Conflicting Expert Testimony and the Search for Gravitational Waves

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How can we make informed decisions about whom to trust given expert disagreement? Can experts on both sides be reasonable in holding conflicting views? Epistemologists have engaged the issue of reasonable expert disagreement generally; here I consider a particular expert dispute in physics, given conflicting accounts from Harry Collins and Allan Franklin, over Joseph Weber's alleged detection of gravitational waves. Finding common ground between Collins and Franklin, I offer a characterization of the gravity wave dispute as both social and evidential. While experimental evidence alone may not have forced resolution of the dispute, there were also credibility-based reasons warranting epistemic trust and distrust. Thus we see how social factors can have evidential significance and how expert disagreement can be reasonable.

1. Introduction. How can we make informed judgments about whom to trust given expert disagreement? Can experts on opposing sides be reasonable in maintaining their conflicting views? Recently epistemologists have engaged the issue of expert disagreement generally: Feldman (2006) argues that mutually reasonable epistemic disagreements are impossible, while Rosen (2001, 71) insists that "reasonable people can disagree, even when confronted with a single body of evidence" (see Kelly 2005; Christensen 2007; Elga 2007; Feldman 2007). Here I consider a particular expert disagreement in modern physics, Joseph Weber's dispute with his colleagues in experimental physics over his detection of gravitational waves.

The main point of contention in this dispute was not whether gravi-

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‡I extend my deep appreciation to Arthur Fine, Alison Wylie, Monica Aufrecht, and Negin Almassi for their help in bringing this paper to fruition, and to Allan Franklin and Boaz Miller for their careful and constructive criticism. Earlier drafts were presented at the University of Toronto and to the Canadian Society for History and Philosophy of Science; many thanks to the conference organizers and participants. All errors are mine alone.

Philosophy of Science, 76 (December 2009) pp. 570–584. 0031-8248/2009/7605-0002$10.00
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https://doi.org/10.1086/605789 Published online by Cambridge University Press

tational waves exist as predicted by the general theory of relativity (see Einstein and Rosen 1937; Kennefick 2007) but whether Weber had successfully detected them. In theory we are beset constantly by gravitational waves from massive objects across the universe, stretching and squishing us imperceptibly, but the effect is small and hard to detect; so when Weber began publishing papers (1969, 1970a, 1970b, 1971, 1972)[1] offering observational evidence of gravitational waves, his work was met with acclaim and cautious optimism. Given conventional cosmological wisdom, existing waves were expected to be too weak to be detected by Weber's device (see Press and Thorne 1972; Misner, Thorne, and Wheeler 1973; Sejnowski 1974), but his work was taken seriously enough that others began building and operating similar devices. Although Weber had been relatively alone in developing the techniques and apparatus for detecting gravitational waves, by the early 1970s others had detectors running to replicate his findings.[2] As these attempts failed and Weber continued to offer results, however, his field turned against him; consensus opinion calcified so firmly that after 1975 Weber's gravity wave work was largely ignored. Yet he remained unrepentant, arguing until his death that evidence of his observation of gravitational waves was "overwhelming" (Bartusiak 2000, 114).

This case is also interesting because it has received conflicting analyses from Harry Collins (1975, 1981, 1985, 1994, 2004) and Allan Franklin (1994, 1999, 2005). Collins, who has studied gravity wave physicists for over 3 decades, emphasizes the social factors in the formation of opinion against Weber. Franklin analyzes the role of experiment in scientific knowledge and argues that careful study of events shows that Weber was dismissed on evidential, squarely epistemological grounds. Here I investigate this two-tiered dispute. While Franklin is right to describe the position against Weber as reasonable, I suggest, Collins is also right that Weber's unpopular position could also be reasonably maintained. Reasonable expert disagreement was possible, I suggest, in part because of the social-evidential dimensions of credibility. At least some considerations that Franklin regards as evidential and properly epistemological are so in context, making room for social factors of evidential significance. At the same time, at least some considerations Collins regards as social are evidential in a way that philosophers of science and epistemologists can also appreciate. In short, this case demonstrates the importance of ex-

1. Weber's earlier papers on gravitational radiation (1960, 1962, 1966, 1967, 1968a, 1968b) concern theory, experimental setup, and data collection, but not evidence of wave detection.

2. Logan (1973) discusses these other devices and how they differed from Weber's device.

https://doi.org/10.1086/605789 Published online by Cambridge University Press

perimenter credibility as social and evidential: with an eye to credibility, we can see how reasonable disagreement can be possible among expert peers.

2. Brief History of Gravitational Waves. Collins's work on gravity wave physicists is a paradigm case in the sociology of scientific knowledge (SSK), used to illustrate SSK theses, including Collins's "experimenter's regress" (1985, 1994, 2002), which claims that disputes over experimental results cannot be resolved by appeal to experiment alone. Something else is needed to limit acceptable interpretations of experimental results; without this something extra there is nothing to stop endless debate on the meaning and significance of results, nothing to break the regress of disputes. "The something *can* include a range of things from high theoretical argument to low dirty tricks," Collins explains (1994, 502). Franklin meanwhile defends an "epistemology of experiment" (2005, 4–7), not a theory of knowledge but rather a set of strategies for validating disputed scientific claims. There are no formal criteria to determine when evidential considerations against disputed observations make scientific rejection of such observations reasonable; nonetheless, Franklin argues, cases like the gravity wave dispute show the resolution of scientific disputes to be an evidential, reasoned process. The two accounts of gravity wave physics also differ methodologically. Collins interviews participating scientists and takes conference Q&A and hallway conversations into consideration, while Franklin sticks to journals and conference proceedings (Collins 1994, 497–498; 2004, 135; Franklin 1999, 14–15). Still, they frequently agree on the historical details of Weber's professional rise and fall.

After a 1955 sabbatical at Princeton University working with John Wheeler, Weber published a 1961 monograph on gravitational radiation, including a proposal for wave detection which he spent the decade bringing to fruition (see Weber 1961; Bartusiak 2000; Yodh and Wallis 2001). Weber sought to detect waves by looking for fluctuations in the intensity of waves as measured by changes in the vibrations of an isolated cylindrical aluminum bar (a "Weber bar"). You and I do not notice these fluctuations, as we are not very massive, nor well isolated, but the bar is in a controlled environment equipped with carefully monitored piezoelectric crystals. As the fluctuations stretch spacetime and the bar, resulting current is registered by the crystals. This happens quietly; isolating signal from background noise is not easy. Weber's approach was to locate signal peaks above noise to be attributed to gravity waves. Weber reported in 1969 17 "events" detected simultaneously by bars in Maryland and Illinois, and continued to report events into the 1970s. From 1970 through 1975 several bars were constructed and operated in Weber's style, but no published papers gave corroborating results (see Braginsky et al. 1973;

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Drever et al. 1973; Levine and Garwin 1973a, 1973b, 1974; Tyson 1973a, 1973b; Tyson, Maclennan, and Lanzerotti 1973). Criticisms were raised, including errors in Weber's data analysis computer program and a notorious false positive Collins calls "the four hour error." Weber had noted a coincidence between bars in Maryland and Rochester, which would have been a nice piece of evidence, but there was no coincidence: the Maryland bar was on U.S. East Coast time and the Rochester bar on Greenwich Mean Time, so the "coincidence" concerned events 4 hours apart. Much was made of this error as proof that Weber not only could but did produce false positives from noise (Garwin 1974). After 1975 his work was relatively ignored; as he defended his results, the field moved to cryogenic bars and interferometry (see Taylor and Weisberg 1982; Fairbank et al. 1988; Kennefick 2007).

From such considerations Franklin argues that rejection of Weber's work was reasonable; although neither infallible nor algorithmic, it was based "on valid experimental evidence and on reasoned and critical discussion" (2005, 193), not cognitive, social, or career interests. So, Franklin argues, Collins is wrong to label this an experimenter's regress. Collins grants that those against Weber were reasonable but says that Weber was reasonable too, and had things ended differently, that outcome would have been reasonable as well—"Reasonable people are always disagreeing" (1994, 502). His point is that if experimental evidence alone did not "force" (Collins 1981, 34) the anti-Weber movement, something extra-experimental must explain the formation of this majority opinion.[3] Collins takes Franklin's admission that the dispute's resolution was not algorithmic as a key concession: if the process was not algorithmic, further explanation is needed. The potential for misinterpretation is high here, I suggest, as the "something extra" Collins locates in this case is not overtly political and so plausibly understood by Franklin as epistemological. This point becomes clearer through closer analysis of the dispute.

2.1. Replication. The failure of other devices to replicate Weber's data may seem a serious problem, but Weber argued and others agreed that these devices were dissimilar enough from his original to make problematic their status *as* replications. Based on 1972 interviews with those involved, Collins observes, "*they are not interested* in building what they perceive as isomorphic models of the detecting apparatus" (1975, 210; emphasis in original). The detectors differed from the original for many reasons.

3. As Collins puts it, "given the tractability, as it were, of all the negative evidence, it did not *have* to add up so decisively. There was a way of assembling the evidence, noting the flaws in each grain, such that outright rejection of the high flux claim was not the necessary inference" (1985, 91).

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For one, copying Weber's device exactly would require prolonged study of his bar, while the basic idea for a detector could be deduced from general relativity. Anyway, copying Weber's bar exactly held no prestige: as one interviewee said, "If it confirmed the first researcher's findings, it would do nothing for *them*, but would win the Nobel prize for *him*, while on the other hand, if it disconfirmed the results there would be nothing positive to show for their work" (Collins 1975, 210; emphasis in original). Franklin's epistemology of experiment also reminds us that different approaches, because of their differences, often provide better corroboration than would an exact copy (2005, 4–7). Collins (1975, 211) notes that as long as it was unclear whether Weber was observing gravity waves, whether he had actually built a good detector, there was no reason for those building devices to follow him exactly. As Drever put it, "my aim was *not* to try to find out if Weber's work was right or wrong, but to find out more about gravitational waves. Therefore, our devices were developed to be rather different from the ones which he has built" (Shaviv and Rosen 1975, 265). Replication is complicated further because what *constitutes* an event is also debatable. Weber (1973) challenged others' characterization of their results as negative where their devices yielded some two-detector coincidences; whether such coincidences were impressive enough to be events was itself a matter of interpretation. In these ways an absence of replication of results does not unequivocally undermine Weber's detection claim.

2.2. Theory. If Weber was detecting high-flux gravity waves, then the universe was much busier than posited by cosmological theory, since only truly cataclysmic and rare events were thought to yield such waves (see Press and Thorne 1972; Sejnowski 1974, 43–45). Yet in any conflict between theory and experiment, reasonable people reasonably disagree whether to adjust theory, alter auxiliary assumptions, reject the results, or abide conflict until a solution presents itself. For his part Weber seemed to take the last approach. He made no claim that cosmological theory should be abandoned; his focus was experiment, he said, and he simply noted that the world could be populated differently than present theory allows (Weber 1970b). This attitude was not unique to Weber alone (see Sejnowski 1974, 45), and Franklin and Collins agree that conflict with theory was not decisive against his claim (Collins 1981, 35–36; Franklin 1999, 32).

2.3. Calibration. Beyond the design of detectors Weber and his peers differed over their proper calibration (see Shaviv and Rosen 1975). To calibrate a detector's sensitivity a known electrostatic pulse would be injected into the antenna and resultant output measured. For this Weber

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preferred a nonlinear algorithm sensitive to changes in signal amplitude but not phase; others preferred a linear algorithm sensitive to amplitude and signal phase. The linear algorithm was arguably more efficient in identifying calibration pulses in Weber's bar and others. When the linear algorithm was applied to Weber's data, none of the peaks seen with the nonlinear algorithm were revealed, while application of the nonlinear algorithm to others' data revealed no new peaks either. Given this asymmetry it is tempting to infer that Weber preferred his algorithm *because* it produced the desired result. Yet Weber argued that his approach was preferable for its resultant "proliferation" (the production of many output pulses over threshold for each input pulse) and its greater sensitivity to the longer pulses, characteristic, he argued, of the waves he was detecting. His critics agreed that their approach was better only for short pulses and that the existence of longer signals could not be entirely ruled out.[4] Algorithm choice is something on which scientists with different research priorities can reasonably diverge, and while it is problematic that other detectors showed no peaks even using Weber's algorithm, this is only decisive on the assumption that the detectors are sufficiently similar in design and application. Furthermore, it was open to Weber to challenge the use of electrostatic pulses for calibration; in fact, his student Sinsky (1968) developed a gravity-based calibration method that attributed greater sensitivity to Weber's bar than electrostatic pulses. Sinsky's method was difficult to perform, however, and he left the field soon thereafter (Collins 2004, 64).

2.4. Experimental Errors. As noted, Weber's case was weakened by problems with his experimentation. Weber's defense on these matters is considered in detail below, but note that these problems did not invalidate all of Weber's data directly. He could admit particular errors while regarding his detection claim as evidentially grounded overall.

3. Weber and Garwin in *Physics Today*. One major catalyst of consensus opinion against Weber according to Collins was IBM physicist Richard Garwin's critique—publications, talks, and letters characterizing Weber's research as not just inconclusive but groundless. Garwin began traditionally by publishing negative results from his own bar antenna (Levine and Garwin 1973a). This device was the smallest then operating, a tenth the mass of Weber's, so one might regard its null result as weak evidence

4. Franklin notes, "Although calibration, and its success or failure, played a significant role in the dispute, it was not decisive. . . . The reason for this was precisely because the scientists involved seriously considered the question of whether or not an injected electrostatic energy pulse was an adequate surrogate for a gravity wave" (1999, 32).

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against Weber's claim. Collins (1981, 43) reports, however, that interviewees regarded this small experiment as significant because of "the way they wrote it up"; where others were "tentative" in presenting negative results, Garwin was "very convincing." At the June 1974 "Fifth Cambridge" Conference on Relativity (CCR-5), Garwin recapitulated his null result and urged David Douglass to publicize his criticisms of Weber's computer program (Collins 1981, 46). Garwin's campaign reached its widest audience and tipping point, Collins contends, in a letter to *Physics Today* alleging that Weber's lab had "published no credible evidence at all for their claim of detection of gravitational radiation" (Garwin 1974, 9).

Two of Collins's interviewees described Garwin's criticism of Weber as "not science," as "not doing physics any longer"; a member of Garwin's team noted, "It's not clear that it was ever physics, but it certainly wasn't by then. If we were looking for gravity waves we would have adopted an entirely different approach" (Collins 1981, 47). Reasons given by Garwin and his team are scientific and social, but not personal. Garwin held no animosity toward Weber but believed and sought to convince others that his work was "pathological science" in the sense characterized by chemist Irving Langmuir (Collins 2004, 164). Langmuir (1968, 4) described a series of experiments performed by scientists convinced that they were seeing phenomena when, in fact, they were "not measuring anything at all." From these cases Langmuir distilled key features of pathological science, features Garwin apparently saw in Weber's work (Collins 1981, 48). If Garwin was not doing physics any longer, it is because he went beyond just publishing his results to actively disputing Weber's credibility as a trustworthy researcher. This endeavor was scientific and social to the extent that it aimed to shift community perceptions of Weber's work and promote better science. As long as Weber's project was respectable, graduate students might find his project worth pursuing, which Garwin feared would wreck promising careers and needlessly prolong an errant research program. Langmuir had warned that pathological science is rarely exposed quickly, and Garwin hoped to avoid a long process (Collins 1981, 1985).

Franklin disputes Collins's claim that Garwin "acted as one might expect a scientist to act who realized that evidence and arguments alone are insufficient to settle unambiguously [the debate]" (Collins 1985, 95). Instead, "Garwin's behavior could also be that of a scientist who believed that Weber's results were wrong, and that valuable time and resources were being devoted to the investigation of an incorrect result" (Franklin 1999, 32). Close analysis of the exchange in *Physics Today*, I suggest, supports an interpretation of Garwin's approach that finds him arguing against Weber's evidence by arguing against the credibility of the speaker vouching for it. Unlike Collins, I make no claim that Garwin's critique

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of Weber in *Physics Today* was a tipping point for consensus opinion against Weber,[5] but I suggest that this critique functions by appealing to experimenter credibility as evidentially relevant.

There are no official guidelines for letters to *Physics Today*[6] but in his 1974 letter Garwin makes uncommon use of this popular forum to transmit criticisms broadly. Gravity waves were familiar to readers of *Physics Today* from features by Weber (1968c), Logan (1973), and Sejnowski (1974). Ostensibly Garwin is responding to Logan and Sejnowski, but he engages neither beyond perfunctory citations; instead, informing *Physics Today* readers of "the present situation" in gravity wave physics, Garwin gives several criticisms of Weber's experimental results and methods. After initially acknowledging Weber's pioneer status in experimental gravitation, Garwin offers the bold assessment that Weber's lab had "published no credible evidence at all for their claim of detection of gravitational radiation" (1974, 9). For this sweeping claim, the premises are surprisingly piecemeal. Let us consider these premises and Weber's replies.

3.1. Computer Error. Garwin relays Douglass's CCR-5 criticisms of Weber's computer program. Weber had sent a tape to Douglass and others to check his work, and Douglass had found a program error that Garwin claims was responsible for creating "nearly all of the so-called 'real' coincidences of June 1–5, 1973" (1974, 9). In his letter Weber replies that the error is real but does not undermine his 1973 data, as "we have never reported results in publications or at meeting based entirely on computer analysis of one series of tapes"; coincidences identified by computer were also identified by "real time counting and pen-and-ink records" (1974, 13). Weber also claims that two double-checking labs processed his tape incorrectly, producing errant checking results that Garwin disseminated (see Shaviv and Rosen 1975, 247). As Garwin studied neither the tapes nor the program, Weber asks, why is he writing to *Physics Today* about it? Note that Garwin's criticism, if accurate, does not impugn all of Weber's data directly, only the data on this tape.

3.2. Histogram Bins. Garwin and his colleague Levine doubted that coincident events due to gravity waves would be confined to single 0.1-second histogram bins, as exhibited in Weber's data. To test this, Levine

5. I do not claim, e.g., that these letters are more important than the Seventh International Conference on General Relativity (GR7) in Tel Aviv (see Shaviv and Rosen 1975), on which Franklin quite reasonably places much emphasis. I attend here to the *Physics Today* letters specifically because of my interest in the particular way in which Garwin defends his "no credible evidence at all" conclusion.

6. Marty Hanna, *Physics Today* letters editor, personal communication, 2007.

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"simulated" the Maryland apparatus using his IBM detector; in his simulation the histograms for coincident pulses were not confined to 0.1-second bins but spread across several. From this, Garwin concludes that "it has been demonstrated that the coincidence data of [Weber et al. 1973] *did not* result from gravity waves, and furthermore *could not* have resulted from gravity waves" (1974, 9). Here the force of the criticism relies on sufficient similarity between the apparatuses. If Levine's experiment showed that test pulses bled across multiple bins with his device, does this properly undermine Weber's data? The reader must judge the comparative force of Levine's simulation.

3.3. The 4-Hour Error. Regarding "other facts . . . discussed at CCR-5," Garwin shares the 4-hour error revealed by Weber as an apparent coincidence at a meeting in Warsaw. In his letter Weber responds, obliquely, "We have reported a zero-delay excess of coincidences over accidentals, but have not claimed a positive result in open-literature publications or at meetings" (1974, 13; cf. Shaviv and Rosen 1975, 248). How does this response fit with Garwin's claim that Weber provided the errant information at meetings? Consider the difference between presenting an apparent coincidence as a positive result, an "event," and mentioning it without this categorization. Even if real, the 4-hour error was not statistically significant enough to be an event, Weber argues, and after proper correction the data showed a larger coincident peak (1974, 13).

3.4. Random Data Selectively Presented. Garwin ends by reporting with consternation Weber's remark, at CCR-5, that when his team fails to find coincidences, "we try harder." Coupled with the fact that Weber had published positive but not negative results, Garwin began to wonder whether Weber was finding false positives in randomness. As a check Levine compiled a histogram of coincidences between two random data streams, presented in Garwin's letter to illustrate the possibility that Weber in pathological fashion was measuring nothing at all. Weber must at least publish his processing algorithm for data selection, argues Garwin, to prove that he is not just giving selectively reported random data. Weber replies by directing readers to a preprint of Shaviv and Rosen 1975, which "gives data on periods when no coincidences are observed and answers the questions raised by Garwin concerning selection of data, histogram widths, and calibration" (1974, 13).

Weber ends his letter by remarking, "Computing errors have been an important factor in the politics but not in the physics of our experiment" (1974, 13). Were Garwin's criticisms in *Physics Today* political? He argues that Weber's computer program caused several mistakes; that single-bin

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results could not have been made by actual gravity waves; that Weber was so keen on positive results that he mixed up time zones; that random data selectively analyzed could produce coincidences too. From these premises readers are supposed to conclude that Weber published "no credible evidence" for his gravity wave claim. If this assessment makes sense, however, it is not because Garwin has conclusively shown all the evidence to be errant but because significant doubts have been raised about those providing the evidence. As far as experimental evidence goes, exposing error eliminates particular pieces of evidence for the detection claim; as evidence against the reliability of those testifying to it, widespread notice of such errors can hurt one's reputation as a careful experimentalist. Without that reputation and without corroborating results from other labs, the positive case for Weber's observation of gravitational waves becomes quite thin.

4. Credibility and Disagreement. Where Franklin sees Weber's loss of credibility as straightforwardly a matter of evidence (1999, 31), Collins sees it as a social-political process (1981, 54). I argue that there is common ground to be found between these positions when we recognize credibility as both social and evidential. Consider the issue of reasonableness in the conflict between Weber and his peers, an issue on which Collins and Franklin are at odds. Franklin sees Collins's position as antiscience: "the regress argument, despite Collins's disclaimer, casts doubt on experimental evidence and on its use in science and therefore on the status of scientific knowledge. . . . The regress is broken by negotiation within the scientific community, but the decision is not based on anything that might be called epistemological criteria" (2005, 200). Yet arguing as Franklin does that Weber was rejected on epistemological and reasonable grounds, including "reasoned and critical discussion," is compatible with the idea that rejection involved extra-experimental evidence. That is, reasoned and critical discussion can be the sort of social-epistemic enterprise capable of evidentially supplementing disputed experimental evidence.

Helen Longino challenges a rational-social dichotomy presumed by many philosophers and sociologists; contra "the dichotomous understanding of these notions, if an epistemic practice is cognitively rational, then it cannot be social" (2002, 1), she argues that "cognitive capacities are, in science, exercised socially, that is, interactively" (2002, 8). For their part Godin and Gingras also challenge a social-epistemic dichotomy: constructivist sociologists and their philosopher-critics both err, they argue, in assuming an opposition between the epistemological and social aspects of how scientific communities resolve disputes. They attribute this tendency to Franklin and Collins, although Collins protests that he endorses

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no distinction between epistemological and social.[7] Franklin sees discussion and argumentation as epistemological—consistent with this is the recognition of scientific argumentation as a collective process guided by contingent community-specific rules, norms, and presumptions constraining how discussions go. "Though philosophers call these criteria 'epistemic'," Godin and Gingras argue, "they in fact simply summarize and formalize what can be observed to be 'rules of the game' . . . in the scientific field" (2002, 148). It is open to us, I suggest, to recognize these considerations as both epistemic and social.

Credibility can be social and evidential in at least two ways. The first relates directly to the approach of Garwin's letter, where Weber's evidence is dismissed in total although direct criticism is raised against only some of it. This criticism casts doubt on Weber's credibility as a researcher, which then casts doubt on his evidence in total as untrustworthy offerings of a questionable experimentalist. Since the trustworthiness of experimental results is in part a product of an experimentalist's reputation in his scientific community, credibility is a social factor with epistemic significance.[8]

The fact that "reasoned and critical" scientific discussion is governed by contingent, community-specific rules, norms, and assumptions shows a second way in which credibility is social and evidential. Those with different theoretical, methodological, and interpretive commitments may reasonably disagree about the case to be made for and against Weber's gravity wave claim. Such commitments may influence, for example, one's assessment of the superior calibration algorithm or whether other devices were sufficiently similar to Weber's to constitute failed replications of results. Of course not all methodological, theoretical, and interpretive commitments that scientists could endorse are live options for all scientific communities at all times, as Kuhn recognized. One's status as a full member of a community depends at least in part on accepting rules limiting the subset of acceptable commitments. When a scientist challenges the paradigm governing his field, often this "reflects not on the paradigm but on the man. Inevitably he will be seen by his colleagues as 'the carpenter who blames his tools'" (Kuhn 1996, 79). One's credibility and the credibility of one's research for one's peers depend in part on one's acceptance of the commitments governing argument and discussion in a field. This

7. Where he seems to hold a distinction, Collins says, he is quoting others' usage (2002, 153).

8. I do not posit a biconditional relationship between experimenter credibility and correctness of experimental results; rather, the claim is that loss of credibility can contribute evidentially to undermining the epistemic trustworthiness of one's experimental results.

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is social and evidential because rejecting widely held commitments is for those endorsing these commitments evidence against one's trustworthiness. In this way "rules of interaction in the field" can have epistemological and sociological significance.

What does this case tell us about the epistemology of disagreement generally? Consider Feldman, who defends what he describes as a skeptical view on reasonable disagreement: quite often, he argues, the reasonable response to genuine epistemic disagreement among epistemic peers after full disclosure of the evidence is suspension of judgment (2006, 217). Here epistemic peers are comparably informed people of similar intelligence and reasoning abilities, and full disclosure occurs when the disagreement has been discussed fully, nothing relevant to their beliefs withheld, so each knows the other's reasons (2006, 2007). In such situations epistemic peers have shared their evidence, so either one should move to endorse the other's position, both should suspend judgment, or one or both must be unreasonable.

Obscured by this analysis, however, is the effect on each expert's total evidence when these experts are those vouching for the experimental evidence, as in the gravity wave case. Weber and his peers could reasonably disagree in part because of their asymmetric access to the experimental evidence. Results made available by Weber and others are only as reliable as those providing those results; issues of researchers' credibility transfer to the credibility of the experimental evidence in question. From his position Weber might possibly know that his total experimental evidence for gravity wave detection is stronger than publicized problems with his experimentation suggest; from the inside he may know that his published data are not random data selectively published, his data collection conscientious and careful. His peers do not have this access, however, and cannot take Weber's assurance of these things.

In theory, full disclosure involves experts sharing their reasons for endorsing particular methodological, theoretical, and interpretive commitments over others. Such conversations can happen, but in practice scientists are unlikely to do so for every commitment relevant to the assessment of the dispute. Full disclosure seems a demanding requirement for disputes in experimental physics, where arguments and reasons crucially rest on experimental evidence testified to by researchers. The premise that results have been competently, conscientiously gathered and presented is key, and the justification of this premise is grounded in researcher trustworthiness. Physicists such as Weber and his peers may discuss conflicting views on the evidence extensively, but the need for epistemic trust in girding the experimental evidence is not easily displaced. The fact that the disputing parties are the trusted and distrusted parties testifying to

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the evidence in dispute, then, makes room for reasonable expert disagreement.

5. Conclusion. As earlier efforts to detect gravitational waves with bar antennas failed, in 1991 physicists requested a special appropriation from Congress to build interferometers in Washington and Louisiana. The project had approval from the National Science Foundation (NSF) and an external review board, but due to cost the Laser Interferometer Gravitational-Wave Observatory (LIGO) was a rare NSF project requiring line-item approval in the federal budget (Bartusiak 2000, 138). LIGO was funded against opposition from Weber (among others), who argued that he could detect gravitational waves at a fraction of the cost. In a letter to Barbara Mikulski, senator from Weber's state of Maryland and committee chair on NSF oversight, Weber wrote, "A small (100 kilogram) available silicon crystal can be instrumented to have sensitivity 10^{10} better than LIGO at the total cost of less than $500,000" (Collins 2004, 361).

Visitors to LIGO in Washington now find a portrait of Weber generous yet contrary to his self-conception. A Weber bar is displayed in the visitors' center with praise from physicist Kip Thorne: "Joseph Weber was the first person with sufficient insight to realize that it is *not* utterly hopeless to try to detect gravitational waves." Little recognition is given to his failure to convince his peers or to the possibility that he may have been right. There is no ambiguity about the real science here: it is the interferometer stretching across the desert, not the bar in the visitors' center, unplugged, a museum curiosity. Its placard reads: "Dr. Weber's reports of successful detections were not corroborated by other groups and the detection controversy remains part of his scientific legacy. Of greater importance to the LIGO community, however, is his legacy of exploration, his willingness to take risks, and his confident determination to carefully gauge effects that the rest of the world had deemed too small to measure." The dispute is over; no one now says that the evidence that Weber saw gravitational waves is overwhelming. Yet dissidents like Weber may seem a problem for Franklin's "old-fashioned notion . . . [that] science provides us with knowledge about the world which is based on experimental evidence and on reasoned and critical discussion" (1999, 1). Does science give us knowledge when experts like Weber offer careful dissent? I think it can, but as the gravity wave case suggests, our evidence for this knowledge is sometimes experimental and extra-experimental. Thus, we see how credibility assessments can license different assessments of the evidence by those with different commitments and different access to that evidence; thus, we see how expert disagreement is possible and sometimes reasonable.

https://doi.org/10.1086/605789 Published online by Cambridge University Press

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