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Essay review

Everything you did not necessarily want to know about gravitational waves. And why

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Gravity's shadow. The search for gravitational waves
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1. Introduction

Many academics are impressed by fat books. To me they recall Gaston Bachelard's psychoanalysis of the concept of mass. The unconscious—academic or not—appreciates the sheer quantity and volume of objects. As Bachelard writes, 'for a greedy child, the biggest fruit is the best, the one which speaks most clearly to his desire and which is the most substantial object of that desire. The notion of mass concretises the very desire to eat' (Bachelard, 1968, p. 18). Similarly, many academics have the desire of the massive book even at the price of dilution and the spontaneous awe of big books is often the result of an unpsychoanalized academic unconscious.

Collins is quite conscious of the extraordinary length of his story, as he himself habituated his readers to books that are short and to the point, before offering the present contribution, which has the faint odour of self-indulgence. He thus readily admits that 'some might wonder why some of the parts are so brutally long' (p. 17). To answer that question, he simply says that he titled Part I 'À la recherche des ondes perdues' in wry acknowledgment that 'it does go on a bit' (p. 17). But that is not an answer to a 'why question' and with all due respect, Collins is not Proust (who was anything but 'brutal') and history (or sociology) is not a novel even though it can take a narrative form. Far from being the

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result of a long-winded argument (as the link with Proust suggests), the length of the book is simply the result of the multiplication of long interview extracts; indeed, almost a third of the book is made up of interview transcripts. And in case a fatigued reader might wish to blame a lazy editor, Collins assures us that the original draft 'was 1,000 closely printed pages' and that it is only thanks to 'the advice of [his] publishers' that the printed book is 'somewhat shorter than the original manuscript' (p. xxii). Confusing the publishing of archives and the writing of a book, Collins even thinks the cuts diminished the value of the book as a 'historical resource' but nonetheless accepted to remove or abridge 'certain lengthy quotations or extracts of documents' (p. xxii).[1] As a better explanation for the length of the book Collins should have paraphrased the oft-quoted sentence by Pascal, who wrote to a friend: 'I have made this [book] longer [than usual] only because I have not had time to make it shorter'.[2]

2. Style and audiences

Collins divides his story of the hunt for gravitational waves into five parts. Part I covers the first generation of gravitational wave detectors from the late 1960s to the mid1970s. These detectors are based on Joseph Weber's original concept of a solid metal cylinder kept at room temperature. Weighing about a ton, the cylinder was kept in an evacuated tank and insulated from all outside disturbances other than the occasional gravitational wave, which would then generate vibrations in the bar whose presence would come to light through piezo electric crystal detectors. Using such a 'bar'—as physicists called it—Weber announced in a series of papers in 1968–1970 that he did in fact detect gravitational waves for the first time (Chapter 4 covers 'The first published results'). This story is now well known among sociologists of scientific knowledge (SSK) as Collins has told it many times and used it as the basis for his analysis of the experimenters' regress. Although much debated in the literature, this concept is presented here in a more descriptive and 'black-boxed' manner that avoids the detailed criticisms that philosophers of science have levelled against it. Part II, 'Two new technologies', cover the next generation of bar detectors developed over the period 1975–1995. Unlike the previous detectors, these bars were cooled at very low temperature (liquid helium or below) in order to diminish noise. This part also chronicles the project of using spheres instead of bars and concludes with the emergence of an alternative approach based on interferometers. This 'straightforward account' (p. 17) is followed by Part III, 'Bar Wars', in which Collins analyses the conflicts that led to the demise of bars and the rise of interferometers. Part IV follows the development of the interferometry approach incarnated in the project LIGO (Laser Interferometer Gravitational-Wave Observatory) from a small to a 'big science' project. This change in scale was accompanied by the appearance on the scene of new macro actors such as the National Science Foundation and its committees charged with making decisions on the future of the field. Part V, 'Becoming a new science' follows in detail the scientific work done on LIGO concentrating on the question of the sensitivity of the apparatus and the

[1] Note to the dedicated: the missing quotations are now available on Collins's website—see www.cardiff.ac.uk/socsi/gravwave/webquote/ for access to the thirty-five page document; the quotes will also be made available on the American Institute of Physics website.

[2] The original French is: 'Je n'ai fait celle-ci plus longue que parce que je n'ai pas eu le loisir de la faire plus courte' (Pascal, 1926, Vol. 2, p. 195).

setting of upper limits to wave detection. LIGO being an ongoing project, Collins did his best to follow it until the final printing of his book. He thus added a 'Coda: March–April 2004' to relate events that have occurred 'since the main manuscript was finished in the summer of 2003' (p. 813). Finally, Part VI, 'Science, scientists, and sociology' discusses many questions concerning the links between actors and analysts, the methodology of the case study and the future of SSK.

Collins' obsession with details that are not always linked to the main argument contributes to making the book too long and makes many sections sound like a monthly technical report from the engineer-in-chief of the project. One example should suffice to give the tone:

> Another extraordinary event, in June 2002, was the parting of a mirror suspension wire on the Hanford 2-kilometer device with the subsequent collapse of a mirror, causing the detector's control magnets to tear. There are stops designed to prevent the mirror from falling too far and causing such damage, but in this instance they failed. The cause of the fall was an earthquake on the Chinese border (minor earthquakes of this magnitude happen twice a month or so), which shook the interferometer enough to make a servo oscillate. This in turn moved a mirror so far out of alignment that it directed the infrared laser beam onto a suspension wire and softened it to breaking point. (pp. 735–736)

Not only does the description of this micro event continue for another ten lines but it is immediately followed by the description of an earlier event: 'In May 2002, a strange noise, related to the inverse of the cube of the frequency, became apparent in the Hanford 2-kilometer interferometer' (p. 736). Certainly useful for the constructors of the apparatus and the supervising committee (if there was one), these details have no obvious meaning or interest, as they are not used to shed light on a particular sociological question. Other than fattening the book, what is the function of such exhaustive (and tedious) descriptions? To answer this question we have to identify the audiences Collins had in mind while writing his book.

Faced with the embarrassment of riches provided by his recorded interviews, Collins used three criteria in selecting material for his book: 1) sociological interest, 2) 'just plain interest', and 3) 'a sense of duty to the history' (p. 17). While the first criteria makes sense for a sociologist of science, the second and third are more problematic and suggest a different audience for the book than the readers of this journal. One could ask 'just plain interest' for whom? Can anything be interesting 'in itself'? Obviously not and it all depends on the audience. Collins clearly expects that his definition of 'interesting' will be endorsed by the 'nonspecialists' to whom he tries to bring 'the esoteric world of the social studies of science' and the 'nonscientists' to whom he brings 'the esoteric world of physics' (p. xvi). This is indeed aiming high. The 'sense of duty to the history' makes sense if one considers the debt Collins has accumulated toward the many scientists he has interviewed over the years and without whose collaboration the book (and the published papers) would not exist. It is thus easy to understand that in writing this book, he 'had the community of gravitational wave scientists very much in mind' (p. xvi). The apparently pointless technical descriptions mentioned above can thus be explained by Collins's belief that such details will doubtlessly interest members of this community. In addition, their presence in the book may have served the secondary purpose of demonstrating to Collins's physicists' friends that he masters the physics he is talking about. Never mind the fact that those details add nothing to the specific sociological question being addressed, they show the scientists that the sociologist can master the details they love so much.

Although they are not his primary target, Collins does not totally ignore those he calls ‘analysts of science’ (typically readers of this journal). He admits that ‘the main argument of part I of the book will be familiar’ to them but he insists that the narrative does not constitute ‘just a repetition of published work’ as these 200 pages in twelve chapters contain much more details though ‘no new analytic ideas’. He also warns those readers that if they skip that part they will miss in Chapter 5 the ‘new metaphor’ that he proposes to understand the way heterodox science survives or fails to do so (p. 18; more on this below). However, as previously suggested, those ‘analysts of science’ are not his principle audience as Collins does not really engage them in any depth or at any length. The book is thus less analytical and polemic than is usually the case in the specialized literature on the sociology of scientific knowledge. The tone is much more pedagogic as the text essentially provides a long narrative of the history of the many attempts to detect gravitational waves. Hence readers are spared the details of the persistent debates that surround some of the interpretations of events put forward in this book.[3]

The absence of these debates is related to the fact that the first intended audience, the ‘non specialists’, fix the general style and tone of the book, dictated as it is by the tradition of trade books devoted to popularization of science.[4] The story is told in the first person as the reader follows Collins through his many trips to meet scientists and interview them. This is the ‘engagingly written’ aspect emphasised on the fourth cover, a typical trait of trade books written by journalists where the ‘I’ is supposed to give flesh and blood and ‘human interest’ to an otherwise rather ‘academic’ story. Thus the reader meets hundreds of ‘I asked him’, ‘he told me’, the presence of which makes sure he/she does not forget that the author is indeed the one who made the interviews. To be true to the chosen style, Collins adds from time to time a bit of this supposedly ‘engaging’ human touch by noting his private melancholic views: ‘My notebook records, “Very sad business seeing a field killed. So many good ideas and hopes, and careers, going down the tubes” (p. 446); or ‘At this time I was suffering from an undiagnosed cracked tibia (I thought it was a twisted ankle), and the intense pain, relieved by huge doses of painkiller, meant I had to cease observations by about 5:00 p.m. each day and lie down in my hotel room’ (p. 633 n. 22). And we have also the mandatory description of the environment:

> Later I was to spend a week or so at Hanford, and its strange beauty grew on me. The business side of the installation—the offices, laboratories, workshops, and so forth—are the apex of the L formed by the interferometer’s arm, and normal access to them is from the outside of the L. But if one crosses the bridge to the inside, the road stops suddenly and enters a private space which is little changed from the original landscape. I used to walk across this bridge just before sunset and sit down inside the apex of the L, where I would be completely on my own. The buildings and the concrete culverts cut off the continuous hum of machinery, so the only

[3] See for example, Franklin (1994) and Collins (1994) for his answer. Most of Collins’s references to this literature are limited to brief footnotes (see for example, p. 176 n. 16), which most of the times refer to his own previous works.

[4] One could ask why the book tries to attain such heterogeneous groups. A sociological explanation would have to take into account the transformation of the field of academic publication over the last twenty years, which has seen a decline in the sales of academic books. Publishers have tried to compensate for this decline by targeting the trade market, thus creating hybrid books like the one under review. On these transformations, see Thompson (2005).

> sounds came from nature. Every evening a glorious sunset filled the sky over Rattlesnake Mountain in the far distance, perhaps colored by the dust of the desert. (pp. 536–537)

All the standard elements of romanticism are there: strange beauty, nature, sunset and of course: solitude at last. And the reader is not spared the following clichéd portrait of a scientist: 'As I recall, he [Weber] wears a baggy navy suit, a white shirt, and a tie. His trousers are a little short, his shoes like they are on the end of sticks, and this with bush of hair, adds to the impression of Einstein-like brilliance mingled with eccentricity' (p. 804). From the point of view of the 'analyst of science', this style allows for too many meandering and ultimately pointless developments and is, to my taste, very naïve (as in 'naïve painters'). Collins' self-proclaimed 'irreverence', supposedly stemming from his presentation of science 'as itself essentially mundane', (p. xvi) simply reproduces the usual journalist-style popularization of science and is no more 'irreverent' than James D. Watson's *Double helix*, Nicholas Wade's *The Nobel duel* or Gary Taubes' *Nobel dreams: Power, deceit and the ultimate experiment*, to name only a few of the dozens of trade books devoted to describing scientific competition with a human touch. In this respect, the difference between the 'sociological history' of Collins and journalists' accounts is often vanishingly small given the overriding dominance of a narrative constructed around long quotations.

3. What are metaphors for?

The style and tone of the book are not the only aspects of the writing that are determined by the targeted audiences. It is quite typical of books aiming at high level popularisation of an esoteric field to accumulate metaphors as a way of explaining phenomena. Collins does not offer the systematic and sustained sociological analysis of the scientific practices and controversies that one would expect from a sociologist. Instead of using, for example, a middle-range theory of the dynamic of science, he seems to find it sufficient to comment on the narrative by using many different metaphors that are supposed to shed light on what has been described. Thus, instead of using typical SSK categories like interpretive flexibility, degree of externality or cognitive and social interests, Collins takes the trouble to develop the metaphor of 'space-time' in which the sociologist follows the 'ripples' in 'social space-time' much as the physicist follows the 'ripples' of waves in 'physical space-time' (pp. 12–14). Instead of direct sociological explanations of the dynamics of a controversy we get a mix of heterogeneous metaphors about the 'unborn embryo' that has to find its 'way out of the chrysalis', following which the 'adolescent creature is assailed from all sides'. And two lines later we 'change the metaphor' and learn that Weber's results 'were coldly floating, lifelessly, and invisibly in space, where "no one can hear you scream"'—the quotation is attributed in a footnote to 'Ridley Scott's film Alien' (p. 155). This substitution of metaphors for sociological analysis reaches its acme in Chapter 5 (titled 'The reservoir of doubt') where Collins develops a convoluted comparison between 'a conical island in a reservoir that is steadily filling behind a dam' in which the water 'is the growing unhappiness of physicists with the tension in the network—it is the water of doubt—and if it continues to rise it will drown one of the inhabitant of the island'. To remove the water one must move stones off the dam either at the surface or further below it. Each stone represents 'an assumption or theory belonging to astrophysics or to the theory of the detector' of gravitational waves (pp. 101–102). Then (on p. 103) we learn that if a 'really heavy foundational stone is removed, most of the water of scientific consensus will run out'. This long and confusing detour serves merely to tell us that in a controversy scientists have to answer arguments or find flaws in the opponents' arguments! Hence, 'stone T can be removed by saying that we have done our calculations about the sensitivity of the detector incorrectly and really it is much more sensitive than we thought' (p. 103). But does that really help to understand anything? To answer this question it is worth recalling what the usual function of a metaphor is.

From a cognitive point of view, a metaphor can be useful when it helps to shed light on an unknown phenomenon by linking it to another phenomenon that is better known to the audience to which the explanation is addressed. It then contributes to understanding by showing that the unknown or unfamiliar phenomenon is like the known one in some respect. In short: a metaphor helps to bring the unknown within reach of the understanding of what is already known. One could argue that a metaphor never explains anything in itself as long as it is not developed into a full-fledged middle-range theory—which can be based on an analogy—composed of definite concepts used in relation with each other. Metaphors can stimulate further analysis and suggest lines of development but they do not in themselves provide an explanation.[5] In Chapter 22, Collins does provide elements of a sociological model of 'scientific cultures' and defines three dimensions of scientific judgment—'evidential significance' (re-labelling Pinch's idea of 'degree of externality'), 'evidential collectivism' and 'evidential threshold'—which are useful to make sense of scientists' decisions to publish a given set of data at a given time. This more sustained sociological analysis, which contrasts with the metaphorical style of the main body of the text, may be due to the fact that this chapter essentially recapitulates the results of a paper published in the *American Journal of Sociology* in 1998.[6]

Coming back to metaphors, since the familiarity with a given lexicon varies with the audience, a good professor will use the appropriate metaphor for his/her chosen audience. Also, he/she will tend to use one that is obvious and does not need much explanation. Otherwise one just adds a layer of complexity instead of removing one through comparison. If I say that 'economists are aping physicists', I should not have to explain in two pages (or ten minutes) what it is that apes usually do, otherwise the chosen metaphor does not do its work and I should change it for one which works for the chosen audience. Collins's 'reservoir of doubt' and 'dam of disbelief' are anything but simple metaphors which clarify a complex phenomenon. Rather, they translate an otherwise relatively simple scientific controversy into a complex metaphor with stones, water, dam and conical islands, thus completely inverting the cognitive function of metaphors. Far from helping to understand the unknown through the known they simply add layers of description with more things to remember (was it the dam or the water that 'represented' disbelief or scientific consensus? And were the stones the theory or the experiments? And so on …). But why bother since Collins himself does not seem to give much credit to his own metaphors: right after this arduous presentation he tells his readers that 'The theoretical physicist Kip

[5] The literature on metaphors is huge, but the classics are still Black (1962), Chs. III and XIII, and Black (1990), Chs. 3 and 4. Note that the philosophical analysis of metaphors rarely relates them to audiences (which are taken for granted). The relation between audiences and arguments is developed in Perelman & Olbrechts-Tyteca (1969).

[6] It is usually a standard practice to acknowledge somewhere in the book the papers which have been used in it, in a more or less revised form. Here this information is lacking, although it is obvious that major parts of this book were previously published.

Thorne puts the matter rather nicely, using a different metaphor: He talks of 'wriggle room'. As theoretical and other arguments gain detail and consensus the wriggle room for a new finding becomes less and less' (p. 103). Apart from showing that Collins did talk to Thorne about metaphors and that it is easy to multiply them ad infinitum, I fail to see what the reader can learn from this kind of narrative.

4. Autistic sociology of science?[7]

A striking feature of Collins sociological thinking is its highly self-referential nature. Although he has included many papers by other sociologists and historians of science in the list of references at the end of the book, some of these entries are never used and his analysis does not really build on the corpus of sociology of science accumulated since 1945.

Let us begin with the case of Robert K. Merton. Presenting his own notions of 'constitutive forum' as the space in which 'scientists put their ideas together in a visible way so as to constitute and certify new knowledge and occasionally to dispute one another's claim' (p. 103), Collins adds in a footnote that Merton 'built a social theory based on this kind of constitutive forum activity' and then dismisses it with the blunt assertion that 'Merton's theory is an excellent prescription for good scientific debate but a flawed description' (ibid. n. 9). He also explains that there is another forum, the 'contingent forum' in which all things excluded from the constitutive forum are present and play a role in scientists' decisions to accept or not a given scientific result. These include statements about the scientist's country or university of origin, their being 'unprofessional', 'liars' etc. According to Collins, 'these kinds of assessments [of the integrity and competence of scientists] are not normally thought of as belonging to science proper' (p. 129). The problem with Collins's model of two forums is that it is in fact a step backward from Merton's. For the elements that Collins relegates to the 'contingent forum' were for Merton part and parcel of the scientific community and he theorized them through such concepts as 'Matthew effect', 'cumulative advantages' and 'ambivalence'. The hierarchy of institutions is also part of the Mertonian theory of the scientific community, although it is interpreted as a functional solution to a problem of processing information coming from different sources. One may of course disagree with that particular functional explanation but the fact remains that his theory is not as simplistic as depicted by Collins, since it takes into account institutional aspects of science completely absent from the micro-sociological approach of SSK.

Collins misses another opportunity to take previous research into account when he tries to explain why 'it is Rainer Weiss of MIT who is now almost universally credited with the conceptualization of an interferometer' (p. 266) capable of detecting gravitational waves, whereas 'the first person actually to build such a device was Robert Forward', a member of Weber's team (p. 265). After offering two reasons related to the fact that Weiss was the first to 'analyze the mode of best operation, the sensitivity and the source of noise' and that 'many of Forward's ideas reached him from Weiss through a mutual acquaintance' he adds a third reason 'more sociological: Weiss is still a leading figure in the

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[7] The idea of an 'autistic science' comes from the critics of classical economics who funded the journal *Post – Autistic Economics Review*. See www.paecon.net.

interferometric search for gravitational waves and has inducted many of the rest of the team into the project ... This sociological reason does, almost inevitably, lead to the contributions of others to be less salient in scientists' minds' (p. 266). It is significant that Collins's individualist view of science has no place for the fact that the position of a scientist in the hierarchical structure of institutions can also play a role. More sensitive to these structural aspects of science, the Mertonian idea of cumulative advantages (Merton, 1973, pp. 457–458), and Bourdieu's concept of symbolic capital (Bourdieu, 1975, 2004) which takes into account the structure of the scientific field where all positions are not equal (Weiss being part of the prestigious MIT), can explain the lack of recognition of Forward without going into the minds of scientists. From a structural point of view, the 'target diagram' (Collins, 2004, p. 9), which shows 'rings of the scientific community more or less distant from the core' is too simplistic in its unidimensional approach based on distance. A better model would be a set of relatively autonomous fields with intersections, thus making the links between them based not on a linear measure of distance from a centre but on structural relations between fields.

Moreover, merely opposing 'description' to 'prescription' is not enough, since Collins himself admits that 'even if arguments are rarely settled by reason [in the short term], we must continue to act as though they were, because only by going on in that way can we preserve the idea of science' (p. 764). Notwithstanding the fact that the mention of 'in the short term' (why in brackets?) can be interpreted so loosely that one could always say that arguments are not 'really' playing a central role in the closure of debates or (conversely) that they do but only in the 'long term' (whatever that means), it remains that what Collins is proposing here is simply the idea of a regulative ideal of communication that is in essence what Merton was talking about, although for him, it was an implicit norm inculcated through socialization in the scientific community.

Collins's arguments would have been more convincing if, instead of diminishing the contributions of others or imagining unrealistic models for the sake of showing how inadequate they are (p. 312), he had chosen to build on them and improve where necessary instead of proposing ad hoc explanations of his own where Mertonian concepts are in fact useful, as they are for institutional matters, a level of analysis which has essentially vanished in SSK. More surprisingly, instead of using the relevant literature, Collins seems to prefer transferring the job of explaining a sociological phenomenon to the scientists he interviews. Hence, discussing the difference in publication practices between natural and social sciences, Collins presents the spontaneous sociology of a physicist instead of the existing sociological work on this well worked question. Thus, it is left to the physicist Kip Thorne to explain that 'in physics it seems much easier to get things published that people are sceptical about, and get them published in reputable journals, than it is in softer areas of science. Perhaps because people view them as less dangerous, because you know there will be confirming experiments and confirming theory' (p. 331). After a long citation from which this extract is taken, no further discussion follows, as if Thorne's view was self-evident and needed no more sociological analysis.[8]

Collins's lack of engagement with the work of his colleagues is not limited to Merton. Although he is prompt to footnote his own contributions (even a minor book review as on

[8] Collins might counter that he does provide sociological comments insofar as in a footnote he refers to a paper by Zuckerman and Merton on peer review (p. 331 n. 4). But this brief reference is not directly compared with Thorne's views nor analysed in any depth.

p. 797 n. 12), he is loath to footnote the original literature.[9] This lack of interest for the 'external world' of other sociologists is also evident in a brief section titled 'The political sphere' where Collins notes that since his analysis 'has been conducted for an esoteric science, gravitational wave physics' one can ask if this kind of analysis could 'speak to the case of sciences that are exposed more to the public domain' such as 'global warming, the safety of nuclear power stations, the human food chain, health and so forth' (p. 781). As most readers know, these cases have been abundantly studied by sociologists but Collins writes: 'To explore the difference, *we can use our imaginations* to convert gravitational wave science into a public domain science. Imagine—let me stress that it is entirely fantasy ...' (p. 781; my emphasis). Well, there is no point in citing further. Why imagine anything when sociologists have provided plenty of real cases where the 'public' interact with science?

For those who do not like the idea that disciplinary autism is at work here (imagined worlds are preferred to the real one ...), they may perhaps find Collins's candid confession that he has 'a low opinion of much of what gets published in the social science literature' (p. 330) more revealing. Given such an attitude, it would indeed be a waste of time to read all those papers scattered about in so many different journals, and even more so to try to build on them.

Scholarship is often about details. Despite more than 800 pages of technical details, Collins seems to care little for historical and sociological ones. Discussing Max Planck's view about how new theories are accepted, he writes: 'Planck's famous dictum states, very roughly, that ideas that lose credibility die only as their upholders die: "Science progresses funeral by funeral ..."' (p. 347). The presence of quotation marks may suggest that he is in fact quoting Planck. But this is not the case and it would have taken him about ten minutes (the actual time I took) to find the exact quotation in Planck's autobiography. It reads: 'a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it' (Planck, 1949, pp. 33–34). The original quotation is not much longer than Collins's gloss but it is sociologically much more informative since it points to argumentation in science and to socialization into a new theory. Applied to scientific matters, this kind of 'rough' explanation can give rise to very poor popularization. Consider the following description of relativity: 'as things *approach* the speed of light, mass *forgets* its constancy and *starts* to increase while time *seems* to slow to a crawl' (p. 4; my emphasis). Of course, mass does not 'start' to increase only when it approaches the speed of light. Being a continuous (and nonlinear) function of speed, mass increases much before it 'approaches' that limiting speed ... And time does not 'seem' to slow down but is in fact measured to be slower by comparing clocks at rest and in relative motion. Why bother about these seemingly pedantic details? Because a less casual approach to the topic could have provided 'nonscientists' with a simple though exact explanation of the basic features of special relativity and with a richer understanding of Planck's views.

[9] When talking about cold fusion for example (p. 318) no reference is made to any previous research. And when he does dare to give reference on cold fusion (as on p. 351) he refers only to his own popular summary of the case, in Collins & Pinch (1993), and to Bart Simon's paper (1999). Among the many other sources on this topic, Collins could not miss the latter since he himself used the idea of 'life after death' developed by Simon who, Collins adds in a footnote, 'uses a similar scheme *to the one used here*' (p. 329). Why not write: 'I used a similar scheme to the one used *there*'?

And it is not farfetched to think that those readers expect this rigorous treatment from a university press. Another example of his peculiar conception of scholarship is Collins's statement that 'we know that about half the papers in the scientific journals are never cited by anyone' (p. 364). This supposed 'common knowledge', not footnoted to any source is in fact false though it is part of the folklore of many scientists. Known in scientometrics as 'uncitedness' this question has been much discussed, and the proportion of non-cited articles is in fact usually much lower than that, varies with disciplines and depends on the method of calculation.[10] One thing is certain: for the case of one of the best physics journals, the *Physical Review* series (in which many important papers on gravitational waves have been published), only 6.6% of the 353,268 papers published between July 1893 and June 2003 have never been cited (Redner, 2005). We are far from 50%. Of course, one can be utterly uninterested in such kind of data, but then silence is better than circulating urban legends on supposedly well known bibliometric 'facts'.

If he had an ecumenical view of the field of sociology of science instead of a sectarian one, Collins would realize that whereas SSK brought new tools to look at the micro-dynamic of scientific controversies, this approach is ill equipped to deal with institutions and the complex relations between the scientific field, the political field and the field of media to name only a few social spaces having their own internal logic. And it is not promoting eclecticism to note that different approaches—which are not opposed but complementary—can be combined to interpret controversies and provide a coherent sociological explanation of their dynamics which, at times, involve different scales.[11] Moreover, as John Thompson nicely put it, 'all research takes place in a context which is shaped in part by previous research and reflexion, and it is an accepted part of scientific and scholarly practice that researchers should take account of the results of previous research as expressed in outputs of various kinds' (Thompson 2005, p. 82). And he could have added that without this implicit 'rule of the game', the field can only become anomic.

5. The embedded sociologist

While Collins conceives LIGO as being socially constructed, the interviews he uses abundantly are taken for granted and transcribed for what they say, with few, if any, distancing comments. Unlike Weber's gravity wave detector, they are not deconstructed but simply transcribed and given to the reader. Having criticised Allan Franklin's choice of limiting himself to printed sources to reconstruct historical cases of physics (pp. 135, 176), Collins goes to the other extreme and uses essentially interviews as if they had no limitations. This debate is of course pointless as there is no reason to limit oneself to any particular kind of source.[12] Using many different ones in order to construct a more robust history is the way to go. The danger with interviews, however, is that constantly rubbing shoulders with scientists in order to get their views, as Collins did, makes it difficult to keep things at arms' length. And while Collins is quick to point out that to

[10] For an entry into this literature, see Stern (1990). For debates on the level of uncitedness see the letters in *Science*, *251* (Abt et al., 1991).

[11] I have argued along this line in more detail in Gingras (1995), pp. 138–147, and Godin & Gingras (2002), pp. 144–148.

[12] The tendency during debates to present choices in dichotomous terms where they cover in fact a continuum of positions is analyzed in Boudon (1994).

understand the spread of ideas 'published proceedings are not so useful, except under unusual circumstances' (p. 135; see also pp. 754–755), or that citation analysis has also its limitations (p. 207), one searches in vain for a similar caveat about the uses of interviews.

Collins seriously underestimates the level of his embeddedness and its effects on his analysis. Although he tries to convince his readers that 'this very book may cause discomfort' among some of his scientists' friends (p. 12) I think, on the contrary, that most readers will find that the book pretty much sides with LIGO scientists. Whereas SSK sociologists long insisted on the central notion of impartiality, Collins often offers his own judgements on scientific issues. He affirms, for example, that 'sometimes in the not-too-distant future, it will be agreed that gravitational waves have been detected' (p. xv); that 'the decision to built two interferometers was the correct one' (p. 733) and that LIGO 'is no bigger than it should be' (p. 734) to mention only a few. Pro-LIGO scientists may agree; anti-LIGO scientists won't. In any case, one usually expects from a constructivist sociologist something like: even though many scientists are convinced that the new apparatus will detect these waves in the near future, it could happen that their prediction will not be confirmed and that an alternative theory will suggest that the properties of these waves are so bizarre that LIGO cannot see them, and so on. But here Collins prefers to side with LIGO scientists. Curiously, and even though the attribution of discoveries is often a contested affair among scientists, Collins does not hesitate to provide 'for scientist–readers' his own 'rough list of inventions' (pp. 556–557), distributing recognition along the way: some ideas 'belong to Weiss', while Drever 'deserves at least half the credit for inventing power recycling', and so on. By distributing symbolic capital among scientists, Collins inadvertently confirms Merton's brilliant analysis of the social function of historians of science:

> recognition is finally allocated by those guardians of posthumous fame, the historians of science. From the most disciplined scholarly works to the vulgarized and sentimentalized accounts designed for the millions, great attention is paid to priority of discovery, to the iteration and reiteration of 'firsts'. In this way, many historians of science help maintain the prevailing institutional emphasis on the importance of priority. (Merton, 1973, p. 301)

In distributing credit, Collins pays tribute to the scientists he came to know personally though he admits that his lists 'almost certainly contains mistakes'. He also admits that this is of course a retrospective reconstruction, as sociologists of science should know, and that this is a 'scientist's accounting rather than a sociologist's analysis' (p. 556). Having become ubiquitous in the field in the course of his analysis, Collins plays the role of the scientist as well as that of the sociologist. In doing so however there is a risk of being perceived as the spokesperson of the LIGO project rather than as a distanced analyst of controversies.

An interesting detail suggests that scientists critical of LIGO did in fact perceive Collins as far from impartial. Analysing the debate between pro- and anti-LIGO scientists, Collins quotes an article in the *New York Times* in which a physicist says that he surveyed seventy scientists on the question and among the sixty who replied they were '4 to 1 against LIGO' (p. 500). In a footnote, Collins explains that he allotted his own pro- and anti-LIGO score to each comment of this survey and found that his 'own subjective assessment of the balance was 2:1 against LIGO rather than the 4:1 reported' by the scientist. He 'was hoping to reproduce these anonymous remarks on the web quote so that readers could make their own judgement, but the survey, [he] found to [his] surprise, was considered a private matter'. He then adds: 'This is a pity and is not an isolated instance of unwillingness among 'anti-LIGO group' to release material evidence to this analyst' (p. 501 n. 11). But why would these scientists be unwilling to collaborate with 'this analyst'? Collins offers no sociological explanation other than finding their behaviour 'surprising', a reaction that makes sense only if one implicitly applies a kind of Mertonian norm of 'organized skepticism' according to which data should be accessible to all the scientific community for scrutiny (Merton, 1973, pp. 277–278). And breaking such norms often generates moral judgements on the part of the members of the community.

Viewed from a distance, which embedding makes difficult, there is, however, nothing really 'surprising' going on here, as the context suggests an obvious sociological explanation. First of all, one has to recall that surveys and polls are typically political tools used by actors in strategies to convince some organizations or an entire population that there is a majority of people thinking like them on a given topic (Champagne, 1990). As a political resource, polls are typically private and used in public debates by those who pay for them or prepare them and there is no strategic reason to give them 'free' to perceived opponents who could then reinterpret them as they see fit in order to deconstruct the original interpretation of the data. So, a sociologist like Collins should not be surprised that actors refuse access to their poll. Moreover, the fact that this was 'not an isolated instance of unwillingness' to collaborate strongly suggests that these actors probably perceived Collins as being on the side of the pro-LIGO scientists. At least they did not perceive him as a 'neutral' agent. The point here is not to determine whether they were right or wrong in their perception and in their decision not to collaborate with a sociologist but, rather, to note that Collins seems to be blind to that aspect of his interactions with them. As he cannot understand why they behaved as they did, he is left with making moral judgements about their behaviour. Given the general tone of the book with regard to LIGO—which is quite positive—it is not farfetched to suppose that those who were opposed to LIGO did perceive a bias and thus refused to collaborate. Like the journalists who were confident they could remain objective despite being embedded within the American troops during the Iraq war, Collins seems convinced that even though he now counts many of theses scientists as his friends (p. 12), these relationships did not affect his analysis. But like many who find this attitude a bit naïve (if not disingenuous) on the part of journalists, one can have doubts about the analysis of a sociologist whose methodology incorporates no device for distancing himself from the subjects with whom he interacts.

6. Mea culpa?

In Chapter 43, near the end the end of the book, Collins looks back on the development of SSK since the 1970s, noting that the very 'success of this way of looking at science was sufficient to inspire some quite radical claims, including some made by me' (p. 792). Since he does not provide any example of those statements, it may be worth recalling some of those 'radical claims': 'the natural world *in no way* constrains what is believed to be' (Collins, 1981a, p. 54; my emphasis). A slightly different (and more ambiguous) formulation is: 'the natural world has *a small or non-existent role* in the construction of scientific knowledge' (Collins, 1981b, p. 3; my emphasis) and, finally, 'the natural world must be treated *as though it did not affect our perception of it*' (Collins, 1983, p. 88; my emphasis). On reflection, however, Collins sees no reason to change anything 'except for a few rhetorical flourishes' (Collins, 2004, p. 797). The problem with this casual attitude toward argumentation is that what Collins seems to consider as 'rhetorical flourishes' were of course read as serious statements by most readers (including scientists and philosophers) who usually take it for granted that writers know their grammar and do mean what they say and what they write. One can even suggest that without these 'flourishes' there would hardly have ever been any 'science war', no 'unpleasant war of words between a group of self-appointed spokespersons for science and the social scientists' (p. 793). Here again a more distanced and more symmetrical view of this 'war' would have made it plain that the 'self-appointed spokespersons for science' were not facing 'the social scientists' as Collins suggests but some self-appointed spokespersons for social science, who were far from representative of 'the social scientist'.[13] Apparently insensitive to the fact that the kind of statements he now considers 'radical' may have played an important role in the 'science wars', Collins now pontificates that the sociology of scientific knowledge 'will disappear as one more academic fashion' unless it 'makes contact with the natural scientists' (p. xvi), and that 'to continue to exist, the sociology of scientific knowledge has to establish the right of certain non-scientists to comment on aspects of science that have traditionally been taken to be the business of scientists alone' (pp. 775–776). Well, after having been among the first to make scientists and philosophers nervous by throwing at them 'radical' statements about the social construction of science, the fact that he does not acknowledge that he may have been part of the cause of this problem does not lend much credibility to such statements. For among 'the social scientists' there are many who never had problems with scientists for the simple reason that they never indulged in the most radical and ambiguous statements that made the 'science wars' possible. While Collins is right to observe that 'there is a danger that social analysis of science will become a private conversation within a narrow disciplinary group' he seems blind to the fact that this observation may apply only to a rather small subset of sociologists of science who have themselves limited their exchanges to their own parochial 'core set'.

Unfortunately, any really fruitful exchange with Collins is made difficult by a fuzzy use of language, which at times makes it impossible to fix any definite interpretation on a given statement. Discussing the way the 'wider social forces' influence science, Collins argues that 'the clearest example in the book is *the influence of the funding* of LIGO on the attempts by the bar groups to maintain their credibility' (p. 785; my emphasis). But as funding is linked to peer review, it is usually seen (even by Merton) as part of the scientific field and not an example of the 'wider social forces'. Concluding that the NSF review committee made it 'politically impossible to continue to fund' bar detectors (p. 446), Collins makes use of such an elastic definition of 'political' that any decision can only be 'political'. But if the concept is to have any teeth, it should be limited to the role of the political field on science as in the recent case where Rep. Joe Barton, a Texan Republican, wrote to the Director of the NSF to request information on scientists funded for research confirming global warming. The same politician also asked the scientists themselves to provide him with their data and computer programs.[14] This is clearly politics, without scare quotes, with its own logic and not a metaphoric use of the term. Nothing is gained in playing on the meaning of terms, except that it may facilitate backtracking. Thus, it is hard to

[13] See for example the contributions in Labinger & Collins (2001). I propose a sociological analysis of the effects of the science war on SSK in Gingras (2000).
[14] See Monastersky (2005).

dispute a sentence like 'as we have seen in the pages of this book, different truths do survive side by side in science, *at least in the short term*' (p. 792; my emphasis). The last part of the sentence makes it impossible to disagree with the first part as no sensible scientist or sociologist would deny that it takes time to get a consensus in science and that a period of transition where doubt is legitimate is bound to happen. And given that the future is by definition open, one cannot predict whether this period of uncertainty will be short (as in the case of high-temperature supraconductivity) or very long (as in the debate over the age of the earth). It is a pity that so much ink has been wasted debating the question of 'relativism' on such (deliberately?) ambiguous formulations and there is no point in discussing this question further here except to say that far from being a good methodological principle, the idea of treating nature 'as though it did not affect our perception of it' (Collins, 1983, p. 88) introduces a major bias since it a priori excludes what should in fact be an empirical question to be decided case by case.

7. Conclusion

As we say in French, *qui trop embrasse mal étreint*, and in trying to do too many things at once, talking to too many audiences at the same time, Collins fails to do any of these things well. Given that arguments are usually tailored to an audience, they can hardly be formulated to convince simultaneously such heterogeneous groups as nonscientists, scientists and sociologists. Nonetheless, as a kind of primary source of information, this book will be useful to historians, philosophers and sociologists of science who want to gain access to the major events in the history of gravitational waves and to the views of the scientists involved, who are quoted at length. Those who do not have the patience to read such a huge book will do well to go directly to the many papers Collins has published on this topic over the last thirty years. But, as should be obvious by now, the book hardly provides a model for writing convincing sociological accounts of scientific controversies.

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