

Scientists as Experts: Observations on "Objections to Astrology"

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When the scientist leaves his role as researcher and takes on the role of advocate, he finds the temptation to use his scientific credentials to give the statements the ring of authority difficult to resist. Nor is such use necessarily illegitimate if his credentials bear directly on the matter at hand. The question of relevant credentials must be raised, however, each time a scientist publicly advocates some point of view or course of action. Otherwise, the appearance of impartiality and, above all, of expertise that scientific opinions convey may tempt some of us to accept these opinions as the final word. This danger is heightened when the opinion is a collective one, with the common (and seemingly unanimous) support of dozens of names. So it is with the statement "Objections to Astrology: A Statement by 186 Leading Scientists," originally published in *The Humanist* magazine (Bok et al. 1975) and quickly picked up by the media.¹

Most of the signers of this statement are astronomers, chemists, or physicists, with a smattering of biologists and scientists with other specialties included. Eighteen have won the Nobel Prize; many others are eminent. The statement argues that there is no scientific foundation for astrology's tenets; that the authors are disturbed by the wide dissemination of horoscopes and astrological charts and forecasts; that the widespread interest in astrology reflects the uncertainty of our times; and that the continued public support for astrology can only contribute to the growth of "irrationalism and obscurantism." They state that there is strong evidence to counter the claims of astrology, although they do not state what this evidence is.

1. An article entitled "Scientists Scan Astrology, Find a Universe of Hokum" (Rensberger 1975) appeared in the *New York Times* on the front page. Other media quickly followed. See Anonymous 1975.

A statement of this kind is interesting from a multitude of perspectives: as an example of the popularization of scientific findings, or at least of scientific attitudes; as the latest in a long history of scientific approaches to astrology (White 1923-1958); as the latest move in the "war against superstition"; and as yet another example of the pretentiousness of scientific orthodoxy (Sagan 1976). What I would like to do, however, is to look at the statement in the light of its implications for public debate on controversial scientific questions. Is this the form in which scientific opinions ought to be expressed? Are the authors of the statement indeed the experts in the case? How ought scientists to present controversial issues, and their opinions on them, to the public?

Some historical precedents

The publication of "Objections to Astrology" was not the first time that a group of scientists had done battle with a belief dear to the public, but which scientists regarded as superstition. The belief in witchcraft was far more pernicious in its social effects than is astrology; it was also more deeply rooted in European social structure and ideas. Its eventual defeat, which Hugh Trevor-Roper attributes to the rise of mechanistic ideas of nature, did not come until the seventeenth century (Trevor-Roper 1969). The witchcraft craze was ending just as modern science was beginning. It thus became the first in a series of popular superstitions against which scientists had to struggle. But science developed a powerful weapon for dealing with "irrational" beliefs: technical expertise.

The growth of science meant the growth of a body of technical experts to whom difficult questions might be referred. Unlike the Church, which had always attempted to deal with technical questions at the highest level, science promoted a division of labor: expertise became specialized. A question might be referred to the specialist in a given area and not simply to the most powerful authority in science. One of the uses to which the French Crown early put its Academy of Sciences was that of dealing with difficult technical questions (Hahn 1971). The questions were researched by small committees, which would issue reports that represented the formal opinion of the Academy. The eminence of the Academy's members, the emphasis on mutual criticism, and the formal nature of Academy proceedings were seen as a protection against the issuing of opinions that were casual or without a scientific basis.

One of the more famous controversies in which the Academy partici-

pated was its confrontation with Mesmerism. At the end of the eighteenth century, Mesmerism enjoyed a popularity that is comparable to that of astrology today (Darnton 1970). Friedrich Mesmer's treatments included the use of "magnetic fluid" to put people into trances, effect cures, and improve health. Hands and "magnetized" objects were passed over, or placed on, bodies to secure the effects desired. (Not surprisingly, the system was popular with ladies who were treated by Mesmer's male colleagues.) Nor was the system merely empirical; a well-developed, if erroneous, theory was promulgated to explain Mesmerism's effects, which we today would recognize as those of hypnotism or simple suggestion.

The Academy of Sciences, its sister institution the Royal Society of Medicine, and doctors from the faculty of medicine participated in two investigations of Mesmerism in 1784. The prestige of the investigating committees (which included Benjamin Franklin) could hardly have been greater. Through a series of experiences and controlled experiments, the savants found that Mesmerist methods did not necessarily lead to the claimed effects and that the effects could be produced without Mesmerist methods (Darnton 1970, pp. 62-64). These findings and their implications were hotly disputed by Mesmerists. And well they might be, for although Mesmer's theory had little merit, the phenomenon of hypnotism, which deserved study, went unnoticed by the committees.

Nearly two centuries later the American government found itself embroiled in the controversy over the existence of Unidentified Flying Objects, more commonly known as "flying saucers." Many witnesses reported seeing strange aerial craft performing odd maneuvers in the sky, landing on the ground, disgorging (temporarily) occupants, stopping cars on highways, chasing airplanes, and so forth (Jacobs 1975). After years of prodding, the government finally consented to commission an independent study and contracted with the University of Colorado to perform it. The commission, headed by Edward U. Condon, a well-known physicist, intensively investigated 59 cases of UFO sightings. These case studies included field investigations, photogrammetric analyses, magnetic sensors (used after the fact), and interrogation of the witnesses of the alleged sightings. Extensive mathematical calculations were used to process the evidence in some of the cases. When the commission issued its report in 1968, it stated that it had found little of scientific value in the study of UFOs: "Our general conclusion is that nothing has come from the study of UFOs in the past 21 years that has added to scientific knowledge. Careful consideration of the record as it is available to us leads us to conclude that

further extensive study of UFOs probably cannot be justified in the expectation that science will be advanced thereby" (Condon 1969, p. 1).

The report, even before its publication, was highly controversial and elicited considerable criticism from UFO advocates, some of whom were members of the scientific community and even of the commission itself (Saunders and Harkness 1968; Boffey 1968; Surrock 1974). Doubts were grave enough that the Science and Astronautics Committee of the American House of Representatives called its own experts to testify before it (U.S. House of Representatives Committee on Science and Astronautics 1968). These experts generally disagreed with the findings of Condon and his associates. Nonetheless, a committee appointed by the National Academy of Sciences reviewed the Condon Committee's work and found it "a very creditable effort to apply objectively the relevant techniques of science to the solution of the UFO problem" (Anonymous 1969).

The conflict between experts

The UFO controversy raises a very interesting question: What if the experts disagree? This happens often enough where mundane issues are concerned, but it is especially likely in matters which are on the frontiers of science. The National Academy of Sciences attempted to weigh the testimony of experts on both sides of the UFO controversy, but their report makes it obvious that they did not really grapple with the issue of scientific method.² There is one case, however, which shows that conflict between experts can occasionally produce important insights. The debate in the case involved the installation of a new American antiballistic-missile system, called Safeguard. The system would be designed to intercept and destroy incoming ballistic missiles. Its primary purpose was to protect American offensive missiles, our "second-strike capability," in the verbose jargon of military strategy. Such a question raises a host of issues, ranging from the ethics of nuclear war to the intentions of the Soviet Union. But the greatest conflict grew out of the fact that the experts could not agree even on the basic data.

The debate took place in many phases, but the one we are interested in took place in 1969. Presentations were made before congressional committees, letters were written to the *New York Times*, a book was pub-

2. According to one writer, such superficial jobs are not unusual for the Academy. See Boffey 1975.

lished by one side in the dispute, and many private statements were circulated.³ One of the participants, Albert Wohlstetter, felt that the opponents of the system were not being fair in their presentation of the data. He thought many of their statements were misleading in a way that made their argument appear stronger than it was. So he took an unusual step. He asked the Operations Research Society of America (ORSA) to examine the methods he and his opponents used.

Operations research is the study of how operations can be made more efficient. Many of the issues in the Safeguard debate were clearly issues of operations rather than of basic science. It is important to recognize, also, that ORSA was being asked to examine only the *methods* used in the debate; it was not being asked whether Safeguard was a good idea or not. ORSA agreed to study the question and appointed a six-member, ad hoc committee, which included four past presidents of the society.⁴

The committee's findings gave Wohlstetter a resounding vindication and made his opponents look either very careless or very dishonest. The ORSA report stated:

The evidence strongly supports the disturbing conclusion that when prominent experts outside the Administration supported their opinions on Safeguard deployment with arguments or results of an operations-research nature, these analyses were often inappropriate, misleading, or factually in error. Moreover, in many instances, elementary standards for proper presentation of results to permit verification and meaningful dialogue were not met. Failure to distinguish properly between the roles of the analyst and the advocate contributed to the lack of complete research to clarify the presentation. (Operations Research Society of America 1971, p. 1176)

As one might guess, these findings were received with but little grace by those whose reputations had been damaged. Several members of ORSA threatened to resign in protest. The report was criticized as being unfair, and the committee which had written it was accused of partiality (Operations Research Society of America 1971: Appendices IV-D, IV-H, IV-I; Wade 1971; Panofsky 1972).

3. See U.S. Senate Committee on Government Operations 1969. This pamphlet gives a sampling of some of the opinions in the debate along with some of the original documents.

4. See Operations Research Society of America 1971. The entire issue is devoted to an examination of the Safeguard debate. Documents regarding the establishment of the committee are contained in Appendix IV.

Thus, even calling in theoretically impartial experts to rule on the work of other experts did not produce an end to the conflict of expert opinions. It is therefore necessary to ask: How can one who is not an expert judge between experts when their opinions conflict? One interesting set of solutions to this problem has been evolved by our legal system, which often has to deal with the problem of the conflict of expert testimony. Let us examine how expert testimony is handled by the courts.

Expert opinion in court

The sociologist Max Weber considered rational procedure the hallmark of modern jurisprudence (Weber 1967, pp. 301-21), and indeed the laws of evidence attempt to make the decision-making processes of the jury as rational as possible by carefully controlling the evidence it receives.⁵ Admittedly, the laws of evidence are based on commonsense assumptions (Bentham 1825); but in the specific area of expert testimony precedent has now built a strong tradition. Expert testimony in court is subject to strict rules, some of them even more strict than the requirements for publication in a scientific journal. There are three basic requirements for such testimony:

- (1) The expert may testify only about areas which depend on skills or knowledge beyond the competence of laymen.
- (2) An expert witness must possess special qualifications of background, training, and experience.
- (3) The expert may testify only to matters within his area of expertise. In doing so he can be required to use well-established principles to support his opinion. (Adapted from Waltz 1975)

That an expert must be qualified before his testimony can be accepted seems obvious, but what is not always so obvious is that his expertise must be in the *exact* area on which he will testify. An excellent example of this point can be seen in the famous Sacco and Vanzetti case. This international cause célèbre concerned two young Italian anarchists accused of killing a paymaster and a guard in a 1920 robbery. One of the

5. Unfortunately, controlling the input of information is only one aspect of the jury's rationality; what they do with the information they get is another matter.

critical issues in the case was whether the fatal bullets had come from a gun found in the possession of one of them—Sacco—when he was arrested. This issue involved a delicate question of forensic ballistics, an area of expertise only recently opened up at the time the trial took place in 1921.⁶ Their trial nicely demonstrates some of the pitfalls of expert testimony:

Several firearms experts appeared in behalf of both the defense and the prosecution. These experts contradicted each other on the question of whether the bullet had been fired from Sacco's gun and on many subsidiary issues involved in identifying the bullet. One of the "experts" was in fact a charlatan, a "Doctor" Albert Hamilton. The others included men with considerable knowledge about firearms but very little knowledge about the area of forensic ballistics. Thus the question of whether Sacco's gun had fired the fatal bullet was clouded by a conflict of testimonies that betrayed the experts' lack of qualifications.

In 1927 a man arrived on the scene who was capable of putting the confusion to rest. His name was Calvin Goddard. For several years he had been perfecting the techniques of bullet identification. He had joined the founders of the Bureau of Forensic Ballistics in New York—the first institute of its kind in the world. In a short time he was so adept at using the instruments the team had developed that he became the undisputed leader of the bureau. One of these instruments, the comparison microscope, was to play an important role in the Sacco and Vanzetti case.

Goddard offered his services to both the prosecution and defense in the case. The defense refused, but the prosecution accepted. Goddard set to work in an atmosphere of considerable tension, since the other "experts" recognized that he came equipped with tools they did not realize even existed. When Goddard examined the fatal bullet and a test bullet from Sacco's gun in the comparison microscope, one expert withdrew his previous testimony and admitted that Goddard's methods were far in advance of his own. Another expert also admitted his error. Goddard found that the fatal bullet and a shell casing found at the scene of the crime had both come from Sacco's gun. Sacco and Vanzetti died in the electric chair.

6. The following account is taken from Thorwald (1965, pp. 435-50). He deals only with the ballistics issue although other issues were equally, if not more, important in the trial.

The basis of expert testimony

Qualifications are, however, only half the story. The method by which the expert arrives at his decision, or what is called the "basis" of his testimony, is equally important. If the expert cannot show an unbroken line of necessary inferences from the evidence in the case to his conclusion, his conclusion can be impeached. Furthermore, the principles he uses in assessing the data have to be generally accepted in his technical specialty, and findings that he cites should be those that are also accepted. He is thus constrained by the state of his art. Going beyond the state of the art in his testimony is not permitted.

What can happen when experts rule on matters not well understood in their technical specialty is illustrated by a case from the history of science. Although the case was not tried in a court, it might well have been. It took place in 1772 and involved a piece of a stone said to have fallen from the sky. Three scientists (or *physiciens*, as they were then called), members of the French Academy of Sciences, were asked to examine the stone fragment and render a verdict on it: Could it really have fallen from the sky?

The eminence of the scientists was attested by their membership in the Academy, and one of them was Antoine Lavoisier, the father of modern chemistry. They were thus highly qualified. In their report on their examination of the "mineral" (Fougeroux et al. 1777) the three noted that stories of such "thunder stones" had always been looked upon by savants with suspicion and concluded that the fragment which they had examined had not fallen from the sky. It was, they suggested, most probably ferrous sandstone which had been struck by lightning. They based their conclusions on a mineralogical analysis which, although it would seem superficial now, was adequate for the period in which they lived. Their case would have done well in court.

Yet, their verdict was wrong. Stones do fall from the sky, and the stone which they had examined was most probably part of a meteorite. This case demonstrates the difficulty with expert testimony on questions on the frontiers of scientific knowledge. The person who is testifying may be the foremost researcher in his field, but the field itself may not be far enough advanced to include matters on which judgment is being made. The phenomenon of meteorites was still too little studied to render the verdict of the three academicians very useful; a mere 31 years later, in 1803, the whole scientific community would take a different view of the

matter. The changed opinion would be based on the chemical analysis of meteorites from a dozen falls and the close investigation of several cases.⁷ But this information was unavailable in 1772, and a decision by scientists at that time was unlikely to go beyond the current state of science.

Expertise on astrology

The previous historical examples have been presented to set the stage for a look at "Objections to Astrology." The first point to consider is that of the relevant expertise for entering into such a debate. Ideally, expertise would come naturally from researchers active in the study of astrology through scientific experimentation, clinical observation, and statistical analysis. The qualifications of such a group to act as experts would seem obvious.

Alternatively, since the group of active researchers is rather small, one might accept a kind of derivative expertise. This secondary expertise might be gained through familiarizing oneself with the relevant studies, literature, and so forth. It frequently happens in science that those who have not done research in a particular field themselves are excellent critics of the methods and conclusions of others' research and can suggest refinements in methods or point out false inferences from collected data. Such expertise would be useful in the astrology debate.

However, it is not clear how many of the 186 signers of the protocol have either of these types of expertise. The eminence of many of these men and women does not allow us to dismiss their opinions lightly, even if they have not familiarized themselves with the relevant literature. Yet we can question whether they have the right to state that "there is no scientific foundation for [astrology's] tenets" without having done the necessary homework. This is especially true since much of the data that does exist on the question seems to indicate that the following statement is ill-considered: "It is simply a mistake to imagine that the forces exerted by stars and planets at the moment of birth can in any way shape our futures."⁸ Whether these data will ultimately prove valuable or not is a secondary issue. *The major issue is whether judgment should precede study.* As Jean-Baptiste Biot (1774-1862) said in a paper on meteorites: "Always, in doubt-

7. There is not a good general account of the meteorite controversy. See Paneth 1949.

8. This statement appears in the second paragraph of "Objections" and is contradicted by data such as those presented by Gauquelin (1976).

ful questions, the ignorant simply believe, the half-savant decides, but the informed person examines: for he does not have the temerity to erect boundaries to the power of nature" (Biot 1803, p. 7).

It is certainly incorrect to assume that because a person is an astronomer he is necessarily an expert on astrology. Neither is a nuclear physicist nor a physiologist necessarily an expert in nuclear medicine, although the latter involves both of their specialties. As the Sacco and Vanzetti case clearly shows, being an expert in a specialty does not permit one to assume expertise in a related field in question. Astrology concerns celestial bodies and human beings, but this does not imply that an expert in either field is necessarily an expert on the correlation of events in the two spheres.⁹

How should expert opinion be presented?

Even if one assumes that all the scientists who signed the protocol possessed the requisite expertise, one might still ask how their opinion ought to have been presented to the public? We might contrast here the operant procedures of the mass media with those of the courts.

Quite often, when a controversial event takes place, the newspapers or wire services request the opinion of an "expert." This opinion is often solicited by interview or over the telephone and is often printed in excerpted form. This rather casual procedure, while perhaps better than no follow-up at all, has several defects. First, the choice of an expert is often dictated more by the demands of time and convenience than by concern for ultimate qualification; the lack of a cross-examination such as that which might take place in a court often encourages this lack of rigor. Second, the opinion is sought on the spur of the moment rather than with the careful deliberation necessary to prepare a court testimony. Finally, the printing of an abbreviated or summarized opinion, often in paraphrased form, is likely to do further violence to reason.

"Objections to Astrology" avoids many of these pitfalls but not all of them. How many of the signers would survive a cross-examination in court on their qualifications to serve as an expert witness on astrology?¹⁰ But of course the general public, at whom the statement was aimed, would not be aware of this point.

9. This point was made to me very forcibly by my colleague Gordon Moss, an interdisciplinary researcher who studies the influence of human relations on illness.

10. For examples of such cross-examinations, see Waltz 1975 and Levine 1968.

Nor is the public encouraged to examine the evidence itself. A take-our-word-for-it stance pervades the entire statement. Even a well-educated person would hardly be placed in a position to make a better judgment by the statement since the evidence on which an independent decision could be made is scarcely presented.¹¹ The strong conclusions presented in the protocol might be appropriate as the final result of an extensive research program by a scientific commission. But in its present form it encourages the person reading it to accept a kind of proof by authority. Interestingly enough, this is exactly the same request made by newspaper astrologers. One has to recognize the historical reality that the judgments of scientists have often proved ephemeral, which is all the more reason to make clear the evidence and arguments on which their judgments are based.

What ought to be done?

The signers of the protocol might consider the example set by their French colleagues. In 1960 a book was published in France by Louis Pauwels and Jacques Bergier (1960) entitled *Le Matin des Magiciens* ("The Morning of the Magicians"). It dealt with secret societies, mysterious human powers, the Nazis, alchemy, and similar subjects. Its authors advocated what they called "fantastic realism." The public was quite enthusiastic about the book, and its success led to the publication of a well-illustrated periodical, *Planète*, which continued to serve up the same heady brew of mysticism, "fantastic science," anthropology, and speculation. Alarmed by the current of irrationalism and occultism that *Planète* seemed to evoke, the French Union Rationaliste issued a book of its own, *Le Crepuscule des Magiciens* ("The Twilight of the Magicians"), in which the approach, methods, and conclusions of Pauwels and Bergier were attacked.¹²

I would like to second the suggestion made by Carl Sagan that a scientific symposium on astrology, similar to those offered by the American Association for the Advancement of Science on UFOs (1969) and on Immanuel Velikovsky's ideas (1974) might be useful. Proponents of

11. The article by Lawrence E. Jerome (1975) is a step in the right direction, but an adversary process would clearly have given a fairer account, as Gauquelin's article shows.

12. Gilifret 1965. A copy of this book was kindly sent to me by J. Marchand, Secretary to the Union Rationaliste. Unfortunately this book seems to be out of print. The address of the Union Rationaliste is: 16 Rue de l'Ecole Polytechnique, Paris, France.

astrology, particularly those with numerical data, should confront critics of astrology. This procedure would seem closer to the scientific ideal and, if its results were published, would afford educated persons at least a chance to weigh both sides of the argument. The scientific support that this method might appear to lend to astrology must be weighed against the harm done by authoritative statements that encourage one to believe without proof that astrology is bunk. To encourage the public to believe in science without understanding it should never be the aim of scientists.

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