Gravitational waves and scientific realism

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I started following the attempts to detect gravitational waves (GW) using terrestrial detectors shortly after the business started in the 1960s. My fieldwork began in 1972 and I have been pursuing the subject ever since. The first (1975) paper I wrote about GW set out what I would later (1985) call 'the experimenter's regress'. It argued that, on its own, replicating the experiments could not prove whether some claimed phenomenon was real or not. This was because one could not tell whether the replicating experiments had been performed adequately. Because experiment is a skilful business and depends on tacit knowledge, the only reliable way of knowing whether an experiment has been properly carried out is to examine the results but if the right results from such an experiment are what is in question then there is no reliable criterion and the argument about whose experiments were good and whose were poor can go on for ever. In later work I tried to show that Joe Weber's early claims were killed, not just by the negative results of others but by the determined efforts of certain scientists to get everyone to agree about how to divide the whole set of experiments into competent and incompetent.

Over the nearly half-century of terrestrial GW detection work up to 2015 there have been about half-a-dozen claims to have seen the waves with detectors more sensitive than Weber's but all of them have led to fierce arguments and eventual consignment to the waste-bin of scientific history. I've described them in my 2004 book and shown how each of them illustrates the problem of the experimenter's regress. I've also described, in 2011 and 2013, two passages of analysis of fake data, known as the 'Equinox Event' and 'Big Dog'; these were 'blind injections' deliberately but secretly inserted into the huge interferometers to cause the scientists to practice and rehearse their skills in preparation for the real thing. In these cases I showed that the data by itself was not decisive because the analysis rested on a whole raft of philosophical assumptions or taken-for-granted procedures the validity of which could not be proved by calculation or logic so even though the injected data should have been decisive (if fake) the process of reaching an agreement about what it meant involved social agreement.

Though in my early work I thought what I was doing was proving something about the nature of the physical world – that it was a 'social construct' – by 1981 I reached the conclusion that

no such thing could be proved. All I could show was that *Nature* or the data that was taken to represent *Nature*, was not decisive in the short term so long as there were scientists ready to argue against any particular interpretation; data is 'interpretatively flexible'. But whether our long term conclusions are forced on us by something like *Nature*'s 'hidden hand' we do not know. By 1981 the conclusion I had reached was simply that though we could not know whether realism was true or not, we could know that it was fatal to the social analysis of science. The question asked by the social analyst of science is: "Why do scientists believe 'p' rather than 'not-p' and how do they come to this belief?" If the trump of reality is always up the sleeve of the analyst there is little chance that the question will be pushed to the limit because social inquiry can be trumped anytime the analyst fancies: 'Scientists came to believe this because it is true, rational, or whatever.' Thus, since 1981 my position has been 'methodological relativism' in which reality-trumps are not allowed. (See also Collins 2015 for a discussion of this in the context of the scientific realism debate.)

Now, the question is, how does the wonderful event of September 14th 2015 affect all of this? I have already indicated the problem by calling it 'a wonderful event' rather than 'the bit of interpretively flexible data that turned up on September 14th' and I'll now make things worse. I learned of the event on a Monday evening from my regular scanning of the scientists' emails; this was just a few hours after it was first seen. As with most of the other scientists, I at first assumed it was just another of the many false alarms which afflict such a sensitive apparatus. But by Thursday morning I had concluded it was the 'real thing' and I had started to write the book I knew I was going to have to write (*Gravity's Kiss* should be out in the spring). The real thing! How can I say such a thing?

Well, the reason I have chosen to spend more than 40 years immersed with a group of scientists is because I like the scientists and I love the completely crazy project: to try to spot cosmic events by recording changes down to 1/10,000 of the diameter of a proton in a 4 kilometer interferometer arm – something which a large number of scientists said would never work. It was the remote chance that the impossible would be made possible before I died that kept me going along with pleasure in the company of the honest, larger than life, and sometimes slightly mad characters who thought it was worth devoting *their* lives to it. In other words, for most of the time I was taking the role of a native and that is what made the whole thing viable. And when I saw what was happening in the first few days after September 14th I was seeing it as a native and loving it as native – wallowing in realism – a

realism that carried all the way through to the press conferences on 11th of February when the secret was announced to the world: 'We have detected gravitational waves; we've done it.'

But the job of the sociologist, and certainly the job of the methodological relativist, is also to step back – to see the same world from the estranged perspective. Why do people, such as you dear reader, believe any of it? Just why is this gravitational wave detection different from all other gravitational wave detections? And what use would it be to say because 'this time it is real'. The answer has to be in terms of why no-one is doubting (I should say, no-one in the mainstream is doubting). What has happened here is that, in terms of the title of my 1985 book, we have seen 'the order of things' changing; from now on when gravitational wave research is done it will be done against a different background of taken-for-granted assumptions and it will be possible to judge the competence of the experiments by reference to their outcomes. The job of the methodological relativist is to document and try to explain that social transformation without reference to reality.

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