

Annotated Interactional Expertise? Collins and Barish argue about noise with Collins's commentary

Except from 2002/2003 draft of *Gravity's Shadow*

In the year 1999 my project went through a crisis. My draft paper on the management of the project (roughly the content of Chapters 31, 32, 34, 35, 37, and 38 [these chapter numbers may not refer to the book as published]), was severely criticised by a large number of my respondents. Among the criticisms was Barry Barish's charge that my work was anecdotal and journalistic rather than scientific. Gary Sanders said that he couldn't distinguish my approach from that of an anthropologist -- which later became journalist.

Maybe, part of the problem was that my account was too like the kind of account they could have provided. Maybe the scientists wanted to see something that they did not already know. The way I saw it, I would be doing my job satisfactorily, if not brilliantly, if I conveyed the world of physics to a wider audience even if there were no surprises for the physicists in what I wrote. Maybe they wanted to see a systematic ordering of the documents that belonged to the project of the kind that they might find in an 'official history.' Or maybe it was something less than this still -- maybe what they wanted was an orderly recounting of the world as they saw it backed up with 'hard evidence.' Whatever, the new crisis provided the opportunity for a lot more discussion between me and my critical respondents about methodological relativism. Here is one of those discussions, between Barry Barish and myself. It goes right to the heart of the problem and, *inter alia*, sheds some more light on the meaning of interactional expertise.

Imagine Barish and I sitting in a side room, part way through a scientific meeting, pretty well shouting at each other, though in a frustrated manner rather than showing real anger. I think we were both enjoying it in the way that one can enjoy pushing a strongly held position in a serious academic argument when you are sure that you can convince the other person if only they'll listen. And I really don't think there was ever any real animosity between Barish, Sanders and me, or if there was, it didn't show. Even when we were shouting at each other I knew I respected them and I was always given to feel that they respected me.

... I use square brackets to mark edited and clarifying comments in the transcript, double square brackets to represent interruptions, and curly brackets for commentary made after the event. There is more than one issue here and one of the central ones -- the role of scientific documents in my research -- will be the entire subject of the next chapter.

I need to add one qualification and that is the way I represent Bob Spero in this debate. A lot of the time in what follows below I am taking a devil's advocate position putting what I see as Bob Spero's position. But I am not sure that Bob would actually want to claim for his own everything that I put into his mouth; in fact, I suspect I invent a slightly more radical Bob Spero than the original. In subsequent correspondence Bob suggested that he would not want to

endorse quite such a radical character for the unknown noises as I impute to him. So, rather than change any of the things that were actually said, I've changed `Spero' to `Spero*' wherever it seemed appropriate, with the imputation that the real Spero ought not be held responsible for all the claims I make on Spero*s behalf, though Spero* would never have existed in the absence of the inspiration provided by Spero. The point is, however, that Spero* does not have to be identical to Spero for the purposes of the argument.

The Debate with Commentary

[2000] Collins: The most important thing, which I wish I could convince you of, is that it's not my job to read the [scientific] papers -- [I mean that] I should read the papers, but it's not my job to reach a scientific conclusion from reading the scientific papers.

Barish: Oh, I understand that, but I think you misinterpreted what I said. I think that if other people have the same confusion, or misunderstanding, that I have, that some of your work is dangerously close to journalism instead of sociology. What that traces back to is that some of the conclusions are based on techniques that are more journalistic than academic.

Collins: The discussions [that I record] and so on, and the quotations [that I use in the papers]? {That is, that most of my evidence is based on scientists' talk rather than their writings for the public record.}

Barish: Yeah -- and that it's mostly anecdotal -- free floating anecdotal. If you get confirmation of one person from another you maybe make your conclusion: That's a journalistic [conclusion]. Or you go to three primary sources or whatever. If you get differences you pick that as something to discuss that's interesting.* But I think you lose some depth by not -- I'm not saying you should make a scientific conclusion -- but I'm saying [that] by not doing a more academic analysis of some of the same stuff and seeing what you can glean from it -- and I invited you, since you had picked the 40 meter -- and I think you'll find it's [the documentation] very lacking -- to look at the documentation of the conclusions on the 40 meter.

If this is a science and [Bob] Spero is really making scientific statements, show me the documents so I can refute them, instead of anecdotal discussions and comments that come down the hall and this or that. If somebody wants to tell me that this is not subject to analysis, I want to see the argument. And if you can tell me that all he does is make a claim, then bullshit -- I don't care. I want to see why. I challenge him through you right now, because I don't see the documents. I've read everything -- OK -- that he's ever written. What is it [the unknown noise source]? Because maybe it isn't subject [to analysis], maybe this has some tricks in it, something I don't see, but to tell me that he just knows in his heart somewhere that this is not subject to analysis like the rest of science -- Show me something! What is it that isn't [subject to that kind of analysis].

*{Barish's description of this part of my method is astute. Where scientists agree with each other in a case study like this, I generally act as though their consensual viewpoint represents a fact of nature.

Where they disagree, I find it interesting to reconstruct and make sense of their alternative views. But that choice does not follow from a journalistic impulse; it follows from the fact that I am interested in how different views are maintained when scientists start with the 'same' scientific inputs and, given the possibility of maintaining different views, how the disagreements are eventually resolved.}

Collins: Well, there's lots of interferometer science that hasn't been subject to analysis.

Barish: What?

Collins: Well, the very, very, early days. I mean you couldn't say in the early days of high sensitivity [interferometry] [[Barish: not in the early days, no]] No -- the difference is this, in a sense what Spero* is claiming is that the bit of interferometry that he wanted to continue doing, still had the character of the early days of interferometry.

{Here I am saying that Spero* is wanting to say that, to some extent, interferometry still has the character it had when Drever and others were inventing brand new ways -- such as power-recycling and signal-recycling -- of making interferometers more sensitive. These kinds of unforeseeable inventions cannot be accomplished by systematic timetabled work -- at least, that is not a sufficient condition for their achievement. Barish is saying that we now understand the noise sources in interferometers so well that we can see how to reduce them without intuitive breakthroughs -- we can see all the noise sources that are there and we know all the principles we need to know in order to drive them down.}

Barish: Why does he say that? [That interferometry is still in a state that requires inspiration or intuition.]

Collins: It's an empirical claim in the last resort. But that's why I've set it up to say it's a difference in viewpoint. Three years will show us. {ie, when LIGO goes on air we will find out if Spero* was right or wrong.}

Barish: But look -- now you claim that it's noise he's concerned about. {Earlier it appeared that the problem that Spero* was talking about was the ability to lock the interferometer; this confusion may well have been my fault as a result of some much earlier interactions. Earlier in this conversation, however, I had made it clear that we were talking about noise and explained that Spero* thought that the deep problem was noise.} Then he should show me a noise curve that couldn't be explained. We have a very good model -- I'll show you -- that explains [it] from his days -- I haven't redone it -- that is used to predict the sensitivities [ie the noise levels].

You can ask: What confidence do we have that we should get a certain sensitivity [ie noise level] on LIGO? Part of that confidence comes from the fact that the same models are used to fit the [noise] data on the 40 meter. {Sensitivity is the complement of noise. The lower the noise the higher the sensitivity.}

Collins: But let me remind you -- his [Spero*'s] claim -- his and Vogt's -- and this is a quote in the paper -- 'every time we reassembled the interferometer in a new configuration we found twice as much noise as we expected and we couldn't get rid of it.' {The paper I am referring to is one of my own that Barish had read. The relevant quotation, from an interview with Robbie -Vogt in 1996, reads as follows: {{{[24 October, 1996]}}} [1996]`... in the Caltech 40 meter, when we recombined the beam, we found that the noise is a factor of two above the predicted noise. You know, we have been working on that for 8 months and it's still the same. You know, I believe it's easier to work on the 40 meter than the 4 kilometre. And so if we run into these problems in the 40 metre and we can't turn it on, there can be very severe repercussions.'}

Barish: That's bad experimentation.

Collins: You say it's bad experimentation -- he says its 'magic' -- I'll use your phrase. I'm standing in the middle -- you see. {The word 'magic' was introduced by Barish and we are now using it as a mutually agreed shorthand to mean 'aspects of the natural world that are not systematically understood.' The original phrase of Barish's, used in discussion with me in 1999, was: 'Despite the image often projected ... running these interferometers is not magic. It is amenable to analysis and systematic studies.' Incidentally 'bad experimentation' does not mean that the experimenters were bad scientists, it means only that there are badly put together features of the experiment of the sort that are inevitable in any difficult design when it is first put together. Though it is not expressed here, Barish did, as a matter of fact, think that Spero et al were bad experimenters for other reasons, namely that they were not willing to move forward fast enough in implementing the means of eliminating this kind of problem, the means being improved electronics and computer modelling of the apparatus.}

Barish: But did he discover anything other than the noise sources we talk about? When you trace it down its all experimental noise. It's all electronic noise and so forth -- it's just how well you did your experiment. We all do bad experiments ... {Barish has divided the world of noise into two. There is what he will shortly refer to as 'technical noise' which is what you expect to find in any piece of apparatus when it is first made and can be eliminated through careful design, and fundamental noise, which would come from some source that no one had thought of. Barish makes two claims throughout this discussion: there is what I will call his strong claim -- that all the noise left in LIGO is technical noise, and his weak claim -- that though there is a chance that there are fundamental noises that are still unknown, Spero never produced any evidence that would prove it or that would be of any help in uncovering and eliminating them.}

Collins: I presume he thinks there are still unknown sources of noise.

Barish: He never found an unknown source -- all he found were technical issues.

...

So that's why I say `go read the documents.' What sources of noise other than lousy experimentation -- which we all do -- did he discover?

Collins: But supposing I read the documents and I went back to him and said `look Bob' [[Barish: but his arguments about the future have to come from some real experience. I dare you to go find something they discovered in the lab that wasn't already -- other than technical crap.]] Yeah, but look -- I don't want to disagree with you, I'm just trying to explain my position. I can't go to Bob Spero, who's been working on interferometers all his life, and say `look Bob, I'm a sociologist, and you don't understand interferometry.'

{Here I am doing something slightly disreputable. Just in the way I criticise scientists for always turning a sociological point into a personal one, I am personalising the essentially theoretical position of methodological relativism. I am saying I would make myself ridiculous were I to criticise a physicist on a matter of physics. This is true, but suppose I were the best interferometer physicist in the world: if I was doing the sociology of the physics I would still have to turn off my physics-based opinions. That is, I would have to `estrangle' myself from the physics in order to do the sociology, so it would still not be appropriate for me to tell Spero that he was wrong. Historians find themselves in this position when they do retrospective studies in a sociological style. If I were less concerned with winning the point in the argument, and if the conversation had been less heated at this point, I might have tried to put the case in a way that sounds less like an `excuse.' Still, the point stands however disreputable it is. Outsiders should be very careful about forming opinions on disputed points of physics when the disputants are physicists.}

Barish: I'm not asking you to.

Collins: But that's what it would amount to.

Barish: Weeell!

Collins: And that's what I mustn't do. There's lots of historians and sociologists who do do that and it's very important not to. It's actually much harder not to. {Because in doing it you too easily put yourself on the side of the powerful.}

Barish: There's a lot of good debates. This isn't a very good one in my mind because it's basically -- it should be based on something. {Here Barish is referring to his debate with Spero, not with me -- though since I am acting as devil's advocate in behalf of Spero* it is also the debate he is having with me.} I should be able to grab onto something -- you should be able to grab onto something. Like `he says there's this X source of noise, we fought it for years, there's some trick and we beat it in some ways. You guys haven't figured out what it is, now you go and you're gonna get hit by it,' and he can show you some things that they saw that were just unexplainable -- OK? There are no unexplainable things that I can see other than that the instrument wasn't -- the noise was bad because there was bad RF [Radio Frequency] or this and that. {That is, technical noise.} There's no evidence that there was this hidden noise source that was important that's not yet understood and that's gonna raise its head now and beat

us because they didn't get around it. There is none! [Metaphorically thumping the table]

Collins: But he must think there is.

Barish: Well where is it?

Collins: I don't know.

Barish: [increasingly heated] Then as a scientist he should have written the damn thing down and documented why there was this [[Collins: well not everybody can. For instance we recently discovered a new noise source in sapphire which nobody had anticipated. You know it's got to be the case that unexpected things come up every now and again.]] {I am referring to what became known as the 'Braginsky bombshell' -- the discovery that what had seemed the obviously most promising material for the very advanced mirrors of the next generation of interferometers had serious and overlooked problems to do with 'thermo-elasticity.' Barish could have responded that this was simply a mistake rather than some fundamental new discovery because no new principle was involved. Instead he took a line that was less effective for his case.} Yeah but that wasn't an experiment, that was when you do an analysis of what you think something's gonna do: Did you think of all the possibilities?

Collins: But it could have been the other way round -- we could have done the experiment first.

Barish: But that doesn't mean we won't hit new noise sources because we're moving into new territory, and it's not a scientific issue, it's a kind of emotional issue whether you think there's something that's gonna come -- and that's where he is, and Mr Vogt is, as far as I can tell -- that somehow you can't possibly get down there without hitting something brand new, but there's no evidence for it at all. {Here Barish concedes, as he is bound to, that there might be unanticipated sources of noise but sticks to his argument that there is no evidence that there actually is any unknown source. This is his 'weak' position.}

It doesn't mean that we won't hit something we've overlooked -- I'm not saying that -- but there's no evidence that comes from anywhere, so at this point it's not a scientific argument, it's an argument that you couldn't possibly -- You didn't have to do any 40 meter stuff to find that. {That is, to say that there could be unknown noise sources is a truism.} All this interferometry he talks about. His credentials to say this are zero. He never found an unknown source [the last said with great stress].

Collins: He said he did [Barish: What?] He and Vogt both say that they found unknown sources whenever they put the interferometer back in a new configuration.

Barish: No no, I call that technical sources.

Collins: But he doesn't agree with you.

Barish: Then let me see one [source of noise] that I can read about. What is it?

Collins: He says it was a noise source we didn't expect. We didn't expect this noise source -- that's his claim.

Barish: There are none. ... [Barish says that if Spero or Vogt could give him hard evidence or information about unknown noise sources he would `gobble it up.']

Barish: [pointing to a diagram on his computer which shows the noise sources that have been theoretically be modelled compared with those which have been experimentally measured and showing that there is a pretty good match between the two] The claim is that these are the noise sources that matter, anything else is what I call technical noise. Technical noise is a way of saying that you haven't done your experiment very well -- you know the way you have your [electrical] grounds done. The noise that comes from science -- from physics -- are listed here [ie, on the theoretical plot]. None of these -- I claim that none of these [ie no new ones] have been observed by Mr Spero -- we may have missed some down here. But the 40 meter -- I'll try to find you a curve -- this track here has been traced out and fit. And he didn't find anything fundamental -- he didn't find anything fundamental up here. ... It's just a fact. Unless there's something that I have never been told. Which I can't deny. There may be something that I have not been told, but I have gone and looked at every document ever written on the 40 meter ... This LIGO curve ... Every noise that we know about has been analysed in a model. That same model fits the 40 meter.

Collins: OK -- I'll go back to him. I'll say that what I heard is that there is no noise source on the 40 meter that hasn't been analysed.

Barish: I'll show you the closest to one that isn't well understood. Look, I worry about these things -- it isn't that I'm burying my head. ... This is the 40 meter [indicating figure], and this is the displacement sensitivity; this jagged thing is the measured noise curve, and over in here, this line is the fit to shot noise and thermal noise, this line here -- when you add it up, fits all this in here -- is the seismic noise. And the only discrepancy we have is about a factor of two in this region here. It's down by about a factor of two. So our calculation of the shot noise seems perfect, our calculation of the seismic noise ... also seems perfect, and our calculation of the thermal noise is within a factor of two. The thermal noise is very hard to do a perfect calculation [for] because the problem is that you have a device that has finite dimensions and it has surfaces and there's different modes that get set up in here, and you sum over these modes and the calculation that was done is the sum of a bunch of things and you're not sure that you've done each term right because they're geometric and ... we know we can't calculate that to five or ten percent. So there's somewhat of a difference.

Now you can argue that maybe we're not really measuring the thermal noise or it's something else that's bothering us in that region, and I can't deny it. However, our belief is that we're probably not quite as good at calculating in that particular region as we think we are. {Thus at this point Barish is agreeing that there is

a region where the measured noise is a factor of two greater than the calculated noise, but he is pretty sure it is because they have not done the calculation right.}

Now, there's nothing else here. Even the little spikes in here are all explainable. [They come from resonances in the pendulum support wires.] You calculate those and you get the line that goes through here which fits the data precisely. So this is all pretty well analysed, I mean as well as most things get analysed.

Now, let me tell you one other thing. This [sensitivity] is [about a 10^{-19}] meters displacement. To get to LIGO sensitivity you don't have to do [two] orders of magnitude better than this [in noise reduction]. [If] You do this well [ie, as well as the 40 meter] over a longer baseline you gain because of the baseline. This is already at the level you have to do in LIGO, you just have to do it over a longer baseline -- which is what I call engineering. This displacement sensitivity is what we asked for in LIGO -- [10^{-19}] meters. [[Collins: 40 compared to 4000 ...]] Yeah we gain a factor of a [hundred] giving 10^{-21} giving this curve for displacement sensitivity and then we ... OK? {The 4 kilometre LIGO interferometers are designed to have a displacement sensitivity of 10^{-21} .}

So this is already at the level [required in terms of noise elimination] and there is no unknown source -- there's maybe a factor of two, but nothing that is a factor of 20.

ASIDE TO THE READER -- READ THIS CAREFULLY BEFORE GOING ON. IF YOU HAVE UNDERSTOOD THE TECHNICAL PART OF THIS BOOK YOU SHOULD HAVE BEEN ABLE TO FOLLOW THE ARGUMENT SO FAR. LET ME REPEAT THE LAST POINT: BARISH HAS SAID THAT THERE CANNOT BE ANY UNKNOWN NOISE SOURCES THAT WILL PREVENT LIGO GETTING TO A SENSITIVITY OF 10^{-21} BECAUSE TO REACH THAT SENSITIVITY ALL LIGO HAS TO DO IS BEAT DOWN THE NOISE TO THE SAME LEVEL AS HAS ALREADY BEEN ACCOMPLISHED IN THE 40 METER; THE SHEER EXTRA LENGTH OF LIGO WILL DO THE REST.

IF YOU CAN UNDERSTAND THAT YOU MIGHT BE WONDERING WHAT I AM GOING TO SAY TO BARISH NEXT BECAUSE, ON THE FACE OF IT, THIS LAST POINT IS A 'KILLER.' IF ALL LIGO NEEDS TO DO TO REACH THE DESIGN SENSITIVITY IS SCALE UP FROM THE 40 METER WHAT MORE RESEARCH WAS THERE FOR THE 40 METER TO DO? HOW COULD THERE BE ANY ADDITIONAL FATAL NOISE SOURCES. BOB SPERO* AND THE 40 METER TEAM MUST SIMPLY HAVE BEEN WRONG AND MY BELOVED METHODOLOGICAL RELATIVISM IS UNSUSTAINABLE -- AT LEAST BY ME. SO IF YOU THINK YOU HAVE AS MUCH INTERACTIONAL EXPERTISE AS ME YOU MIGHT BE ABLE TO WORK OUT WHAT I SHOULD SAY NEXT. AND IF YOU CAN, IT MAY BE THAT MY LONG SOJOURN WITH THE GRAVITATIONAL WAVE PHYSICISTS WAS NOT REALLY NECESSARY.

IF YOU CANNOT ANTICIPATE WHAT I WILL SAY NEXT, OR ANY EQUALLY SATISFACTORY ALTERNATIVE, IT SHOWS THAT YOU HAVE LESS INTERACTIONAL EXPERTISE THAN ME IN SPITE OF THE FEELING -- THAT I HOPE I HAVE ENGENDERED IN YOU BY MY CLEAR EXPOSITION OF THE SCIENCE IN THIS BOOK -- THAT YOU ACTUALLY KNOW AS MUCH AS ME. SO HAVE A THINK ABOUT IT AND SEE IF YOU CAN SEE HOW ONE MIGHT CONTINUE THE ARGUMENT.

Collins: What configuration was this interferometer running in? Was it power-recycled at the time?

Barish: No.

Collins: So if you say to get to 10^{-21} you only need the extra length, why do you need to use power-recycling? {Here I am asking Barish whether it is not more than a matter of just scaling up.}

Barish: In order to get the shot noise limit, which is this for us [indicating figure], we need more light in the big interferometer. It's photons per frequency bin, or per second, so to fill up the 4 kilometre you need more light. {This is a technically important point because to reach the sensitivity required, the 4 kilometre interferometer is actually going to have to run in a configuration of the kind that was not fully explored by the 40 meter. This is the 'power-recycled' configuration. I go on to exploit this point.}

Collins: So could there be some source of noise that's configuration dependent?

Barish: Of course.

Collins: Could it be that when you power-recycle [the interferometer] there could be some sorts of noise that aren't represented on this curve?

Barish: But [that's not] fundamental noise. Its noise that has to do with how well we bounce the light around or make a resonance, it's what I call technical noise. I agree there can be technical noise. ... Technical noise is boring, it's hard work, and they weren't very good at it on the 40 meter. Fundamental noise is a different matter.

...

Collins: I'm just bullshitting now, but [could there be] any unexpected non-linear effect that could be the result of putting in the power-recycling mirror or something?

Barish: But that again is what I call technical noise, unless it's quantum mechanical or something.

Collins: Well let it be quantum mechanical then.

Barish: We're not in the region where quantum mechanical -- [... ..] We're not yet in those regions.

Is there something? I won't guarantee to you that we haven't overlooked something but there should be --

Collins: It's the quotes you see. Vogt's quotes in that paper: that when they reassembled the interferometer into more complicated configurations they found noises that they didn't expect -- And they're what you call technical noises?

Barish: They're technical noises, not fundamental noises. ... Beating technical noise can be just as hard but it's not fundamental. It just means you have to do work. If we build the instrument and we run into a noise source that we haven't thought about -- let's say like the one in sapphire that you talked about. Well, hard work isn't going to solve the problem -- you've picked a material that has a certain fundamental noise limitation due to thermo-elastic effects that you aren't going to get below, so that this graph that we drew isn't right -- so it's fundamental noise. If it's not fundamental noise it's subject to just improving the things that limit you. {I think that at this point Barish is being over-generous; in his terms the sapphire problem is not fundamental, it's just something that was missed in the design but could have been foreseen in principle.}

Collins: You see, I'm being devil's advocate because I'm trying to put their viewpoint.

Barish: But it's science not viewpoints. Somebody needs to say 'Look we see ...'

Collins: No but the sociologist deals with viewpoints unfortunately [laughter]

Barish: But if you're going to do sociology of science you have to deal with science.

Collins: But that's where we came in at the beginning. It's very important in a sense not to. It's important for me not to, because I'd make myself look ridiculous {I'm making excuses again}. In other words you can deal with science so long as it's settled science.

Barish: You have to deal with it enough to know whether the debate has any sense.

...

Further Reflections

Now let me draw out some of the questions and ironies upon which this debate turns. Barry starts by telling me that I have not done a good enough job of reading and analysing the physics papers. He's right. I did try to read those physics papers but I found I was not learning much from them. I can pretty well understand what a physics paper is saying on the surface, but a physics paper has three deficiencies as far as the sociologist is concerned. First, there might be a lot of information in what the paper does not say. Just by ignoring certain possibilities the physics paper can say a huge amount. We have seen this with great clarity in the treatment of Joe Weber's later work; much of the force of the later papers by those who did not follow Weber's line is found in the fact that they did not mention him at all. (For example, this is the only kind of 'criticism' there is of his 1996 paper.) In the case of Weber's work the claims were sufficiently big and striking to enable us to understand the significance of the silences in other papers, but when we come to debates that involve fine details it is a different matter. To read the silences properly in a detailed debate such as the one about noise you have to know the

background physics in a way that comes from a level of immersion in the community that can be gained only by contributing to the science.

Second, and this is simply the complement of the point just made, the important things a paper does say may be said in such an enigmatic way that only a skilled physicist will see them. Again, really deep immersion is needed to get this from the papers.

Third, the physics paper is written in a certain stylistic convention that is designed to hide the work that went into producing it and for that reason is a very unsuitable source for the sociologist, who is interested in work: physics papers are written to look as though all the conclusions that they reach came out of pure and simple scientific reasoning whereas sociologists know (and this has been demonstrated over and over again in this book), that there is much more to it than this. Physics papers are designed to hide the sociology of the physics.

Fourth, what Barry really wants me to get out of reading that series of papers is not what is in them, but what is not in them as a series. This is something like the point made above about single papers. He wants me to understand how little the 40 meter team really learned about interferometer noise and he believes I will be able to see this just by seeing what is missing from the whole series of papers. But to use the papers in this way again requires that I have an extremely good idea of what might be there but isn't and this would require me to be able to imaginatively create a proto-science of noise which I cannot do with confidence. But worse, Barish's reading of the non-existence of interesting noise sources in the series of papers turns on an assumption about publication practices. The 40 meter team confess that they did not publish a lot, but say that this does not mean that they did not discover a lot. They say they were developing an immense understanding of interferometer noise even if they were not writing it down. This is not an unreasonable position. High-energy physicists have a well defined idea of what a 'unit of knowledge' consists of -- a publication -- and this may not fit other areas of physics. Indeed, at the highest ranks of the interferometer community there are notorious non-publishers, such as Rai Weiss. Weiss has published almost nothing of his immense depth of knowledge about interferometry. Furthermore, by definition, published papers cannot contain the rafts of 'tacit knowledge' and the like that the skilled scientist comes to embody through practice in the field, sometimes without fully realising it (Chapter XX). When a field is developing -- when scientists are feeling their way into new experiments -- it would be impossible for them to publish everything they were beginning to understand in a groping way. The contention of the 40 meter team is that the science was still at that early stage.

Barry thinks that my work gives the impression of being anecdotal and journalistic because it is based on discussions with the scientists rather than careful reading of the papers. Maybe it does give that impression! So, to put that right, in the footnote that follows this have sentence will be found a list of papers on noise sources. I have read them all but it has changed nothing. On the contrary, I think it would have been fatal to a proper analysis of the argument to have based it on these papers.

Perhaps more interesting is the question of what I am supposed to know as a sociologist of science. Barry clearly thinks that if only I knew more physics I would see that on this noise matter he is right and Spero* is wrong. Disreputably, I claim that I (a mere sociologist), would make myself ridiculous by (effectively) telling Bob Spero, who has worked for much of his life on interferometry and still works on it at the famous Jet Propulsion Laboratory in Pasadena, that he is wrong about sources of interferometer noise. This is true, but I should have argued that irrespective of how much physics I know, as a sociologist I would still have been trying to reconstruct Spero's position rather than reach a conclusion about the physics.

But now it gets complicated and I might be accused of disingenuousness, because I spend a lot of the discussion with Barry arguing, not sociology, but physics. I could not have conducted the discussion without knowing a certain amount of physics and, if I say so myself, I think I make some quite good points one of which I have indicated with the 'aside to the reader.' Barry had claimed that all you needed to get from the 40 meter sensitivity to LIGO's sensitivity, was more length; I said that this couldn't be right because, if it was, they would have no need for the power-recycled configuration. This was an especially good point because the unknown noise sources that Vogt (and Spero*) had claimed to have been puzzled by, revealed themselves when the 40 meter was assembled in the new configurations. Furthermore, if there are any fundamental and unexplained noise sources yet to be found in LIGO, this is the place they are most likely to come from {that's me making a physics argument}. So, in order to play devil's advocate I had to understand the difference between various interferometer configurations and to think pretty fast 'on my feet' about them and their consequences.

Ironically, this means that Barry is right in the sense that to do this kind of sociology of physics you have to be able to understand some physics. If you have to understand some physics it must surely be better to understand more physics. So how can I counter the claim he makes (above), 'I think you lose some depth ... by not doing a more academic analysis of some of the same stuff and seeing what you can glean from it.'

I cannot counter it. If I knew more physics I would, potentially, be able to write richer sociology of science. Nevertheless, as I have argued above, I don't think the sociology would be much better or much richer. That is my 'get out.' That is how we square the circle -- how we resolve the irony of my demonstrating, by holding my corner in a physics argument, that I don't need to know much physics -- which I manage to do only because I understand some physics.

Thus, to repeat the argument of Chapter 43XX in this context, I have, or aspire to have, 'interactional expertise' in physics. Interactional expertise is enough expertise to be able to interact in an engaging way with my respondents when talking physics. But don't I offer a bit more than this in the conversation set out above? At one point I put Barry Barish on the back foot (or let us say I do for argument's sake). I point out that though he has told me that all LIGO needs to do is scale up, in fact it also has to pioneer a new interferometer configuration and that opens the door to unexplored noise regimes. Isn't this verging on contributory expertise? The

answer is no, and we can use a distinction made earlier to see why. Though my understanding of interferometry is better than my understanding of 'the rest mass of neutrinos' [an example used earlier of something I do not understand], I am still not in a position to act on it. The leadership of LIGO has to decide what to do next under conditions of some uncertainty. If we take my intervention to be as clever as I like to think it was, what it reveals is what we all should have known all along, that Barish cannot be quite as certain about what is going to happen as he sometimes makes out. But deep understanding means knowing what to do next in virtue of that understanding in spite of the absence of complete certainty. My expertise, good interactional expertise though it is (or let us suppose it is for argument's sake), does not provide me with assurance about how to act next; I am just talking (interacting), with no responsibility. Barish's deeper understanding is what tells him how to act -- that is what contributory expertise is like. Contributory expertise entitles you to make scientific judgements whereas interactional expertise does not. Irrespective of any flea-bites I might come up with, Barry's scientific judgement -- the judgement he will act on -- is that there are no unknown fundamental noise sources to be found in LIGO. In spite of appearances, and what seems to me a fairly creditable performance, I am not contradicting him at this level. At best, I am merely defending someone else's point of view. This does not carry the responsibility of setting a new course of action; nothing turns on what I think.