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The Meaning of Data: Open and Closed Evidential Cultures in the Search for Gravitational Waves¹

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The meaning of scientific “data” depends on the “evidential culture” of laboratories. Using transcripts of interviews and conversations with scientists, open and closed evidential cultures are analyzed under three dimensions. For example, an Italian laboratory’s evidential collectivism and an American laboratory’s evidential individualism are contrasted. In this case—the detection of gravitational radiation—evidential cultures are found to be homologous with institutional settings. The data interpretation of the long-standing small science is being influenced by the growing global dominance of a new big science. The interesting technique of “involuntary blinding” has been used to enforce a uniform approach.

MIT campus (Cambridge, Massachusetts), the “Laughing Seafoods” restaurant, December 1996.—Over dinner, two groups of experimenters are arguing about how gravitational radiation data should be interpreted. The data they are talking about are coincident bursts of energy affecting two or more widely separated resonant metal bars. The team from Frascati

¹ I am grateful to Ingemar Bohlin, Christa Knellwolf, Peter Dear, Dan Kennefick, Ian Welsh, and Robert Evans for helpful comments on various drafts of this paper. Many of the lively remarks of audiences at the Wellcome Institute in London, Cardiff University, Sussex University, and the University of New South Wales have also been incorporated. The constructive comments of *AJS* referees have influenced the text significantly. I am in debt to the many scientists who, over the years, have treated me as a colleague and have allowed me to share in a great scientific adventure spanning two millennia; I have learned much from their vigorous intellectual curiosity and refreshing optimism. All the respondents quoted at length read the paper and offered useful feedback, most of which has been incorporated; I do not thank them personally here so as to maintain a semblance of anonymity. Two physicists not quoted at length, Sam Finn and Peter Saulson, read a draft of the paper with enormous care and gave me encouragement and advice. I am, of course, entirely responsible for the paper’s faults. This research received generous financial support from the U.K. Economic and Social Research Council (grants R000235603 and R000236826). Correspondence may be directed to H. M. Collins, KES, Cardiff University, 50 Park Place, Cardiff, CF1 3AT, United Kingdom. E-mail: collinshm@cardiff.ac.uk

has suggested that it is reasonable to publish reports that say that coincidences have been seen, even if they do not claim that they are gravitational waves. The group from Louisiana State University (LSU) disagrees and would prefer to publish nothing.

LSU2: That's a difficult thing—it's not the—it's a reasonable thing to do, but it's not what most physicists want to see. Normally, you get to the end of an experiment and you give a result. "I didn't find the X-particle, and my results are consistent with 'X-particles will not be produced this way because their cross section must be less than something.'" That's a negative result, and a positive result is "I saw the thing. . . ."

If you say, "I saw something very unusual, that doesn't seem as though it could be due to chance," then, what do you do—that's the grey area—you're not making a negative claim, you're not making a positive claim, you're somehow in the middle.

Frascati1: You know, one consequence of all this is the following—[it is] very strange—that you do a coincidence experiment; if you find nothing—in other words, if you find a number of coincidences equal to the number of accidentals—then you publish, and you give the upper limit [that is, you say what the maximum flux of gravity waves can be]. If you find an excess [a positive result], you don't publish—that's the conclusion [laughter]. Don't you think so?

LSU1: There is a danger of that—there is.

Frascati1: If you find nothing, you publish; if you find an excess, you don't publish—I mean an excess with minimum probability of 1% or 1 per 100. You see, the only excuse not to publish is if one is afraid that by publishing there might be bad consequences, like not getting money from the funding agency, or things of that sort. Then you can be excused for not publishing, but it is a bad thing. But—of course . . .

LSU1: Well, we have been in that situation too—in years past—I don't think we are anymore. But—there's . . .

LSU2: The problem is particularly difficult for us. It's not the normal problem in science, I don't think—frankly. Or at least, a fair fraction of science. If we really did see a gravitational wave at our current level of sensitivity, it would be in conflict with what many people believe is well-founded, established facts. It might be in conflict with the general theory of relativity, which is not . . .

Frascati1: No! . . .

LSU2: Well, if we saw gravity waves, what possible conclusions can you draw? What is the theory? . . .

Frascati1: Well, first point is that a coincidence excess might be due to another phenomenon, not gravitational waves—a very important phenomenon, even, at this point, even more important, that nobody knows about.

LSU2: That's a second possibility, that there's unanticipated physics operating. Well, that would be—that's a good reason to publish if you've got evidence that's pretty clear. But in my opinion, you have to be pretty certain that the chance of this being ordinary accidental behavior has to be pretty small to make it worth it to even start to believe such a thing.

Frascati1: But the definition of small is not easy. . . .

Frascati1: I think each one follows his own philosophy. My personal phi-

osophy is the following: if I have a situation where I don't know whether I should do something or not to do it, I do it. Because the outcome is better than no outcome. Now this does not apply . . .

LSU1: I was gonna' say that that could lead to overpopulation of the world. [laughter]

Frascati1: Because, if you don't do it, it's nothing—if you do it, it's something.¹

INTRODUCTION

The above conversation represents a clash between open and closed “evidential cultures.” The evidential cultures are related to the institutional contexts in which the two laboratories find themselves. To understand the passage sociologically, one needs to know something of the history of the field of physics in which the laboratories work, something of recent events in that field, and something of physics itself. In this kind of study, one has to know a little science in order to understand the sociology, and this means the structure of the article presents a chicken-and-egg problem: Should it be physics first or sociology first? I have tried to solve the problem in the following way:

Following this introduction, I briefly compare sociology of scientific knowledge (SSK), the approach used in this article, and earlier sociology of science. I indicate the methodological leaning of the article and the scope of the case study.

The next section contains a first description of gravitational radiation research and an indication of the features of the field that makes it an especially interesting site for sociological research. After that, I introduce the idea of evidential cultures and illustrate it with examples and quotations covering publication strategy. I then set the evidential cultures in their institutional context, introducing a little more physics on the way.

The second main section of the article introduces two recent incidents in the field. It begins, perforce, with more physics, concentrating on data analysis. It moves on to discuss the solution to the clash of cultures that has been put in place by the American laboratory against the wishes of the Italian laboratory.

A description of a recent controversial claim to have detected gravita-

¹ With the exception of a few E-mails, all the quotations in the paper are taken from interviews or conversations recorded by the author. All have been cleared for publication by the speakers. Most respondents felt more comfortable with the anonymous “style” that has been adopted throughout. Of course, most insiders will easily recognize many speakers, but occasionally, because I was asked, or because I thought it more appropriate, I have disguised identity more thoroughly. I retain a master copy of the paper with a complete key. Several respondents asked me to edit their “uhms,” “ers,” “you knows,” and “sort ofs,” and I have removed them throughout.

tional waves brings us back to the Laughing Seafoods conversation. The article ends with the conclusion that the meaning of data in physics depends on evidential culture and institutional setting.

APPROACH AND METHOD

The sociology of scientific knowledge (SSK), the style of analysis adopted in this article, is a quarter of a century old. Nevertheless, it is still frequently referred to as a new and controversial approach. Compared to the earlier, Mertonian tradition, it differs in several ways. It does not start with the assumption that there is a single epistemologically efficacious set of social norms that guides scientists to the truth. Though this article could be said to follow in the Mertonian tradition in the sense of being concerned with patterns of scientific action, it compares the actions of one group of scientists with the rather different actions of another group, without making a judgment about which is better.

It might be said that even in this the approach follows Merton, for in his paper on ambivalence (Merton 1963), he discusses the tension between the desire for priority in science and the imperative to be right. But SSK, though it describes individual dilemmas and strains within patterns of action, is primarily concerned with groups, their social settings, and their scientific cultures, or "forms-of-life" (Wittgenstein 1953; Winch 1958). Moreover, systematic differences between groups are shown to affect what counts as scientific knowledge. The analyst has to enter far more deeply into the science than was necessary in the Mertonian tradition, sometimes embracing competing scientific positions. To do this, views about scientific truth have to be set aside, at least in respect of the matter under examination and at least temporarily.³

³ For sociological ambivalence, see Merton (1963). The tradition of the analysis of the norms of science goes back at least as far as Merton (1942). (See also Merton 1973; Shapin 1994). The tradition of empirical and historical studies of the SSK type goes back at least as far as Ludwik Fleck, though it was not recognized as a tradition until much later and some of the contributors listed below might resist the label. Early works include Fleck ([1935] 1979), Kuhn (1961, 1962), Collins (1975, 1981a, 1992), Holton (1978), Latour and Woolgar (1979), Shapin (1979), Knorr-Cetina (1981), MacKenzie (1981), Pickering (1981, 1984), Pinch (1981, 1986), Travis (1981), Collins and Pinch (1982), Gieryn (1983), Lynch (1985), Galison (1987), Shapin and Schaffer (1987). Within this later tradition, this article stresses the way networks of scientists and of laboratories negotiate the meaning of data between themselves. This style of research goes back at least as far as the study reported in Collins (1975). While adopting "methodological relativism" (Collins 1981c) and the principle of symmetry (Bloor 1973), it resists the extension of symmetry to the relationship between human and nonhuman entities (Callon and Latour 1992; Collins and Yearley 1992). For easily accessible reviews of the field written from the perspective of more recent trends, see articles in the *Annual Review of Sociology* by Collins (1983a) and Shapin (1995).

This setting aside of the scientific truth has led some commentators to imagine that SSK endorses irrationality or some such, but, on the contrary, it differs from other ways of analyzing science in imputing *more* rationality to scientists than they allow themselves. When a minority of scientists hold scientific views that most other scientists reject, the former are often taken to be acting irrationally or unreasonably. SSK, however, with its "symmetrical" approach (Bloor 1973, 1976), tries to recapture the rationale of all sides; the mistake is to think that rationality can lead in only one direction (Franklin 1994; Collins 1994). One might say that SSK tries to make sense where scientists—and those sociologists who accept the majority's interpretation of matters scientific—make nonsense (of the minority view). That is a difference between sociology of scientific knowledge and science (and the older sociological tradition) that many have misunderstood. It arises because it is scientists' responsibility to make science and, therefore, to make the nonsense with which the sense can be contrasted; it is not sociologists' responsibility to make either.

To carry through this kind of analysis, the sociologist must adopt a stance widely known as "methodological relativism." Methodological relativism implies that, for the purpose of analysis, the sociologist "brackets out" received views of scientific sense and nonsense. Instead, the focus is on the interpretative flexibility that is available to scientists in respect to their findings; the networks of beliefs, practices, and interests that favor one interpretation over another; and, ideally, the way that one interpretation rather than another comes to predominate.⁴ If received views about scientific truth are not set aside, the whole enterprise becomes biased at best or circular at worst. Whether or not this methodological relativism implies a commitment to an epistemological or ontological position has been the topic of much philosophical debate, which need not concern us here. Here, all that we need in the way of epistemology and ontology is some slack.

The more narrow methodological stance adopted in this article is "participant comprehension" (Collins 1984). Participant comprehension is an interpretation of participant observation under which the field-worker tries to acquire as high a degree of native competence as possible and interaction is maximized without worrying about disturbing the field site; this ideal should always direct the research effort, even though the degree of native competence attained will vary from study to study. In this case, my understanding of the field of gravitational radiation detection has been gained from discussions between 1971 and 1976 with most of the major actors in the field and from more intense contact with the field since 1993.

⁴ The approach has been referred to as the empirical program of relativism, or EPOR (Collins 1981b, 1983b)

Recent work has been made possible by financial support covering the whole of my time in the year 1995–96 and most of my time from 1996 to 2001. During these periods, I have, so far, carried out around 150 tape-recorded interviews in the United States, Britain, Germany, Italy, and Australia; attended around a dozen conferences and committee meetings (including private meetings at funding agencies); engaged in scores of E-mail and telephone interchanges with physicists; spent periods in laboratories and in on-site visits throughout the world; and, perhaps most important, joked, chatted, and argued with physicists about their work and mine, in corridors, cafés, cars, buses, restaurants, and a boat.⁵ Had my early papers not been seen as reasonably accurate and responsible portrayals of events, I would not have been able to reenter the field in the 1990s. That said, I would not claim to have achieved anything like full native competence in gravitational radiation research—I cannot actually do it myself—but I believe I have gained enough understanding to be able to carry out the kind of sociological analysis presented here.⁶ On two memorable occasions I have even managed to argue my corner while talking minor matters of gravitational wave physics with physicists, and on a third occasion I was told that my critical queries would affect the design of a major new instrument.

This article has been seen by the principle characters discussed within it and by other sympathetic physicist informants, and they have found no fault with the physics. Needless to say, some respondents would have preferred the analysis to be less neutral, and this is always likely given that explaining one side's position looks like attacking the other's. Other physicists commended the paper as an accurate appreciation of a recent episode in the history of their enterprise. The opinions offered by scientists

⁵ The places I have visited once or many times so far in connection with this project include (number of visits in brackets) the universities of Bristol (3+), Reading, Glasgow (2+), Sussex, Cardiff (2+), Hannover (2), Leiden, Maryland (5+), MIT (3+), Irvine, Stanford (3), Caltech (5+), Rochester (3), Louisiana State (5+), Tor Vergata-Rome, Padua, Milan, Perth, Adelaide, Canberra. I have also been to industrial labs, including IBM (3), Bell Labs (3), Hughes Aircraft, CSIRO Sydney, NASA-Goddard Space Center. Other locations include the Max Planck Institute—Munich, Sophia-Antipolis, the LIGO site at Hanford Nuclear Reservation (2), the LIGO site at Livingston, Louisiana (2), the GEO600 site near Hannover (2), the VIRGO site near Pisa (2), the Frascati labs (3+), the NSF in Washington (4+); the offices of April Burke and Kevin Kelly in Washington; the homes of Joel Sinsky in Baltimore, David Zipoy in Punta Gorda, Florida, and Bob Forward near Inverness. I have also attended conferences or committee meetings in Washington, Pisa, Orsay-Paris, Jerusalem, Geneva, Boston, Hanford, Livingston, and the California Institute of Technology (Caltech).

⁶ In this judgment, I benefit from the experience of other case studies, including one, on parapsychology, in which I became a full-blown expert, and another, on the theory of amorphous semiconductors, which I had to abandon because I could understand none of the science.

on the merits of the sociological analysis were not correlated with their position in the scientific debate.

Participant comprehension licenses a certain approach to interview material. Interview extracts *illustrate* what analysts understand to be going on as a result of their experiences in the field and are used to convey this understanding to a less expert audience; they are not thought of as *data*.⁷ The scientific warrant of this paper as a whole is its internal logic and the fieldwork it reports.

There is an interesting tension between methodological relativism and participant comprehension, because few native physicists are relativists. An acute problem arises when the analyst looks at a scientific controversy, because then the job is to acquire native comprehension of both sides of a debate—something that natives rarely need. One might express this as an issue of methodology. One of my failings as a participant-observer/comprehender is that I cannot generate the levels of disdain for physicists that one physicist can generate for another; I know what I am missing because, given that I have no commitment to symmetry in my own field, I can readily generate such levels of disdain for some of my fellow social scientists.⁸ Fortunately, given the qualifications and compromises described above, science does not have to be perfect to be science; it is fortunate, because otherwise there would be no social science nor any physical science either.

THE DETECTION OF GRAVITATIONAL RADIATION

According to the General Theory of Relativity, gravitational radiation is emitted when masses move. Because they are so weak, only huge cosmic catastrophes, such as the explosion or collision of stars, can generate detectable fluxes of gravitational waves. If the shape of gravitational waves is ever revealed in detail, a new field of astronomy will come into existence; gravitational radiation is the only direct way to see into black holes and many other features of the universe. The better part of a billion dollars is now being spent worldwide in order to realize the aim of a new gravitational astronomy using the technique of "interferometry." At the same time, the older and cheaper program of detection, pioneered in the 1960s by Joseph Weber of the University of Maryland and using resonant metal

⁷ Compare the notion of interview material found in Mulkay, Potter, and Yearley (1983) and the rebuttal in Collins (1983b).

⁸ The classic participant-observer study (Festinger et al. 1956) was flawed for the opposite reason: the "participants" had too much disdain for their subjects. Festinger et al. were not trying to "comprehend" but only to observe the "irrational" objects of their study (Collins 1984).

bars, continues with instruments of increasing sensitivity. In this technique, a gravitational wave would make its presence felt as a coincident input of energy on two or more bars separated by thousands of miles.

In the early 1970s, Weber announced that he had seen gravitational waves with his bars. In earlier papers, I reported on the controversy that followed (Collins 1975, 1981a, [1985] 1992). Weber's results were widely disbelieved, and by 1975 they had few supporters. The episode is an important part of the background to the story that follows.

Gravitational radiation detectors, like many pathbreaking instruments, have a very poor "signal to noise ratio." According to the dominant theories, in spite of improvements in sensitivity by a factor of perhaps a million over the last four decades, none of the detectors that will be completed before around the year 2006 ought to be able to see the effect unless they are very lucky.⁹

The estimate of an instrument's likelihood of seeing gravitational waves is based on calculations of its sensitivity, the gravitational energy that ought to be emitted during cosmic catastrophes—such as the spiralling-in and coalescence of neutron stars in orbit round each other—and the likely frequency of such events at various distances from us in the heavens. The opportunity for a lucky break arises because one or two such events could take place relatively close to us, in our own or a nearby galaxy, giving a unusually strong signal at the surface of the earth.¹⁰ Even with luck, however, it is difficult to tell when a disturbance in a detector is a signal and when it is just a peak of accidental noise, because a "strong" gravitational wave is still about the weakest thing to be measured since measurement began.

It is because gravitational waves are so weak that to separate them from noise in the detector it is necessary as a first step to look for coincident signals on two or more widely separated detectors. There is one research group that runs two gravitational antennae that are widely separated (in Rome and Geneva), but the credibility of a finding is boosted if the coincidences are seen by apparatuses belonging to separate laboratories. In effect, since the middle 1970s, a device for detecting bursts of gravitational waves has consisted of two or more antennae run by two or more groups, most often located in two or more countries.¹¹

⁹ *Pace* the theory of bars developed by Weber and Guiliano Preparata of the University of Milan (Weber 1984; Preparata 1990).

¹⁰ On the other hand, we might be wrong about the constitution of the heavens (see below) and each new generation of detectors might just turn out to have crossed the crucial sensitivity barrier for as yet unknown events and processes.

¹¹ This is true only for signals that come in bursts. Detection of the waves from continuous sources, such as those from pulsars, can, in principle, be accomplished with only one detector. The random background gravitational radiation left over from the big

Collaboration and Conflict

When science is done by individuals or by small teams, experiments are worked up, data are taken, analyzed, and interpreted, and then "results" are published, all under the control of one laboratory. The boundary between the private world of the laboratory and the public world of journals and conferences can be policed by scientists working on their own or leading a physically and morally integrated team to a consensus. Of course, the wider world of science might not accept the results, and controversy and rejection might follow, as in the case of Weber's early findings. Nevertheless, the differences between developing an experiment, evaluating the data, and announcing the results for further evaluation beyond the laboratory are clear. This is important, because it is our distance from the untidy world of the laboratory, with its continual adjustments, choices, and need for active interpretation, that helps separate science from mundane activities. The idea of the laboratory as a place where the consequences of nature's agency are observed and reported with a minimum of human intervention is maintained by keeping the laboratory closed.¹² But, in the case of gravitational radiation data, because findings are sets of coincident readings, responsibility for initial interpretation is the prerogative of more than one team. All science is, in the last resort, communal, but in the search for bursts of gravitational waves, international collaboration is a *condition* for the *initial* announcement of a credible result.

Such an intrinsically intimate relationship between diverse groups of scientists is very unusual. There are, of course, many examples of replication by other groups or collaborations in which the findings of different

bang or produced by the huge numbers of collapses and explosions taking place throughout the universe needs two detectors that are close together. Such results would still need to be replicated if they were to be believed, but they are not intrinsically "coincidence experiments." Until very recently, the history of the search for gravitational radiation was the history of the search for bursts of energy, and here, when I refer to gravitational radiation, I will be referring to bursts. All the incidents reported here concern the search for bursts.

¹² This is a very important principle in the history and sociology of scientific knowledge. It enables one to establish the difference between an "experiment," a "result" announced in public, and a "demonstration," which is a display of well-established results, perhaps using techniques to enhance the effect that would be considered to be cheating in an experiment. (See, e.g., Gooding 1985; Shapin and Schaffer 1987; Collins 1988). Notoriously, historians and sociologists of scientific knowledge invade the privacy of the laboratory and threaten these distinctions. Holton's famous study of Millikan's oil drop experiment and many other historical and sociological analyses of famous experiments (see n. 3 above) exhibit the problems. This very article may cause discomfort because of the way it exposes what would normally be the private thoughts and conversations of scientists, not yet honed and refined for public consumption. I am sorry that my job sometimes makes me appear to trespass upon the conventions of friendship.

laboratories are aggregated, but in this case, there are no findings at all until the raw outputs of the groups are combined. Because of this, clashing research styles will cause the laboratories to pull against each other like convicts from a chain gang trying to run in opposite directions. As with the convicts, once one knows what to look for, the predicament is striking, giving the analyst a great advantage.

There is no reason to think that this forced grinding together of potentially different scientific cultures makes for untypical science in any other way. The three dimensions of evidential culture to be discussed below are likely to be found across the sciences, wherever conditions are appropriate, even though they may be less obvious in places where different laboratories are isolated within separate research programs. It remains to be seen, however, if divergencies in evidential culture are typical only of relatively small sciences. Big sciences, such as high-energy physics and gravitational wave interferometry (see below), may or may not be able to sustain a variety of styles in spite of being global enterprises.

THREE DIMENSIONS OF EVIDENTIAL CULTURE

The LSU group and Frascati group are chained together by their joint research on gravitational radiation. As revealed in the Laughing Seafoods conversation, their evidential cultures do pull in opposite directions. I am going to suggest that evidential culture has three dimensions: "evidential collectivism" versus "evidential individualism"; high versus low evidential significance; and high versus low evidential thresholds.

Evidential Collectivism and Evidential Individualism

Evidential individualists believe that it is the job of the individual or individual laboratory to take responsibility for the validity and meaning of scientific results. Under this philosophy, as much responsibility as possible is gathered into the individual or individual laboratory. In contrast, evidential collectivists believe that it is the job of the scientific collective to assess results from an early stage. In the case of both individualists and collectivists, the ultimate arbiter is, of course, the scientific community, but the individualist will consider it a matter of professional failure if the community rejects results sent forth from the laboratory while the collectivist thinks discussion by the core-set (Collins 1992)—whether the outcome is acceptance, rejection, or a demand for further clarification—to be a normal part of the scientific process.¹³

¹³ Evidential collectivism differs from the norm of "organized skepticism" (Merton 1942). Organized skepticism is a collective activity, but it is meant to *check* the validity of *individuals'* findings rather than interpret them. In any case, the idea of organized

The two philosophies can be usefully compared to styles of car driving.¹⁴ The first duty of a British driver, or an American driver is not to upset the equanimity of others who are driving acceptably—this is an individualist ethos. Rome, on the other hand, represents a case of driver collectivism. For example, on Rome's crowded ring-road, I once stopped my car on a busy roundabout while I conspicuously consulted a map. In Britain, such behavior would be almost unthinkable, and every passing driver would hoot and gesticulate, exhibiting what has become known as "road rage." In Rome, the approaching traffic simply skirted around the obstacle I presented without a second glance. Another driving example was offered to me by an American physicist. He explained that once, in Rome, he was parallel parked nose to tail on a road with a dense and slow-moving stream of traffic passing close to the parked cars. But, as his initial maneuvers indicated his desire to leave the parking space, the traffic squeezed itself across the road in such a way as to leave him room to extricate himself and join the traffic stream. The initial act of parking was, one might say, antisocial, illustrating, as he initially thought, the careless individualism of Roman drivers, but the cooperation of the community of drivers made it into a reasonable act.

In sum, in this part of Italy, responsibility for avoiding accidents and ensuring a smooth flow of traffic is passed to the community of drivers; this works well, without engendering fury. In Britain and America, the responsibility remains with the individual, and departures from the norms are met with sanctions.¹⁵

It has been put to me that the terms "individualism" and "collectivism" are here being used the wrong way round, and it is the Roman drivers who are the irresponsible individualists, while the American and British drivers are far less antisocial. But there is no such thing as a society made up entirely of individualists. The deep point is that in Rome it is the com-

skepticism has its roots in the supposed ready reproducibility of scientific findings rather than the intentionality of scientists' actions. For a discussion of the complexity of replication, understood through the notion of the "experimenters' regress," see Collins (1992). The Mertonian norm of "communism" refers to the collective *ownership* of results, not the collective establishment of results.

¹⁴ I am not suggesting that the boundaries of the collectivist driver culture coincide with the boundaries of evidential collectivism in science, merely that this is a similar set of social relationships found in a different field of activity.

¹⁵ That this is a matter of driving norms rather than national character can be seen because the relationships between car drivers and pedestrians is reversed in Italy and America. In most of America, drivers take it as their responsibility to avoid damaging pedestrians, who seem to think it their right to cause any amount of inconvenience to the driver. In Britain, it is the pedestrian who always defers to the car on pain of injury or, at least, indignation, and the same applies in Italy.

munity that is given responsibility for maintaining order, and that is why the individual can act with what looks like carelessness; the collectivity "repairs" any potential disruption before it becomes serious. In the British-American approach, it is individuals who are meant to retain responsibility for the smooth organization of traffic society.

These alternative ways of organizing society in respect of car driving reflect similar choices made throughout social and political life; after all, what we are talking about is different ways of dividing labor—in this case, it just happens to be cognitive labor. Consider that there are two ways to organize an undergraduate course in sociology during periods of high politicization of the subject. One can insist that every teacher of sociology presents an unbiased course, so that if, for example, they favor a Marxist approach, they also put the counterarguments, or one can allow each teacher to teach according to his or her biases but make sure that the faculty as a whole is balanced. The first of these solutions is individualist, the second is collectivist in the sense I am using the terms here. Consider again that those who insist that reflexivity is a vital part of the sociological analysis of scientific knowledge are individualists in the sense used here, because they believe that it is the duty of the *individual* to produce a complete analysis: the analysis must include not only a discussion of the social influences on the science under examination but also an analysis of the social influences on the analyst. The collectivist (such as myself, in this instance) believes it is satisfactory to complete the analysis of the social influences on the science, leaving other members of the community to analyze, if they are interested, the social influences on me (as in Ashmore 1989).

To return to gravitational radiation, I am arguing that the Frascatian approach (shared with the laboratory in Perth, Australia, to be introduced below) is set within the kind of collective ethos that is found on roads in and around Rome. The Louisianian approach to science is better compared to the driving style found on the majority of British and American roads.

Sociology would be much easier if only social life arranged itself as neatly as the analyst would like. Unfortunately, evidential collectivism is not the uniform style of Italian science or Australian science nor even any single laboratory within those countries. For example, the Frascati laboratory includes many members who are evidential individualists, and the discussion about whether to publish the contentious findings has been carried forward as much within the Frascati laboratory as between it and the rest of the world. For this reason, I will henceforth refer to the subset of Frascatians who do share the evidential culture under discussion as the "Frascati Team," while retaining the usage "Frascati group" for the

whole laboratory. What is more, there may well be groups of evidential collectivists scattered within the dominant culture of evidential individualism of Britain and America.

In spite of these reservations, it remains intriguing that the principle Italian physics journal, *Il Nuovo Cimento*, seems to express a collectivist philosophy. I asked its editor about his policy in respect to a recently published and notably "adventurous" paper. Joseph Weber (Weber and Radak 1996) claimed that his gravity wave data correlated with "gamma ray bursters." (The existence of this paper, I might add, was unknown to the very large majority of American gravitational radiation physicists, who generally no longer read *Il Nuovo Cimento* and who, in any case, gain most of their information from the informal networks within which Weber no longer has a voice.) The editor said:

Well, this is a complex issue, and it is very much a matter of the judgment of the editor in this case. . . . My opinion as an editor is that not all the papers have to be necessarily correct, but we must make every effort possible to publish papers which are not wrong—a priori wrong; if a paper is [obviously] wrong and we publish, this is unacceptable. But if a paper is not obviously wrong, and it is on a topic which is important, I think it can stimulate discussion. And the important thing is finally to have an answer—to promote the discussion and to reach a definite answer.

Now, in this sense, even if something is not absolutely proved to be final, it could generate other people to look, in order to disprove it, for example—to stimulate a search. . . .

This is a general approach that I have been following. There are, for example, other cases of theories which are not settled, which I decided to publish anyway in order to provoke—to generate discussion. . . .

So long as things are not done randomly, but are done by an editor in order to converge to an answer, that is OK.

While I will deal only with the contemporary scene, it is tempting to speculate about the historical roots of these phenomena. Steven Shapin's *Social History of Truth* (1994) examines the origins of scientists' actions in the norms of English gentlemanly behavior in the 17th century. The norms described by Shapin can, with a little stretching, be mapped onto evidential individualism. Questioning another gentleman's assertions could be construed as branding him a liar—a charge that could lead to a duel. The prohibition on "giving the lie" imposed a reciprocal obligation, making the validity of observations very much the responsibility of the individual, even while obeisance was given to the idea that the community was the ultimate arbiter. Thus, it may be that in exploring the relationship between English gentlemanly norms and science, Shapin has discovered the origins of evidential individualism and that other national traditions

or religious cultures may have given rise to different expectations of the way scientists should act, the traces of which are also still visible in the forms of life of modern science.¹⁶

Evidential Significance

Pinch (1981, 1986) was the first to discuss a component of what I am calling evidential culture. He described the search for solar neutrinos. As Pinch explained, experimenters try to detect neutrinos through their ability to transmute the chlorine atoms in a tank of cleaning fluid into radioactive argon. Putatively, radioactive argon atoms betray themselves as marks on a chart recorder. The experimenters can report this as "marks on a chart" (which is the only thing they see directly), "radioactive argon," "solar neutrinos," or other phenomena at different levels of generality. The higher the evidential significance, that is the longer the chain of inference, the more important the findings. But the more important the claim, the greater the risk of engendering opposition, and the longer the chain of inference, the more ways there are of being wrong. Thus, a choice of high evidential significance entails what we might call high "interpretative risk," while claiming low evidential significance involves only low interpretative risk.

In a similar manner, in gravitational radiation detection, the same coincidences can be reported simply as "coincidences" or as "gravitational waves." A claim to have seen gravitational waves is far harder to support and far less likely to be credible than a claim to have seen some coincidences. In the Laughing Seafoods conversation, we can see that the Frascati Team is happy to announce unadorned "coincidences," whereas the Louisiana group wants to announce nothing unless they are sure they have gravitational waves.

Evidential Threshold

A third independent dimension of evidential culture is choice of evidential threshold and concerns "statistical risk." Irrespective of whether the collectivity or the individual is considered to be the proper locus of scientific findings and irrespective of the degree of interpretative risk endorsed, a scientific culture might be more or less risk averse in terms of the level of certainty in the data that is taken to merit announcement or publication.

¹⁶ There is evidence that the same situation applied at one time in respect to England and Germany. Infeld in his autobiography (1941, p. 190) remarks that the attitude of prewar English journals was "better no paper than a wrong paper," while German journals felt "better a wrong paper than no paper at all."

The choice might be as simple as whether journals are full of two- or three-standard deviation results, as in the social sciences, or seven- or eight-standard deviation results or better, as in high-energy physics.¹⁷ In our language, the social sciences have a low evidential threshold, while high-energy physics has a high evidential threshold.

One might argue that the whole field of gravitational radiation research would never have started were it not for Joe Weber's high-risk—in both interpretative and statistical senses—reporting of putative phenomena. Weber may now be largely thought to have been wrong, but most scientists with a long acquaintance with gravitational wave physics would agree that he founded a field as a result of pressing forward obstinately with his claims.

In the Laughing Seafoods conversation and in the section on signal thresholds to be discussed below, one sees clearly that the Frascati Team endorses a strategy that involves high statistical risk—that is, low evidential thresholds—together with low interpretative risk (low evidential significance). The Louisiana group reveals the opposite position.

OPEN AND CLOSED EVIDENTIAL CULTURES

Putting together these three dimensions enables us to imagine an “evidential culture space.” In figure 1, the Louisianians are at one corner of the imaginary space—low collectivism, high significance, and high threshold, with the Frascati Team at the opposite corner—high collectivism, low significance, and low threshold.

What all three dimensions have in common is that the position adopted by the Frascatians on all of them tends to early release of relatively unprocessed data, whereas the position adopted by the Louisianians causes them to restrict access to their results until they have been much more highly processed. The Frascatians have an open evidential culture; the Louisianians have a closed evidential culture.

These two corners of evidential culture space also represent the change from the early days of gravitational radiation detection—when it was reasonable to “point a detector at the sky” and report what was seen—to the present—when what it is legitimate to see is very much more constrained by theoretical considerations (see below). The tension between the corners of the evidential culture space was experienced within the Frascati laboratory itself, and the positions there were described to me as the symptoms of, respectively, an *experimental animus* and a *mathematical animus*.

¹⁷ For a comparison of the “epistemic cultures” of different disciplines, see Knorr-Cetina (1991).

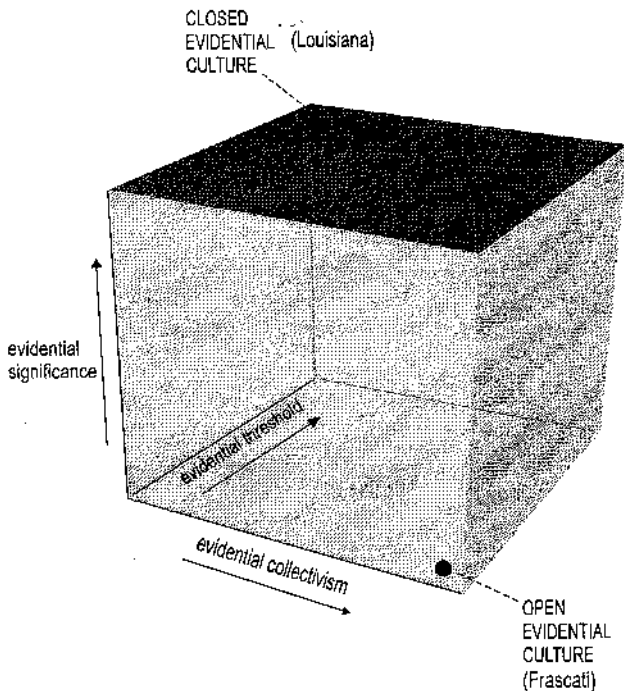


FIG. 1.—Evidential culture space

These are useful labels in this case because of the current role of theory in holding back speculation.¹⁸

The experimental animus is very well illustrated by the notorious 1989 publication (Aglietta et al.), which claimed that the room-temperature resonant bars run by Weber in Maryland and the Frascati Team near Rome found coincident signals, which were themselves in coincidence with neutrinos emitted by the 1987 visible supernova. In that paper, it was made clear that the energy level required to produce the signals was equivalent to the complete conversion into energy of a mass equal to 2,400 suns—something that even the members of the Frascati Team considered well beyond belief.¹⁹ At the same time, the levels of statistical significance re-

¹⁸ In a field such as biology, it might well be experiment that holds back "irresponsible" theorizing, so the terms "mathematical animus" and "experimental animus" would label different points in the same space.

¹⁹ But Joe Weber, Giuliano Preparata, and their colleagues would consider the claim reasonable, because under their theory of the bars' enhanced cross-section, only a fraction of a solar mass was required to produce the effect they apparently saw.

ported in the paper were not such as to avoid the charge of statistical massage. Nevertheless, the Frascati Team considered that this data should be published and looked at by the rest of the community; as far as they were concerned, something had been found, and it could not be wished away. A mathematical animus sees experiment as the servant of theory; it would restrict the presentation of data to that which makes sense in theoretical terms.

Again, the Frascati Team's evidential culture is well set out in the attitude to publication expressed in the following quotations recorded on separate occasions in 1996. Both reflect a leaning toward low evidential significance, the first combining it with high evidential collectivism, the second with readiness to adopt a low evidential threshold.

Frascati1: You should say "look"—no, it's a difference of how you publish. You should say "look, we found this result—we inform you—so if it's the case that you find the same results, you will report it to us." You should not say that you found gravitational waves. . . . Other people should say that.

Frascati2: There is a coincidence excess over what is expected. Now, is this coincidence excess enough to make an extraordinary claim or not? What is the probability that the excess is by chance? Calculating this, it is a few percent I understand. Is a few percent enough or not? Well maybe it is not enough to conjecture that these are gravitational waves, because to do that, you need extraordinary evidence. But it is enough to publish the results saying, honestly, "We have these results."

In contrast to the Frascati Team, a member of the LSU group provided the following by way of explanation of their unwillingness to be associated with anything but the most secure results:

LSU1: On thinking about it, I'm pretty sure that foremost in my mind was there is no way that I want to do anything that is going to have me labeled as a crackpot. . . . I suspect that underlying all of this is the desire to really say, "Well, OK, if my work is going to be significant, I've got to make sure that no one thinks that it's crazy."²⁰

The low evidential significance strategy would be fine if the development phase of an experiment could be kept clearly separate from the process of making a claim about the existence of new phenomena. As a member of another sympathetic group put it, "It is only by poking around at the data that you sometimes manage to make a discovery. Because of reproducibility, results must be the same or similar in the next experiment—and because of the ability to repeat the experiments . . . data massage is not as easy as [some] like to claim." But keeping report and interpretation

²⁰ We should read what is said here as an indicator of what counts as "crackpot behavior" or "craziness" in the respondent's reference group.

distinct are not easy; the meaning of a paper is in the eye of the beholder. It is not always the case that papers are read or interpreted in the same way as the author intends:

LSU: You were telling me that the paper you wrote with Weber, you did not claim gravitational waves. Everyone thought you did but . . .

Frascati1: Well, at CERN they said, "Oh—you publish a paper with Weber—Oh! Oh!" "Well—have you read the paper?" "No—just the title." I forget what it was, but it gave the impression that we found something. Look, I spent six months looking at the data—I did a data analysis. Why shouldn't I publish what I found?

Another passage from the Laughing Seafoods conversation makes the same point.

Collins: Let me intervene in this again. What [Frascati1] was saying earlier was that he doesn't want to publish this as a claim for gravity waves, but just publish as—you know—"we found this data." But what you're worried about—tell me if I'm right—is that in fact people won't read the paper this way. People will read the paper as a claim for gravity waves whatever it says.

Unidentified speaker: Sure—people want to reduce it to yes or no.

Collins: People will reduce it to yes or no.

LSU2: That would be my guess.

Collins: OK, but hang on. Let me ask this question: Why are you worried about that? . . . Why are you worried if everybody does read it in this way? You know, if it's not written but they read it, what's the problem?

LSU2: I'm thinking about it. One is, you could certainly expect a lot of ridicule from your colleagues at some point—maybe not overt but covert—if they find the result somehow ridiculous. I mean, you know, everyone values the good opinion of their colleague. . . .

Frascati1: This is true.

There are no Americans among the gravitational wave research community who are sympathetic to an open evidential culture, and some of them, perhaps in keeping with a strong individualistic ethos, despise it. The most forceful expression of this point was put to me as follows:

It was mostly at these general relativity meetings. . . . They would give their presentations in such a way that they would lead you, they would show you this data and they would show you the events, and they would show you some statistics they'd done, but never enough of it so that you could really get your arms around it. And they left you with this tantalizing notion that they could go either way. They either could make a claim that they had detected something if they had wanted to, if they had gone the next step in their presentation, or they could back off and say, well, yeah, maybe the statistics isn't good enough. And they left you at that critical juncture. . . . What would happen is that you had to draw the inference. Now that gave them the freedom at any point later on, or maybe even . . . to say well if we choose to say this, we have detected it, or if we choose to interpret that way we haven't detected it because the statistic isn't good enough. It was this . . . ambiguity—OK? That got to me—OK?

Another quotation expresses the point still more strongly in the context of anticipated future problems associated with the development of the World Wide Web.

The problem with unedited data dissemination is that it is irresponsible. The person who was there when the data was taken, who knows the idiosyncratic parts of the apparatus, who has experience with the kind of statistics that comes out of the gadget, and yes, the person who has something to lose if he (she) is wrong needs to validate and then disseminate the data. . . . It has all to do with plain old honesty. When one publishes data and a result, it has a pedigree associated with it, and your reputation as a scientist rests with how well you have analyzed and interpreted it. It should not be possible (as has been the case in this field before) to equivocate and claim validity for an observation at one moment and then deny it later when challenged.²¹

This member of the community believed that the potential ambivalence in an "open" research claim could too easily be used to the researcher's advantage. In this view, a researcher could stake a claim, as it were, to the Nobel Prize, while not being exposed to the risk of being wrong. Less forceful Americans, such as the members of the LSU group, also disagreed strongly with the Frascati Team's approach.

We just don't agree on the theory of data analysis . . . when he says, "I'm not claiming gravity waves, I'm just putting out what I have." And that's perfectly acceptable, except for the fact that if you look at Weber's early papers, . . . "I wasn't claiming gravity waves, I just said what I had," and it is that legacy, I guess, in American gravitational wave physics, that has me very, very cautious of doing that kind of publishing. . . .

And that's never gonna change, because it's just a different way of looking at the physics we do; and we just have to understand that we, as individuals, are different. And that's where your problem comes, actually, as a sociologist, and you're trying to say, "OK, what is it that enters into these things?" And it is much more than just the science, as you know.²²

HISTORICAL AND INSTITUTIONAL CONTEXT

Both Frascati and LSU have been involved in the search for gravitational radiation from the early 1970s. They use "resonant bars"; this, as explained, is basically the same technology as was pioneered by Weber from the late 1960s onward, but the Frascati and LSU bars are cooled to liquid helium temperatures or below. Such devices are known as "cryogenic"

²¹ This comment was made in the context of a discussion about the propriety of broadcasting detector data on the Web. It expressed, then, a general concern about relaxing the distinction between the inside and the outside of the laboratory.

²² Respondents almost invariably think of clashes between scientific cultures as matters of the diverse personalities of individuals.

bars. The LSU group gets all its funding from the U.S. National Science Foundation, and its members are typical, research-active university faculty. The Frascati group is located within the complex of government-supported research facilities at Frascati, near Rome, and draws its members from various universities. The LSU group is the only state-of-the-art resonant bar group left in the United States, while, at the time of writing, the Frascati group was the most advanced resonant bar group in Europe and possibly the world.²³ The group based in Perth—which will figure later in the discussion—also runs an advanced cryogenic bar.

Up to early 1997, the time at which this account is set, the LSU group had never been party to any claim to have seen signals consistent with gravitational waves; over two decades, they had offered only upper limits: "Gravitational waves can be no stronger than such and such, because our bar is of such and such a sensitivity and has seen nothing." The Frascati group, on the other hand, has issued at least two positive reports jointly with Weber in Maryland: one published in 1982 (Ferrari et al.), and one published in 1989, which claimed to see coincidences associated with the rare, "local" supernova that exploded in 1987 (Aglietta et al. 1989). Both papers have been rejected by the wider community, with the supernova paper attracting particularly harsh criticism (Dickson and Schutz 1995).²⁴

The Perth group has recently joined with the Frascati Team to report another positive finding. By February 1997, the Perth group had made at least two conference announcements to the effect that they had recently found strongly suggestive coincidences between their bar and that of the Frascati group.²⁵

Since the late 1980s, the field of gravitational radiation research has been transformed by the advance of a newer, bigger, and much more expensive technology. Research groups at MIT and Caltech combined to lobby for funding to build giant laser-interferometers, which should be more sensitive than the resonant bar technology.²⁶ After a long campaign, funding for the American effort was granted by Congress in 1992. The

²³ This can be said because their most advanced bar was cooled to a lower temperature than any other bar. In 1997, they were joined by a group in Padua using a bar of almost identical design. Weber still keeps two room-temperature bars working. According to Weber's theory, the transducer design of his bars, which differs from the design used by the cryogenic bar groups, means that his devices are more sensitive than theirs. This view is not known to be shared by anyone else.

²⁴ Subsequently strongly rejected by the Frascati group.

²⁵ Remember—the "Frascati group" is the whole laboratory—the "Frascati Team" is the subset that occupies the experimental animus corner of evidential culture space.

²⁶ The resonant bar groups believe that improving their technology would lead to instruments that would be at least as sensitive as laser interferometers within narrow wavebands, even if they cannot match the broadband sensitivity.

American version of the new technology is known as LIGO—which stands for Laser Interferometer Gravitational Observatory—and will cost in excess of \$300 million. The earliest that LIGO might be expected to see gravitational radiation signals is 2002, but most would accept that a few more years are likely to elapse before the devices have a good chance of seeing the waves. The American effort comprises two interferometers, one in Washington State and one in Louisiana (not strongly connected with LSU), each with arms four kilometers long. A European counterpart, with arms three kilometers long, is being built near Pisa by a combined French and Italian collaboration, and there is a German-British group building a 600-meter apparatus near Hannover, while a Japanese group has a 300-meter device. The huge expenditure on interferometry compares with the resonant bar effort, which has probably cost less than \$30 million for the three-decade-long worldwide effort.

From the point of view of those who still experiment with resonant bars, the growth of LIGO has meant that the American environment has become very unreceptive to claims that include any element of speculation. Nowadays, LIGO is the dominant force in American gravitational radiation astronomy, and I argue that it increasingly influences the activities of the bar community in a number of ways.

Reputational Drag

Because of the history of the controversy over Weber's work and the funding battle over LIGO, American researchers are sensitive to insecure discovery claims that might diminish their reputation in the eyes of the wider scientific community. LIGO gained financial support from Congress only after a long and difficult campaign. Even then, in the early years, those working with LIGO did not feel that its future was assured. Under the U.S. system, additional expenditure has to be approved every year, and the demise of the Super-Conducting Super-Collider, after substantial outlay, left researchers feeling vulnerable; a number of physicists migrated to LIGO from the dead Super-Conducting Super-Collider project, with this memory burned deep. Many believed that the difficulty associated with the initial funding of LIGO was partly a result of the controversy over Weber's results. They felt that the future of LIGO could be jeopardized by what they saw as the potential for further reckless reporting of incorrect results by members of the bar community.

Large funding decisions are not made by scientists but by members of Congress and other nonspecialists who may not see the fine distinctions within scientific fields. It is easy to associate contentious results announced by one set of gravitational radiation researchers with that of another set engaged in the search for the same phenomenon, even if they use an en-

tirely different kind of apparatus informed by a different philosophy. Indeed, sometimes physicists themselves seem to have difficulty in making these distinctions. The following interview extract is taken from my conversation with a leading member of the LIGO research effort based at "TECH":

Respondent: Anyway, so what we had to fight was that problem, the problem that this was a field for crazy people; that was number one. And none of the academic places liked it, [TECH] in particular. They looked at this, and they said this was for the birds. Here I was pushing like hell to do this by another technique [interferometry], and I couldn't get any backing in this place, intellectual backing. And the reason was, in part, because it had this very speckled history, and it looked, also, very difficult to do. . . .

You see, I lived in this hostile environment here at [TECH], and they were just gonna' say, "See, you guys are all crazy, and here's more of this bullshit that you guys are presenting to us. How could you do that." . . . I was hoping it would disappear.

Collins: So because you think . . . "Wow, this is amazing" . . . so because, let's say, [named resonant bar researchers] made another one of these crazy claims, let's call it, this endangered LIGO?

Respondent: Yes.

Collins: Just explain it to me again, because I find it very hard to comprehend.

Respondent: Well, we were fighting with the astronomers still at the time, very much so, about the value of doing LIGO in the beginning, and we were continually being tainted by, "You guys are all nuts!" And this just was further evidence of it. So, I mean, they lumped us together with that.

Collins: Yeah, but people surely would have known enough to know that you were separate groups.

Respondent: Doesn't matter. This's a field. "Look—it's pathological science again." That's where it came up again. You see. Now, maybe I overreacted, but I felt that . . . if [named resonant bar researcher group] does something cuckoo . . . we're all gonna get it.

Thus, given that positive reports by the resonant bar community are almost certain to be false according the theory that has informed the design of LIGO, they can only bring trouble or ridicule for a project that is already unpopular among the majority of astronomers. In the early days, this could have damaged LIGO's funding, and it can still damage LIGO's credibility. In contrast, Italian interferometer scientists expressed no such fears, even though they use the same technology.

Big Money Strain

The American gravitational radiation detection community can be thought of as made up of three components: there is Weber's remaining small effort that has been almost completely marginalized both intellectu-

ally and in terms of financial support (it was initially funded by NSF); there is the remaining state-of-the-art resonant bar community, centered at LSU; and there is the LIGO group, centered around Caltech and MIT, which has turned the field into a "big science."

On the face of it, the bar groups should have none of the worries about funding described in the last section; they have always been a relatively inexpensive speculative enterprise. Nevertheless, the LSU group shares the cautious approach of the rest of the American community. Perhaps part of the reason is that they feel they have to be sensitive to the concerns of the interferometer groups because they share a common funding source. Because LIGO is the largest project ever funded by the National Science Foundation, its success and failure represent success and failure for NSF policy. It is almost inevitable, then, that LSU is sensitive to LIGO's fears and what they perceive to be the Gravitational Physics Program Director's interests.²⁷ As one American scientist put it to me:

If there is any one single individual in my opinion in the United States that has influenced the direction and the attitudes that people have on gravity waves, it would probably be [the program director], because he has been the man in charge of funding research.²⁸

and again:

[The program director] does have very definite views, and by being in the position of power that he is and by the way that the American funding system works, why, he has a good bit to say on what work gets done—no way to avoid it.

Members of the LSU group did report to me that they had felt, in the past, that their project would have been in jeopardy should they have reported anything too dubious:²⁹

²⁷ Under the American funding system, decisions are made as much by powerful program directors as by the committees they appoint. There are good and bad aspects to the procedure. On the whole, scientists prefer NSF program directors, who are well-informed members of the relevant scientific community, to committees. Furthermore, individual program directors are more likely to make brave decisions—for example, over interdisciplinary work or adventurous lines of research—than committees ruled by majorities. But program directors' preferences are understood by the community, and when decisions go against a favorite project, disappointment may be personalized while accusations of "bias" are more easily directed at an individual than at a committee.

²⁸ To echo the point made in the previous footnote, this respondent went on to say: "and he is a respected theorist himself, or was. One of his calculations was one of the really fundamental ones to show that gravity waves should exist, before he became a science bureaucrat." At the time of writing, this particular program director was taking a research sabbatical, doing more theoretical work.

²⁹ Though, as I will go on to say, this effect, if it exists, is a subtle matter—it is not a matter of crude tailoring of results to fit self-interest.

Collins: You said earlier that there was a stage when you felt that if you had released findings, you felt your grant might be in jeopardy.

LSU1: I know, absolutely, it was, earlier. There wasn't any temptation, because very early on, our experiment didn't work well enough to be happy with it; but had I tried to say that there was anything, we never would have—it was one of those cases where, as [another member of the group] said, it would be so improbable that everyone would say this man cannot be trusted.

At the time of writing, the Louisianians are continuing to seek new funding in an atmosphere of increasing difficulty for resonant bar research. Thus, in so far as U.S. science funding discourages speculative reporting in this area, the pressures have not gone away.³⁰

The structural and institutional pressures on the Italian labs are more ambivalent. The Frascati laboratory seems relatively rich, and its funding seems secure so long as it is seen as *productive*—that is, producing results. The duty to publish that they feel is understood both within and without their laboratory:

LSU1: [The leader of the Frascati lab] argues that he's had people funding his research for all of these years and he really owes them papers.

Frascati2: You cannot decide not to publish the results of these experiments. There are many people working hard, taking data, making the detector work—then you cannot expect that people don't want to publish their results. They should be published, otherwise, why are we doing these experiments? ~

Furthermore, though there is less money overall in Italy, neither kind of gravitational radiation research seems to be in jeopardy. Thus, a second ultra-low temperature resonant bar detector has just been built in Padua, even while the Americans are considering closing down their one remaining resonant bar group. Also, there is far less evidence of financial pressures on the Italian-French three-kilometer laser-interferometer. While the American building program has, according to a number of physicists, been very much "front-loaded" so as to make sure most of the construction money is spent before Congress has a chance to change its mind, the Pisa installation is proceeding in a leisurely way, driven far less obviously by financial pressure. Thus, the Pisa installation has completed its so-called mid-station without even beginning to build the three-kilometer arms—one of the most expensive parts of the project—seemingly secure in the knowledge that whenever they decide to build the arms, the money will be there to finish the job. There is, likewise, no apparent

³⁰ By the middle of 1997, this funding had been initially refused, leaving the LSU group's resonant bar program without a clear future. By coincidence, they recently reported some very interesting continuous wave results.

competition between the Italian resonant bar groups and the interferometers for a single source of finance; relations between the groups of experimenters pursuing the two kinds of program are cordial, without the tensions that attend the U.S. scene. The setting, then, seems to have features of ordered civil service laboratories rather than the hectic competition between universities, between different communities of scientists, and between different congressional interests that characterizes the American scene.

Theoretical Stress

A "big science" initiative, such as LIGO, must promise success to government and its representatives. LIGO represents a step-function increase in funds for gravitational radiation research and promises a step-function increase in sensitivity; the new machine is meant to be good enough to guarantee not only the detection of gravitational radiation but the founding of a new field of gravitational astronomy.³¹ Given that this is not a matter of gradual development based on existing technology, there is only one source for such a guarantee—theory. Thus, during the course of the search for gravitational radiation from the late 1960s onward, the importance of theory has grown. With the argument over the funding of LIGO, theory has come to dominate experiment, at least in America. It is theory that has justified the spending of hundreds of millions of dollars. For this reason, I treat theory under historical and institutional context.

The most widely accepted theory paints a picture in which, in terms of the emission of "visible" gravitational waves, the sky is almost completely black; under this sky, it is LIGO, which has the only chance of observing and understanding any glimmers of gravitational waves that disturb the gloom, and under this theory, it is virtually impossible for the current resonant bars to see anything except extraordinarily rare events, such as might occur a few times a century.

Theory has also come to dominate nearer to the heart of the LIGO project, because at the levels of sensitivity predicted for the first detections, the only way data will be extracted from noise will be to look for

³¹ A physicist respondent pointed out to me that as far as scientists are concerned, there is an ambiguity about the notion of success: building a sufficiently sensitive gravitational radiation detector and finding no waves would be a scientific success because it would show that the existing theories are wrong. "Negative success," however, does not generate further funding (just as tenured posts generally only go to those who find positive, not negative results). An example of a negative experiment is the search for solar neutrinos—the flux of neutrinos coming from the Sun is far lower than theory would predict. As Pinch (1986) points out, however, the first detectors were justified and funded on the promise of finding a large flux of neutrinos.

data signatures that fit precalculated templates (e.g., for in-spiraling binary neutron stars). Again, instead of just looking to see what is there, data will not count unless they match a template. Theory is needed to calculate the templates, and maverick theories must be suppressed if the consensus needed for this kind of experimental philosophy is to be maintained.

Finally, by predicting that the ultimate limit of sensitivity, set by quantum theory, is better for interferometers than for bars, theory has changed the balance of credibility. The technology is so far from reaching the ultimate limit that this is not currently a matter of any practical importance but it has still had a powerful effect on the bars' prospects. Theory, in sum, has aggregated influence in many ways; in this field, theory, once the servant of experiment, has become like a massive star with a gravitational field that affects the shape of everything near it.

Bright sky over Frascati and black sky over Louisiana.—In harmony with their evidential culture and institutional position, the Frascati Team feels that theorists' claims about the structure of the heavens should not dominate experimental work. In 1995, I was told: "If we find something, it is because there is something new. Because nature is kind to us." And again a year later:

I think that—we are aiming to make a discovery. And no discovery can be made if one has a mind which is within a frame that does not give the freedom to think something different, something new. It seems to me so obvious. Actually, we started this experiment with this idea in mind because I said that already, and I repeat it now—if the gravitational waves that are expected are exactly those which are predicted by the physics, nobody will see gravitational waves from now for the next fifty years.³² I hope we will both live long enough to find out. So, even if the laser [interferometry] people don't say that, I think everybody hopes that the situation is different.³³

The Frascati Team, then, seems less bound by the strong theoretical consensus of American physics. The sky seems brighter over the Frascati

³² This assumes that though LIGO in theory will be sensitive enough to see the waves within a decade, development work will be as drawn out as it has been for the cryogenic bars. It could be, however, that cryogenic bars have had such an extraordinarily painful development phase because any adjustment, however small, takes three to four months while the apparatus is warmed and cooled again. Barring accident to the main vacuum pipes, interferometers have an advantage in this respect.

³³ Sam Finn pointed out to me that it is possible to have an adventurous view of the cosmos and still be conservative in terms of evidential practice; it certainly is true that one does not follow strictly from the other, though without an adventurous view of the cosmos, one is less likely to want to release results early.

Team than over most of the United States (Maryland is an exception).³⁴ For example, one member speculated that relatively high energies—enough to provide a half-dozen or so pulses a year—might be emitted by coalescing MACHOs (MASSive, Condensed, Halo Objects). Such objects, which would be invisible because they do not emit heat, light, or shorter wavelengths, might make up part of the “dark matter” component of the universe. These massive but invisible stars could fill the area around our galaxy.

In contrast, there was no such speculation in public among the Louisiana team with respect to the bars. They seemed happy to accept the calculations of the major theory groups that stand behind LIGO. The axis of Louisiana's arguments turned on the narrowband sensitivity of resonant bars, which would enable them to compete in terms of the observation of certain kinds of sources, the improved sensitivity of further generations of resonant spheres, and the much greater directional sensitivity of resonant technology. But this argument was conducted almost entirely within the framework of the levels of energy and the cosmic scenarios informing the development of LIGO.

Academic Drift

Because LIGO is such a large project, and because the future of resonant bar work is not clear, interferometry groups may represent the best employment market for academic apprentices trained in resonant bar experimentation. One young physicist explained to me his attraction to LIGO in the following way:

I'm certainly just following my gut. I'm certainly following the interferometers. That's where the money is, that's where the people are, that's where the scientific opportunity is, so that's where I concentrate most of my attention. It's just that—it's one of those unconscious choices that—it's a value judgment. It's the kind of value judgment that people make when, you know, you decide whether to have children or not, or things like that. They often don't make those judgments very explicitly, but they know they're based on some kind of understanding of things.

Given that in the United States it is taken to be the duty of a project leader to find secure employment for graduate students and postdoctoral fellows who move on, it is important for the resonant bar groups to maintain good relations with the rest of the physics community; they must not marginalize themselves intellectually or in terms of the informal academic networks that lead to jobs.

³⁴ Joseph Weber is at the University of Maryland.

In the case of the LSU group, an added complication is that one of the LIGO sites is just 45 minutes' drive from LSU, which could bring benefits if relations remain good:

It is our hope that since we're already active in the gravity business, that if they come in and build a LIGO detector here, that this is going to enhance the program here, and it will. We've said that we would make some faculty appointments in gravitational physics and so on. . . . We see our position being with new faculty hires, being someone who would be actively involved in using the detector and also quite naturally that people running it—it's about thirty miles from here . . . there's nothing over there—absolutely nothing. So you're not going to find your Ph.D.-level people living in the country town of Satsuma—they'll base themselves around here. We've got good computing facilities, I think it's just going to be natural.

Furthermore, one of the members of the LSU group is actively seeking to develop research under the aegis of the LIGO project.

The members of the Frascati Team seem well insulated from such pressures, making optimistic plans for further developments in resonant mass technology (resonant spheres are the latest idea). On the other hand, members of the Frascati group who share the mathematical animus, many of them junior, may be more influenced by job prospects and professional networks. The three-kilometer interferometer group may eventually be a source of employment for them, just because of its great size as compared to the resonant bar program. Furthermore, partly because all searches for gravitational bursts are coincidence experiments, the whole community is becoming more and more globalized, and the cultural influence of the U.S. community is bound to be felt by physicists who frequently hold temporary fellowships away from their own laboratories and who interact with all the members of the gravitational research community several times a year at conferences—interacting, for purely demographic reasons, with far more interferometer scientists than resonant bar scientists. Globalization is, of course, well advanced among high-energy physicists who now form a sizable proportion of gravitational wave researchers on both sides of the Atlantic.

The Growing Influence of High-Energy Physics

American physics practice is led by the elite, competitive science of high-energy physics. Its influence on gravitational wave research has become marked as LIGO's personnel have become dominated by high-energy physicists.

From the early days, Weber's data analysis practices have been unfavorably compared with those of high-energy physics. In my interviews, I find American groups holding up high-energy physics as the reference

group for how to do good data analysis. Members of the Frascati group, on the other hand, were less impressed, believing that the delay histogram method (see below) was better.

No. The concept of standard deviation is used by most physicists, particularly the high-energy physicists. It is not the concept which I think is appropriate for our experiment. . . .

But if I convert this to standard deviations, and say four standard deviations, then any high-energy physicist will say "not enough," because they think in a different way.

Here we see the clash between choices of evidential threshold in high-energy physics and resonant bar work.

Sharon Traweek's (1988) anthropological study of high-energy physicists reveals some of the features of that community that encourage evidential individualism. For example, she repeats a story of a physicist who prematurely reported the discovery of an "exotic meson" before it had been confirmed with sufficient statistical rigor. The man angered the community and lost a promotion (p. 118). She also reports that high-energy physicists say that even if one has absolute trust in an experimentalist, one should multiply his claimed amount of possible error by at least three (p. 117).

Traweek also reports that American high-energy physicists from different groups do not talk to each other very much, while interaction is needed to reduce distrust of the methods of the resonant bar community, such as the use of delay histograms. Members of resonant bar groups have complained to me about the initial arrogance, competitiveness, and unwillingness to communicate of the new entrants from high-energy physics (a problem that seems to be fast resolving itself).

Without being evaluative, it does seem that there is a difference in approach between high-energy physicists and certain resonant bar groups. It has to do, I would suggest, with the relative "normalcy" of high-energy physics as a science. Over many years, as a result of experimental bravado combined with engineering excellence, new particle after new particle has been produced. A certain pattern of expectations for the nature and duration of the experimental cycle has been built up, and with it, a well-defined image of what constitutes a mistake arising out of an insufficiently rigorous approach. The relationship between theory and experiment has been fruitful, with the experimentalists showing time after time that, given the resources, they can build the machines that will see anything that theorists say it is possible to see. In contrast, the whole history of gravitational wave research has been a matter of looking for something that ought not be there. All the ingenuity has been directed at struggling to keep going while trying to do the impossible rather than regularly doing the possible. Any signals have to be extracted from noise using any and every means;

the problem has been not to drive away false signals but to maintain some grounds for hope. This has given rise to a very different experimental culture. The pre-high-energy physics culture has survived in Frascati, while the Louisianians predilections have been reinforced by the influence from high-energy physics.

INVOLUNTARY BLINDING AND THE PERTH-ROME COINCIDENCES

We have explored the cultural and institutional setting of the field of gravitational wave detection. We can now understand the unfolding of two interesting recent events. But first we need to explain some features of data analysis.

Analysis of Coincidences

Delay histograms.—The trouble with gravitational waves is that they cannot be turned off. It is difficult to do a detection experiment on a phenomenon that cannot be turned off, because it is hard to compare a control situation and an experimental situation. Ideally, one would like to be able to shield one's apparatus from gravitational waves from time to time. Then one would compare the output of the detector in its shielded and unshielded periods; any excess of activity in the unshielded periods compared to the shielded periods would be a strong indicator of the existence of gravitational waves. But since gravitational waves are distortions of the very fabric of space time, which lose only an infinitesimal proportion of their energy to matter, they penetrate all imaginable shields.

The invention of the "delay histogram" represents a partial solution to this problem.³⁵ The output of a resonant bar detector is a record of a series of levels of energy at small time intervals over a period of days, weeks, months, or years, depending on how long the detector remains working reliably. Other groups can take this data stream and compare it with the output of their own detectors, looking for coincidences of levels of energy or, more usually, coincident, sudden changes in levels of energy. The way this is done is to compare the two data streams at a variety of temporal offsets, or "delays." If the two data streams being compared were recorded, say, one week apart, then there can be no coincident changes in energy caused by outside sources—any apparent coincidences must be a result of chance combinations of the inevitable peaks of "noise" of the two sys-

³⁵ This interpretation of the delay histogram protocol—in terms of its being a substitute for shielding—is my own invention.

tems. To a first approximation, the same goes for every time delay—positive or negative—except zero.

Actually, because the two detectors are typically separated by several thousand miles, a signal received on one may not be “seen” by the other until several 100ths of a second have elapsed—the time taken for signals, which travel at the speed of light, to traverse the distance between them. More important is that the response of resonant detectors to inputs of energy is “smeared” over time—they experience an energy “kick,” which may take time to register and time to die down. Thus, the definition of coincidence is another point of debate and negotiation. But, if we take “zero-time-delay” to mean “within a second or so,” we will be able to understand the techniques and the arguments.³⁶

Now imagine that we count the number of coincidences between the outputs of two detectors seen over a period such as a month. Then we take the recorded data streams and offset their starting points by, say, one minute and count again; this time we will be counting apparent “coincidences” rather than real coincidences. We then repeat the process with an offset in the other direction—minus one minute instead of plus one minute. We can continue to try as many positive and negative “delays” as we like. The array of numbers produced by this process is plotted on a “delay histogram.” The height of the “bins” represents numbers of “coincidences,” while the horizontal scale represents delays, negative and positive. The zero-delay bin is in the middle. The heights of all the non-zero-delay bins should fluctuate randomly around a mean level that represents the average number of apparent coincidences that might arise from chance matchings of peaks of noise. If, however, there are some peaks of energy that are due to outside signals impacting on the two detectors simultaneously, as in the case of gravitational waves, we would expect these to show up only when there was a zero time-offset. Thus, the central, zero-delay bin in the delay histogram should be higher than the other bins if genuine signals are present. A coincidence experiment compares two data streams in this way and looks for a “coincidence excess” in the zero-

³⁶ Apart from the physics of the meaning of coincidence that I have just described, simple, accurate timekeeping seemed to be a source of trouble over and over again in this field. One would like to be able to compare the time at which events occurred to within an accuracy of a 100th of a second or so, but at some sites, clocks were used that could drift by a second or two per day before they were readjusted. One group, so it was reported to me, used their computer clocks, correcting them a few times a day by telephoning a local radio station. When I expressed surprise at the problems encountered with timekeeping, a respondent explained: “Everyone was working hard just to make their experiment work, and their was little thought given, in the early years, of just how coincidences would be done. Then, when it was necessary to do coincidence work, it was hard to force good timekeeping into the historical way that each experiment’s data taking had developed.”

delay bin. The height of the zero-delay excess shows how many gravitational wave events (or other events that mimic gravitational waves) have been experienced by the bars (see fig. 2).³⁷

Once we think of a gravitational wave experiment as comprising two detectors rather than one and we think of the effect of gravitational waves as being coincident excitations of two detectors rather than excitations of a single detector, we have a way of turning off the effects of gravitational radiation on the experimental apparatus. The effects are turned off in all the non-zero-delay bins and turned on in the zero-delay bin. Unfortunately, this is not a complete substitute for true shielding, because it is not really gravitational radiation signals that are being turned on and off but the influence of anything that can effect both bars simultaneously.³⁸ Nevertheless, it is this invention that makes the experiment, dealing as it does with tiny effects, possible.

The delay histogram method of analysis is very seductive. Even a small zero-delay excess, when set among a large array of delayed bins, looks convincing. The experimenter is bound to ask, if there is nothing there, why did the data-analysis algorithm choose this particular central bin to be the highest one? A quotation from an early interview (1975) is revealing:

More recently, two Japanese scientists have repeated the experiment, again using the amplitude algorithm. They did see the largest number of coincidences in the zero-delay bin, with a factor of between one and one-and-one-half standard deviations.

By the standards of modern physics, that's not a significant effect. Nonetheless, the fact that their computer discovered the largest number of counts at zero-delay may be significant.

Statistical massage.—It is a well-known psychological phenomenon that experimenters tend to analyze experimental data in such a way as to favor their own hypotheses. Nearly always, this is not a matter of dishonesty but unconscious bias (e.g., Rosenthal 1978). Given the nature of gravitational wave detection experiments, there is ample scope for inadvertent bias in the analysis of data. Any delicate apparatus will have noisy, insen-

³⁷ It seems that Joe Weber was the first to use the delay histogram technique. Guido Pizzella told me that Joe Weber invented the technique while Pizzella himself gave it the name of "experimental probability."

³⁸ To give an idea of other effects that have been considered, it has been suggested that two bars separated by the width of the United States might be influenced by the simultaneous switching on of many electric pumps across the United States in response to coordinated toilet flushes during the commercial breaks in televised *Monday Night Football*. I believe I heard the gravitational effects of the simultaneous movement of the massive amounts of water also being discussed in relationship to the interferometers.

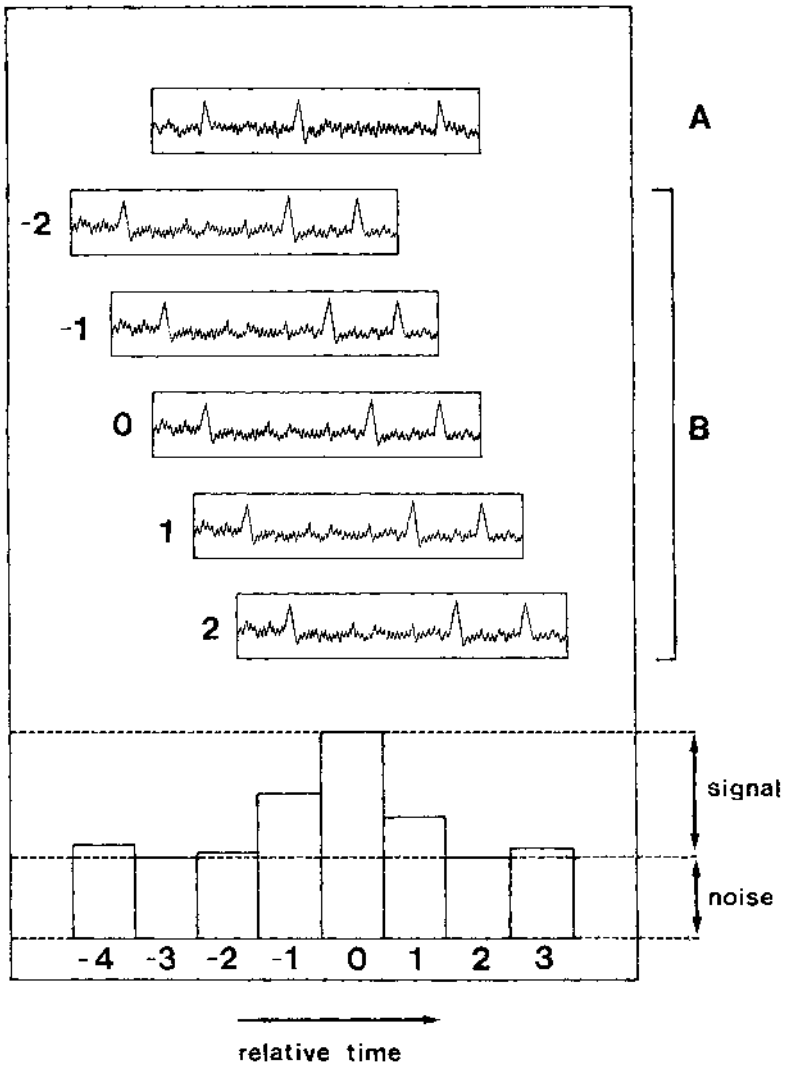


FIG. 2.—Signal extracted from noise by delay histogram

sitive periods and quiet, sensitive periods: the outside world of cosmic ray showers, tides, and earthquakes may vary in its intrusiveness; the immediate environment, both human and physical, may affect the apparatus in many ways; and the internal components, such as seals, electrical connections, liquid gases, and the metal itself, may harden, crack, leak, cease to insulate, and stretch and strain. The tiniest change in any of these

things affects the immensely delicate detector. Every experimenter has to decide when the apparatus is working satisfactorily, so that data are a candidate for analysis, and when it is not, so that the data should be rejected at the outset (Holton 1978). Other parameters that can be changed are the length of the period that is taken into account as a bin, the nature of the initial signal-processing filter, and where the threshold for what counts as a potential signal is set.

The following extract is from my interview with a member of the LSU group in December 1996:

If our apparatus is not working correctly, as we can tell—the noise temperature going up—we'll do a new calibration and build a new filter, and then on the basis of that filter, we would begin recording events again. . . .

Each filter would be different for a different apparatus. And this is something that [named groups] have not understood. [Named group] have felt that they could build a filter on a theoretical model and that would be good; and in theory, you could do it, but in actual fact, you don't understand the actual operation of the apparatus well enough. So it is more complex than one would initially think.

Even this most cautious of experimenters recognized the need for continual monitoring and readjustment of the apparatus in light of its performance. Yet retrospective readjustment of experimental parameters always offers the possibility for the artificial construction of a nonexistent signal—that is, “statistical massage.”³⁹

Given the history of the field, American groups are very sensitive to the possibility of accidental statistical massage. One of the events that caused Weber to lose credibility was his reporting of coincident signals between his own antenna and that of a group at the University of Rochester. It turned out that because of a mistake in timing, the two sets of signals had starting points that were more than four hours different, so Weber had accidentally constructed a “signal” out of what should have been pure noise.

Most scientists, and certainly almost all American and British scientists, believe that the Frascati group's report of signals coincident with Weber's apparatus at the time of the 1987 supernova was a result of unconscious post hoc adjustment and readjustment of parameters until a spurious “signal” emerged. This has not helped the Frascati Team's case in spite of their strong rebuttal of the charge.

³⁹ It is tempting to think that, following standard psychological practice, once parameters have been set in “preliminary runs,” they could be frozen for “experimental runs.” Indeed, this was a justification offered for publishing with low evidential significance (see below)—so that the parameters recorded in the paper could be publicly certified before “freezing.” It seems, however, that the experiment is likely to be too delicate to allow “freezing” in the foreseeable future.

Thresholds.—The scope for statistical massage increases with the number of data points to be analyzed, and this is affected by the threshold that is set for potential gravitational wave events. Two (or more) streams of data are to be compared. Each of these streams may be visualized as a squiggly line with almost random fluctuations up and down; there will be big fluctuations and small fluctuations. Let us say that the inputs in which one is interested are sudden inputs of energy, represented by sudden upturns in the wavy line. The question is: How big must an upturn be in order to count as a potential gravity wave signal? A threshold must be set for the events that are to be compared in the search for coincidences. If the threshold is high, only a few events will be compared. As the threshold is lowered, more and more candidate events will be compared for temporal coincidence.

As might be expected from the Laughing Seafoods conversation, on this point too, the Louisianians take a view consonant with their closed culture, and the Frascati Team takes an open view. The LSU group argued that the threshold should be set so high that very few events were being compared; this would mean that even a single excess event in the zero-delay box would stand out. They argued that, in theory, expecting to see even one event a year on antennae with their sensitivity was extremely optimistic and that the threshold should be set accordingly. Furthermore, they argued, the lower the threshold and the more candidate events being compared, the greater was the scope for any apparent zero-delay excess to be the outcome of unconscious bias in the data analysis; the more data there were to be analyzed, the more opportunity was there to “dig down” and pull something out of the data that was not really there.

The Frascati Team argues that finding one event would be meaningless anyway, because a single event could always be the result of chance and could have no statistical credibility. Such an observation would, therefore, require replication, and if gravitational events are as rare as they are expected to be for instruments of that sensitivity, decades might pass before such a thing would appear again. The high threshold, low event frequency search program is a recipe for finding nothing that can be believed until long after the interferometry program has been completed. In any case, if the threshold is set high, in theory, the amount of energy required to produce the single event would be so great that astrophysicists would refuse to accept it for that reason.

As we have seen, the Frascati Team argues that if the resonant bar program is to have any chance of success, it must be based on the assumption that there is something unexpected in the heavens. What follows is that the threshold should be set low enough for any zero-delay excess to imply a frequency of events, such that repeated observations can be made within a reasonable fraction of an experimenter's lifetime. In other words,

most of the data should be treated as potential signal and exposed to scrutiny; it should not be treated as noise and hidden behind a high threshold.

Involuntary Blinding: The Louisiana Protocol

We are now in a good position to examine one of the most interesting episodes in the history of the search for gravitational radiation. As explained at the outset, coincidence experiments force groups with different cultural propensities to work together.⁴⁰ In spite of the contrasting institutional, financial, and cultural backgrounds of the Frascati Team and the Louisiana group, they must cooperate to produce results. How can Louisiana maintain its preferred strategy when the Frascati Team could take Louisiana's data stream, compare it with their own under their preferred analytic protocols, and in a spirit of evidential collectivism, issue high-risk reports with low evidential significance?

In the early 1990s, the LSU group introduced an intriguing new data exchange protocol to try to get around the problem. From that date, they decided to release data from their own experiment only in a disguised form. The Louisianians' innovation was as follows: Initially, in 1991, they began to release two data streams, one genuine and one false, without saying which was which. Later, they began to release their data streams in the form of a continuous loop with 1,000 "starting points" marked on it. Of these 1,000, only one was genuine, while the other 999 were false. They would not say which was the true starting point. Any other groups receiving their data had to find out for themselves which point represented the start of the data stream.

To do this, other groups would have to construct a delay histogram with 1,000 bins corresponding to the 1,000 potential starting points on the Louisiana data loop. Of these bins, 999 would be the result of comparing data trains with a time offset, and only one would exhibit zero-delay. If they discovered that one bin out of 1,000 was higher than the rest, they might choose to claim that they had discovered a zero-delay excess—that is, a genuine signal. But they would not know if this bin was really the zero-delay bin or if it was another bin with an excess due purely to coincidences in the noise. To know whether they were right, they would have to depend on the Louisiana group. If the second group were to announce a "result" without consulting Louisiana to find out if they had found the right starting point, they would risk making public fools of themselves.

⁴⁰ Note that the principal members of the LSU group and the Frascati group have been friends for many years not only professionally but at the level of personal visits between families; the drama was played out with a minimum of personal animosity.

In effect, the Louisiana data exchange protocol forces any group who wishes to cooperate with them in a coincidence search to run their experiment "blind," whether they like it or not.

Initially, the invention of this protocol, with only two data sets in the first instance, was on the basis of a mutual agreement between LSU and Frascati. It also appears that the Frascati group was initially happy with the extension to 1,000 starting points, since any result based on this protocol would look sound. More recently, however, the Frascati Team has become much less happy. They consider that to continue with the protocol serves the interests of the interferometer groups rather than the bar groups. A member of the Frascati group put it this way, overlooking, perhaps, their initial ready agreement to the protocol:

If we write a paper on coincidences, we might damage the LIGO project. So [the NSF gravitational physics program director] asked the Louisiana people to be very careful in writing these papers. So we have a lot of difficulties in exchanging the data with Louisiana. And finally we reach this agreement: . . . So when searching coincidences—if we find coincidences with these wrong files, it certainly is not good. This is a very clever point.

In fact, the NSF program director did give strong support to the new protocol. As he put it:

I had talks with the people at Louisiana before they got involved with data exchanges. I said, "You've gotta protect yourself, you don't want to risk errors." And they knew that too. They were asking, "How much garbage shall we put in with the good stuff? Should we put in a factor of five or 10 or 100?" And I was ultra-conservative, and I suggested massive amounts of nonsense. And I think they got a practical compromise.

In a later interview, he went on to amplify as follows:

Collins: Who invented that LSU protocol?

Program Director: Well [a member of the LSU group] was talking about this, and I remember having a discussion with him where I asked him what would the chance be if you were exchanging your records blind. . . . "What's the chance that you can get a false alarm?" And he came up with a number which is a few percent. And I said that he should be very careful, and that if I were in his shoes, I would certainly not want it to be more than one in 10,000 or 100,000. And he said, "Well, you have this problem, you have to do data analysis—massive amounts of data analysis, and it gets to be a headache." And I said, "Who cares [laughter], we're playing for big stakes, and we don't want false alarms." But I think he was getting similar advice from lots of people. . . . He worked out some procedure.

The advantages of involuntary blinding.—As far as physicists inclined toward a closed evidential culture are concerned, the major benefit of

compulsory blinding is that no chaos can emerge from irresponsible interpretation of the Louisiana data.⁴¹ Anyone can use it (in principle), but it would be a foolish person who broadcast a result of a coincidence analysis with the LSU data without checking first that what they believed to be the zero-delay box was indeed the zero-delay box. Thus, LSU is effectively enforcing its own evidential culture—compelling the struggling convicts to run in their chosen direction.

Of course, if a blinded analyst does find the right box, this is powerful confirmation that the effect is real and not a result of post hoc statistical massage. Any post hoc tampering to increase the height of a favored bin may be enhancing the wrong one. The procedure seems, then, an ideal way of eliminating both “crazies” and unconscious biases.⁴¹ A third party, a partner in the laser interferometry effort, put it this way:

If he's doing his statistics right and claiming a coincidence at a believable level, he'll have no problem picking out which of the thousand is the right beginning time. But if he's fudging things—even unconsciously—then they'll fudge on the wrong one, or something, or won't even be motivated to start fudging because it will get lost in a morass. I think it's very clever. You know, not all social or psychological problems have technological solutions, but I think in this case, someone was clever enough to have come up with a technical solution for a psychological problem. So I think it's beautiful. . . .

Scientists from other fields would say, “Oh look, those gravity wave people can never do anything right, here's another piece of crap from them. . . .” Claims that are believable—whether they're true or not of course is very subtle as you guys [sociologists] know—but they have to be believable. So this is a way of getting around the biggest hurdle of believability, namely, the incredible temptation . . . of massaging the statistics. This will make it virtually impossible, and furthermore, it makes it certifiably virtually impossible for anybody from outside who would look at it; that's why I think it's so clever. So that either there won't be a false find or there might be a believable claim—either way, it's better than being almost certain of an unbelievable claim.

It is true that under this protocol the Frascati group has made a few wrong guesses about the starting point. They have, however, checked these out privately with the Louisiana group before publishing. From the point of view of anyone not working in gravitational waves, it looks, then, as though the protocol is excellent; it looks especially good to anyone working on interferometers. As one scientist put it to me, stressing the view of the interferometer community:

Respondent: And they've had several times when [a member of the Fras-

⁴¹ “Crazies” include complete outsiders who might take the data from any pair of laboratories, perhaps from the Internet, and analyze them in their own preferred way, without knowing anything about the experimental apparatus.

cati group] has said, "number 62—that's the one," and they [Louisiana] said, "Nope!"

Collins: That's happened a few times has it?

Respondent: Yep! . . . "Oh well it wasn't 62, it was 54" . . . "Now wait a minute . . . how many guesses are you going to get before we invalidate the procedure?"

The disadvantages of involuntary blinding.—The Frascati Team confirmed that they had indeed made some wrong guesses. Is involuntary blinding, then, simply a brilliant solution to a "psychological" (that is, sociological) problem? Not according to the Frascati Team; for them, cutting risk to near zero is not the best policy. Going back to the aim set out at the beginning of the paper, we can explore their rationale.

First, the 1,000 starting point procedure imposes a lot of work on the recipient of the data, because he or she must construct a 1,000-box histogram—in the normal way, a delay histogram would contain many fewer boxes than this. More serious, a member of the Frascati Team pointed out that the only effect that an analyst can discover under this protocol is one that is strong enough to have occurred by chance less than one time in one thousand (i.e., more than three standard deviations above background). If the effect is less strong than this, one or more of the non-zero-delay bins is likely to show a higher peak than the zero-delay bin, purely as a result of chance. For example, if there is a genuine effect, such that there is a 1% chance of its being due to chance (2.5 SDs—a result that would be considered more than adequate in the social sciences), the 1,000 starting point protocol is likely to disguise it among around 10 results of equal or greater apparent significance. Thus, work on a 1% signal is impossible. In physics, 2.5 standard deviations is not much and would not normally be thought to amount to a significant result, but this is not a normal field of physics; we know we are going to stay near or within the noise for a long time to come, and therefore, we know it will be a long time before we escape from the developmental phase of the experiment.

A member of the Frascati Team argued that it is necessary to continue to understand the phenomenon and develop the apparatus in appropriate directions, and for this purpose, it is necessary to work with any signals that might be there, however poor their statistical significance: "I still think it's important that we find 1% excess, that you cannot find it in that way because you require better than one per 1,000. That's the problem." To enhance the signal, in a situation where the signal and the apparatus are so ill-understood that it is impossible to design the best filters and set other experimental parameters to their optimum level by prior theoretical considerations, one should adjust the parameters in such a way as to enhance the height of the zero-delay bin, and to do this, one has to know which it is.

Interestingly, this argument carries forward two positions found in the early days of gravitational radiation research. Joe Weber, who was trained as a naval radar operator during World War II, first organized his data analysis on the basis of a radar search. Here one continually tunes and retunes the "receiver" and the analysis parameters until the signal is enhanced as far as possible using the signal itself as a reference point. In circumstances where one is certain that the signal must be there, or that the major risk is not finding a signal that might be present, this procedure is optimal. In circumstances where the existence of a signal is uncertain and the major risk is falsely reporting something that is not there, the procedure is dangerous because it encourages post hoc manipulation.

Thinking along these lines, it is tempting to reconstruct both the history and current situation in gravitational radiation research as a battle between those for whom the major risk is finding a false signal and those for whom it is missing a real signal. The interferometer and bar groups occupy the two poles of the dichotomy. Since the interferometers will not be "on air" for several years, the potential damage caused to them by a false bar signal is their principal worry. The bars, on the other hand, are losing credibility under the interferometers' "strong theory regime" and will soon be overtaken in sensitivity; their very survival could depend on them finding signals fast. Of course, under this model alone, the Louisianians ought to want to run in the same directions as the Frascatians.

In any case, according to the Frascati Team, there are yet more disadvantages to blind analysis. Once one has a signal to work with, one might be able to enhance its value as evidence in ways other than simply boosting the signal-to-noise ratio. One might find, for example, that not only does one have coincidences—albeit at a low level of statistical significance—but the ratios of the levels of energy of the coincident signals are consistent between the two detectors; this would be further evidence for a common source. Or one might find that the signal peak, low though it is in absolute terms, correlates with the times when the antenna is pointed in some particular direction—putatively, a strong source of gravitational waves. These are ways of boosting the credibility of a signal but only after one has a vestigial signal to work with; it can be done well only if the vestigial signal is in the public domain. From the point of view of evidential collectivism, compulsory blinding is a mechanism for suppressing the development of instruments and data analysis techniques.

The Perth-Rome Coincidences

The tensions and differences of view we have described have recently found a new focus as a result of the discovery of the apparent coincidences between the output of the Frascati antenna and that belonging to the

group at Perth.⁴² The new signals, like all those that have been announced, imply an energy level that requires what the scientists refer to as “new physics” or “new cosmology” to make sense; in short, they are as difficult to accept as all the previous claims. Nevertheless, at a conference in Pisa, held in March 1996, the leader of the Perth group suggested that they be announced; the leader of the Frascati Team, who was organizing the session, was less enthusiastic.

Perth: Because we're not down to be giving any talks . . . in that bar session, and . . . I don't want to give the impression that we're not working on the bars. . . .

. . . And so I thought it might be an opportunity just to say that we're seeing some interesting coincidences—we're trying to understand it.

Frascati: If they ask me, I will make no comment. . . . Because you see, we are in this difficult situation, since we are publishing papers on the supernova. . . .

Perth: I think it's not right . . . because of the fuss that there's been in the past, to deny the data.

Frascati felt too bruised by the reception of the supernova claims to want to be controversial again, but Perth went ahead and offered the results to a skeptical audience. The leader of the Perth group explained to me much later: “We were not going to be bullied by people who have their own agenda. We believed that what we had seen was reasonable and interesting and that you should tell the story as the story goes—as it unfolds.”

Over subsequent months, the evidence for coincidences between Perth and Frascati grew stronger. Once more, the Frascati Team felt it was time to publish something positive. They believed, however, that, as well as those of the Perth group, the results of the LSU group should be included in any report. That is the context of the Laughing Seafoods conversation.

We have now understood enough of the technical, institutional, national, and cultural environments of these groups to understand the state of the argument in December 1996 when the Laughing Seafoods conversation took place. The immediately pressing point of the conversation and of various other discussions that took place at the same data analysis meeting was whether the LSU group would agree to their names and their data being included in a joint publication that would include a report of the coincident signals between Frascati and Perth.⁴³ The Frascati group

⁴² In fact, the Frascati antenna discussed here is located at CERN in Geneva, but I will stick to this usage to save confusion.

⁴³ The conference was the first Gravitational Wave Data Analysis Conference, held in Boston, on December 6–8, 1996. LSU, it should be stressed, was not suppressing positive data that would support the Rome-Perth coincidences. But it was generally agreed that even the presentation of neutral data by the LSU group in the same paper

also wanted the Louisiana protocol relaxed for the sake of future collaboration and development, and they wanted the threshold for potential signals to be set low. The leader of the LSU group was trying to make up his mind what to do. His position was becoming less clear cut, in spite of the pressures toward the retention of uninterpreted data discussed above. The LSU group had been running detectors for two decades with very little to show for it. For some time, they had been seeking funds from NSF for a new generation of more sensitive spherical resonant detectors. If this development does not go forward, they may conclude, that they should be saying more about what they have done (especially as the leader of the group approaches retirement).⁴⁴ As one member of the group put it in discussion with me:

LSU1: And it may be that [we have] been too cautious, er, in saying we want this to be a real field. And we want to develop the science—the science is important stuff to do because no one really understands gravity yet; they don't understand why it's the way it is, so since we can begin to do these experiments, we have to do them, we have to do them responsibly. And I can't say that I looked at it this way when I started, but on the other hand, on thinking about it, I'm pretty sure that foremost in my mind was there is no way that I want to do anything that is going to have me labeled as a crackpot. . . .

When someone then begins to find experimental things that may or may not be there, then, again, you've got to be very careful, and I suspect that underlying all of this is the desire to really say, "Well, OK, if my work is going to be significant, I've got to make sure that no one thinks that it's crazy."

Collins: Yeah, you've got a gamble to make though, haven't you?

Because you can always make absolutely sure that nobody thinks you're a crackpot by never finding anything.

LSU1: Oh—that's true—absolutely—and Weber accuses me of that. In years past, Weber has said, "There are two mistakes you can make—one is to find things—anything—you know, to find coincidences or something like that given any data," and I've said this at a number of talks I have given—you can make the other error and that is to try too hard, and that is what Weber accused me of; he said, "You can also try too hard to not see events." And I've been very aware of that.

Later, discussing whether LSU would agree to publish with Frascati, the conversation went on:

as the positive reports by the Rome-Perth collaboration would boost the credibility of the whole exercise. As it was, Frascati was becoming marginalized.

⁴⁴ An NSF review committee, which met at the end of 1996, decided not to fund the more sensitive detector and to close down all work on resonant bars in the United States once foreign resonant detectors had reached an unambiguously higher level of sensitivity. At the time of the interviews reported here, this was unknown to the LSU group.

Collins: So [to add your name] will add more weight to the paper, but you must also have a worry that if the paper is dismissed, its gonna take credibility away from you?

LSU1: Yeah, but we can stand the shot now; that we can stand. But on the other hand, maybe there is something there—I am very aware of that. And so do you throw away 25 years of working on this? And this is not thinking of a Nobel Prize in this . . . it's just saying, "OK, well we've not published much"—do you want your 25 years of work just to quietly disappear—while other people go ahead and get on with advancing the field.

Now we know what was going on in Laughing Seafoods.

SUMMARY AND CONCLUSION

I have examined the clash between two evidential cultures. The closed evidential culture of the American group of gravitational radiation researchers is homologous with the institutional and structural forces in which they are embedded. The open evidential culture of the Frascati Team fits with the looser set of constraints under which they work, though the growing cultural homogeneity of big science makes things more ambivalent in that case and gives rise to disagreements within the same institutional setting.

I have looked at two recent episodes of resonant bar research in light of these differing backgrounds and cultures—involuntary blinding and the putative coincidences of the Rome and Perth groups. In both cases, the dilemmas that the groups of scientists face about what to count as data and what to count as noise and how to present and analyze their potential findings are set within a network of considerations that stretches in many directions. The decisions that have to be made rest, inevitably, on the sort of reasoning that is familiar in any walk of life. The young physicist quoted earlier who had decided to invest his effort into interferometry made the point in respect of career choice: "It's the kind of value judgment that people make when, you know, you decide whether to have children or not, or things like that." I have tried to show that this kind of tacit decision making, informed by background assumptions, many of which are not recognizable as "scientific," goes into demarcating interesting data from uninteresting noise. The very notion of "data" depends on different scientific traditions and, in this case, patterns of institutional forces.

My guess would be that, in the immediate future, the growing global power of interferometry will result in the further marginalization of evidential collectivism and open evidential cultures; in this field, the one right way to do science will become more and more firmly entrenched. But it will be intriguing to watch events in the first years of the new millenium

as the interferometers belonging to different national teams see their first signals at the margin of noise. For closed evidential cultures, the prospect is not without its problems, and even now, some scientists are trying to organize the way the findings will be made public.

VISIBLE AND INVISIBLE FORCES

I have described the relationship between the evidential cultures of the two groups, their view of the heavens, and their institutional positions as a homology. I choose that word because it would be quite wrong to draw crude causal connections between the structures and pressures within the respective environments and the scientific choices made by the individuals. The history of scientific heroism is the history of scientists resisting institutional pressures, and there is no reason to suppose that the principal actors who speak from these pages are not just as capable of resisting external pressure as their famous forebears. Of course, given that every scientist discussed here would readily concede that, in the last resort, blinding is the only sure way to eliminate unconscious bias in data analysis, they would also have to accept the possibility that structural forces *might* affect their larger judgments in subtle and invisible ways. It would be very difficult to turn this "might" into anything definite, as the complex workings of law courts reveal. Fortunately, we do not have to prove the matter one way or another for any individual scientist; we have only to describe the pressures and the plausible patterns of action within the scientific community.

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