occasion to visit there frequently. If agreeable to you,

Mrs. James McDonald

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on my next trip to New Mexico, I could easily make a run over to Tucson and talk with you about this personally. I do not wish in any way to be presumptuous and I wish to assure you that my only interest is that Jim's work not be lost. I would much appreciate learning of your wishes in this respect.

With best personal regards,

Sincerely yours,

J. Allen Hynek

JAH:agl
Enclosure