They give you the keys and say ‘drive it!’ Managers, referred expertise, and other expertises

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Abstract

On the face of it, the directors of new large scientific projects have an impossible task. They have to make technical decisions about sciences in which they have never made a research contribution—sciences in which they have no contributory expertise. Furthermore, these decisions must be accepted and respected by the scientists who are making research contributions. The problem is discussed in two interviews conducted with two directors of large scientific projects. The paradox is resolved for the managers by their use of interactional and referred expertise. The same analysis might be applicable to management in general. An Appendix, co-authored with Jeff Shrager, compares the notion of referred expertise with contributory expertise.

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1. Introduction

What kind of expertise do managers need? Clearly they need management expertise, which is about matters of finance, organization, human resources and, in the case of commercial firms, making, advertising, buying and selling. There remains, however, an unresolved question about how much a manager needs to know about the products sold by his or her firm. On the one hand there is the tradition (perhaps mythical) of the manager working his or her way up from the shop floor so that every aspect of the firm’s work is known at first hand. On the other hand, there is the MBA approach, in which management is seen as a set of portable skills, applicable to any firm independent of the particular product or service.

Here we discuss the directing of big scientific projects which, though it has its own set of special requirements,
might also carry lessons for management in general. The thing that is probably most different about science is the moral relationship between manager and ‘workers’. Some of the ‘workers’ will be more distinguished as scientists than those who manage them. While to be the CEO is the natural ambition of every smart new recruit to a firm there is, among some scientists, a suspicion that managing science is not quite as good as ‘doing’ science and some distinguished scientists will spend their careers trying to duck management roles even if it means sacrificing pay and power. One of the hardest things for the new manager of a big science project is, then, to win the respect of that group who consider scientific brilliance to count above all else. Without respect the manager may not get all from the scientists that there is to be given. Worse, since science managers are drawn from the ranks of scientists, they cleave to the same academic traditions and values. Fortunately, every scientist will come to cherish a manager who creates the conditions for scientific success by guiding their project wisely. Not for nothing are the directors of big projects among those considered for the Nobel Prize when such a big project leads to a major discovery.

1.1. The interviews and the interviewees

The two interviews presented below are with two highly accomplished science managers directing projects with budgets of hundreds of millions of dollars. What is special about these interviews is that they were carried out not long after the directors took over responsibility for projects whose basic science was new to them. Gary Sanders did his Ph.D., wrote his first research papers, and underwent his management ‘apprenticeship’, in high energy physics. He then spent about ten years as the Project Manager—the second in command—of the Laser Interferometer Gravitational-Wave Observatory (LIGO), as it transformed itself from a small science project to a $300M instrument (Collins, 2004a). In 2004, about eighteen months before the interview, he became the Director of the Thirty Meter Telescope (TMT) project, potentially a $700M enterprise.

Like Sanders, Jay Marx’s induction into science at all levels was in the setting of high-energy physics. Marx did have experience managing a big project in a field other than his own as the Director for the construction of the Advanced Light Source, a world-leading facility which produced a beam of X-rays for research in biology, materials science, chemistry and other sciences. In March 2006, just a week before the interview, Marx was appointed as the Executive Director of LIGO, though, as can be seen, he had been involved in LIGO management decisions for some time already.

The interviews turned on the way Sanders and Marx used their expertises to make the decisions that their new roles demanded in light of the fact that they had little or no time to master the science they were managing. Marx, a high-energy physicist had to make decisions to do with the building of interferometers—a completely different science from those he had encountered—while Sanders, who was also an expert in high-energy physics, and had learned something about interferometry—had to make decisions about telescopes, again a completely different science from those he knew anything about. In short, both men had small to vanishing contributory expertise in the respective fields that they were being asked to manage. As we will go on to argue, to do their job properly there may well be no need for them ever to master the science they are managing after the fashion of a full-blown expert—indeed, given the multiple specialisms within these sciences, it would be impossible for them to master all of them.

The main topic of these interviews is the role of ‘referred expertise’ and ‘interactional expertise’ in decision-making, and how these blended with any fragments of existing ‘contributory expertise’ that could be directly applied to the new setting. Interactional expertise and contributory expertise are described and discussed in other contributions to this special issue and in a variety of other places. Referred expertise, on the other hand, has not been discussed much outside the ‘Periodic Table of Expertises’. The term, ‘referred expertise’, draws on the notion of referred pain. Referred pain is felt when, say, an injury to the back gives rise to pain in the leg. In the case of referred expertise, skills that have been learned in one scientific area are indirectly applied to another. What is meant by ‘indirectly’? ‘Directly’ means that some technical skill such as solving differential equations (we will see real examples in the interviews below), is just as relevant for the new scientific area as...
the old one—the same thing as was done in the old location—as far as the solving is concerned the location might not have changed. 'Indirect application' means some more diffuse body of experience is applied to a set of technical activities very different to those within which it was mastered.4

1.2. The dark matter question

The idea of referred expertise emerged as a possible answer to a puzzle: after working with gravitational wave scientists from a decade or so, Collins believed he had developed interactional expertise in the domain. He noted that managers in the area did not possess contributory expertise in gravitational wave physics, at least in so far as they could not invent bits of the instrument or show front-line virtuosity in data analysis or any of the other technical specialties from which the science was built. On the face of it, their expertise seemed mostly interactional—not much different to that of Collins himself. In the spirit of a reductio ad absurdum Collins then asked himself ‘Why shouldn’t I (or someone like me), run LIGO?’ In other words, what do the managers have that someone with interactional expertise alone does not have? Among other possibilities was referred expertise; the managers did have contributory expertise in other domains and it could be that something of that contributory expertise could be referred, indirectly, to the new area.5

Though the nature of these technical expertises is what drives the interviews, the entire transcripts are presented verbatim since they contain many other interesting comments on the nature of the problem facing managers of new sciences and, perhaps, managers in general. These other comments fill out the answer to the question about what a science manager has to do in addition to making technical decisions. At the end of the paper we sum up all the things the managers possess that a purely interactional expert like Collins does not possess, hoping to elucidate ‘the dark matter of management’.

1.3. Ethnomethodology of management or technical analysis

The interviews presented below are not exactly ‘interviews’ in the normal social science sense. In normal (interpretative), interviews the analyst asks a series of questions, or sparks a conversation, which elicits the nature of the actor’s world. The actor’s frame of reference is then reassembled in terms of the analyst’s technical concepts—in this case the concepts would be the three types of expertise in question. In such interviews, only the analyst, along with the referees of any publications, the readers, and the rest of the social science community, has access to the technical concepts. In the interviews presented below, however, especially in the case of Gary Sanders, the relationship was much more symmetrical. As we will see, in the course of the ‘interview’ Sanders helps to develop and elucidate the technical concepts. This was possible because Sanders had been around during the development of the concepts. Collins had known Sanders as the Project Manager of LIGO for around ten years in the course of fieldwork in the gravitational wave physics. To Collins, Sanders was initially an interviewee and respondent, but, as Collins acquired interactional expertise in gravitational wave physics, the relationship became more that of acquaintance or friend—one whom Collins would naturally sit down with over a meal or coffee to chat about the state of the physics research and his role as manager. The ‘dark matter question’ would have been put to Sanders in an exploratory way soon after the idea began to form. Sanders was also present at a seminar at California Institute of Technology which was one of the first places where this initial classification of types of expertise was put forward in public. In that presentation, as in one or two others at which Sanders was in the audience, Collins used an interaction with Sanders as a principal example of interactional expertise: in Kyoto Collins had explained to Sanders a technical aspect of a Tokyo-based detector in sufficient detail to convince him that his belief about one of its sources of noise—internal reflections from the beam tube—was incorrect. In these seminars Sanders readily accepted the point of the illustration. Therefore it is not so remarkable that the ‘interview’ between Sanders and Collins is more of a joint exploration of the sociological concepts. This, perhaps, raises an interesting question about ethnomethodology—the study of the methods by which ordinary people make sense of their world. In this case technical concepts drawn from social science are being developed and becoming part and parcel of the world of the ‘ordinary people’ (here the ‘ordinary’ people are scientists) a reversal of the usual order of things.6

Marx was also aware of the expertise classification since he had read Collins’s (2004a) book on the history and sociology of the gravitational wave field in the course of the process of recruitment to the Directorship of LIGO. Marx, however, had not been involved in any extended discussion of the ideas and has not contributed to the development of the ideas in the same way.

4 See the Appendix to this paper for further refinement of this issue.
5 For first discussion of this point see Collins (2004a), pp. 777–778. It goes almost without saying that the ideas of interactional expertise and referred expertise are quite outside the monolithic framework of Dreyfus & Dreyfus (1986). These expertises, which essentially do not turn on practical involvement, cannot be counted as expertises at all under the Dreyfus framework. For a critique of Dreyfus that points to the various other kinds of expertise excluded see Selinger & Crease (2006). Selinger and Crease’s critique, unfortunately, sometimes strays into the realm of attributional models of expertise (Collins & Evans, 2007, pp. 2–3).
6 In the course of the project other physicists too became interested in the project and helped to develop some of the sociological ideas.
1.4. The projects

There is an important difference between the two interviews that has to do with the nature of the respective projects. Sanders came in ‘on the ground floor’ of an enterprise where all the big decisions still had to be made and nothing had been built. Marx, on the other hand, took over a project in which most of the big decisions had been made and many were already expressed in hardware and buildings. The first generation instrument (or perhaps it should be called ‘the prototype’) — Initial LIGO — was already up and running. Marx’s most important job was to make sure that the already almost completely designed second instrument — Advanced LIGO (or AdLIGO) — received the $200M or so that Washington was teetering on the brink of giving it. Furthermore, Marx was taking over an existing team with a number of experienced leaders who had over-seen the development of different dimension of the project over two decades, and a very accomplished deputy, Stan Whitcomb, who had taken on the role of Acting Director for the transitional period and had himself been a strong candidate for the Directorship. We may be sure that Whitcomb was able to steer Marx through the initial stages. There are, then, two reasons why Marx’s interview tends to stress political decisions rather than technical decisions: First we are much earlier in the managerial transition with Marx and there had been less time for him to learn the technicalities. These technicalities were, in any case, known ‘inside out’ by Whitcomb and others. Second, we are at a much later stage in project maturity with Marx; in so far as there were pressing problems they were political rather than technical (with the one exception that will be discussed in the later part of the interview).

This is not to say that Marx did not immediately make his mark on the project, this mark being to emphasise the importance of AdLIGO in relation to Initial LIGO whenever tensions arose over distribution of time, effort, and other resources. With Initial LIGO reaching its design sensitivity just around the time that Marx took over, there was a chance that the opportunity to make the first detection of a gravitational wave would divert attention from what, it might be argued, was the true long term goal of the project. This was speedily to construct a more sensitive device that could see not just the one signal needed to count as ‘a discovery’, but the many and frequent signals needed to found gravitational wave astronomy.\(^7\)

Marx, on reading this description of differences between the two projects, stressed that there was a still deeper distinction to be made in managerial roles. He wrote (e-mail of 10 August):

The balance between technical and non-technical decisions is very different in the two projects that are discussed in this paper. Gary [Sanders] makes many technical decisions and I make many fewer.

Gary is leading the design of a new facility. This involves many, many design decisions and choices between technical alternatives. That is the heart of what Gary is doing (besides putting together a team, managing it, and developing and implementing a strategy to get the telescope funded).

My job is to direct a scientific laboratory and an ongoing scientific research program. And so my job involves decisions about resources, use of people, hiring, funding strategies, policy, how to do the collaborative science, and dealing with the public, funding agencies, the scientific community and the political community. There are very few technical decisions that come to my level. And in such cases my job is to be sure that the right decision is made but in such a way as to keep the collaborative environment in LIGO whole. So the process matters a lot.

Although Advanced LIGO is a big project in its own right, I have appointed a project director, a project manager and the rest of a project team. I will oversee their work and watch what is going on, but the technical decisions have to be made by that team within their organizational structure. The Advanced LIGO project director has the responsibility for the project succeeding. My responsibility is to be sure that he is succeeding.

This difference in role again means that most of the development of the concepts that refer to technical management are to be found in the first interview — that with Sanders — while Marx’s interview, though it does contain material relevant to the technical points, mainly rounds out the other aspects of management.

1.5. The technologies

Preceding the appointment of Marx, the last big decisions made by the LIGO team concerned aspects of the design of AdLIGO. An almost interminable debate had taken place over whether AdLIGO should have its mirrors made from sapphire or fused silica. Sapphire at first seemed to promise a much better performance but it meant growing faultless crystals of sapphire larger than had ever been grown before (boules of around 120 kilograms from which mirrors weighing 40 kg would be cut and polished), and it meant exploring the properties of the rival materials in great detail (see, for example, Collins, 2001). During the long process of studying and measuring, new problems revealed themselves in sapphire while the less glamorous fused silica showed more and more promise. Eventually the decision was made that a marginal improvement in performance was not worth the risks and trouble associated

\(^7\) This position is developed in Collins (2004a) but is not favoured by some of the pioneers of the LIGO project who believe more effort should be put into making sure that Initial LIGO makes the first discovery.
with sapphire especially as there were regions of the spectrum where fused silica would do better in any case.

A more fraught and heated debate concerned the way the major parts of the AdLIGO interferometer were to be isolated from external noise. Here the protagonists were the ‘stiff’, or active, suspension—a complex feedback system of hydraulic micro-actuators linked to sensing circuits—against the ‘soft’, or passive, suspension—large and soft cantilever springs which acted largely as passive dampers with only a few sensors and feedbacks. Here the champion of the soft suspension was a maverick physicist and engineer whose style of work, while inventive, did not fit naturally into the ordered timetables and systematic milestones of big science. In the end the stiff suspensions won but the champion of the springs has never given up his advocacy and his approach has found favour with some non-American interferometer projects. Marx was to discover, then, that the hard/soft suspension debate was not quite finished when he took over and as a result he had to make the first technical decision of his tenure.

We will get a good sense of the decisions facing Sanders in his interview but one technicality needs explaining. Modern ground-based telescopes use ‘adaptive optics’. What this means is that one or more of the telescope mirrors is deformable under computer control and can be made to compensate for differential changes in diffractive index of parts of the atmosphere. To put this in plainer language, the stars twinkle when viewed from the ground because their light passes through an atmosphere full of regions in which temperature and pressure vary rapidly in small ways and this affects the passage of the light. If the changes can be sensed, and the deformable elements of the mirrors moved to compensate as fast as they happen, the twinkle can be removed and the telescope will have a clearer view. Working out how to remove the twinkle is mind-bogglingly complicated and, as Sanders soon discovered, there are two competing methods for such large, wide-field telescopes. One method, ‘multi-conjugative adaptive optics’ fires laser beams into the sky and watches what happens to the ‘artificial stars’ before trying to compensate the entire visual field and it uses the corrected image to assure success. The other method, ‘multi-object adaptive optics’, starts by looking at these artificial stars and very quickly corrects only selected and interesting patches in the image field without measuring how the correction has succeeded.

In all cases of big science, the way difficult decisions are made is to delegate groups, with project leaders, to study the problems, organize measurements, and come together from time to time to report progress and debate the issues. Once as much information as possible has been gathered, the management team makes the decision. With luck the right decision will emerge ‘naturally’ as more and more information is gathered but often this does not happen. Sometimes the timetable of the project as a whole demands a decision before the arguments have reached a satisfactory level of persuasiveness. Sometimes the protagonists of some approaches will find themselves unable to agree with the position reached by the majority. Sometimes the management team will disagree with the majority. In the case of the stiff versus soft suspensions for LIGO, the losing team never accepted for a moment that they were wrong.

1.6. The interviews

In the interviews that follow, all indented passages are transcripts while non-indented passages are paraphrases. In the case of Marx’s interview the indented passages were edited by him after the interview though adjustment were made of style and clarity only with occasional additions of new information—there are no changes of substance. Nevertheless, this is why Marx’s interview turns appear less ‘conversational’ than in most interview transcripts. Sanders interview has been hardly edited at all. Curly brackets indicate a commentary added later, the author of the remark in curly brackets being indicated by initials. Given that opportunities to conduct interviews like this are rare, the choice has been made not to edit the materials on those few occasions where repetition or redundancy could have been eliminated.

2. Interview with Gary Sanders, Laguna Beach, 22 October 2005, late afternoon

COLLINS: A good way to explore [what we are trying to get at would be to ask about] moments when you were nervous about whether the things you knew could be transferred to the new project.

SANDERS: There’s a couple of areas: things that have to do with telescopes and astronomy, in which I was ignorant, and the other has to do with things that have to do with managing the big science project about which I was not ignorant, but I was ignorant about the reactions of this community.

Let me also say I cannot remember a time since I’ve been on TMT [Thirty Meter Telescope] that I was nervous about my expertise. I’m very comfortable in knowing what I know and knowing what I don’t know, and I’m comfortable in using what you call referred expertise and throwing out something and listening to reaction, and testing whether I’ve trod on ground that there’s something I don’t know that I ought to know, and seeing if there’s feedback or pushback. But confident that whatever I’ve put out, based on what I am expert in, is a good valid starting point.

When I came to the job I knew very little about telescopes. I did not know what a Cassegrain telescope was, what a Gregorian telescope is, what a Schmidt telescope is. I could not describe what an alt-az mount is as opposed to an equatorial mount, and so on. And so one of the first things I did was go find the two or three books which are the equivalent of Peter Saulson’s book in gravitational waves [a standard reference for gravitational wave detection which the sociologist-author of this
that there is ‘a’ design that is somewhat unique. to make that design succeed as opposed to some ideal techniques that will meet the requirements your job is I’ve always said to people that once you find a set of were always partly right and always partly wrong. And comments, and they were from very fine scientists, and they Fabry-Perot interferometers—I heard all these argu- people had delay-line interferometers and others had Argon-Ion lasers—I heard all these arguments—le LIGO, I already came from a field where people swore ways of doing it that were probably pretty good as well. statements that they had made. And then use my expertise, expertise, whatever it is I had to try and understand the tradeoffs that people had used—and the value judg-ments that they had made. And then use my expertise, because, ultimately, I had to decide.

One of the first jobs that had to be done, technically, was the project had three teams, who had been the designers of three precursor telescope projects. And they were all trying to build roughly 30m telescopes, they all had the same science goals, but if you looked at the designs, they were quite different. That told me that different designs could be used to accomplish the same goals, so that when one of the experts swore that their approach was the only valid approach, or by far the superior approach, I automatically had two counter-examples that were pretty good, done by pretty good groups of experts, and I also knew from my reading that the people who had done these designs really were pretty experienced experts in the telescope field; they had lived through the design and building of previous large scale telescopes or the instruments that use them, and they were well known—experts.—So I knew there was a diversity of technique and if I thought, because of referred expertise, that the diversity of different techniques could all be combined in ways that met the goals of the project in different ways, I was confident because I had three clear examples. That put me in a position of using my expertise, whatever it is I had to try and understand the tradeoffs that people had used—and the value judgements that they had made. And then use my expertise, because, ultimately, I had to decide.

COLLINS: Let me just go back a step. Presumably the experience of having people come up to you and say ‘my way is the only decent way’—you would have heard those things before in other projects and you would also have encountered the fact that other people had other ways of doing it that were probably pretty good as well.

SANDERS: Right, I already came as you know from LIGO, I already came from a field where people swore to me that Argon-Ion lasers were the only way to do it and other people said ‘solid-state lasers’ and some people had delay-line interferometers and others had Fabry-Perot interferometers—I heard all these arguments, and they were from very fine scientists, and they were always partly right and always partly wrong. And I’ve always said to people that once you find a set of techniques that will meet the requirements your job is to make that design succeed as opposed to some ideal that there is ‘a’ design that is somewhat unique.

Nevertheless I imposed my own judgments, some of which were based on referred expertise—I’ll give you some examples. So in the course of picking the specific design that we were going to use – combining the elements of this—I had to make the final choices—I’ll give you an example . . . The primary mirror is a conic section and looks up: light comes in, bounces off the primary mirror, and goes up to a focus. And that’s where the secondary mirror is and the secondary mirror could be either convex or concave. And there were arguments from the precursor teams about which was better, and the convex version would make the total length of the telescope a meter-and-a-half shorter on a telescope that’s 30 or 40 meters long, which meant the enclosure of the telescope would be a little cheaper—a million or two dollars cheaper. The concave mirror was something I knew from LIGO—referred expertise—could be optically tested in a simple way, because you can take a concave mirror with [its] well defined focus and set up your instrument and do an optical measurement, whereas for a convex you have to do a set of measurements and then stitch them together which has some errors. I knew that from LIGO—I knew enough about metrology—and I knew that—everyone in the telescope business said that optical testing of these mirrors was one of the really hard parts. I made the judgment based on what I knew from LIGO, based on a prejudice, that I wanted to have simple optical testing and I wanted to be able to test while the mirror was in the telescope, which meant craning in a camera to sit at a certain focus and doing a test, as opposed to having to remove it and go to a special set-up. So, on the basis of what I learned in LIGO, and a prejudice about ease of calibration and testing, I made a judgement that we were going to go with the Gregorian or concave, even though we had experts saying that it is possible to do the optical testing with the convex.

Another technical judgement: After the light has bounced back down by the secondary towards the primary, there are two styles of telescope. In one there’s a hole in the primary and the light is directed through the hole and the instruments are hung under the telescope in rather confined spaces. In the other, as it comes down to the primary mirror there’s actually a tertiary mirror which kicks the image out to the side where you can put instruments on platforms on the side and have a lot more space—called Nasmyth platforms. There were arguments that it was optically quieter and better shielded down below the telescope but I also knew—this is referred expertise in my case—or judgment—based on working in high energy physics where people build accelerators and they have to be used for 30 years, and they’re going to bring instruments and detectors in and out and they need the flexibility in the system—the ability to respond to new ideas. I looked at the design where instruments had to fit under the telescope, where there’s drives and bearings and structures and so on, and the ones where there’s these big
platforms on the side, and made the judgment that we were going to go with the Nasmyth platforms because I felt that if you were going to spend N million dollars on a telescope that was going to be used for 50 years, you needed a robust and flexible way to accommodate instruments that might look very different over the decades. I didn’t want to build something which 30 years from now, someone was trying to figure out how to stuff an instrument under the telescope in a confined space. I wanted to give design freedom. And that prejudice came from my experience in high energy physics where we built accelerators and made sure we had accelerator floors that were flexible where we brought the beams out and where we could react to new kinds of ideas. It’s a judgment that I made that would result in a more useful, over its fifty year lifetime, more flexible facility. Is that referred expertise or is that just taste or judgment?

COLLINS: I think that’s referred expertise. The first example you gave, I was thinking wasn’t really referred expertise because it’s too direct an application.

SANDERS: It’s too close—we’re talking about optical testing.

COLLINS: It’s something you already knew.

SANDERS: I actually knew something about optical testing.

COLLINS: It wasn’t referred expertise from one field to another, it was just you taking what you knew and putting it there. The second case is a much more general—

SANDERS: A much more general experience of flexibility in another field warned me that in a high cost investment like this ‘let’s make sure we don’t design new instruments out of the system.’ ‘Let’s make it flexible and robust.’ So that’s referred expertise—OK? Let me think of other examples:

So I found myself making key design decisions, not really knowing the history, the lore, the tradition, the lessons learned in the telescope. Even though people were telling me I also learned that when a bunch of scientists come and give me competing options I’ve learned to listen and make my own judgment. There are no stone tablets with ‘the’ right answer.

(In the last few paragraphs we have encountered three examples of applications of expertise. The first, reiterated in the very last paragraph above, is expertise about how to discount technical claims made with great certainty by specialists in narrow domains. The second is expertise in the metrology of mirrors. The third is expertise in the best location in a big device for the siting of instruments.

The second of these is the easiest to deal with. It is the direct application of something learned in one field to another field. It is worth noting, however, that Sanders does not claim to have contributory expertise in the metrology of mirrors—he can’t make the measurements himself—but he does have excellent interactional expertise—he knows what the measurements are all about and can speak about them like an expert. So the second of these expertises is the direct application of an interactional expertise acquired in another field.

The first of the expertises, on the other hand, seems to be as near as we can get to an ideal type of referred expertise. Because of his contributory experience in other sciences (backed up by his managerial experience turning on interactional expertise in these other sciences), Sanders knows that statements made with great apparent authority by narrow domain experts are not always what they seem. As a scientist he will know the true level of certainty of this kind of claim, and as he explains in the interview, he knows this because he has often seen such statements countered by others made with equal apparent authority by specialists in other domains. This is referred expertise because it is a matter of taking a diffuse skill learned in one technical area and applying it in another.

The last of the expertises—an understanding of the siting of instruments—could be thought of as a referred expertise or as the direct application of knowledge from another field. Given that one reason Sanders was chosen to run the TMT was so that he could apply his expertise in big science to the more traditional field of astronomy, it is probably easiest to think of it in the latter terms. Sanders knows big scientific installations and here is applying this knowledge to this area. Of course, to apply it to the new area with confidence he needed the referred expertise of discounting confident statements about why the Nasmyth platforms would not work. HMC)

COLLINS: So presumably a counter-argument to this flexible [Nasmyth platform] one is that you’re gonna have to have a third optic, which is another set of potential distortions and all that sort of thing.

SANDERS: Right, so there a set of arguments about throughput—although even an instrument slung under the telescope has to have something to match in, so the number of optics in the optical train [is the same]. In the end the argument carried the day and there were people in the project before . . . that were pushing that argument. There were others who held the other view. And they came to me with very strong views. ‘Those guys who put them up on Nasmyth platforms, they’re more subject to vibration; the instruments are above the primary mirror so they can see emission in infra-red light that will cause background to the instrument—by hiding it under the telescope you can optically shield it’—There were all kinds of arguments—OK? But in the end, from the big accelerators, I said, I’m not going to build a

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8 For the idea that distance from the front-line of scientific research lends spurious certainty to scientific findings (distance lends enchantment), see Collins 1992 [1985].
facility where I’m hamstrung. Like I thought that was a judgement I was importing from another field.

{In the above two turns we see Collins trying and failing to make an innovative technical point about the need for an extra optic. Collins, of course, has no kind of expertise in telescopes so does not realize that even though the Nasmyth platform design requires an extra optic, the other design needs something similar. Here, then, we see a failure of interactional expertise which, when it is corrected by Sanders, could be an illustration of a tiny fraction of the process of learning it.

Interestingly, Collins had made another such mistake earlier on. In the introductory section of the paper, the idea of adaptive optics is explained. In the first draft of the paper Collins wrote the explanation himself, basing it on what he remembered from the conversation that preceded the interview with Sanders. It turns out his version was quite wrong and Sanders corrected it when he returned the draft, quipping that Collins hadn’t yet mastered the interactional expertise of adaptive optics.}

 COLLINS: Another element of the referred expertise is that even though some of these people said there are going to be really serious problems if you do it that way, you knew how to discount some of what they were saying.

 SANDERS: I knew that if they were worried about wind shake from being up on the Nasmyth platforms it was a mechanical engineering, an aerodynamics problem—a little stiffer—it could be designed out. If we’re talking about being out in the open and being exposed to more light that could be scattered into the instruments and could cause some background it was a matter of optical design—of cooling some optics—of shielding things—it’s a design issue—I knew there wasn’t anything fundamental about it. But it did impose some requirements on the design. That also is a kind of referred expertise—you learn when you’re in a regime where a situation is a design issue, it’s an issue—you just work on it.

Are there other examples? The observing style. I already mentioned earlier in the day the notion where it’s classical observing where, ‘It’s my night—I’ll do with what I want,’ versus queue observing. Having come from fields, like high-energy physics, where you don’t get days of beam without proposals, ranking, priorities—there’s an ethical imperative to give the time away on the facility to the most meritorious science, modulo, readiness and conditions. And the idea that: ‘I’m brilliant—give me the telescope for a night and I’ll do what I want’, [is unheard of]. It is a design issue because we are going to design the computing systems to handle the queue of pre-prioritised observations. If I really believed in classical observing in the tweed jacket, I wouldn’t design any of the software and the systems we need. We’d just have a committee to give people nights and we would show up and the observing assistant would say ‘master—what would you like me to point at tonight.’ But we are going to spend a considerable amount of money building a system where we store in data bases the observations a person wants, the conditions they need, we sample the conditions, we look at the priorities and in a automatic way we make the judgments what we’re going to do tonight and when to switch from one to another as conditions vary. That’s a design choice. And it’s something I brought from other fields in doing research in really big, expensive facilities. You just cannot justify not having that capability.

{This again seems to be the direct application of Sanders’s knowledge of how big science should work. HMC}

 COLLINS: Let me just step back again. As we know, when you first went into LIGO there was resentment from interferometer specialists who did not think you knew anything about interferometry, Did you have the same thing here? Did you fear it? Did you encounter it?

Sanders says that he experienced no resentment in the TMT because they had decided that they wanted the project done in the big science way and they chose him to do it—unlike the LIGO experience. Since Collins did no interviews with the TMT scientists, who might have harboured hidden resentments, this line of inquiry will not be pursued here. We do discuss the possibility that the shift from small science to big science organization could be a learning experience for the scientific community as a whole, which is discovering that there is a big science model and that it can deliver certain goods. Sanders also says that in astronomy there are end-to-end entrenched tribal approaches that are not found in LIGO.

 COLLINS: Let us go back to anything you felt you couldn’t handle in virtue of the fact that you’ve changed fields.

 SANDERS: Areas that I am technically tentative about?—I said I was never worried but I guess I have to change that. I was afraid that I would not understand adaptive optics well enough. This whole art of sensing the wavefront distortion and doing the mathematics to unfold the correct corrections, is an extremely arcane field. The people who do it are actually mathematicians. And they really are gurus. In LIGO it’s a combination of the [mentions names of knowledgeable LIGO scientists] and I was just concerned that I would not understand it well enough. But I’ve found that, remarkably, what you call interactional expertise was not hard to achieve. I couldn’t design an adaptive optics system but I really do after six to nine months in the field, I really do understand the different kinds of adaptive optics in the field and the way that they work and I can draw a schematic and define the algorithm, and understand the technological readiness of the different techniques—which ones are really ready to apply to the sky and which ones need to be demonstrated, and
certain components have to be developed. The interesting thing is that in retrospect it was easier than I thought to understand it because of what I learned in LIGO about optics and control systems, which I never learned in high energy physics. So in some sense I would say that I was able to apply my expertise more easily to the most arcane part of the telescope business because I had picked up a broader palate of technical expertise by having been in more than one field.

From high-energy physics I brought certain things; from LIGO I brought different expertises, and having that broader palette made a difference. That still is referred expertise—you know, I understood control systems better so I actually found I could understand adaptive optics better. It’s a new set of problems so I made judgments about which adaptive optics algorithms we were going to use at first light and which ones we were not.

COLLINS: You said something very interesting in the middle of that. You said you could not actually design the thing but nevertheless you had the interactional expertise to sit down and argue through—

SANDERS:—I can sit down in a group of adaptive optics experts who will come to me and say ‘Gary you’re wrong, multi-object adaptive optics will be ready at first light and it will give the following advantages—and others will say, ‘no, it’s multi-conjugative adaptive optics’ and I can give them four reasons why we should go with multi-conjugative adaptive optics based on the kind of science we want to do, the readiness of the technical components, when we need them, and so on. And I will see as I am talking about it that the room is looking back at me and they’re saying ‘he does have a line, he’s thought it through, and yes.’

But if someone said to me, ‘OK Sanders, we agree with you, now go and design a multi-conjugative adaptive optics system,’ I couldn’t do it. I couldn’t sit down and write out the equations—But I can draw a diagram of what each part does, where the logical readiness of each one is, what the hard parts are—I know the language. I actually feel qualified to make the decisions. That’s on the border of contributory expertise.

COLLINS: It’s on the border of contributory and interactional—yeh!

{At this point we encounter a problem that haunts the distinction between contributory and interactional expertise but should not be treated as too scary. The point is that people like Sanders and Marx who, let us say for argument’s sake, possess only interactional expertise in the technicalities of the fields they manage, also make contributions to those fields. Why are they not contributory experts just like other specialist contributory experts? All we have to say is that their specialism is ‘directing’, as opposed to, say, solving adaptive-optics equations, or designing multi-stage mirror suspensions, and we have it. This problem was pointed out by Simon Cole in relationship to the way his social science expertise bore on fingerprinting, and on many occasions Collins has been told that in spite of his protestations that he is ‘not a physicist’ he really is.

We simply have to accept that someone who is an interactional expert looked at through one lens can be seen as a contributory expert when the level of magnification is changed. On the one hand, all the specialists who are involved in, say, the LIGO project, from mirror-polishers to astrophysics modelers, are contributory experts in gravitational wave detection. Maybe Collins is too! But to leave it at that is to miss the point that the reason they can all be contributory experts in gravitational wave detection—the reason there can even be a LIGO project—is that they are interactional experts in each others specialities. If there were no such thing as interactional expertise then the LIGO project would simply fall apart into its constituent contributory specialist groups. If the fact that from some perspectives an interactional expert looks like a contributory expert is taken as fatal to the classification then a wealth of explanatory power is lost. But the fact also is that the terms can be used with clarity and without ambiguity so long as they are used carefully, and then they do seem to have explanatory power.

It is a shame that the Periodic Table of Expertises differs from the periodic table of the elements in that carbon remains carbon if you look at it from near or far or from straight on or sideways whereas an interactional expertise can become contributory expertise when the perspective changes. It is, then, a different kind of classification to the periodic table. There certainly are other kinds of classification. Thus diamond and graphite are both carbon when looked at another way but that does not seem to scare anyone away from talking about the difference between them.

SANDERS: So it’s a little bit different to someone who’s answering a questionnaire who’s trying to mimic the experts in the field because he’s learned the language, the vocabulary, and the concepts, enough to interact. I actually am in a position where even in LIGO I can’t design the LIGO interferometer. I can’t sit down and write down all the transfer functions and work out the noise budget in the way a [mentions the name of an interferometer scientist] can. But if he gave a talk on it I could follow the thread. I can understand the important parts and the hard parts, partly by listening and partly by quantitatively understanding, but I couldn’t come back and compose the symphony. But, I was in a position where I had to decide. So it’s a matter of who I listen to and which parts seem like they carry the argument—what it is that we want. That’s more than interactional but it’s not quite contributory in, I think, your usual sense of the word.

9 Collins & Evans (2007), pp. 70–76.
(That for someone with interactional expertise it is possible to follow an argument in detail but not necessarily possible to compose it, is a common experience. HMC)

COLLINS: Well, one of my arguments is that if you get enough interactional expertise, you could in principle contribute to decision-making just in the way you’re saying.

(Here, momentarily, Sanders fails to give enough credit to the power of interactional expertise: it is not just about answering questionnaires—it is about mastering the tacit knowledge associated with all that can be said about an area. HMC)

SANDERS: And if you’re like me, who tends to be brought in at the top of the organization with a certain amount of decision-making power, you naturally face that. They give you the keys to the Thirty Meter Telescope on Day One and say ‘drive it.’

COLLINS: So the way it works out is if you get enough interactional expertise you wind up making contributions, but that’s still not the same as having contributory expertise in the sense of being actually able to ‘do the thing.’

SANDERS: Yeh, that’s right. . . . I can’t do what [mentions two names of gravitational wave scientists] actually do, but someone put me in charge of them. And I think I did a pretty good job. And what the hell is that? I think you need another word. And this referred expertise thing—there I am trying to develop this interactional expertise by learning the vocabulary, so I know what multi-conjugative adaptive optics is, and I know what a Gregorian secondary is, and I can carry on the conversation and sound like I know what I am talking about, but I’m also trying to learn how to confidently make the decisions, and some of it is based on stuff I’ve learned in other fields. It’s not interactional expertise it’s imported.

COLLINS: What I’m trying to build—what I am trying to say about someone in your situation—is that you’ve got a combination of interactional expertise and referred expertise.

SANDERS: That mix enables me to do what I’m doing.

COLLINS: Yeh, and you can do it with the interactional plus the referred without the contributory that relates to that specific field.

SANDERS: Up to now in this conversation we’re only talking about the technical aspect. But the other part of what I do—is the business of taking a group of people and turning them into a team and delegating—and making them interact in a systems way. And directing them towards goals and milestones and importing it from one field to another—that’s a cultural thing—one field has a certain cultural history and we’re adapted to this (or not)—another field is completely naked and wondering what this is all going to be like. And I bring it in and I start doing it and teaching and coaching by example: Is that referred expertise?

COLLINS: No, that’s contributory expertise. That’s contributory expertise in management.

SANDERS: You are right. You are right, and I guess that is why I was brought into the 30 meter telescope—this guy knows how to manage—he’s going to bring in his true expertise—so it’s not imported [referred], it’s the naked thing.

COLLINS: It’s like a mathematician can be employed in one field and then switch to another and use the same mathematics.

SANDERS:—could apply group theory to several different things.

. . .

COLLINS: That’s very, very rich. Is there anything else you can think of that you should say?

SANDERS: A decision I’m going to face in the next year: We are designing eight instruments. These instruments cost from $10M to $50M each. I’m supposed to hit a cost target of $700M [for instruments plus telescope]. I’m gonna have to decide that the project is only going to deliver one, or two, or three instruments. I have a science advisory committee that is constantly looking at these designs—they gave us science priorities—and assessing whether the designs will meet the science requirements and the relative priority of the science, knowing, by the way, that the science is going to change over the next decade. In the end, the decision over which one, or two, or three instruments is included is mine. I have to justify it to my Board and so on, but I’m going to go through a process and make a decision, and like every decision I make, I have to inform the Board and give them my reasons and assume they will say ‘this was well done.’

I’m not an observing astronomer. I have to listen to the arguments of planets versus galaxy formation versus stellar populations, and this instrument should be a first light instrument and this instrument should be first and the drum-beat has already begun; the campaign for my instrument—I have eight teams designing instruments—to be one of the first light suite. And I have to make a decision. What is the science that we want to do in the first three years? What are the instruments that are probably based on technologies that will be ready. What are the instruments that are of a technical complexity that we can actually commission them and make them work in the first three years and then get science done. It’s a little bit like your question about the 6-year grant and what you can get done in 6 years. It’s a combination of technology readiness, science goals—which are the science goals we want to go after first in the discovery space?—and practical things. Is that referred
expertise? Some of it’s management, some of it’s technical, some of it is based on the kind of insights I got from other fields. But in the end, guess what? The guy who’s never spent a night on a mountain opening the shutter and doing an astronomy observation is gonna be the guy who’s gonna say ‘I selected these two instruments, and these are my reasons.’ What the hell is that?

COLLINS: Well, I dunno—that’s the most mysterious expertise of all.

SANDERS: It’s a mixture of technology, science judgment, and implementation—’Are we ready to implement this one as opposed to that one?’ ‘This one’s going to be harder.’

COLLINS: A big element is what the Greeks used to call ‘wisdom’ [phronesis]. And we can’t do much better than that nowadays [laughter]. You know, in the end it probably emerges from—you get good at it as a result of the experience of making lots of decisions I suspect.

SANDERS: If you go through a career of doing what you are talking about: contributory, interactional, and referred expertise and combining them, and making decisions, in the end, people start bringing you imponderable combinations of these things.

COLLINS: [It’s mysterious but] let’s put it this way: you can think of all the reasons why a person would not be fit to do it. They would be too swayed by some small technical argument and wouldn’t have had the experience to know that it actually could be soluble.

SANDERS: Right: I’m afraid of that.

COLLINS: I’m sure you could sit down and you could think of a lot of ways in which you could make the decision badly.

SANDERS: There’s even judgments I’ve already made on this telescope: ‘that technology is not going to be ready in five years; I believe that.’ And the people who are developing it think I’m a fool.

COLLINS: Another important element of it is knowing what speed to make the decision at, I would guess. How long do you hang on to get enough knowledge to make the decision and when is enough knowledge enough to actually make the decision.

SANDERS: In my experience I push that in three different ways. Sometimes I just know, and I’m quite confident, and I push that out there. I will always listen if there’s a good argument that comes back. I’m not George W. Bush who will doggedly stick with a bad choice. But I’m confident and it doesn’t take me long. There are others where I really will let it Mull for months at a time and go through a process and let it get to a point—I mean the choice in LIGO between the stiff and the soft seismic isolation was not an easy choice—and even by that time I kind of knew a little—but it was not an easy choice. And history has proven that it still is a choice that could be re-dissected. And then there’s a middle ground where it’s not very quick and it’s not very long, where you design a process to give you all the smarts you can get in the time you are willing to allow for it. And the other is I’m pretty confident and I put it out there as a trial balloon and I consciously give people a chance to push back. But they know that I have put one out there as a first among equals. So people have to size me up also and understand that the burden is on the push back. But I really am open to the push back. And those are three approaches that I take. And the push back one is not usually stretched out. One is fast and allows for an honest push back and the other is ‘I really think this one is imponderable and how much time do I have to get as smart as I can before I have to make the decision?’ And if I wait any longer I don’t really have the time to get smarter, or at this point I know I’m not going to get smarter. When we make the stiff versus soft Barry [Barish—Director of LIGO who resigned about a year after Sanders left] and I thought we were about as smart as we were going to get. Extending the time was not going to make us much smarter so it was time to make a decision.

[It is hard to know whether to call this referred expertise or contributory expertise in science management. HMC]

COLLINS: Sometimes the time gets longer when you don’t expect it as in the case of Sapphire versus silica test masses in LIGO.

SANDERS: I was amazed. When I left LIGO I though the decision was pretty well made. I knew there were people pushing to stretch it out some. I wondered whether if I had stayed the decision would have been made more quickly. So that’s a good inquiry. I think the amount of time they took was excruciatingly long. I wouldn’t say that they made the wrong decision and I’m not close enough into it now to know whether they really had that much time. I think they probably did.

COLLINS: I suppose another element of it is how important is the decision. Cause you’ve got to have a sense sometimes that there are just two ways of doing this and it is not going to make a lot of difference which one you choose.

SANDERS: I told you earlier that at some point you’re allowed—as I came to the three different telescope designs and saw that they all met the requirements. My approach was to use, perhaps judgment, referred expertise, or whatever, to make a choice. Then I said to the team ‘Guys we have three good designs. We’re going to pick one. Then our job is to make that one work.’ The better is enemy of the good enough. The good enough is ‘good enough.’

(Once more a referred expertise could be talked of as a science management expertise. HMC)
COLLINS: This was very much exemplified in LIGO in the case of Ron, who found it hard to accept that anything was good enough.

SANDERS: He’s the classic ‘the good enough is the enemy of the better.’ He turns it around.

{Ron Drever is a brilliant inventor who found it hard to fit into the routines of big science.}\(^\text{10}\) Here is another referred expertise and limitations of different categories of scientists. HMC}

2.1. Subsequent e-mail from Gary Sanders, 7 June 2006

Nine months later Sanders was asked whether he would be willing to allow the interview to be published. He responded as follows.

Harry:

TMT just had its Conceptual Design Review. The usual deal. A dozen eminent . . . reviewers, a week of dry runs and 4 days of presentations, breakouts, etc. But it went well. . . . I have to admit that I thought of you as I finished the big 4 day review, the first one for TMT, heard the debriefing, and realized that they never laid a glove on me. I answered all the questions thrown at me just like in LIGO. I thought, Harry should see another example of referred expertise and a great deal of catching up to interactional expertise. It has been an interesting two years.

Having now come through transition for a second time, I find it fascinating to try to parse expertise and the different kinds of judgments. The interview looks right even today. No new perspectives. I would say the same things.

[Here is] A quote from the report of the TMT review: “This is a well-scoped project, technically challenging, yet within reach. It will enable a new era in astronomy that is exciting and highly motivating. The project has a great team, [with acceptance of] rigorous project management by a community not always accustomed to such discipline.”

Cheers,

Gary

3. Interview with Jay Marx, 19 March 2006, early afternoon, in an office of the conference center during a LIGO Scientific Collaboration meeting in Hanford

COLLINS: Jay, one of the reasons I want to talk to you now is because this is an absolutely fascinating and unique moment. . . . You’ve got to chase around a lot to find someone who is just taking over a project from another field and I’m really interested in how this business works. How you can come from one field and move into a field in which you are not a specialist and manage it? So why don’t you tell me how that works.

MARX: First of all it’s important to understand my job in LIGO. LIGO has two major components: The LIGO Laboratory which operates the LIGO observatories and collects the data that is used to search for gravity waves, participates in R&D aimed at future capabilities such as Advanced LIGO and contributes to the analysis of the data. The other is the 500 member, 40 institution LIGO Scientific Collaboration which also participates in the R&D and carries out the bulk of the data analysis. The LIGO Laboratory, with involvement from some parts of the LIGO Scientific Collaboration, will be responsible for the Advanced LIGO Project. My job is to direct the LIGO Laboratory, to work with the leadership of the collaboration to assure that the best science is done, and to represent LIGO and its interests in Washington, with the public, with the scientific community and with stakeholders such as Caltech, MIT and the National Science Foundation.

Given all that, I can tell you that it is hard to move into a new field in a high level position, but I have a lot of relevant experience. I’ve worked at high management levels in unfamiliar fields twice before. I think the key to understanding the challenge is to realize that there’s a number of dimensions to consider. There’s the science, there’s the technology, there’s the politics, and there’s the sociology—the way the people interact and the culture of the field.

The first thing I am doing is to understand in which of those areas lack of knowledge is going to cause problems first. And in my experience the thing that will cause problems least is my ignorance about the science. It will embarrass me but it won’t cause big problems right away, so I give myself more time to learn that.

The thing that will cause the biggest problems is not understanding the political environment, not understanding the sociology, and the culture of the scientific collaboration. Your stuff! Because that’s where immediate and serious damage can be done. Technology is in the middle, and key there is for me to know what I don’t know. To not put myself in a position to make decisions when I don’t really know what I am talking about. And so early on I need to put my ego aside and be able to get advice from enough of the right people to understand the issues. So it’s a kind of triage of knowledge—I give myself a lot of time to learn the science but try to learn the politics and the sociology as quickly as I can. And for the technology I try to learn quickly, rely on expert advice and on my ability to ask the right questions to uncover the hidden assumptions and things that might be swept under the rug.

\(^\text{10}\) Collins (2004a).
COLLINS: So in terms of learning the science and all the rest of it, let’s just get a rough timetable. How long have you been doing it so far?

MARX: I started my education in the fall when I was first approached. I read your book, ‘Gravity’s shadow’ to learn the history, and culture of the field. And since the middle of December when I agreed to take the job I’ve devoted every airplane flight and spare moment to reading about the science and technology—the instrument science as well as gravitational science. I’ve been studying Peter Saulson’s book about the design and technologies of interferometers for detecting gravitational radiation. I’ve been trying to watch Kip Thorne’s video course about gravity and gravitational radiation. So there are good places to get the information for a beginner but it’s a matter of having the time to learn the material at sufficient depth. I also ask a lot of questions, even very elementary ones, of a lot of people working on LIGO.

COLLINS: This morning we sat through three of four papers [at the LSC meeting]. Were you following them?

MARX: Yes—There’s the astrophysics to understand. My physics education is the basis for me to understand the basic concepts. Understanding about pulsars is an example. I have a mental picture of these things beaming, the physical processes that cause the beaming, and I have learned about what attributes affect gravity waves from pulsars. Things like the eccentricity, the spin, rotation and the moment of inertia. So the basic physics of pulsars is pretty clear to me and there’s a template in my head to help me to understand that stuff at a useful, but certainly non-expert level.

COLLINS: But you couldn’t sit down and work out, say, the flux of gravitational waves that would be emitted by a pulsar of a certain eccentricity.

MARX: I can’t do that. So there’s a difference in my mind between understanding the science and understanding about the science—and having this sort of mental picture to guide what one’s thinking about the science. Part of that picture is to learn how important factors depend on others and the orders of magnitude of things. For example, how the frequency of gravity waves from a pair of inspiraling objects depends on their mass. Being able to make a real calculation is really understanding the science and I’m certainly not there.

{The above paragraph seems to be a fine description of interactional expertise; Collins too can understand pulsars in a way that, for example, tells him how the masses of the stars in an inspiraling system will affect their frequency—qualitatively, not quantitatively. HMC.]

COLLINS: And do you intend, or do you think it’s important to be able to do those kinds of calculations?

MARX: I doubt that I’ll ever do them, but I would like to eventually understand the most important calculations so that I can understand the physics at a deeper level. This is because I have a real interest in the science as much as it being important to my new position in LIGO.

I once spent about ten years of my life having moved from particle physics to lead a big project in nuclear physics and I worked pretty hard to try to learn the science. It mattered to my own feeling of fitting in and it mattered to my colleagues in nuclear physics. And I think what mattered most to my colleagues was not how much I knew but more that I was actually trying to learn the science because it showed that I was just not a hired manager. That I was there as somebody who cared about the science and therefore was going to make decisions with the science in mind. I believe that this will be important for LIGO as well.

COLLINS: But let me put it like this: the rational you’ve just given me is essentially a political or social psychological rationale, which is very understandable: ‘show that you’re a serious punter,’ as it were. This rational is very similar to mine when I got into this—I knew that if people were going to accept me I’d better go to nearly all the meetings and I’d better put the same kind of work in it that people in the field put into it otherwise you just look like some sort of spectator. But do you think you will make better decisions when the point comes that you can do those calculations?

MARX: Good question. No, I don’t think I’ll make better decisions. So there’s another experience I’ve had that maybe will answer the question. There was another era in my life when I directed the building of a synchrotron radiation source. The science that people do with synchrotron radiation had no intrinsic interest to me—it was things like surface science, it was chemistry, macroscopic physics and biology. And I found that I just couldn’t get myself to be at all interested in learning the science. So I just learned what I needed in order to do the managing job—the scientific vocabulary and a few concepts that I needed to interact with the relevant scientific communities, and a few sexy examples of the science that could be done with the facility that I could discuss in Washington and in presentations. It’s fair to say that I learned as little about the science as I could get away with. But I certainly did a good job and made the right decisions. Even controversial ones that impacted the science that could be done.

I feel very different about LIGO. In fact one of the things that appealed to me about taking on this job, with all the stress associated with joining a new field, was the chance to learn some new science that really has always been fascinating to me. As I teenager I got turned on by cosmology and astrophysics. And the science of interferometers is becoming more interesting to me the more that I learn.
COLLINS: So that’s what brought you into it. So let me try and get at it a different way. Have you been in the job long enough to have had to make a decision yet?

MARX: Yes

COLLINS: Tell me what it was.

MARX: Do you mean a political decision?

COLLINS: Tell me about any kind of decision you’ve made so far.

MARX: I’ll describe a political decision first. There was concern from a big supporter we have in the NSF that there might be problems at the very highest levels because I am not a professorial faculty member at Caltech. I’ll be frank. The worry is that having a second-class citizen at Caltech running LIGO is going to be a disadvantage for LIGO and that the educational component of LIGO might suffer. The underlying political concern was that anything unusual or unfamiliar might endanger Advanced LIGO at the National Science Board level.

So this person [names person], was trying to make some constructive suggestions about things that might be done to deal with this issue of perception, things that were really symbolic in my view. I had to decide what, if anything to do. I decided to set up an advisory body that you will hear about in my talk tomorrow—it’s the LIGO Academic Advisory Council. I didn’t want to do anything symbolic but I set up a group of respected faculty members from several universities in LIGO (including Caltech and MIT) to advise me about educational issues in LIGO. We’re actually going to do some real work and try to improve things. So the decision was to take the concern seriously but try to put together some mechanism that would actually contribute something useful.

COLLINS: So that’s a political decision. What about scientific decisions? . . .

MARX: A decision that directly impacts the science will be to resolve the question about how the collaboration will treat the data in the current year-long run. Whether to present preliminary results from the early data at conferences and publish the results and then have additional publication and conference presentations from data from the whole run. Or whether to publish at several points during the run as the volume of the data increases significantly. There’ll soon be a decision about how to treat that question. Now maybe that’s political but it’s related to how the science is done in LIGO.

And of course the most serious scientific decision will come if we think we have a detection but there is not an ironclad case. The question of whether the evidence is convincing enough to publish could be very controversial within LIGO. That will be a decision about science with big consequences that will also require not only an understanding of the data and analysis methods, but also an appreciation of the sociology of the collaboration and of the field.

COLLINS: Have you given any thought to that? Let’s imagine that we get some signal that is liminal. That is where the sociology is going to be—Is it there or is it just below? Have you thought about the factors on either side of that kind of debate—about when you go public with it.

MARX: I’ve thought about similar situations I’ve been in—I’m being careful here—and I realize that there’s such a strong cultural component. In my previous experience questions like this have arisen. For example, the question of whether there was enough evidence to claim the discovery of the Quark–Gluon Plasma in data from the STAR experiment. In the end the process is important and the thing that must be avoided is having a significant part of the collaboration objecting to the results because they will undercut its credibility. So it’s really a question of how to try to get a significant consensus, but that can often lead down relatively conservative paths as it did in that case. A challenge is how to get people, even those who have scientific arguments in the other direction, to feel that their concerns have been heard and addressed in some sensible way and not just pushed aside. And in the end you hope that the strength of the logic will carry the day. If it doesn’t, then there probably isn’t enough evidence to publish. I’ve been involved in such things in the past, and I expect that the experience will apply here.

COLLINS: My sense is that high-energy—at least, American high-energy—is very, very careful about putting out anything that might be the slightest bit dubious. Is that right?

MARX: It seems so to me. And I think that comes, fortunately or unfortunately, with the visibility and cost of big science as well as the inherently conservative nature of big scientific collaborations because consensus is needed. The impact of having trust in the field damaged by incorrect results is just too high. But the problem is that as a result of the understandable caution it can take a long time for the results to come out.

It seems clear to me that the gravity wave community is also especially gun-shy and for understandable reasons given the history of the field. But this can lead to an overly risk-averse culture. I think one of the challenges will be to get people to shift mind-sets from setting limits on the observability of gravity waves to actually saying ‘we expect to discover something—we’ve got to look hard for a real signal.’

COLLINS: My own view, for what it is worth, is that the community is too careful—it’s not high energy physics it’s a brand new kind of experiment, and in a brand new kind of experiment you’re entitled to be not entirely certain about what you are finding when you first find something.
MARX: As long as you find something that doesn’t have an alternate and uninteresting interpretation. So long as someone doesn’t come along afterwards and point out that you made a stupid mistake that could have been avoided with more care. There’s a difference between a false alarm posing as a signal where if you worked a little harder you’d realize it was trivial, and a signal that you can’t explain by known environmental or other factors. That kind of signal may or may not be due to a gravity wave, but it is worth taking seriously.

COLLINS: It’s probably too early to get to the real nitty-gritty but it would be nice if we were sitting here discussing how you decided that [the soft suspension championed by ‘Broadside’ [a pseudonym] was going to be given another trial.]

MARX: But there was a related decision recently too which was controversial, with some very strong feelings, about reviving his work, spending about half a million dollars on building a prototype of his system and next winter or spring after testing seeing how it stacks up against the baseline stiff suspension system for use in the HAM chambers in Advanced LIGO.

COLLINS: And were you involved in that decision?

MARX: I was very much involved.

COLLINS: So tell me about that one.

MARX: Reaching a decision involved a process that included a review, developing additional information, modelling and finally advice to me by our Change Control Board that considered the arguments on either side. It was a process that my deputy put a lot effort and knowledge into making happen.

The process involved, on one side, people who felt that the issue had been laid to rest some time in the past. People who for good reason felt that the Lab’s staff was already overburdened and adding one more relatively big prototype and testing activity was going to be a problem. And there were also people who weren’t so confident about [Broadside’s] abilities to carry something like this through to the point where there would be enough evaluation and testing done so you could take a hard-nosed look at his approach versus the other one and say which was better. So there is a lot of concern that this either may not pay off or have a lot of negative impact beyond the money. Though the money itself was significant.

On the other side it was clear to me that if [Broadside’s] approach worked it could pay off by saving considerable amounts of money for Advanced LIGO, about $10M. This is important because the cost estimated for Advanced LIGO is very near the edge of what is politically feasible. And so we have concerns that the NSF could say ‘Advanced LIGO is a wonderful project but we can’t quite give you that much money’. In such a situation something that could reduce the cost by $10M or so would make a big difference, possibly helping to preserve the full scope of Advanced LIGO (to upgrade all three interferometers) and so getting the maximum scientific benefit out of the project.

So I thought it was worth the gamble.

But what was involved in my reaching a decision involved working at two different levels. One was walking around and talking to everyone involved, and really trying to understand about the technical issues and the concerns about taking on another significant prototyping task. I had to give people a chance to beat me up if they felt this was the wrong thing to do; they needed to have their views aired. And I needed to be open-minded and listen to the arguments on both sides. And to ask the kinds of questions that uncover the unspoken assumptions or the overly optimistic or pessimistic ways of looking at the issues.

And we got people who were credible proponents of this to put together written documents that really tried to lay out the risks, the benefits, the unknowns and a realistic plan for getting it done in the time we had before it would be too late to change the baseline. The documents were circulated and we set up a committee of experts to review and evaluate the approach and the plans. That committee recommended that additional things that were needed such as more modelling. The additional information was developed and the modelling was done.

We had meetings where people complained that they didn’t believe this or didn’t believe that—we talked it through with them. And finally in the end there was a formal meeting of the LIGO Laboratory Change Control Board which advises about all kinds of changes in plans, budgets, technical scope, etc. This issue was so controversial that for the first time in a long time, after intense discussion in closed meeting, there was a formal recorded vote. The vote was not at all unanimous, but it did favor going ahead with the prototype. I accepted the advice of this group and I approved it.

COLLINS: In making that decision how much technical understanding of suspensions did you need? Because so far everything you’ve said has been in terms of risk, cost, benefit, will [Broadside] deliver it on time, is he the right kind of person . . . , and all that kind of stuff.

MARX: In addition to relying on experts and colleagues I tried to learn as much as I could. I went to Stanford and spent a day there with some engineers looking at some prototype stiff suspensions; I spent time with [Broadside] trying to really understand his approach and its strengths and weaknesses. I can’t claim to have come away with a very deep technical understanding but after 30 years of doing this kind of scientific and technical management, you develop an instinct for where there are weaknesses and you learn certain questions to ask that help bring the right information to light.
COLLINS: Give me some examples

MARX: Typical kind of things to ask will focus on what might have been swept under the rug. For example, the wrong question to ask is about which coupling modes between different degrees of freedom were taken into account in the modelling. The right question is which ones weren’t taken into account. Everybody makes approximations. Everybody has things that they think are not big effects that they ignore in their analysis. You ask questions to probe the impact of what has been ignored—if there’s a system that is at some level of prototyping you ask about the difference between that and the parameters and requirements of the final system and whether differences matter or not, and why. So it’s about trying to get the uncertainties and the assumptions on the table, to see those clearly and understand what they mean. Without that you can make the wrong decision by believing people’s wishful thinking or optimism. Or even because some people can be so in love with their ideas that they can’t see the weaknesses or risks. Everybody’s honest but that’s where the proponents can fool themselves and that’s where other people can be fooled.

[Here is another kind of referred expertise. Experience in one technical domain of the kind of mistakes that can be made by asking the wrong questions turns out to bear on another technical domain. HMC]

COLLINS: Do you have any sense that if you knew more about suspensions you would have been better equipped to make this kind of decision?

MARX: Yes. I would have been more confident

[COLLINS: Confident!] in my decision from the technical perspective. I relied on very knowledgeable and objective people like my deputy to help fill in my technical naivety. But there was also a very strong strategic component to the decision and I was very confident about how well I understood that aspect of things. And remember that we were deciding to build and test a prototype, not deciding to use this technology in Advanced LIGO. That could come later.

Let me talk about money just to illustrate this aspect. We probably will spend about half-a-million dollars developing and testing [Broadside’s] prototype, but if this thing works we’ll save maybe ten million dollars so it’s a gamble with a big pay-off. If the investment was half-a-million dollars and the potential pay-off was only two million dollars—I’m making this up as I go along—I’m not sure I would have been so enthused about this. Looking at it as a strategic gamble, the ratio of payoff to investment has an impact on the level of personal technical confidence I needed. The less the benefit compared to the cost, the more confident I would have wanted to be technically because I would have needed to be more sure that it would work, technically.

COLLINS: You’d have needed to be more confident to make that decision as the difference gets finer.

MARX: I would have needed to be more confident because as the benefit narrows down you want to be more sure that it has a good chance of paying off.

[It is interesting that here the way a financial calculation works out bears on the importance of the technical knowledge. Oddly, the bigger the potential financial benefit compared to the cost the less important is technical knowledge. At first sight this seems to reflect the strange paradox that the larger the grant for a scientific project the more do non-scientific considerations play a role in its funding. The resolution of the paradox is that in both cases the larger the financial outfall the more do the opportunity costs fall on wider sections of society (writ large in things as big as the space program, writ small in this case). HMC]

COLLINS: You’ve put it all in terms of cost-benefit whereas when I talk to [Broadside] he puts it in terms of absolute performance . . .

MARX: I think [Broadside] believes in the absolute performance. He’s an inventive scientist and he want to see his ideas proven out at the highest level possible. But what he’s looking at is the absolute performance in one part of this very complicated system. And what is not clear to me, and maybe not to him, is whether at some point the improved performance in this one area is over-shadowed by other factors when all components must work together as an integrated system. But on the other hand, improved performance in this area might also lead to benefits in some future instrument. So, he’s right—better performance does help in the medium term and in the long term. But when considering investing lots of money and effort, someone in my position has to consider the relative risks, not just the potential upsides. This is especially true when there is already another viable solution that has already been adopted as the baseline. The potential payoff has to be worth the risk.

Collins mentions the fact that Broadside’s early designs were problematic because of their height and he needed an additional annulus in the chambers but that now he’s miniaturized it. Jay says that is right and that it was important. It reduces the complexity and what you have to throw away.

[Here Collins has used a piece of his interactional expertise in gravitational wave physics successfully. HMC]

MARX: A very important part of this process by the way, was the LIGO committee that had been set up to review what [Broadside] had done and make some suggestions about whether it was really ready for prime-time—whether things were mature enough even to consider taking this step. And one of the major recommendations
of that committee was that we need more modelling done. And our lead engineer took up the challenge over the Xmas holidays and did the modelling. The results of the modelling were very encouraging. If that modelling had either not been done, or showed that the system was more on the edge, things might have turned out differently.

Marx agrees with Collins’s comment that it is good to keep one or two mavericks around.

MARX: You do need that because they are a tremendous stimulus and it is important to have some people looking ahead to the needs of the next generation of instruments. You do have to understand what they can do and what they can’t do.

COLLINS: In my terminology, what I’m suggesting at this point, is that you’ve got a lot of what I call referred expertise. That’s things you’ve brought from other projects and can apply to this even though you can’t do the details of the science—even if it’s actually necessary to do that—if you don’t know the science quite as well as you would like to. Whilst we’re sitting here with the tape-recorder can you just give me an inventory of your bits of referred expertise that you’ve managed to use so far?

MARX: OK. One very important one is having been part of big science collaborations for a long time. I participated in and lead enough of them to have seen a range of cultures and sociologies, but also to have a sort of toolkit of techniques that are used in big science collaborations to get things done and to deal with the issues and controversies that come up. Many of these techniques appear here in LIGO and at the 90% level are the same as I’m used to. So nothing so far about how the collaboration does its business, sets its priorities, goes about the science seems unfamiliar to me. And it’s been very nice that at various times [a very knowledgeable named person] in the field has asked my advice and he’s actually seemed pleased by it. So there’s a kind of referred knowledge that seems to be working.

Another one is dealing with funding people. I’ve been doing that forever. And my approach is different from that of many other people in that, at least in my experience, a lot of other people see the funders as ‘people who have what I want and are going to do me damage if I’m not careful.’ I’ve always worked hand-in-hand with the people in the funding agencies. And I think this came about because twenty-five years ago I spent a year on sabbatical at the Department of Energy so I really got a chance to see what they have to live with, what they need to do their job and what their constraints are like. And so I’ve always tried to really work with them in a teaming mode and that seems to be working here as well. So that knowledge and experience carries over very well from my past work in big science.

Then there’s just dealing with big, complex technical systems and knowing what kind of questions to ask. There’s enough technical similarity to things I’ve done in the past to give me a decent starting point. So while I’m still struggling to really understand all the details of how the interferometers work, I find some of it, for example the vacuum system, feedback systems and beam controls are very familiar to me from particle accelerators. The beams are different, the hardware’s different, but some of the concepts are very familiar so that helps a lot. And I do have some experience with optics and metrology from my involvement in soft x-ray beamlines for the Advanced Light Source.

[This seems like the direct application of interactional expertise from another field. HMC]

And the other place where my experience is relevant is strategic and management issues related to Advanced LIGO because I’ve led a number of big science projects in the past.

Probably if I’ve contributed to LIGO so far it’s helping the leaders of Advanced LIGO to get the momentum for preparing for a project start in 2008 going again. Getting them to focus on what they need to do to prepare for real construction in a year-and-a-half. That’s one of the things that struck me when I started getting involved in LIGO, and I think it’s true. The collaboration had fallen into a mindset that Advanced LIGO is way far off and we’re just doing the R + D. But it’s only a year-and-a-half before there’s construction funds and it’s a few months to a do-or-die review. So the first thing I had to do on joining LIGO was get the right people to take this very seriously and begin to work on the right things and really focus. I have had a lot of experience doing just that. I think it’s been helpful.

COLLINS: Let me ask you about more detailed things. I’ll just prompt you and see if it makes sense. I’m guessing here that one of the things you’re good at is if someone like [Broadside] comes up to you and says I’ve got this idea, and it’s absolutely fantastic, and it can’t fail, etc. etc. The first thing you’ve got to know is ‘don’t take this at face value’ [MARX: right!]. And the second thing you’ve got to know is a set of questions to ask that will reveal the weaknesses. [MARX: and the third is ‘don’t turn the person off.’] So those things are common across the board.

And the other thing is you must have listened to a lot of debates between disagreeing parties. [MARX: yes!] Presumably you know something about how those debates go. Do you recognize certain standard poses that people adopt.

MARX: Yes. First of all there are the many levels of these debates. The words people say. Most people try to be very technical and scientific. There’s a next level down that is often a clash of egos that is unspoken but real. And often there are very real and unspoken philosophical differences about what’s important. And people can be very locked into their position.
In my experience it isn’t often one side will say, ‘yes, they’re right I’m wrong.’ And so in the end you’re really dealing with people issues. It’s a question of just how to get to the point where people will not feel they were personally screwed by the decision and at least they can understand why the decision was made not to choose their side of the issue. And sometimes if the differences aren’t so big at the deeper levels you can actually get people to work together on a common solution. And in some occasions the issue is purely technical. Those are fairly easy because then there’s some logic you can apply to reach a crisp resolution that everyone can see is right.

We agree that there are a surprising number of people out there who do not understand that if you are going to manage successfully you have to keep walking into peoples’ offices and explaining things to them.

MARX: And the trouble with these big collaborations is that there is a lot more walking about to do and there are many more people to talk to. And if you are going to have people talk to each other and have something self-organise into a consensus there are many more connections to make. That’s one of the difficulties. With LIGO one of the biggest problems in this area is that so much work gets done by e-mail and the telephone. The bandwidth is so much less than face-to-face conversation, so much gets lost when people can’t see each other’s facial expressions and body language that carries a lot of information in real conversation. That worries me.

We agree that the people who think that e-mail and video-conferencing can replace face-to-face conversation are missing something important.

MARX: I can’t tell you how many times I’ve put a word in an e-mail that caused problems while if I’d said the same word in a face-to-face conversation it wouldn’t have cause a problem or even been noticed because somebody’s looking at me and they know what I mean. As a result I spend much more time thinking carefully about what I’m going to write down than I would if I’d say it. It’s a huge time-waster. But the bandwidth is so much narrower with e-mail that the chances for misinterpretation are so great.

COLLINS: So what else do you think I should be asking you?

MARX: I’ll pass on that.

3.1. Subsequent e-mail from Jay Marx, 6 May 2006

I wrote to Marx subsequently, asking him if anything more worth reporting in the way of decisions had happened. His reply consisted of a useful summary of some of what we had already discussed.

1. Technical decisions—Thus far there has only been one.

My role [in the hard/soft suspension decision] was to learn enough to develop interactional expertise and to apply my referred expertise to be sure that the decision process within LIGO would involve the right people, sufficient detailed analysis and a decision process that would allow the opponents to make their case and be heard. In the end the decision was referred to a change control board that evaluated the information and made a recommendation to me. The recommendation (not unanimous and made after heated discussion) was to take the risk and go ahead with the development. I accepted the recommendation. If the recommendation went the other way and there wasn’t a show-stopping reason I probably would have authorized the development anyway. The recommendation of the change control board was not unanimous, but strongly in favor of going ahead so my part was relatively easy.

2. Political or sociological decisions—many

I have been making lots of political and sociological (my use of the word) decisions. All are comfortable because I am very experienced with big collaborations, dealing with institutions and with government agencies. That’s presumably why I was hired. Examples include — * Planning strategy for preparation for the Advanced LIGO baseline review, for how we would handle the review, for dealing with the NSF about how they structure the review and the review team. * Strengthening connections within Caltech and MIT. Building relationships with key people. Doing so now when there are no big problems so when I need their help in the future, I will not be an unknown. * Dealing with issues at Caltech and NSF related to my not being a professor—taking external requirements or demands and shaping a response that leads to something that will do some good for LIGO or at least no harm. The LIGO Board and the LIGO Academic Advisory Council. The story behind these should be discussed face to face. Too much to write. * Politics behind the LIGO-Virgo MOU [Memorandum of Understanding] for joint data analysis. The politics involves the Italian funding agency, INFN and NSF. Another long and complicated story which is yet to play out. (There is nothing but positive working relationships between the LIGO and Virgo leadership people and the governing bodies of both collaborations are extremely supportive of the MOU— INFN has some of its own agendas. NSF see things my way) 3. Dealing with reporters, people from funding agencies and political types Here there are decisions about what to say, how to say it, how to focus people’s interest. Communications at different levels for different people, especially about science. Understanding their expectations, biases, etc. These factors inform my decisions about how and when to communicate with various such people who have an interest in LIGO. I bring a reasonable level of experience and, I hope, a good instinct.

4. Decisions about people, their capabilities and how to choose the best match for the best assignment. Impor-
tant as we approach the advanced LIGO project. Like a sports coach having to know what players to play each position and what combination makes the best team. Again, I have experience and, I hope, a good instinct.

5. Decisions about modifying the LIGO organization to optimize our capabilities to operate the observatories, do the science, do the enhancements and prepare and successfully execute Advanced LIGO...with a relatively small staff. Here again I'm experienced.

So you see that much of my role, so far, has been in management of people, organizations, LIGO itself, political situations, etc. The technical decisions on a day to day basis are made smoothly by the experienced people in LIGO. I step in if something is very controversial or has big impact....jay

4. Summary: the dark matter of management

The 'dark matter question' concerns what managers have that a purely interactional expert, like Collins, does not have. The main point of this paper has been to elucidate that component of the dark matter that has to do with technical expertises and to pull it apart into contributory, interactional, and referred expertises. One thing that has been regularly illustrated is the managers' understanding that they need to gain interactional expertise of the technical area but that this interactional expertise does not have to be converted to contributory expertise in order for them to make good decisions. We have also seen the immense power of interactional expertise in understanding the specialisms and in defending those specialisms in committees of peers. We have also seen examples of the way interactional expertise gained in one specialism can be directly applied to another, as in Sanders's application of his understanding of the metrology of lenses, gained from LIGO, which could be transferred without modification to the TMT. We have seen examples of the direct transfer of contributory expertise, as exemplified by Sanders's direct use of experience in running a big science—as in his decisions about where to site the observing instruments and how to organize the priorities for use of the instrument when completed—both technical decisions. Now let us list the things which we have classified as referred expertise. In the order they appear in the interviews they are:

(1) An understanding of the weight that should be given to a seemingly decisive technical argument made by a domain specialist and that this is usually less than the specialist thinks should be given to it.

(2) An understanding of how long to give the process of gathering information before a decision is made.

(3) An understanding of the capabilities and limitations of different styles of scientist.

(4) An understanding of the right kind of questions to ask so as to draw out the limitations in a technical design.

There are also understandings that we are not quite sure whether to classify as referred expertise or just science-management contributory expertise. These are:

(5) Knowledge of the ways to make a decision: for example, when to throw out a quasi-decision and wait for ‘push back’ and how to do this in the three different ways that Sanders described.

(6) How to make sure that the ‘good enough’ does not get driven out by the ‘better’.

The problem of classifying these instances, as in the case of when an interactional expertise should be counted as a contributory expertise, should not be thought of as fatal. Perhaps a bundle of referred and other expertises simply becomes science management expertise when it is practised over and over!

Finally let us return to the dark matter question as a whole and outline the other things that the managers must have that a purely interactional expert does not have. We ignore the question of personal qualities: drive, charm, brilliance, political skill, articulateness, and so forth, since we are looking for more systematic answers. Here then are some more elements that belong on the list, many of which can be drawn from the second part of Marx's e-mail (above).

(7) Plain, MBA-type, management skills such as financial management, the drawing up of progress charts, and an understanding of how to organize a big science project.

(8) Status that accrues with having made significant published contributions to science in earlier phases of their careers; they have demonstrated that they have the contributory skills of scientists albeit not necessarily in the fields they are now managing. This will open many doors that would be closed to a non-scientist And will win them respect and status in their new fields and among those decision-makers in the levels of above them who they have to impress.

(9) Likewise a successful manager will have useful networks of colleagues in science who can advise them and ‘put in a word’ for their projects and their capabilities.

(10) They will also understand the politics and ‘culture’ of the assessment and funding regimes within which they have to succeed and can interact smoothly with them, also knowing what is a credible approach and what is not.

There are, then, many good reasons for a purely interactional expert with no experience of science not to take over the running of a large scientific project.
Appendix. More on the definition of referred expertise (co-authored by Jeff Shrager)

Though the idea is introduced in Collins and Evans’s Rethinking expertise, this paper is the first dedicated discussion of referred expertise. As can be seen, we are still reaching for ways to define the term more sharply. This Appendix is an attempt to reach a little further by contrasting referred expertise with other expertises which are close to it.

First consider ‘directly applied expertises’. One example which is relatively easy to deal with is that mentioned in the introductory section of the paper—solving differential equations. The solving is easily recognized as ‘directly’ applicable in a new area in virtue of the fact that it could be delegated to someone who knew nothing of the new area. While forming the right differential equations, and applying them appropriately, are skills that depend on understanding the science in which they are being used, the solution of the ready-formed equations could be delegated to a mathematician (or computer program!) who knew nothing of the particular scientific context. In this sense, solving differential equations for LIGO is exactly the same as solving differential equations for the Thirty Meter Telescope, so the skill is properly described as being directly applied.

From this we can impute that a referred expertise will depend on a grasp of some elements of the tacit knowledge pertaining to the particular science in question. But this cannot be all there is to it.

Consider the case of a plumber. The skill of a plumber cannot be applied without an understanding of the particular context in which it is to be used on each occasion. Every time a fresh house which requires some substantial addition or alteration to the plumbing is encountered, the plumber will have to draw on experience and expertise to know how to apply generalized expertise in the novel circumstances. Yet we do not want to say that application of plumbing skills to new houses is an example of referred expertise or referred expertise will collapse into contributory expertise. The skill of plumbing, we have to say, is a contributory expertise, not a referred expertise.

The resolution may work as follows: consider two individuals, P and E, who are just learning to become plumbers after following other less lucrative careers into middle age. P has been a professor of cultural studies whereas E has worked as an electrician. E is likely to bring with him certain expertises that are useful to a plumber that P does not possess, and it is these expertises—that are not ‘just plain’ plumbing skill (which neither P nor E have), but which give E much better standing than P in plumbing school—that can be called ‘referred’. These might include knowing what it is like to work in a manual job; knowing what it is to learn a manual skill and how to go about it efficiently—and that includes knowing how hard it is to learn such a skill and therefore how much effort and practice is needed; knowing how to deal with customers; knowing how much to charge; knowing how to extract money; knowing what you can ‘get away with’ in terms of quality when working in the houses of different kinds of client; and so forth. None of these skills are intrinsic to either laying cables or fixing pipes but they are part of being both an electrician and a plumber. The ex-professor of cultural studies does not possess them, the ex-electrician does. That example seems to show a way to differentiate between referred expertise and contributory expertise in such cases.

It is, however, victim to another possible ambiguity. This is that the referred expertise of the plumber/electrician as described above merges into the contributory expertise of managing small businesses. This reflects the point made in the paper in respect of science: ‘Perhaps a bundle of referred and other expertises simply becomes science management expertise when it is practiced over and over!’

Could management expertise (of the kind we are talking about) simply be an especially big bundle of referred expertises? This is the kind of problem that may be best left for future consideration but, before finishing, here are some indications of possible directions for further analysis.

First, the examples of referred expertise which the interviews exemplify are often kinds of meta-expertise. Sometimes they are meta-expertises in the sense of the Periodic Table—expertises used to judge other expertises and experts. Sometimes they are expertises about how to learn and apply other expertises in different contexts.

Second, a characteristic of all the examples to which we have been drawn is that they are learned from experiences that come in large chunks. No one makes changes such as from electrician to plumber, or interferometer scientist to telescope scientist, more than a few times in their lives. One’s referred expertise, then, is the kind of thing to which one refers by saying: ‘I have some experience of that’ rather than ‘I can do that’. Or, when one says ‘I can do that’, one says it in a slightly ironic sense—one is making the claim ‘I am as likely to succeed as anyone else and more likely than most’ rather than ‘This is part of my proven, day-to-day repertoire of abilities’. In sum, referred expertise is, perhaps, one the things we mean when we talk of ‘experience’.

References


