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• ABSTRACT

In making decisions about the reality of alleged anomalous events, scientists are likely to weigh both the a priori plausibility of what is alleged and the credibility of the reports which reach them. The present paper is an attempt to examine the anomaly reporting processes which led to the scientific recognition of the reality of meteorites in the eighteenth century. It is shown that scientists fail to make realistic assumptions about anomaly reporting, and that this failure affects the accuracy of the decisions made about anomalies. The treatment of reports about alleged anomalous events is further shown to be related to the scientific community's concerns about protecting its internal processes from external interference. The recognition of meteorites took place only when the savants of the eighteenth century 1) found a way of evaluating the reports, 2) devised a theory to explain them, and 3) received unimpeachable eyewitness testimony of their occurrence.

Science and Social Intelligence about Anomalies:
The Case of Meteorites

Ron Westrum

An under-explored area in the sociology of science is the process by which data from scientific observations or experiments come to be accepted as genuine. While historical material on controversies over data is abundant, there are relatively few studies which attempt to consider these cases in the light of a theoretical framework.[1] Even less studied, however, are those cases in which data originating outside the scientific community have become accepted within it. The control of the boundary between the institution of science and other parts of society by the acceptance or rejection of observations from outside the scientific community is nonetheless critical for the autonomy of science, and for social control within it. What I intend to do here is to examine this problem posed in its most extreme form: the response of the scientific community to reports from non-scientists of hypothetical anomalies.[2] In

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two previous papers on Unidentified Flying Objects and sea-serpents[3] I explored the way in which accounts of these hypothetical events reach the scientific community, and how these reports are considered within it. But both of these studies dealt with objects at best hypothetical, and possibly imaginary.[4] How would the scientific community respond in the case of a 'real' anomaly? Would information be treated in the same way? To find a partial answer to this question I will examine a case which has become a classic one in the history of science: the meteorite controversy of the eighteenth century.

The choice of the meteorite controversy might be criticized on a number of grounds, the most obvious of which is that the term, 'the scientific community', refers to different entities in the eighteenth and twentieth centuries. This criticism has considerable face validity. After much study of the meteorite case, however, I am even more convinced of its relevance to other anomalies than I was when I began. Study of the meteorite case, in conjunction with those of UFOs and sea-serpents, shows suggestive, although far from exact, parallels between the three controversies.

There is, however, a closely related reason why the meteorite controversy ought to be examined from the point of view of the sociology of knowledge. Savants'[5] rejection of meteorites has now become part of the rhetoric used by anomaly advocates to argue that the resistance of scientists to accepting UFO or sea-serpent reports is not based on adequate reasons, but is simply characteristic of scientific resistance to new ideas.[6] Anomaly critics have used the controversy in turn to argue that the evidence for meteorites and UFOs is very different, and that in fact the cases are very far apart.[7] Secondary sources on the controversy are often inaccurate, leading some individuals into making erroneous statements about it.[8] It is important to get the facts in the meteorite case right, if only for purposes of comparison.

The controversy took place during the last years of the eighteenth century and the first years of the nineteenth century. In one sense the controversy began with a formal rejection of three meteoritic stones by the French Academy of Sciences in 1772. But the true debate did not begin until 1794, when the German physicist Chladni published a small book advocating the reality of meteorites.[9] In the same year a widely-publicized fall took place in Siena, Italy.[10] The response to both these events was generally negative.[11] Chladni and other advocates of the reality of meteorites were under constant attack on the grounds that their ideas offended either theology or the ideas of the Enlightenment.[12] At the turn of the century, however, with more falls having been reported, the general attitude changed from outright denial to uncertainty. After 1803 the reality of meteorites became generally accepted.

The last comprehensive histories of the meteorite controversy were written in the early nineteenth century.[13] Although there have been some recent short treatments[14] which give the outline and important details of some aspects of the controversy, these articles have not dealt with the broader issues in the sociology of knowledge to which the meteorite controversy is relevant. These issues will be the focus of this study.

It is often suggested that the reason that scientists reject reports of anomalies is that they consider these things to be implausible for a priori theoretical reasons. Perhaps this is a complete explanation in some cases (ghosts, for example). I would like to argue in what follows, however, that a major factor in the rejection of the existence of such anomalies is the quantity and quality of reports about them which reach scientists. If reports (or their relative absence) are indeed important in scientists' rejection of anomalies, then we ought to investigate the way in which reports from alleged anomaly percipients reach (or don't reach) scientists. I would like to suggest that only by understanding the social intelligence system which transmits these reports — and, in particular, *how scientists regard its processes* — can we truly comprehend scientists' opinions about anomalies like UFOs, sea serpents, and (in the eighteenth century) meteorites. As we will see, the rejection of these anomalies takes place on sociological as well as scientific grounds. Hence our concern here will be to examine the validity of the sociological premises on which this rejection is based.

A Missed Connection

My point of departure will be two memoirs submitted to the French *Académie Royale des Sciences*, one in 1771, the other in 1772. The first dealt with an observation of an impressive fireball meteor,[15] the second with the chemical analysis of a stone alleged to have fallen from the sky.[16] Both papers concerned things happening in the sky, and we know today that the two papers dealt with parts of the same phenomenon: the entry and fall of extraterrestrial bodies through the earth's atmosphere. Today such bodies are called 'meteors' while in transit through the earth's atmosphere, and 'meteorites' if they fall to the ground. At the time, however, no connection was made between these two phenomena, and in spite of the close proximity of the two papers in time, their subjects were not related to each other by the *Académiciens*. Nor were the two phenomena treated equally. While the report of the meteor treated it as a real event, that of the meteorite treated it as a fiction, or at least as some other kind of event which had been badly perceived: stones simply did not fall from the sky.

The failure to make a connection between these two phenomena was very serious, since the explanation of meteors was not possible without the evidence offered by the meteorite. LeRoy, the author of the paper on the meteor, felt that 'Among the multitude of objects of all sorts which physics encompasses, there are none of them more important, or which merit our attention more, than meteors.'[17] But he was no more able to offer a satisfactory explanation of them than had other savants. Edmund Halley, for instance, suggested in one article that they were solid bodies — and then, in another article five years later, that they were caused by the ignition of long trains of gas in the atmosphere.[18] LeRoy himself considered several hypotheses, but was unwilling to accept any of them as convincing. He did not seem to be aware of any reports of falling stones, and did not agree with the suggestion that solid bodies might be the cause of these appearances.[19] The clues which would have allowed the solution to the mystery were not seen as such.

And yet the clues existed. The stimulus for the 1772 chemical analysis had been three 'thunderstones' submitted to the *Académie* in 1769 from independent sources. The historian of the *Académie* even remarked upon the surprising resemblance of the stones to each other.[20] But the report of the chemical analysis of one of the stones, submitted by Fougeroux,. Cadet, and the great Lavoisier, concluded that the stones had not fallen from the sky. They remarked that 'true physicists' had always regarded the existence of such stones as very doubtful,[21] and they saw no reason in the chemical analysis of the stone at hand to change this opinion. This instance was merely one of many rejections which similar stones had to suffer at the hands of savants and learned academies. Because these crucial bits of evidence were rejected, understanding the nature of meteors would have to wait until after the appearance of Chladni's book in 1794.

It is interesting to note that the acceptance of one part of the phenomenon (meteors) and the rejection of the other (meteorites) rested upon unequal opportunities to observe the two. Whereas the meteor was often visible over several thousand square miles, the fall of the meteorite was visible only in a much more restricted area. This inequality meant that while commoners and savants alike might observe the meteor, and in great numbers, the number of witnesses to a meteorite fall was likely to be very small and was unlikely to include those with scientific training. In at least one case (Barbotan in 1790) the sighting of a meteor was readily accepted while savants rejected the fall of the meteoric stones which took place at the end of its path.[22] During the eighteenth century, while articles on meteors either simply reported observations or dealt with possible explanations of their nature, the articles on meteorites were largely concerned with whether such things could actually fall from the sky.

Toward the end of the eighteenth century, this attitude began to change rapidly. In 1794 Chladni published his book, in which he used accounts of meteorite falls and finds to connect meteors with meteorites and suggested that the phenomena were of extraterrestrial origin. Shortly thereafter, as a sequel to several notable falls, the English chemist Howard and the emigré mineralogist de Bournon analyzed several meteoric stones and irons and found surprising similarities in their chemical composition. The iron meteorites contained nickel — a combination known to occur only in these 'rocks' fallen from the sky.[23] The chemical analyses went far toward convincing the savants of the reality of the phenomenon. In 1803, De Drée could remark that

> It was not so long ago that one risked a disdainful smile by seeming to believe that mineral masses could fall from the atmosphere on to our globe; but thanks to the researches made by several savants on these extraordinary minerals, along with circumstantial accounts of the fall of some of them . . . the general attention is now fixed on these astonishing phenomena.[24]

On the same day that these words were read before the *Institut de France*, it was announced that an enormous quantity of stones had fallen near the little town of l'Aigle, France, a mere 70 miles from Paris. Other reports followed; some of the actual stones arrived. The *Institut* sent Jean-Baptiste Biot to investigate. Biot, who had previously announced in favour of meteorites,[25] did a thorough and elegant job of investigation. His report[26] put to rest virtually all the remaining doubts about the reality of meteorites.

The Scientific Context

To understand why the scientific community reacted as it did to meteorite reports, we have to examine the scientific context in which meteorites appeared. The position of science in Western Europe in 1794 was an ambiguous one.[27] On the one hand, science had been institutionalized in a number of learned societies, some of them enjoying royal support.[28] A goodly number of books written by scientists were published annually, and were reinforced by more timely publication of results, theories, and criticisms in scientific journals.[29] Within the scientific community itself, the norm of experimental verification was widely accepted, and disputes about reality were frequently resolved by recourse to observation and experiment. On the other hand, scientists were few, they were seldom supported directly for their work as researchers, and public acceptance was far from assured. It was hard to make a career in science and in fact the exact nature of the scientific role itself was unclear.[30] Recognition by other scientists was often more important to membership in the scientific community than formal training or the holding of an official position. There is no question that a scientific community existed in 1794, but it was one considerably looser and more fragmented than the scientific community of the twentieth century.

The intellectual and social boundaries of this community were accordingly more difficult to defend. Amateurs existed within scientific societies who did not possess any real scientific ability; experts frequently held no important positions; and many scientists were churchmen, who thus had double loyalties. In such a situation it is not surprising that there should be stiff resistance to any attempt to widen the arena of scientific discourse. Everything that went against current notions had to be carefully examined, and if it could not be made to fit, it was consigned to the intellectual rubbish-heap; otherwise the disorganization might become worse.

Particularly suspect were ideas from two sources: ancient authors and the common people. The fascination of the Renaissance with older authors and popular lore had been replaced in the course of scientific development with a deep suspicion of anything that could not be subjected to experiment or observation.[31] This distrust served not only the practical purpose of protecting science from 'superstition' or unfounded accounts, but it provided a most important mechanism for science in protecting its vulnerable boundaries. The insistence that all data used by scientists originate with them or be checked by them not only provided a rudimentary quality control of data, but it kept the processes of science firmly in the hands of scientists. To admit data from outside was not only thought to be dangerous to the integrity of scientific ideas, but it meant a substantial loss of control. Necessarily this meant that any potential threat to the data control system had to be minimized.

It is thus no accident that when meteorites, supported by old accounts and the experiences of the common people, presented themselves at the door of science, they were for a considerable time rudely turned away; for scientists were only too aware of how many other 'superstitions' were also trying to gain admittance.

Lightning-Stones

In the eighteenth century there was still a belief among many peoples that stones fell down with the lightning. These stones, which earlier authors had called *ombriae, brontiae* and *cerauniae*,[32] were often regarded as having supernatural powers.[33] Bishop Pontoppidan observed that the Norwegian peasants believed that these stones were especially useful to women in labour, in that they would aid delivery of the child.[34] Belief regarding the supernatural powers of these stones was so strong in Prussia that Helwing, the minister of Angerbourg, finally had to resort to the use of the Secular Arm to root them out. [35] Accordingly one savant after another took pains to describe how baseless was the idea that these stones had really fallen with the lightning.[36]

These 'figured stones' appeared to one eighteenth-century savant as belonging to three types.[37] The first were evidently fossils, the hardened remains of creatures which had once been living. A second, more interesting, variety were obviously the tools of primitive man which, left behind wherever settlements had formerly been, gave rise to the idea that the lightning had sent down these strange wedges and axes. A third type of 'lightning-stone', which seemed to be a pyrite or marcasite with a black crust, was a little harder to explain. Whereas the first two types had been related to lightning because of their pointed shape (naturally thunderbolts had to be pointed), the reason for the connection of the third type with lightning was not clear. 'We will have to leave it to the chemists to demonstrate its origin against those who believe it to be celestial,' the savant said indifferently.[38] That all three types of stones should have been given the same name was unfortunate for the recognition of meteorites. For while the scientists of the twentieth century would agree with the savants of the eighteenth on the identity of the first two types, they would disagree on the third type, which definitely included some stones which really had fallen from the skies, although not with the lightning. But when real meteorites were brought to savants for consideration, the memory of the erroneous ones was still vivid. No savant wished to be considered foolish by accepting as authentic items analogous to ones already shown to be fraudulent. It took many years before prejudice could be overcome.

The savants of the eighteenth century were also well aware that the ancients had reported that stones had rained down from the sky on several occasions. Probably most savants saw this simply as an example of the credulity of ancient authors, but some were differently disposed. The French historian Fréret, for instance, suggested that his colleagues at the *Académie des Inscriptions et Belles Lettres* take these ancient reports seriously. He argued that many of the 'prodigies' reported by the ancients were real enough: they were simply natural events, perhaps somewhat rare, which had been given a supernatural interpretation.[39] The geologist Guettard, in his notes on Pliny's *Natural History*, also felt that stones could rain from the sky, and cited modern as well as ancient reports of them doing so.[40] He felt that such rains might be caused by hurricanes and volcanic explosions.

But the question of stone-falls was of more than merely historical interest. The three stones which had been sent to the *Académie des Sciences* in 1769 were far from the only recently fallen ones which came into the hands of savants. Testimonial accounts of contemporary falls were even more numerous. Yet the analysis of one of the three stones in 1772 was the first time that a stone said to have fallen was analyzed by a scientific academy and described in a scientific journal. The negative assessment of the stone's origin by such a prestigious body was probably very influential in determining the reaction of other scientific bodies to the stones, and to testimonies about them. The reaction of the *Académie* created an environment in which information transfer about the phenomenon was slow and imperfect. For the verdict of the *Académie* was widely viewed as being the official viewpoint of science itself, not just that of a single academy. To explain why this verdict was changed three decades later, we must examine in detail how individual savants made decisions about the reality of meteorites and the social intelligence system which brought them the information which was the basis for these decisions.

The Mechanism of Conviction

In trying to understand the way savants came to believe in the reality of meteorites, I have found it very useful to borrow a concept from the physiology of the nervous system, the concept of 'summation'.[41] This concept has to do with the way that signals are passed along the nerves. Often a single impulse arriving from a nerve fibre at a nerve synapse will not be transmitted unless another impulse arrives from another fibre within a short time. The synapse then integrates or 'sums' the signals from the fibres and transmits its own signal accordingly. I would like to argue that in a very similar way, anomalous events are subject to 'summation effects' — that is to say, that the reaction to reports of anomalous events is a function of the quantity and quality of reports received. In this case time is not so important as is the fact of having received two or more independent reports which together may produce a sense of conviction — or at least of interest — which a single report would not. Keeping this concept in mind may help us understand the behaviour of many of the savants in the meteorite controversy for whom evidence coming from several independent reports indicated that meteorites were worth studying.

Let us consider this matter of summation effects and their importance a little further. There are many instances in ordinary life where independent evidence from two or more sources produces a sense of conviction that information from a single source does not. Both in the courts and in military intelligence work this kind of concurrent evidence has a greater probative value.[42] It has a special importance, however, where the content of the communication is one which the receiver would tend to doubt. The reception of anomalous events by scientists is, as it were, a special case of this general principle. A single report of an anomalous event will usually be viewed with scepticism, if not dismissed out of hand. But as reports of anomalous events of the same kind multiply, they may begin to receive attention which they would not have received by themselves.

The effect of multiple independent reports seems to work in two ways. The first effect is to orient the scientist toward viewing the events as being worthy of interest. This can be important if it means the expenditure of further time and attention and, even more significant, the search for more evidence. If two or more reports of an unusual event carry common features, especially those which it is difficult to imagine the reporters coming up with on their own, a scientist may feel that there is 'something to' the matter.[43] The common feature may point to a potentially solvable problem,[44] one which would justify the expenditure of effort. The second closely related effect is the cross-validation of the worth of each of the reports. The fact that the reporters mention the same thing tends to make both (or all) the reports appear to possess some probative value. Thus the conjunction of two or more reports may move the scientist from doubt to suspended judgment or even to curiosity. There are many instances of exactly this effect in the meteorite controversy.

In 1790 there was a fall of a large number of stones in the community of La Grange de la Juliac in the south of France.[45] This fall was witnessed by perhaps three hundred persons in the community. A professor at Agen, Saint-Amand, heard about the fall and, highly amused, asked for a formal deposition from the witnesses. To his surprise a legal protocol signed by the mayor and the town attorney arrived along with a sample of the stones. Saint-Amand was no more convinced than before, but sent the document to his friend the physicist Bertholon, who was the editor of the *Journal des Sciences Utiles*. Bertholon then poured scorn on the witnesses:

> If the readers have already had occasion to deplore the error of some individuals, how much more will they be appalled today seeing a whole municipality attest to, consecrate, by a legal protocol in good form, these same popular sensations, which can only excite the pity, not only of physicists, but of all reasonable people . . . What can we add here to such an affidavit? All the reflections which it suggests will present themselves to the philosophical reader in reading this authentic attestation of an obviously wrong fact, or a phenomenon *physically* impossible.[46]

Eleven years later Saint-Amand was reading an article written about the investigations on meteoric stones being conducted by Howard in England. He compared the stones which had been sent to him with the ones described in the article: they appeared to be identical! He commented:

> If this news does not convince me, at least it appears to me very remarkable that all the stones from diverse countries to which are attributed the same origin, present exactly the same characters; and I remain convinced that no matter how absurd an alleged fact appears from the viewpoint of physics, judgment must be suspended, and one must not rush into regarding the fact as impossible.[47]

This kind of summation effect appears at several crucial points in the meteorite controversy. The chemical analysis of meteorites which was undertaken by Howard and which went so far toward convincing the scientific community that the stones were real was stimulated by the influence of Sir Joseph Banks, at that time President of the Royal Society. Banks had noticed a stone on exhibition in London which was supposed to have fallen from the clouds near Wold Cottage, Yorkshire in 1795. Banks noted the resemblance between the stone on exhibition and other stones which had been sent to him as having fallen near Siena, Italy in 1794. This resemblance led him to collect accounts of other falls, and finally to encourage Howard to investigate the resemblances chemically.[48]

Another instance of a summation effect resulted in a short but very important paper by the Abbé A. X. Stütz, at the time an assistant curator of the Kaiser's Natural History Cabinet in Vienna.[49] Stütz had been given a sample from a meteorite fall at Eichstedt by his friend the Baron Von Hompesch. He found a description of a similar 'mineral' supposed to have fallen from the sky in a book by the mineralogist Von Born. These two cases reminded him of yet a third one which had taken place in Yugoslavia in 1751: several witnesses had sworn to seeing a heavy stone descend from a fireball. The bishop of Agram's Consistory had investigated the fall and had sent the stone with an affidavit to the Kaiser's Cabinet where Stütz had seen it. The Agram stone was almost pure iron; it in turn reminded Stütz of another lump of pure iron, a huge mass which had been found on top of a mountain by Peter Simon Pallas, a Russian savant and explorer. It, too, had been said to fall from the sky. It is clear that Stütz was not predisposed to accept the depositions of the witnesses to the Agram fall. But perhaps, Stütz wondered, in view of the common theme running through all these accounts, there might be something in the matter after all?[50]

In many ways a museum is a natural point of summation for anomalous objects,[51] and museums could have played the major role in the meteorite controversy. The sad fact is, however, that often they allowed their precious specimens of meteorites to become lost, or even threw them away.[52] Fortunately, the falls were frequent enough and a sufficient number of specimens survived in museums or private collections to make comparison between them possible.[53]

A summation effect can be produced by passive acquisition of data — as in the case of the Abbé Stütz — but it can also take place as the result of active search. What was probably the most important intellectual event in the controversy was the outcome of such a search for data. Chladni, whom I mentioned earlier, was an independent researcher and travelled widely. While he was visiting the physicist G. C. Lichtenberg in Göttingen in 1792, the conversation turned to meteors. Chladni pressed Lichtenberg to tell him what physicists thought about this unexplained phenomenon. Lichtenberg indicated that while meteors had generally been explained as electrical, he and others felt that this theory was far from satisfactory. What were they then, Chladni wanted to know. Well, suggested Lichtenberg, they might be bodies from beyond the earth. This intrigued Chladni so much that he decided to find out as much as he could about them. He relates:

> With this intention I stayed nearly three weeks longer in Göttingen, to collect as many reports of fireballs as I could put my hands on from the town library. It early presented itself as an historical truth, that often stone- and iron-masses fell down following the appearance of an exploding fireball. . .[54]

One of the pieces of information which he uncovered in this manner was the memoir by Stütz, which made a considerable impression on him. When he published his conclusions in a small book, its contents and title were strongly influenced by material considered by Stütz, in particular the Pallas iron mass. When he wrote, Chladni had never seen a meteorite, but based his conclusions entirely on reports; a library can thus serve as a summation point for reports of anomalies. If the relevant reports can be extracted from the mass of materials in the library's holdings, the researcher may have a ready-made set of data. This is what Chladni found.

Another point, however, must be recognized here. It was not only the reality of the meteorite reports which Chladni discovered from his library research; it was also the connection of meteors and meteorites. The cases fit together not only as a set of similar events, but as a set of pieces needed to solve a puzzle. Only when the different events were juxtaposed did their relation appear as an 'historical truth'. Others (like Stütz) had made the connection before, but only in a speculative, uncertain way. Chladni, convinced by the variety of data which all pointed to the same conclusion, made the connection in an unequivocal manner.

The Social Transmission of Data

Like many relatively rare natural phenomena, meteorite falls were usually witnessed by persons who were not savants. Accounts of the fall, and the meteorites themselves, usually had to be transmitted to savants from persons outside the scientific community. The acquisition of data and specimens from non-scientists is not unusual in science, of course. Many natural history museums, for instance, rely to a large degree on animal collectors who in turn rely on native helpers and trappers. In some cases such relationships become so regularized that they become a major commercial enterprise. Even when there is no money involved, we frequently see flows of information about potentially interesting events in the direction of the 'experts'.[55] A general study of these networks of information flow would make an interesting research topic in itself. Here, however, we must confine our attention to the way in which reports and meteorites came into the hands of savants. Attention has already been called to the role played by museums and libraries as summation points. But how did the reports and specimens arrive at these points?

Let us consider the reports first. Some of the reports were contained in books by ancient authors, in local histories and in the reports of travellers, scientific or otherwise. The overwhelming role, however, was played by scientific journals. The fact of having a report printed in a scientific journal was important for several reasons. First, it meant that the report would be widely disseminated. Second, someone else might write a reply to the first article, providing further coverage of the matter. Third, someone might be influenced to write of their own, similar observations.

Even an article which attempted to debunk the phenomenon might, as an unanticipated effect, spread information about it and cause more attention to be given to it.[56] Lastly, scientific journal articles were frequently indexed and sometimes reprinted in other languages. The net result was that information about meteorites which went into scientific journals was not likely to be lost, nor totally ignored.

Transmission of the actual meteorites shows an equally complex pattern. Typically the witnesses of the fall were peasants, farm workers, or other rural people. Often they would bring the stone to the local priest, nobleman, or government official, who would then transmit it to a savant or higher authority. The stones used in the 1772 chemical analysis were seen to fall near Lucé by several workers. A sample soon came into the hands of the Abbé Bachelay, a correspondent of the *Académie des Sciences*. Bachelay then transmitted the sample with a note to the *Académie*. In another case, the Abbé Domenico Tata heard from his friend the Prince of Tarsia about a stone that five Calabrian shepherds had seen fall in 1755.[57] He asked the Prince to send him the stone, which arrived with a formal affidavit. Tata was very much intrigued by the stone and placed it in the public library, where it might be examined by others. It was lost after a number of years.

The importance of these transmission networks cannot be exaggerated. It is true that in some cases (very few previous to 1803) the savants did investigate the cases themselves on the spot. But most of the time they were dependent on these reports and specimens transmitted to them from non-savants. Yet savants' reactions to these transmissions were far from homogeneous. Some, like Tata, regarded them as very important; but others set a quite different value upon them. We will now explore one effect of the negative attitude, the use of ridicule, on the transmission of these data.

The Effect of Ridicule

When the Prince of Tarsia first related to Tata how a stone had fallen from the air near the River Crati, Tata tells us

> I had trouble containing the desire to laugh which came upon me at hearing the story related to me by this young cavalier with the most naive air of conviction. I

> only asked him if he could send me this stone from Calabria (where he had left it) and procure me a circumstantial report from the eye-witnesses to its fall.[58]

As we have seen, Tata was impressed by the receipt of the stone and the affidavit which accompanied it. Yet he did not publish his account until 1794 (39 years later), after another fall near Siena. His reasons are interesting:

> Since that time, the Prince of San Severo and the Marquis Mauri often tried to persuade me to publish all the details; but other friends dissuaded me. They warned me that the savants and the half-savants (even more to be feared) would attack me on this subject or pretend to be gracious to me while treating me only with incredulity. These reasons decided me in favour of silence.[59]

Nor were these fears unjustified. The behaviour of Saint-Amand and Bertholon in regard to the Barbotan fall many decades later is only one of many similar reactions which took place during the controversy.

Part of the problem was the low status of many of the witnesses. The physicist Patrin, in evaluating the chemical analysis of Howard and de Bournon, noted that although many eminent persons had been involved in getting the stones to the chemists, the actual witnesses were not even named. He stated that 'it is easy to see that *this type of evidence* is not even a probability; because everybody knows that thousands of absurdities have been certified by thousands of witnesses of this nature'.[60] The 'lightning-stones' previously brought forward had been one example of the credulity of the multitude. There was also the problem of optical illusions. The geologist DeLuc believed that the witness to the Wold Cottage fall had seen lightning striking a rock, giving rise to the belief that it descended with the lightning.[61] He felt it was dangerous to treat such accounts seriously, because

> It is thus that events badly seen and badly judged, of which volcanic phenomena above all demonstrate many examples, cause naturalists to err who, believing in the exactitude of the accounts, will form systems based on them which will be without foundation.[62]

Acceptance of this kind of data would short-circuit the quality control system which had taken so long to construct. The system could be protected only by rejecting data so difficult to check.

Another part of the problem was the intrinsic implausibility of what was alleged to happen. Where could the stones come from? Volcanoes and whirlwinds were suggested by some savants as possible agents, and others even went so far as to suggest that the stones might be formed in the air.[63] Chladni suggested that their origin might be cosmic, but this was hardly any better, as far as the majority of the scientific community was concerned. One physicist suggested that even if one of the stones fell at his feet, he still would not believe it.[64] Until Laplace and Biot suggested that they might have their origin in lunar volcanoes,[65] none of the explanations seemed tenable. And probably what could not be explained, had never happened in the first place!

Freud makes a very apposite remark in his *New Introductory Lectures in Psychoanalysis*, where he contrasts two ways of treating an hypothesis. Suppose, he says, someone tells us that the centre of the earth is filled with carbonic acid. We might feel that this is improbable, but if they should show us a way of testing this hypothesis, we might indeed make the test. But now suppose they tell us that the centre of the earth is filled with jam. In this case, we would not even consider the idea. Instead, the focus of attention would be shifted, and 'we shall ask ourselves what sort of person this must be who can arrive at such a notion, or at most we shall ask him where he got it from'.[66] In many ways the meteorite witnesses and their champions were treated like persons who had put forward the jam theory. Instead of the meteorite hypothesis being seriously examined, attention was focussed on the way witnesses might have fallen into error, and great stress was laid on the 'true physicists' avoiding this error themselves.[67] More than once it was suggested that a 'love of the marvellous', very dangerous to science, lay behind the belief and interest in these accounts.[68] In this struggle against 'superstition', ridicule was a very useful tool. Because of its employment, many persons (like Tata) hesitated to forward accounts; others, like Stütz, put forward their hypotheses very reluctantly. Some waited until other reports were made before making their own.[69] Chladni, who daringly published his own conclusions in an unequivocal way, was attacked on many sides.[70] Ridicule was a most effective tool in the controversy; unfortunately, it was directed at the wrong object. There is no question in this case that it slowed the advance of science.

Savants and Social Intelligence

We might describe the routes by which information about alleged anomalies travels to savants as a 'social intelligence system'. Whereas some social scientists might view the meteorite controversy as a case of 'the resistance of scientists to scientific discovery',[71] I feel it would be more accurate to see it as an instance of the resistance of the scientific community to social intelligence about anomalies. Social intelligence about events which fall within accepted models of reality are not likely to encounter the same resistance that meteorites did. In regard to those which fall outside such accepted models, however, many scientists (both in the eighteenth century and today) would agree with Hume's feeling about miracles:

> The knavery and folly of men are such common phenomena, that I would rather believe the most extraordinary events to arise from their concurrence, than to admit of so signal a violation of the laws of nature.[72]

Polanyi has argued very cogently that it may be better to reject anomalous experimental data out of hand than carefully to refute them.[73] Often such data may be due to errors which are difficult to detect, and it may be better to ignore them, rather than to waste valuable scientific effort trying to prove them wrong. If such a course of action can be recommended with experiments by scientists, is not an equally strong course of action implied in confronting the uncontrolled experiences of non-scientists? Why should time be wasted in pursuing events that in all probability have not occurred, when tangible problems await solution?

But scientists' distrust of the social intelligence system is selective. While many scientists are willing to believe that non-scientists' reports of anomalous events are generally to be disregarded, they also believe that somehow the 'genuine' anomalous events *will* be successfully transmitted by the system. It follows from this principle that a lack of reports constitutes evidence that the anomaly does not exist. For instance, John Pringle, a Fellow of the Royal Society of London and later its President, stated in 1759 that it was his belief that meteors never fell to the ground:

> And here I would venture to affirm, that, after perusing all the accounts I could find of these *phenomena*, I have met with no well-vouched instance of such an event; nor is it to be imagined, but that, if these meteors had really fallen, there

> must have been long ago so strong evidence of the fact, as to leave no room to doubt of it at present.[74]

We do not know what information Pringle used in coming to this decision, but one wonders if he knew of the observation by Henry Barham of a meteor striking into the earth in Jamaica about 1700.[75] This observation is all the more significant in that it had appeared in the *Philosophical Transactions* of the same Royal Society of which Pringle was a Fellow.

It must be recognized that the argument against the existence of many controversial anomalies is at least partly sociological. When it is argued that, 'if there really were such a thing as X, I would have heard about it by now', the person speaking is making assumptions not only about the physical properties of the anomaly, but also about the workings of this social intelligence system in regard to anomalous events. Scientists tend to assume that reporting is more complete than it often is. There are a number of reasons why this *fallacy of complete reporting* is in error. As an example we could consider the reporting of meteorite falls in the twentieth century. H. H. Nininger, who devoted most of his life to the search for meteorites, indicated that all three of the following estimates were often in error due to incomplete reporting and inadequate search:

1) the amount of material involved in a given fall;
2) the number of falls in a given area;
3) the overall weight of meteoritic matter falling upon the earth.[76]

In one case the amount of matter involved in a Texas fall was revised upward from 68.2 pounds to 1500 pounds, a twenty-two fold increase.[77] Nininger also found that some finds were not reported to avoid ridicule and embarrassment.[78] We have already seen that ridicule was effective in preventing the publication of some meteorite reports. What would have happened to Pringle's opinion if Tata's report had been printed in 1756 and Pringle had seen it?

A related problem is the *fallacy of centrality*. Many scientists seem to believe that if reports have been made, they personally would know of them. DeLuc's belief that alleged meteorite falls were actually optical illusions led him to suggest that where there are few stones, these optical illusions do not occur. In the Amazon region, where one can go for miles without having seen a stone, there are no meteorite reports: 'having no object which could give rise to such illusions, they are not produced'.[79] One wonders what he would have said if he had been aware of the discovery in 1784 of an enormous mass of 'native iron' standing isolated in a Brazilian coastal region.[80] As with the Siberian mass found by Pallas, it was hard to see where this mass had come from, if not from the sky. And in fact this mass is a meteorite. But DeLuc did not know of this find, and it seems not to have been published until 1816.

Similarly, Charles Blagden in 1784 speculated on whether meteors might be some sort of terrestrial comets. He rejects this hypothesis, saying:

> But such a crowd of revolving bodies could scarcely fail to announce their existence by some other means than merely a luminous train in the night; as, for instance, by meeting or justling sometimes near the earth, or by falling to the earth in consequence of various accidents . . .[81]

He is apparently unaware of any accounts of stone-falls connected with meteors. Surely, he thinks, if such stone-falls had occurred, he would know of them. But they had occurred, and very recently, and he did not know of them.

Considering these mistaken assumptions, one might be tempted to see them as the result of arrogance and dogmatism. But this would be a mistake. For the presumption was, in fact, a very reasonable one. If events less anomalous had been involved, the savants probably would have heard about them; it was precisely the anomalous character of the events which made reporting about them so unsatisfactory. If witnesses and savants who wished to report falls had not been subject to incredulity and ridicule, knowledge about these falls would have been much more widely distributed. In this case, the presumption of non-existence served to prevent the transmission of information which would have indicated that meteorites did exist.

Control of the Data

One of the generic problems with anomalies like Unidentified Flying Objects and sea-serpents is the intangibility of the phenomena. It is bad enough that the phenomena seem implausible; but added to this problem is the difficulty of checking the data, in making independent observations of the phenomena. For instance, virtually the only evidence for salt-water sea-serpents is eyewitness testimony; no photographs of them exist, and there are no carcasses to examine.[82] Again, with Unidentified Flying Objects there seem to be some photographs and alleged 'landing traces'; but the validity of these in turn rests upon the testimony of the witnesses, at least until something truly exotic in either photographs or 'landing traces' turns up which makes them interesting independent of the witnesses' testimony.

The savants of the eighteenth century had little confidence in the testimony of the witnesses to meteorite falls. They, of course, did not have the knowledge that we do of the psychology of perception, nor the large body of experimental evidence of the fallibility of eyewitness testimony. But they did possess enough experience and common sense to be sceptical of eyewitness accounts of anomalies. DeLuc, for instance, believed that optical illusions were to blame for some reports of falls.[83] Others blamed superstitious beliefs.[84]

The real problem, however, was that there was no way of checking what the witnesses had seen, except for the stones themselves. Without some independent proof, acceptance of social intelligence about meteorites would constitute a dangerous precedent. For here would be 'facts' which savants believed, but which they had not personally confirmed. The system of quality control of data could not function if such accounts were admitted into the corpus of science. Some part, however small, of the control of the scientific enterprise would pass into the hands of non-scientists. Should jurists and clergy, to say nothing of ignorant peasants, instruct savants? No: better that such 'facts' be denied, at least until they could be submitted to scientific control.

The meteoric stones provided the possibility for such control. As we have already noted, the first chemical analysis of a meteorite failed to detect its extraterrestrial origin; nor was any use made of the resemblance of meteoric stones to each other. It is important to note in this case that the physical evidence did not speak for itself; it needed to be interpreted. Nearly three decades later, when chemistry had gone through a major revolution and techniques had greatly improved, chemical analysis did show the peculiarities of meteorites, and the similarity of their appearance and composition was then seen as proof of their common origin. The evidence available in 1772 and 1801 was different only in quantity; but the use made of this evidence was much more effective in 1801. For now chemical analysis was seen as a method of data control — as a means of separating true from false meteorites — and the scientist was no longer entirely dependent on the testimony of the witnesses. With control of the data safely in the hands of scientists, meteorites could be accepted into the corpus of science. Research into meteorites could go forward unhindered by doubts and ridicule.

'External' Influences on the Meteorite Controversy

Thus far we have been concerned with the internal 'logic' of scientific discovery. Were forces external to the scientific community also at work?[85]

One external factor which clearly was present was the reports of meteorite falls, which largely originated outside the scientific community. Belief in the falling stones gained currency outside the scientific community before it gained currency inside it; in fact, the external belief and reports were eventually to bring about the internal recognition. But this is not the sort of 'external' factor which would ordinarily be of interest to the sociologist of knowledge. Were social interests affected by the meteorite controversy? Was any particular body of people inside the scientific community identified with belief in meteorites or with doubt about them? The controversy occurs, for instance, virtually in the middle of the French Revolution, which brought enormous changes to the structure of the French scientific community, including the humiliation of the *Académie des Sciences* and its eventual absorption into the *Institut de France*.[86] Surely the meteorites, which were seen mostly by the common people, were exactly the sort of thing that 'Jacobin science' would want to uphold against more theoretical science.[87] Was not Chladni, who did not hold a university post or membership in a prestigious academy, an excellent example of a 'marginal man'?

Unfortunately, this hypothesis will not go very far in helping us understand the acceptance of meteorites. To begin with, the controversy was not merely French, although France was perceived as its centre. The Royal Society of London took part in it, the Genevan journal *Bibliothèque Brittanique* carried much of the disputation, and articles by Chladni and others were published in German journals. Important falls took place in France (Barbotan 1790, Villefranche 1798, and l'Aigle 1803) but they also took place in Italy (Siena 1794), England (Wold Cottage 1795), and India (Benares 1798). Chladni was a marginal figure only from an institutional point of view; he was highly respected intellectually. His major opponents in the controversy were the brothers G. A. and J. A. DeLuc,[88] both Swiss. E.-M.-L. Patrin, a member of the Academy, who disputed both Chladni's theories and Howard's results, never did so in an official capacity, but as an individual savant. While the Academy, thanks to the rejection of the Lucé stone in 1772, was identified with a negative attitude toward meteorites, it *did* very little from 1772 until the last years of the controversy.

The events which led to Biot's investigation of the l'Aigle fall in 1803 (which was to prove the final piece of conviction in the controversy) are interesting in themselves. News of the l'Aigle fall reached Paris relatively quickly. The 'stones from the moon' were displayed in the public gardens, sold by natural history dealers, and sung in the streets.[89] When Chaptal, Minister of the Interior and President of the scientific section of the *Institut*, asked for an investigation, it is not certain that he actually believed that any real event would be discovered. He may simply have been eager to put an end to the public clamour. Here certainly was an external influence, although of a very transient sort.

Now of course it is possible that the events of the Revolution had made such a strong impression on the mind of the Academicians that they were now willing to accept the stones, although they would not have been willing to do so otherwise. And perhaps Chaptal was more responsive to the public's interest than he would have been before because of his own experiences during the Revolution. Yet it is hard to believe that Biot would not have been sent to l'Aigle in any case, given his own previously expressed views,[90] the controversy, and the short distance of l'Aigle from Paris.

There are also other indications. If Revolutionary influences were responsible, one would have thought that the Academicians would have seized every opportunity to laud the intelligence of the common people. This simply did not happen. In Biot's memoir, for instance, the *'peu de lumieres'* of the rural population is treated in a straightforward naturalistic manner; it is neither scorned nor applauded.[91] In an earlier paper by Patrin, however, in which he questioned the work of Howard and deBournon, he stated that the testimonies to the falls were based on *'hearsay* from individuals who are not named, and whose testimony is for the most part insignificant'.[92] At least in the Academicians' writings, there was no attempt to flatter the pride of the masses. Here the 'external causation' hypothesis fails for lack of evidence.

If we pose the question in a different way, however, we can see that some external influences were important. If we consider western Europe as a whole, we can see that the meteorite controversy was intimately connected to the problem of the autonomy of science. How could science resist penetration by faulty accounts of natural phenomena transmitted through the social intelligence system? The scientific community had a vested interest in not admitting data which it could not check, for to do so would have been to compromise its institutional autonomy. The meteorite controversy presents us not so much with the effect of external forces on the content of science, but rather with the scientific community's attempt to defend itself against such external pressures. The response to these pressures was favourable only when science found a criterion for deciding which data should be accepted, and the process of data acquisition was again placed firmly under scientific control.

Meteorites and Other Anomalies

What can the meteorite controversy tell us about the reactions of the scientific community to other anomalies? I think it offers several important lessons. The first is that in some cases the decisions of scientists about anomalous events are dependent on social intelligence. Somehow the relevant information has to reach the relevant people; in some cases this will mean that it will have to cross the boundary between the scientific community and the rest of society. Where anomalous events are involved, however, this kind of communication is fraught with difficulties. Mistrust and ridicule are common components of scientists' response to non-scientists' reports of anomalous events.[93] Even where these responses are not present, indifference and ignorance on the part of non-scientists, and the anticipation of negative reactions from scientists, may impede reporting.

The second lesson is the paradoxical attitude of scientists toward the social intelligence system. On the one hand, scientists are extremely sceptical toward reports of anomalous events coming from the social intelligence system. This is a perfectly natural reaction, grounded in the history of science itself and borne out by repeated experiences of fraud and error in social intelligence about unusual events. The mistrust and ridicule with which reports of unusual events are often greeted by scientists are very functional in the ordinary course of affairs. Yet scientists tend to believe that somehow the *real* anomalous events will be successfully transmitted by social intelligence, and that therefore there will be no great difficulties connected with information about such events. In the meteorite case this presumption was incorrect. Real anomalous events *were* taking place, and savants' negative attitudes toward the reports *did* impede the flow of information. I invite the reader to consider whether similar problems may not today impede the flow of information to scientists about real anomalies, as we know it does in regard to hypothetical ones like UFOs and sea-serpents.

The third lesson is that scientific acceptance is likely to be connected to scientists' ability to control the data in some way. Where this control is difficult to implement, as in the case of ball lightning, for instance, continued scepticism on the part of the scientific community is the probable result.[94] Yet this control itself is often dependent on scientists' willingness to attend to the signals of the social intelligence system long enough to do some useful research. We saw that the cursory examination accorded to the Lućе stone by the *Académie des Sciences* was insufficient. The important chemical analyses at the turn of the century, on the other hand, were the product of strong intellectual commitments to the reality of meteorite falls. If there is a common or interesting element in the data, some scientist has to pay close enough attention to the reports to detect what it is. At times this may involve taking intellectual and professional risks, as Chladni did. And as the meteorite controversy illustrates, paying close attention to the reports is not always sufficient to produce conviction, even if a real anomaly is involved. G. A. DeLuc, for instance, was very knowledgeable about reports, but until the l'Aigle fall he never gave them the least credence; and he accepted the reality of the l'Aigle fall very reluctantly.[95] For most scientists, however, the ability to check the data and the acceptance of the phenomenon go hand in hand.

A fourth lesson we might draw from the controversy, but which we can only briefly discuss here, is the importance of theory in validating data. Several aspects of the meteorite phenomenon seemed inexplicable at various points in the controversy: the fall itself; the globe of fire; the relatively low velocity in the lower atmosphere leading to shallow penetration of the soil. Linguistic confusion was engendered by use of the words 'thunderstone' and 'lightning-stone', and the false connection with storms interfered with a proper understanding of the nature of the falling stones. Not until Laplace and Biot put forward the 'lunar volcano' theory did most savants feel that a possible explanation existed, even though the correct explanation had been earlier propounded by Chladni. LaCroix put the problem very nicely:

> When a phaenomenon is announced, if we are able to ascertain, by a complete enumeration of the different physical agents, that none of them is capable of producing it, the impossibility of the phaenomenon would be the evident result, and consequently the falsity of the account.
>
> But, on the other hand, when we find a cause which establishes the probability of it, if sound logic forbids us to ascribe it exclusively to this cause, it commands us at the same time to substitute doubt for complete negation, and to employ every means possible of confirming the fact, because it is not repugnant to the laws of nature.[96]

To put the matter bluntly, if we cannot explain a phenomenon, we ought to reject reports of its existence, *if these reports come from non-scientists.* And we have already seen that some contemporary philosophers are willing to go further to reject unexplainable data, even from scientists. Certainly the credibility of ball lightning has suffered from the inadequacy of the theories offered to explain it. The logical implications of this question are beyond my scope here,[97] but there is no question that this principle of 'what is unexplainable is impossible' is frequently applied by scientists to reports of anomalous events.[98]

Conclusion

If the sociology of knowledge is of any use at all, it must demonstrate the social influences on beliefs. The meteorite controversy shows one case at least in which the social environment of the scientific community was very important in influencing scientific discovery. But it also puts social influences in proper perspective. The public knew of meteorite falls, but it did not understand them. Scientists had to be persuaded that meteorites fell; but only scientists could provide the necessary insight for their understanding. The meteorite case demonstrates the importance of social intelligence; but the existence of large numbers of non-meteoric 'thunderstones' should also indicate its potential dangers.

How much the meteorite case can be generalized to explain other cases of scientists' rejections of anomaly reports is an open question. Certainly the analogy of the meteorite case to UFOs and sea-serpents is evident. But there are many other types of accepted former anomalies (like the mountain gorilla) or current hypothetical anomalies (like the Bigfoot or Sasquatch) which remain to be studied.[99] It is obvious that the acceptance of parapsychological events by the scientific community, for instance, presents some different problems (as well as some similar ones) to the anomalies studied so far.[100] Until more cases have been studied, our conclusions can be only tentative.

The meteorite controversy does indicate the need for a comprehension of the workings of social intelligence. As we have seen, the assumptions made by scientists about the process by which reports of anomalies reach them are often erroneous. Further details of this incomprehension are available from the two studies of my own already cited. Only with a realistic appreciation of how social intelligence about anomalies works can one expect to evaluate the kind of information one might be getting if an anomaly of such-and-such a kind exists. In particular, the problem posed by the suppression of reports and research on anomalies through ridicule needs to be considered before one decides that absence of information indicates lack of phenomena. On the other hand, the scientist can also make use of organizations dedicated to researching anomalies and the sometimes formidable amount of literature these societies make available.[101] There is also the series of *Sourcebooks* edited by William Corliss, which has largely used scientific sources themselves for records of anomalous events.[102] The existence of these organizations and compilations of anomalous events represents a change in the parameters of the social intelligence process, since they both tend to increase the efficiency of transmission of information about anomalous events.

I would like to conclude by summarizing my basic argument. Individuals in any society are likely to experience events which current scientific doctrine holds to be impossible or implausible. Some of these experiences may represent encounters with phenomena that scientists would wish to study, if they could get information about them. But information about such events is transmitted to scientists in very unreliable and imperfect ways. Therefore it cannot be assumed that absence of reports implies an absence of experiences. For the 'stones falling from the sky' the opportunity to study a large number of reports proved to be very effective. Perhaps it would prove valuable in other cases as well.

• NOTES

The author wishes to acknowledge the helpful critiques of several persons who read earlier drafts of this paper: Barry Barnes, Roger Hahn, David Jacobs, G. R. Levi-Donati, Marcello Truzzi, and three anonymous referees of this journal.

1. Discussions with my father, Professor Edgar F. Westrum, Jr, a physical chemist, have convinced me of the importance of data quality control for the practising scientist. The work of Harry Collins in this regard is very suggestive. See his 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9, No. 2 (May 1975), 205-24, and also his 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experiences of Researchers into Parapsychology', mimeo, in Proceedings of the First International Conference of the Society for Social Studies of Science, Cornell University (4-6 November 1976). See also G. N. Gilbert, 'The Transformation of Research Findings into Scientific Knowledge', *Social Studies of Science*, Vol. 6, Nos. 3 and 4 (September 1976), 281-306.
2. The type of anomalous events I am interested in here are not the same as the 'anomalies' which operate in Kuhn's theory of scientific revolutions. (See T. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn. [Chicago: The University of Chicago Press, 1970].) They do not necessarily pose problems for scientific theory, and unlike the anomalies with which Kuhn is concerned, it is the existence of these anomalies which is controversial, not their interpretation. I have considered several alternative designations for these events, but none, including 'occult', 'controversial', and 'hypothetical', is really satisfactory. Nonetheless, it is evident that UFOs, sea-serpents, ghosts, psychic events, and abominable snowmen all share a common intellectual situation in that 'witnesses' allege to have seen them, while scientists firmly deny that they exist. Charles Fort referred to them as 'damned' data because of their scientifically unacceptable character in terms of current 'dominants' (paradigms). See *The Book of the Damned*, in T. Thayer (ed.), *The Books of Charles Fort* (New York: Henry Holt, 1941). Again, however, it is often scientific intuitions which are offended by these events, rather than scientific theory.
3. R. Westrum, 'Social Intelligence About Anomalies: The Case of Unidentified Flying Objects', *Social Studies of Science*, Vol. 7, No. 3 (August 1977), 271-302, and 'Knowledge about Sea-Serpents', in R. Wallis (ed.), *Sociological Review Monographs*, special number of 'Rejected Knowledge' (forthcoming, 1979).
4. G. Maxwell argues very cogently that 'theoretical entity' is not a useful concept in his 'The Ontological Status of Theoretical Entities', in H. Feigl and G. Maxwell (eds), *Minnesota Studies in the Philosophy of Science*, Vol. III (Minneapolis: University of Minnesota Press, 1962), 3-27. Discussion of this point is outside the scope of this article.
5. I have used 'savant' in preference to other terms as the one in most common use at the time in France to designate the people we now refer to as 'scientists'; the most common British term at the time seems to have been 'philosopher'. Both of these terms, however, were also used to designate non-scientists. It is ironic that

words for the individual specialties were in common use while there was no term to describe scientists in general. See S. Ross, 'Scientist: The Story of a Word', *Annals of Science*, Vol. 18, No. 2 (June 1962), 65-85.

6. The classical expression of this view is that of Charles Fort. See Fort, op. cit. note 2, 19-21. See also A. C. Oudemans, *The Great Sea Serpent* (Leiden: E. J. Brill, 1892), ix-x.

7. P. Morrison, 'The Nature of Scientific Evidence: A Summary', in C. Sagan and T. Page (eds), *UFOs — A Scientific Debate* (Ithaca, NY: Cornell University Press, 1972), 276-90.

8. For instance, the information on meteorites in D. H. Menzel, 'Case Against UFOs', *Physics Today* (June 1976), 13-15, is inaccurate.

9. E. F. F. Chladni, *Über den Ursprung der von Pallas gefundenen und anderer ihr ähnlicher Eisenmassen, und über einige damit in Verbindung stehende Naturscheinungen* (Riga: J. F. Hartknoch, 1794).

10. D. Tata, *Memoria sulla Pioggia di Pietre* (Naples: Nobile and Co., 1794).

11. See Leman, article on 'Pierres meteoriques', in the *Dictionnaire d'Histoire Naturelle* (Paris, 1818), 237-83, at 248.

12. E. F. F. Chladni, *Über Feuermeteore und über die mit denselben herabgefallenen Massen* (Vienna: J. B. Huebner, 1819), 9-10. Chladni would not even publish the names of other physicists who agreed with him, for fear their reputation might be harmed.

13. The best general overview of the meteorite controversy is still M.P.M.S. Bigot de Morogues, *Mémoire Historique et Physique sur la Chute des Pierres Tombées sur la Surface de la Terre à Divers Époques* (Orléans: Jacob Ainé, 1812). Many of the relevant documents are given in J. Izarn, *Pierres Tombées du Ciel ou Lithologie Atmosphèrique* (Paris: Delalain Fils, 1803).

14. D. W. Sears, 'Sketches in the History of Meteoritics, 1: The Birth of the Science', *Meteoritics*, Vol. 10, No. 3 (1975), 215-26; P. M. Sears, 'Notes on the Beginnings of Modern Meteoritics', ibid., Vol. 2, No. 4 (1965), 293-99; G. R. Levi-Donati 'La Polemica sulla "piogetta di sassi" del 1794', *Physis*, Vol. 17 (1975), 94-111.

15. J. B. LeRoy, 'Mémoire sur le Météore ou Globe de Feu, Observé au mois de Juillet dernier dans une grande partie de la France', *Histoire et Mémoires de l'Académie Royale des Sciences de Paris*, 1771 (Paris, 1774), partie 'Memoires', 668-92. (Hereafter *HMARSP.*)

16. A. D. Fougeroux, L.-C. Cadet and A. Lavoisier, 'Rapport . . . d'une observation communiquée par M. l'Abbé Bachelay, sur une pierre qu'on prétend être tombée du Ciel pendant un orage', *Introduction aux Observations sur la Physique, sur l'Histoire Naturelle, et sur les Arts*, Vol. 2 (1777), 251-55. This is apparently a reprint of an article which appeared in *Observations sur la Physique* (July 1772), which I have so far been unable to obtain.

17. LeRoy, op. cit. note 15, 668.

18. E. Halley, 'An Account of Several Extraordinary Meteors or Lights in the Sky', *Philosophical Transactions of the Royal Society of London*, Vol. 29, No. 341 (October-December 1714), 159-64; and 'An Account of the extraordinary METEOR seen all over England, on the 19th of March 1718/9. With a demonstration of the uncommon Height thereof', ibid., Vol. 30, No. 360 (April-May 1719), 978-90. (This journal will be referred to below as *Phil. Trans.)*

19. LeRoy, op. cit. note 15, 680.

20. 'Trois faits singuliers du meme genre', *HMARSP*, 1769 (Paris: 1772), partie 'Historie', 20-21. For reasons that are not clear, the analysis in 1772 (see note 16) dealt with only two of the stones.

21. Fougeroux et al., op. cit. note 16, 251.

22. P. Bertholon, 'Observation d'un globe de feu', *Journal des Sciences Utiles*, Vol. 4, No. 24 (1791), 224-28.

23. E. Howard and the Count de Bournon, 'Experiments and Observations on Certain Stoney and Metalline Substances, Which at Different Times are Said to Have Fallen on the Earth; Also on Various Kinds of Native Iron', *Phil. Trans.*, Vol. 92 (1802), 168-212.

24. E. de Drée, 'Sur les masses minérales dites tombées de l'atmosphère sur notre globe', *Journal de Physique, de Chimie, et d'Histoire Naturelle*, Vol. 56 (April-May 1803), 380-89, citation at 380. (Hereafter *JPCHN*.)

25. J.-B. Biot, 'Note sur des substances pierreuses d'une nature particulière, que l'on assure être tombées sur la terre', *Bulletin des Sciences de la Société Philomatique*, No. 66 (August-September 1802), 159-60; and 'Sur les substances minérales pretendues tombées du ciel, et nouvellement analysées par MM. Howard et Bournon', ibid., No. 68 (October-November 1802), 153-56.

26. J. -B. Biot, *Relation d'un Voyage fait dans le département de l'Orne pour constater la réalité d'un météore observé à l'Aigle le 26 Floréal an 11* (Paris: Baudouin, 1803). (Note: the date given in the title is in error. It should be 6 Floréal, or 26 April 1803, as the text of the book shows),

27. For an overview, see J. Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, NJ: Prentice Hall, 1971), 75-87.

28. D. McKie, 'Scientific Societies to the End of the Eighteenth Century', in A. Ferguson (ed.), *Natural Philosophy Through the Eighteenth Century and Allied Topics* (Totowa, NJ: Rowman and Littlefield, 1972), 133-43.

29. D. M. Knight, *Natural Science Books in English 1600-1900* (New York: Praeger, 1972), especially 63-106; J. -L. and M. Flandrin, 'La Circulation du Livre dans la Société du 18e Siècle', in F. Furet (ed.), *Livre et Société dans la France du XVIIIe Siècle*, Vol. 2 (Paris: Mouton, 1970), 39-72; D. McKie, 'The Scientific Periodical from 1665 to 1798', in Ferguson (ed.), op. cit. note 28, 122-32; D. A. Kronick, *A History of Scientific and Technical Periodicals*, 2nd edn. (Metuchen, NJ: Scarecrow Press, 1976).

30. See for instance R. Hahn, 'Scientific Research as an Occupation in Eighteenth-Century Paris', *Minerva*, Vol. 13 (1975), 501-13; M. Crosland, 'Development of a Professional Career in Science in France', ibid., 38-57; and D. Stimson, *Scientists and Amateurs: A History of the Royal Society* (New York: Greenwood Press, 1968), 161-96.

31. The changing fashions in scientific interests are indicated in the course of the eight volumes of L. Thorndike's *History of Magic and Experimental Science* (New York: Columbia University Press, 1923-58); also R. F. Jones, *Ancients and Moderns, A Study of the Rise of the Scientific Movement in Seventeenth Century England* (St. Louis, Mo.: Washington University Press, 1961), 119-47.

32. F. D. Adams, *The Birth and Development of the Geological Sciences* (New York: Dover, 1954), 118-24.

33. There is a considerable number of cases in which meteorites have actually been the object of worship. See L. LaPaz, *Hunting Meteorites: Their Recovery, Use, and Abuse from Paleolithic to Present*, Topics in Meteoritics No. 6 (Albuquer-

que, NM: University of New Mexico, 1969), 84-94. Also H. N. Nininger, *Out of the Sky* (Denver, Col.: University of Denver Press, 1952), 5-8.

34. E. Pontoppidan, *The Natural History of Norway* (London: A. Linde, 1755), 174-76.

35. A. de Jussieu, 'De l'Origine et des Usages de la Pierre de Foudre', *HMARSP*, 1723 (Paris, 1753), partie 'Histoire', 6-9.

36. See G.-L. L. De Buffon, *Histoire Naturelle*, Vol. VI (Paris: Imprimerie Royale, 1779), 225; article on 'Pierre de Foudre', in *Dictionnaire Raisonné Universel d'Histoire Naturelle* (Lyon: Bruyset, 1791); article on 'Foudre, Pierres de', in J. L. R. d'Alembert and D. Diderot, *Dictionnaire Raisonné des Sciences, des Arts et des Metiers* (Paris, 1757).

37. Mahudel, 'Sur les Prétendues Pierres de Foudre', *Histoire et Mémoires de l'Académie Royale des Inscriptions et Belles Lettres* [*HMARIBL*], Vol. 12 (Paris 1745), partie 'Histoire', 163-69.

38. Ibid., 163.

39. N. Frérét, 'Reflections sur les Prodiges Rapportez dans les Anciens', *HMARIBL*, Vol. 4 (Paris: 1746), partie 'Mémoires', 411-36.

40. Pliny the Elder, *Histoire Naturelle de Pline*, Vol. I (Paris: Desaint, 1771), 404-09.

41. See P. N. R. Usherwood, *Nervous Systems* (London: Edward Arnold, 1973), 88-93.

42. Cf. L. J. Cohen, 'How Can One Testimony Corroborate Another?', in R. S. Cohen (ed.), *Essays in Memory of Imre Lakatos* (Dordrecht: Reidel, 1976), 65-78.

43. The lack of such a non-obvious 'common factor' is one reason R. V. Jones suggests that reports of Unidentified Flying Objects ought to be rejected. See his 'Natural Philosophy of Flying Saucers', *Physics Bulletin*, Vol. 19 (1968), 225-30, at 229. Part of the difficulty may be the reliance on 'conjunctive' concepts. See J. Bruner, J. J. Goodnow and G. A. Austin, *A Study of Thinking* (New York: John Wiley, 1956).

44. See P. B. Medawar, *The Art of the Soluble* (Harmondsworth, Middx.: Penguin Books, 1969).

45. This is the Barbotan fall referred to earlier: Bertholon, op. cit. note 22.

46. Ibid., 226. (Emphasis in original.)

47. H. F. B. de Saint-Amand, letter in *Bibliothèque Brittanique*, Vol. 20 (May 1802), 85-89, at 88.

48. Howard and de Bournon, op. cit. note 23, at 175.

49. A. X. Stütz, 'Über einige vorgeblich vom Himmel gefallene Steine', *Bergbaukunde* (Leipzig, 1790), 398-409.

50. Ibid., 406-09. The importance of similarity is explicitly mentioned by Stütz, but only in passing; it is more implicit in the nature and composition of the paper. Stütz in fact makes more of an experiment of Komus in which iron was reduced by an electric spark, and caused a flaming mass to fall. Stütz wondered if a similar effect might not take place in the atmosphere on a larger scale, which would account for meteors.

51. Dr James Ritchie, Keeper of the Royal Scottish Museum at Edinburgh, in a letter to Rupert Gould, stated:

> Had you the opportunity of seeing the specimens, which are sent here for identification, supposed to be the remains, not of sea serpents, but of as monstrous land animals, and which turn out to be very ordinary things after

all, you would place very little faith in the casual observations, often in difficult conditions, of sea-men and others, even when the observations were supported with affidavits.

Quoted from R. Gould, *The Case for the Sea Serpent* (London: Philip Allan, 1930), 248-49.

52. Chladni, op. cit. note 12, at 5.

53. Private collections of natural history specimens were very popular in the eighteenth century. See Y. Lassius, 'Les Cabinets d' Histoire Naturelle', in R. Taton (ed.), *Enseignement et Diffusion des Sciences en France au XVIIIe Siècle* (Paris: Hermann, 1964), 659-70.

54. Chladni, op. cit. note 12, 8.

55. The exchange of specimens in return for expert opinions on them constitutes an interesting relationship from the point of view of the sociology of knowledge, and is worthy of a more extended discussion than space here permits.

56. See Westrum (1978), op. cit. note 3.

57. Tata, op. cit. note 10.

58. Ibid., 16.

59. Ibid., 23.

60. E.-M.-L. Patrin, 'Considérations sur les Masses de Pierres et de Matières Métalliques qu'on Suppose Tombées de l'Atmosphère', *JPCHN*, Vol. 55 (1802), 376-93, at 379. (Emphasis in original.)

61. G. A. DeLuc, 'Sur la Masse de Fer de Sibérie, et sur les Pierres supposées tombées de l'Atmosphère', *Journal des Mines*, Vol. 13 (1803), 92-107.

62. Ibid., 106.

63. Izarn, op. cit. note 13; also Guettard in Pliny, op. cit. note 40; J. de La Lande, 'lettre de Jerome de la Lande au C. Delamétherie, sur les Pierres de Foudre', *JPCHN*, Vol. 55 (1802), 451-53.

64. Chladni, op. cit. note 12, 9.

65. Biot, op. cit. note 25.

66. S. Freud, trans. J. Strachey, *The Complete Introductory Lectures in Psychoanalysis* (New York: Norton, 1966), 496.

67. See Fougeroux et al., op. cit. note 16, 251.

68. Patrin, op. cit. note 60, 393; C. Barthold, 'Analyse de la Pierre de Tonnerre', *JPCHN*, Vol. 50 (1800), 169-76, at 174.

69. 'I never related this narrative and shewed the concreted substance to any persons (which I should not have done but that the subject was now agitating) . . .' W. Bingley, letter in *Gentleman's Magazine*, Vol. 60, Pt. 2 (1796), 727.

70. Chladni, op. cit. note 12, 8-9.

71. B. Barber, 'Resistance by Scientists to Scientific Discovery', in B. Barber and W. Hirsch (eds), *The Sociology of Science* (Glencoe, Ill.: Free Press, 1962), 539-56.

72. D. Hume, *Enquiries Concerning the Human Understanding and Concerning the Principles of Morals*, 2nd edn. (Oxford: The Clarendon Press, 1962), 128.

73. M. Polanyi, 'The Growth of Science in Society', *Minerva*, Vol. 5 (1967), 533-45.

74. J. Pringle, 'Some Remarks upon the Several Accounts of the Fiery Meteor (Which Appeared on Sunday the 26th of November, 1758) and upon Other Such Bodies', *Phil. Trans.* Vol. 51, Pt. 1 (1760), 259-74, at 272.

75. H. Barham, 'A letter of the curious Naturalist Mr Henry Barham, RSS to the publisher, giving a Relation of a Fiery Meteor seen by him in Jamaica, to strike into the Earth', *Phil. Trans.*, Vol. 30, No. 356 (May-June 1718), 837-38.

76. H. H. Nininger, *Find a Falling Star* (New York: Paul S. Eriksson, 1972), 237-40.

77. Ibid., 237.

78. Ibid., 30, 55-56.

79. DeLuc, op. cit. note 61, 100.

80. Chladni, op. cit. note 12, 343.

81. C. Blagden, 'An Account of some late fiery Meteors: with Observations', *Phil. Trans.*, Vol. 74 (1784), 201--32, at 223-24.

82. B. Heuvelmans, *In the Wake of the Sea Serpents* (New York: Hill and Wang, 1968), 435.

83. DeLuc, op. cit. note 61.

84. Barthold, loc. cit. note 68.

85. A recent summary of literature on 'internal' versus 'external' causation in scientific discovery can be found in R. MacLeod, 'Changing Perspectives in the Social History of Science', in I. Spiegel-Rösing and D. de Solla Price (eds), *Science, Technology and Society: A Cross Disciplinary Perspective* (London: Sage Publications, 1977), 149-95.

86. See R. Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666-1803* (Berkeley, Calif.: University of California Press, 1971).

87. C. C. Gillispie, 'Science in the French Revolution', in Barber and Hirsch (eds), op. cit. note 71, 89-97.

88. Chladni, op. cit. note 12, at 9. J. A. DeLuc was nonetheless a foreign correspondent of the Academy of Sciences.

89. C. P. Brard, article on 'Meteorite', in *Dictionnaire des Sciences Naturelles*, Vol. 30 (Strasbourg: Levrault, 1824), 334-58, at 345.

90. Biot, op. cit. note 25.

91. Biot, op. cit. note 26, 7-8.

92. Patrin, op. cit. note 60, at 377. (Emphasis in original.)

93. See the articles by the author cited in note 3.

94. S. Singer, *The Nature of Ball Lightning* (New York: Plenum Press, 1971); and especially E. Garfield, 'When Citation Analysis Strikes Ball Lightning', *Current Contents*, Vol. 8, No. 20 (17 May 1976), 5-16.

95. G. A. DeLuc, *Observations sur un Ouvrage intitulé Lithologie Atmosphèrique* (Geneva: G. J. Manget, 1803), 39.

96. S. -F. Lacroix, 'On the Stones supposed to have fallen from the Clouds', *Philosophical Magazine*, Vol. 15 (February 1803), 187-88, at 187. This is an obvious echo of the views of Biot, op. cit. note 25.

97. But see J. Agassi, 'When Should We Ignore Evidence in Favour of a Hypothesis?', in J. Agassi, *Science in Flux* (Dordrecht: Reidel, 1975), 127-54.

98. For instance, see W. Markowitz, 'The Physics and Metaphysics of Unidentified Flying Objects', *Science*, Vol. 157 (15 September 1967), 1274-79.

99. On the mountain gorilla, see B. Heuvelmans, *On the Track of Unknown Animals* (New York: Hill and Wang, 1959), 54-56; on the Bigfoot, see J. Napier, *Bigfoot: The Yeti and Sasquatch in Myth and Reality* (London: Jonathan Cape, 1972).

100. See Collins (1976), op. cit. note 1; and also M. McVaugh and S. H. Mauskopf, 'J. B. Rhine's *Extrasensory Perception* and Its Background in Psychical Research', *Isis*, Vol. 67 (June 1976), 161-89.

101. Two such societies are: The International Fortean Organization, 7317 Baltimore Ave., College Park, Md. 20740, USA; and The Society for the Investigation of the Unexplained, Columbia, NJ 07832, USA. Both publish periodicals.

102. W. Corliss, *Strange Phenomena* (2 Vols., 1974); *Strange Artifacts*, Vol. 1 (1974), *Strange Planet* (1975); *Strange Universe* (1975); *Strange Artifacts*, Vol. 2 (1976); *Strange Minds* (1976); *Strange Life* (1976). All privately published by Corliss at Sourcebook Project, Glen Arm, Md. 21057, USA.

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