Quo Vadis?
Science and The UFO Problem

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paper presented at the American Institute of Aeronautics and Astronautics (AIAA) meeting
Pasadena, CA , January, 20, 1975

You have been exposed to a series of interesting treatises on the problem. In the concluding paper of this symposium, I wish to address a question which, I believe, the international scientific and engineering community must now ask itself: "Quo Vadis? — Where do we go from here?" Are we going to continue indefinitely to collect qualitative (or at best, pseudo-quantitative) reports?

Are we going to let these reports accumulate in nonscientific publications, in nonscientific terms with nonscientific interpretations until we have, after another 20 years, 100,000 of these reports instead of the 50,000 that now exist?

Are we going to perpetuate a situation in which this material is merely considered as entertaining reading rather than as a data set accessible to the scientific community?

The time has come to recognize that sufficient observing material exists to answer the question whether or not the UFO phenomenon presents a legitimate scientific problem and how it can be defined in clear scientific terms.

I submit that this has not been done and that, as long as it has not been done, all attempts to interpret the phenomenon will remain speculative in the eyes of the scientific community.

As you may know the AIAA formed several years ago a UFO Subcommittee of its Space Physics and Atmospheric Environment Committees. If we have learned anything in our Committee work, it is that questions must be asked and answers given in the right order.

The Present Situation

Before one tries to answer the question what UFOs are and where they come from, one has to establish whether they represent a real or imagined scientific problem. This can be decided only on the basis of the *observational* material, but it cannot be decided on the basis that possible *interpretations* are acceptable or unacceptable. For example, it is not scientifically sound to reject the existence of the UFO problem by showing that the odds against the extraterrestrial hypothesis are excessively high. This can serve to reject a specific hypothesis, but not the problem itself.

We have also in our Committee work learned why the Number-One-Question, namely the existence or nonexistence of a defined scientific problem, has so far been neglected.

It is important to understand the reasons and they have to do with certain groups of individuals who concern themselves with this vexing but intriguing problem.

Among scientists and engineers there are three groups interested in this problem. Their respective sizes are directly proportional to their ignorance of the subject matter.

The first group comprises a fairly large fraction of the total scientific community, while the members of the third group can be counted on the fingers of your two hands. In between, there is a moderately small group which has made positive efforts to gain sufficient insight to arrive at an assessment of the problem. This group is quite knowledgeable compared to the large first group, but quite ignorant compared to the small third group. The members of our committee may be counted in this middle group, while several of today's speakers are among the exclusive club you may count on your fingers. I also suspect that many in this audience may fall into the first group (or they would be at lunch now and not in this room).

This large first group consists primarily of scientists and engineers who have heard about the problem and are — passively — interested in it but reserve judgment. Used to orient themselves through scientific journals on any problem of interest, they are frustrated by the complete lack of scientific discussion in the recognized journals and by the inaccessibility of the original observing material. They notice that the vacuum in scientific literature is compensated by a flood of pseudo-scientific books and articles in newsstand magazines. (Scientists are generally not prepared to collect *this* type of data from "Playboy" or "Enquirer".) Of course, there are exceptions and one of our speakers here has probably written the most informative book at hand.

It should be understood that, as long as data are not presented in a digested form in scientific journals proving the existence of a well-defined scientific problem, the first group will preserve its curiosity but also its suspicion that wishful thinking is at work.

Before discussing the role of the second group, let us see what the members of the third group are doing. Most of them have spent an enormous amount of work and intelligence on this problem. They have tackled the first question long ago, having convinced themselves that the problem is real and are now intensely working on the second question, the interpretation of the phenomena. They are bored with the first question which the other groups are still wrestling with. This is understandable but regrettable, since there is tremendous talent in this small group. No scientist has been successful who has only satisfied his own curiosity and has convinced himself and his friends of his findings without making the very laborious effort of formulating the problem and his findings in a convincing way, convincing also to his peers. We all have to obey this scientific ground rule. The cry "I have convinced myself on the basis of long years of effort" goes unheard among scientists and that is how it should be.

I admit that the formulation is especially difficult in this case in which quantitative data recording is almost completely absent. Unfortunately, the treatment also has to be of exceptionally high quality in view of the complexity of the problem and the discredit it has suffered in its strange history. I see no way to break the impasse without making this effort to prove the reality of the issue as a scientific problem and its proper definition.

With the first group standing passively by and the third group forging farther and farther ahead (some call it "far out"), it seems to be left to the second group, which I may call the "Assessment Group", to bridge this ever-increasing gap and the existing vacuum in the scientific literature. The alternative is to continue the present ambiguous situation indefinitely.

This brings me to the second part of my talk, namely to the question of how to go about this, but before addressing this question, I must apologize for not including in my discussion two more groups which are active in this field but work primarily outside the scientific and engineering community. One is quite large and consists of national and international organizations devoted to the collection and follow-up of UFO reports. They are most important as a source of data but the sometimes unscientific claims of some of them have not always helped to clarify the issue.

Quite apart from this group there is the vocal group of emotional fanatics — both pro and con — whose main contribution has been to obscure the issue. I believe that a scientific solution to the problem has to come from the scientific community and this is why I did not mention the last two groups in the connection.

The Next Steps

Here I must start with an assumption. The assumption is that question #2, "What are UFOs?" can only be solved by a broad multidisciplinary effort and that such effort will require major institutional and financial support. In other words, I doubt that the spare time efforts of even the most capable and energetic individuals will suffice to supply an answer which can be accepted by the scientific community at large. If this assumption is correct and the source of such support is public funds, proposals for research grants will have to meet favorable consideration not only by the supporting agency but also by reviewing members of the scientific community, and this under a presently unfavorable climate. This will lead immediately back to question #1: "Is there a real, not imaginary, scientific problem, and what is its definition?" If the answer to this question also requires this type of external support, then we are in a "circulus viciosus." Indications are that the vicious circle holds at present. I would therefore like to make some suggestions how to break it.

The most efficient way to attack this question is to use the large existing observational material and to subject it to a carefully designed *statistical analysis*. The studies of Saunders and by Poher and Vallee presented in this Symposium seem to move in the right direction. To work, on the other hand, on new cases with improved methods— desirable as this may be — would in all likelihood require an effort beyond individual possibilities and may actually lead into the same trap in which the Colorado University (Condon) study was caught. Figure 1 illustrates this situation.

According to the best assessment available, only about 5% of the existing reports can be termed "unidentified" and only a fraction of these is of sufficient "credibility" and "strangeness." As a consequence, 95% of the efforts extended to new cases may be wasted and the actual number of "interesting cases" will not provide a statistically significant sample (although it may produce some observational material available now and already computerized in two or three places is so large (order of 50,000) that even the small fraction of "unidentified" cases represents a large number. Furthermore, the "probability" (or "credibility") of the unidentified observations — remarkably enough — does not fall off with increasing

"strangeness", as one would expect. (See Figure 2.) Therefore, we have a significant sample to work with.

What needs to be done then[1] resembles what is presently being done in the atmospheric sciences in an equally difficult and controversial field, namely in weather modification. If the "modified" results differ from the "expected" (normal) results significantly, for example at the 5% significance level, then the modification method is generally considered successful. Refined methods have been developed for this specific field and may give some guidance for the problem at hand.

The distribution of cases as shown in Figure 1 seems to offer itself readily to this type of analysis if applied to the "unidentified" residue with high P/S values on one hand and to the "identified" sector on the other. It is then a question of selecting the proper parameters and determining their variance in the two groups. (Poher and Vallee have tried something along this line.) Such an approach will provide the answer to the basic question: Can the small residue of unidentified cases be considered as noise of the identifiable cases, or does it present a significant signal buried in a large noise? One should realize that the application of a "strangeness" factor is already an expression of a signal character of the residue, though a subjective and qualitative one. It should be possible to express it objectively and quantitatively through the statistical approach suggested. In addition, this method will identify the problem in quantitative scientific or technical terms and test the reality of the problems.

Later on, one may extend the analysis to all not-identified cases and see if they contain the same signal. This will broaden the statistical basis. The data set was also last notified according to time or geography for additional significance testing.

If the results are negative and the residue shows statistically insignificant differences to the remaining cases, this should end the UFO controversy (until somebody comes up with different parameters). If the differences are significant, say in the neighborhood of the 10% significance level, this will provide a solid justification for further scientific research.

It is now necessary to develop this approach by statistical experts and to proceed in the most self-critical way. I believe that this is an avenue open to the efforts of individuals, especially those in the third group, provided they have some computer time at their disposal.

The results *must* be published in the scientific literature and here we run into another obstacle. Individually submitted papers may be rejected by professional journals because of the existing [missing line] should clear the way for such publications in a similar fashion as our subcommittee has done for the "A & A" journal.

What I think should be done is that this group should form a Review Board of highly respected scientists. They should help to bring such papers into the best possible form and subsequently submit and recommend these papers for publication in specific journals attaching their own review. This may open the door to the scientific literature for those authors who want to make use of this possibility.

Finally, I would like to address the question of *case studies* and how they can be improved. Many people have discussed various schemes and we have done the same in our Committee. Most of these schemes run into the problem discussed earlier: They are too costly and require major support. We have, however, come up with one relatively inexpensive idea, which may or may not work. If it does, it could be quite significant and that is why it may be worth trying.

The National Weather Service operates approximately 100 weather radars 24 hours a day in the United States to provide information to the various forecasting, warning and climatological centers. Photographs of the PPI scopes are taken once every hour during fair weather and once every 40 seconds during severe weather. They are stored at the facility of the Environmental Data Service in Asheville, North Carolina.

The radars have range modes of 125 and 250-mile radius, and Figure 3 presents coverage of the United States for the latter mode. Calculation of the volume coverage shows that the 250-mile range provides [missing line] the entire United States.

The corresponding figures for the 125-mile range is about 28%. Overlapping coverage would increase the figures to a maximum of 80%. Search for anomalous trajectories and flight speeds is in principle possible but turns out to be prohibitively expensive in terms of effort as well as costs. If we remember, however, that about 20% of all unidentified cases are "close encounters", including landings, the possibility arises to confirm these reports by actual radar records. For example, in the well-publicized case of Capt. Coyne and his helicopter crew, a double target may be found. A

[1] Implied in these remarks is the viewpoint that the Condon report does not provide the final answer to the UFO PROBLEM. The reasons have been published earlier by our Committee, but among them is the fact that the Condon study fell into the described trap dissipating much of its efforts on identifiable cases and that it lacked a statistical approach. The conclusions by our Committee, based on a "best judgment" approach, were at variance with Condon's own conclusions.

possible drawback of this approach is the lack of good radar coverage at low levels (where most "close encounters" occur) due to the curved earth geometry. Furthermore, best radar coverage is available during bad weather while the unidentified cases are favored by clear weather, according to available statistics.

Nevertheless, the effort is small and inexpensive, and no major support is required. If it is unsuccessful, nothing is lost, but if it is successful it may add a powerful data piece to specific case studies. This information may already exist but has not been recovered.

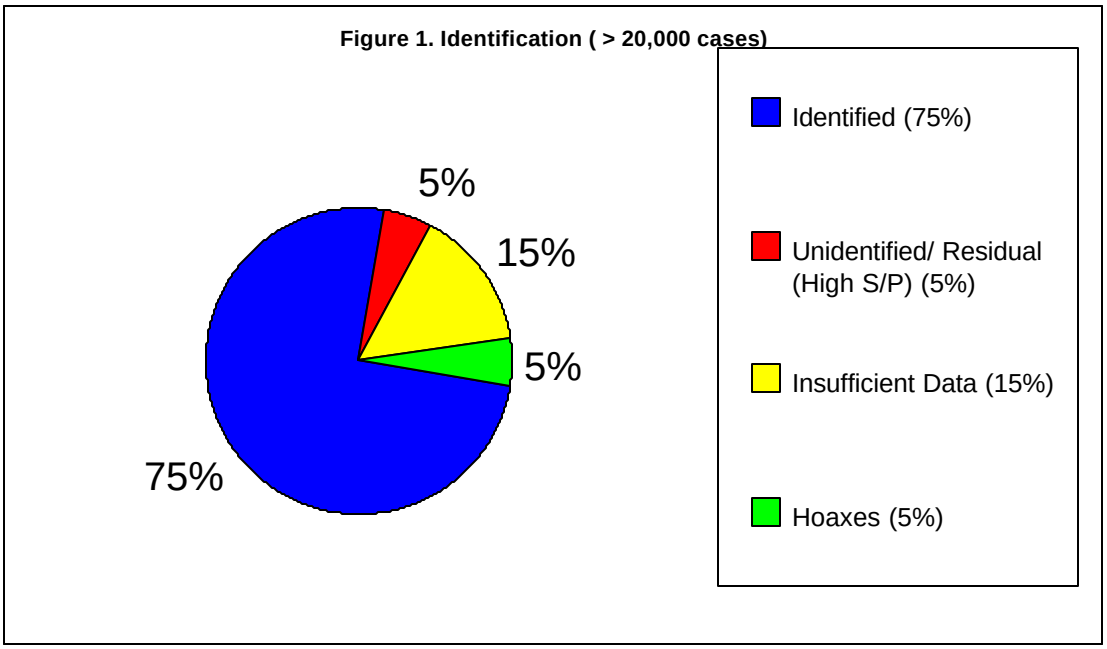

Figure 1. Identification (> 20,000 cases)

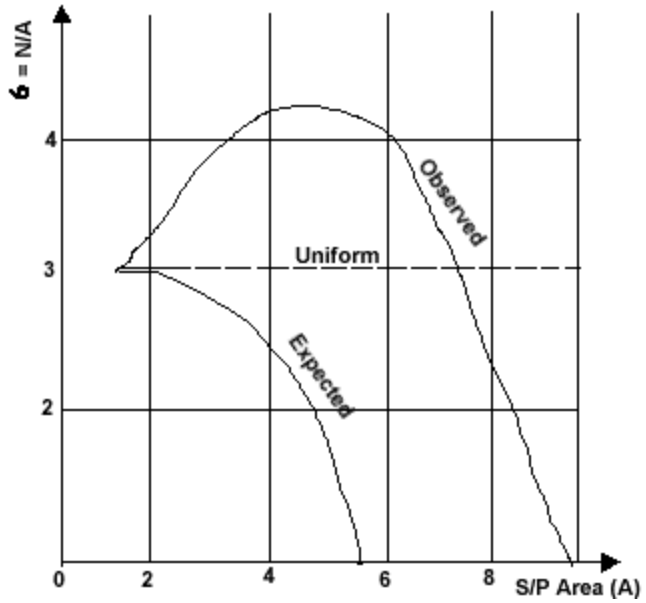

Figure 2. S/P Diagram