

The binary pulsar and the quadrupole formula controversy

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1 Introduction

The recent detection of gravitational waves by the advanced LIGO instruments (Abbott et al. 2016) has brought the phenomenon to public attention in a way never seen before. Given the long and enormous effort to detect this elusive form of radiation, it is interesting to look back at the reasons why scientists became sufficiently convinced of their reality to pursue this difficult experiment. While theoretical work based on Einstein's theory of General Relativity predicted the existence of the phenomenon, it is hardly surprising to learn that a previous observational result played a central role in convincing scientists and funders that the goal of detection was achievable. In this paper we look at the way in which this observational result, the measurement of orbital decay in the binary pulsar PSR 1913+16, interacted with an ongoing debate amongst theorists known as the quadrupole formula controversy (Kennefick 1999 and 2007). We shall see that the observational result at first sharpened and intensified the theoretical debate, before it became one of the reasons why the debate came to a close.

2 Controversy

The background to the story can be sketched relatively briefly (for a fuller account, see Kennefick 2007). The theory of gravitational waves dates to 1916 with Einstein's first paper on the subject, only half a year after his publication of the final form of his general relativity theory. In 1918 Einstein published a paper correcting a certain error from the paper of 1916, and presenting, for the first time, the quadrupole formula, expressing the rate of emission of gravitational wave energy by a system of accelerating masses. The formula gets its name because, as Einstein showed, the power radiated in gravitational waves by a system of accelerating masses is proportional to the square of the third time derivative of the system's mass quadrupole moment.

When Einstein derived the quadrupole formula it was on the basis of the linearized approximation of general relativity. This permitted him to make the calculation relatively straightforward, because in the coordinate system adopted by him

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the linearized equations of gravity take on a form which is directly analogous to the Maxwell equations for electromagnetism, a theory in which the role of radiation was, and is, reasonably well understood. But, since general relativity is a non-linear theory, this linearized approximation can hold only for very weak fields, which specifically excludes systems, such as a binary star system, which are held together by their own gravitational interaction. Since it is only this type of system which (as far as we know today) might be capable of producing detectable gravitational waves, this approximation leaves something to be desired as far as sources go (keep in mind that we are still awaiting the first evidence of gravitational waves from anything other than a binary system). It is thought to be ideal for the study of gravitational wave detectors however. The question then is, does the quadrupole formula give a reasonable approximation of the source strength of possible astrophysical sources of gravitational waves, especially binary stars?

Aspects of this question were debated vigorously from the mid-1950s to the mid-1970s during the renaissance of General Relativity (a period defined by Cliff Will 1986). By the mid seventies most theorists accepted that binary star systems did generate gravitational waves, but whether the quadrupole formula could be correctly applied to them was still doubted by some experts. This quadrupole formula controversy, by that time, appeared to be showing signs of coming to a close, in that the remaining skeptics were obliged to object, from time to time, to comments made by other theorists which treated the problem as largely settled (Havas 1973).

What is interesting about the role of the binary pulsar in this story is that there are good grounds for believing that its primary role was to stimulate the controversy into new life. It is usually thought of as the agency by which the controversy was settled (and this is certainly a role which is of interest to this paper), but another possible reading is that it actually made the controversy more prominent and more contentious and that this served, with time, to bring it to a conclusion by focusing the attention of theorists upon it. One might speculate that we are dealing with a controversy downsizing principle, in analogy with the problem of cosmic downsizing in extragalactic astronomy, which revolves around the observation that over time quasars come to have smaller and smaller black holes. Since black holes should only ever grow in size, it is claimed that this observational effect arises because the big ones have already used up all their fuel and “turned off”. The situation is thought to be similar to that which obtains for stars, where the larger stars, which paradoxically contain more fuel, burn the fuel at a far faster rate and live a much shorter life than do less massive stars.

In the case of scientific controversies we may similarly expect, at any given moment, to find many more small and almost moribund controversies than strident ones, because the former will be more long-lived. The fuel which is only slowly consumed in a small controversy is not the number of issues to be debated. I agree with those who think such points are all but inexhaustible. The fuel is the number of potential participants in the controversy. Where the number of participants is low, each of them may feel comfortable conceding a long period of debate to what is a manageable number of colleagues. As the number involved in the controversy rises, the ability to mediate the controversy by direct personal relations between all participants is strained. The consequences of remaining on the fence become less predictable as they become potentially more serious, since more people involved means potentially more influential people having a vested interest in the outcome. The participants come under pressure to take a definitive position and tend to do so more quickly. To continue with the analogy, the fuel is more quickly processed through the various stages, from open minded participant, to committed protagonist, to close-minded ideologue, at the end of which no further debate is possible. In essence, the controversy which burns most brightly extinguishes itself most quickly. To be sure, I am merely taking

a long-established piece of folk wisdom and dressing it up in academic clothes. The phrase “slow-burning controversy”, already nicely encapsulates the image I am trying to convey.

So let us examine briefly the course of the quadrupole formula controversy in the 1970s. We have already summarized the debate over whether binary stars could emit gravitational waves, a debate which flourished in the late fifties and early sixties. There then followed a period in which it was regarded as settled, by a large majority, that binary stars did undergo radiation damping as a result of gravitational wave emission. The detail of how this occurred was perhaps not regarded as a terribly pressing problem, given that no one was familiar with any known astronomical systems which, according to the quadrupole formula itself, would undergo a measurable decay in their orbits. The state of affairs bore a close approximation to the situation in controversies which have passed the point of crystallization, which is to say that even though there remained some who doubted the consensus opinion that the quadrupole formula was approximately correct, their views did not receive much public airing. In fact, however, it was still possible for their views to be aired, the problem was simply not important enough for major notice to be taken of anyone’s views on the matter.

A thorough account of the views of the skeptics as to why confidence in the wide applicability of the quadrupole formula was misplaced is given in chapters 9 and 11 of Kennefick 2007 (for a more concise account see Kennefick 1999). Briefly, by the early 1970s quite a bit of work had been done by theorists to try to show that binary stars did radiate (to leading order) according to this formula. But skeptics objected that the calculations which had been done lacked mathematical rigor. To quote the abstract (in its entirety) of what might be called the manifesto of the sceptics, a 1976 paper by Jürgen Ehlers, Arnold Rosenblum, Joshua Goldberg and Peter Havas

It is argued that a formula for the energy loss due to gravitational radiation of bound systems such as binaries has not yet been derived either exactly or by means of a consistent approximation method within general relativity, a view which contradicts some widely accepted claims in the literature. The main approaches used to obtain such a formula are critically reviewed, and it is pointed out that the derivations presented so far either contain inconsistencies or are incomplete.

Very few exact results have been published in the history of gravitational wave research, so the most relevant part of this abstract is that decrying the lack of consistency in the approximation schemes used. These schemes typically involved expansions of quantities in powers of v/c where v is a velocity (for instance of one of the stars in the binary) and c is the speed of light. As long as v/c is small, higher order terms in the expansion are ignored. But, objected the sceptics, no effort was made to check whether coefficients in these neglected higher order terms might not be large enough to offset the small size of v/c . In general there was no attempt at error control at all. Physicists objected that, for most stellar binaries, v/c was a truly tiny number, much smaller than in many other calculations of physical interest where they were accustomed to be just as cavalier in their approach. Furthermore mathematicians like consistency in their approximation schemes (as Ehlers et al. advertise above) and this means truncating all terms at the same order in v/c . But physicists are prone to occasionally truncating some quantities at higher orders than others, if they feel that these quantities are more physically relevant. In short, at least part of the argument is whether you feel that the calculation should be conducted according to the reliability standards of physics or mathematics. Of course Ehlers, Rosenblum, Havas and Goldberg were all physicists, but they came from a branch of physics, General Relativity, which was much closer to mathematics than other branches were. Indeed in some Universities the relativists were housed in the mathematics department. Ehlers

and company might have preferred to say that they were upholding the standards of BOTH math and physics before they were confident enough to quote a result, but this irked some of their colleagues, who observed that none of the four were prepared to give a definitive answer to the problem themselves, but were willing to critique the efforts of others!

To give, briefly, two examples of approximations which alarmed the skeptics, one would be the use of point masses to represent the two stars in the binary system. Of course the calculation is greatly simplified by pretending that all of the mass in the system is concentrated at two points in space, but it is known that tidal effects (amongst other things) are missed when doing so. Most astrophysicists would argue that these tidal effects would be very small unless the two bodies were very close, but relativists responded that such confidence came largely from experience derived from Newtonian calculations not General Relativity itself. Another issue concerned the need to use different kinds of approximation to describe the motion of the two stars and to describe the behavior of the wave far from the star. Some way of matching between these two solutions was needed, in order that boundary conditions on the waves could be unambiguously and correctly applied to the motion of the stars. Indeed, as argued in Kennefick 2007, discrepancies between many early calculations of gravitational wave emission from binary stars are probably traceable to just this failure to impose proper boundary conditions. The fact that these were just two of many issues which were debated means that calculations which the sceptics would have applauded on one ground they would criticise on another. Thus what seemed like an impartial attempt to move the field forward to the sceptics, may have seemed like incessant and insatiable nit-picking to others.

One important bone of contention (which both the issues already mentioned fed into) can be described in the following way. Since it is the accelerated motion of the two stars in the binary which is expected to generate the gravitational waves, many physicists felt that it was appropriate, since the motion of binary star systems was well studied, to describe such motion and then calculate the waves which would be thereby produced. The sceptics pointed out, however, that such motion schemes were not demonstrated to be actual solutions of the Einstein equations (Ehlers et al. 1976). It was possible, for instance, to calculate the motion of some binaries by assuming the absence of gravitational radiation and then put that motion into the equations and find out what radiation would be produced by it. But since the motion would be modified by including the radiation, there was a logical inconsistency involved in this approach. Essentially the history of the theoretical endeavor was of a long slow process of painstakingly altering calculational schemes to address various objections, with different researchers insisting on different levels of rigor before they were satisfied that the answer was known to some reasonable level of approximation. From the point of view of the controversy, a key question is, when does the debate end? When everyone is satisfied? Or is it legitimate to cut off this discussion when some participants would rather it be continued? In analyzing the history of this debate, I introduced the concept of the Theoreticians' Regress (Kennefick 2007; modelled on earlier work on the Experimenters' Regress by Harry Collins). This describes the dilemma confronted by theorists whose calculations fail to agree. Because the calculations are complex, finding errors in them is an open-ended process which can lead to debates of arbitrary length. Since the most reliable method of testing a calculation is to see whether it yields the correct result, it is difficult to evaluate the claims of competing calculations when the result is itself at issue. In such a situation one must often fall back on one's expert assessment of the abilities of the theorists themselves. Since you may naturally be more apt to trust a theorist whom you know better, it follows that one's social network may have a bearing on one's view of which calculation is yielding the correct scientific answer.

A good example of the status of the debate on the eve of the discovery of the binary pulsar is the June, 1973 Paris meeting on gravitational waves at which Peter Havas (a European emigré to the United States who specialized in the radiation problem) gave a talk outlining his view that the question whether binary stars did emit gravitational waves at all was still unsettled, and advancing his critique of the main calculations which agreed with the quadrupole formula result (Havas 1973). In the conference proceedings, two of the remarks in response to Havas' talk can be regarded as sharing his skepticism, two as disagreeing with it, and two as neutral (at least phrased in a neutral way). This certainly suggests not only that Havas had leave to raise such issues with his peers, but also that he had an audience part of which, at least, was sympathetic. At the same time, the problem was not at the forefront of theoretical concerns at that moment. It was not considered irrelevant or uninteresting, after all the very fact of the conference being held at all suggests otherwise, but the fact that no astrophysical applications had been discovered certainly lessened its urgency.

Within little over a year the situation was transformed completely.

3 Discovery

Pulsars were discovered in 1967 by Jocelyn Bell and Tony Hewish using the Interplanetary Scintillation Array at the Mullard Radio Astronomy Observatory near Cambridge, England. It quickly became apparent that pulsars were a real-life instance of a long standing theoretical entity, the neutron star, which had been first proposed by Walter Baade and Fritz Zwicky decades previously, in 1933 (see Haensel et al. 2007, pp. 2–4 for a brief history). The problem of gravitationally collapsed objects became of greater theoretical interest following the discovery of quasars by radio astronomers in the fifties and was further stimulated by the pulsar discovery. By the early seventies only a few dozen pulsars were known, and Joe Taylor of the University of Massachusetts, together with his graduate student Russell Hulse, proposed to do a computerized search for them with the large Arecibo dish in Puerto Rico to provide a much larger ensemble of discovered objects. It was a specific aim of Taylor's proposal that such a large number of pulsars might feature one which was part of a binary system (Hulse 1997). This would permit the measurement of the mass of the pulsar, a topic of immense astrophysical interest, since the very idea of neutron stars had arisen following the work of Subramanian Chandrasekhar on the limiting mass of white dwarf stars. That a close binary neutron star system had been suggested as a possible source of detectable gravitational waves as early as 1963 by Freeman Dyson was almost certainly not on Taylor's mind as he began his pulsar search. This was all the more true since Dyson's suggestion had been made in the context of a proposal that arbitrarily advanced alien civilizations might construct such systems for the purpose of interstellar navigation.

In early July 1974 Hulse, down at Arecibo, recorded a pulsar, just barely strong enough to be detected by the system, unusually sensitive for its day as it was, whose position on the sky automatically baptized it with the name PSR 1913+16. After confirmation that this was indeed a pulsar, including measuring its period, Hulse recorded the word "fantastic" on his observing record, referring to the fact that the pulsar had the second shortest period known at that time. At this point he had no notion that it was in a binary system, only the rotational period of the neutron star itself had been measured, not its orbital period. The only foretaste of what was to come was that subsequent attempts to confirm that rapid pulse in these first observations did not agree, to Hulse's frustration. He even went so far as to cross out and erase these subsequent attempts from his log (Hulse 1997).

In late August Hulse returned to this object, in a routine way, to try to confirm its period. As before he found that its period kept changing with each measurement. Indeed, by a curious coincidence, he found that he almost repeated the same set of measurements each time the pulsar came overhead at Arecibo (the dish at Arecibo is so large it is built into a small valley, and thus cannot observe very far from the zenith of the sky). This would turn out to be due to the fact that the pulsar binary has an orbital period of just under 8 hours, and thus completes a little over 3 orbits with every rotation of the Earth. It did not take Hulse long to convince himself that he had discovered a pulsar in a binary system, and it was immediately clear to him and to his advisor Taylor that they were dealing with an extraordinary system. An eight hour orbital period represented an orbiting system involving massive objects with an unprecedentedly small physical separation from each other. Indeed word got around quickly about the new discovery, to the extent that the first theoretical paper commenting on the binary pulsar appeared in late 1974 (Damour and Ruffini 1974), while the discovery paper itself appeared only in 1975 (Hulse and Taylor 1975).

There can be little doubt that interest in the radiation problem from binary stars was reinvigorated by the binary pulsar discovery. Here was a real world example of a system where radiation damping might actually be measurable. Of course there were doubts expressed, on the theoretical side (Damour and Ruffini 1974) that the effect really would be measurable, but the experimenters were nevertheless not ruling it out. In an interview Joe Taylor recalls his own view at the time (interview conducted by the author by phone on 2nd May, 2008) . . .

The person who put us onto that was Bob Wagoner. It happened that once the news was out and it became public that this thing was there and that we were observing it, I responded to a number of invitations to go and give talks about it and ended up making a grand tour around North America where I made five or six stops and one of them was at Stanford and Bob Wagoner there actually gave me his paper predicting the orbital period decay to carry back with me since he knew I was going to be at Harvard a couple of days later and I handed it to Alex Dalgarno the editor of ApJ Letters. So it was Bob's paper (Wagoner 1975) that I first began to take seriously and to recognize that with the current state of the art then, in October 1974 of doing pulsar timing, it was clear that, if his numbers were right, and I assumed they were, it would take us a number of years to see any effect, but not an unreasonable number and if we could improve the timing accuracy a little bit it might happen even sooner and that's more or less what happened.

While relativists were excited about a number of tests of general relativity which could be made for this system whose components were moving under the influence of unprecedentedly strong gravitational forces, it seems that the measurement of the binary pulsar orbital decay came significantly earlier than most people expected, as Taylor agrees (interview, 2nd May, 2008):

I think that's right and that's largely because at that time it wasn't yet recognized that doing really high precision timing of pulsar signals was a very important goal.

Nevertheless the possibility was in the air from late 1974 onwards, and the fact that it would take a significant amount of time gave the theorists ample time in which to apply new techniques and increased effort to the problem of analyzing the orbital evolution of such a system as it responded to its own gravitational wave emission.

To what extent was this activity on the theoretical side visible to the experimenters? Given that their result, when available, was likely to have a decisive effect on the controversy, it is remarkable that they went totally unaware of it until they

finally had a result to announce. This announcement was made, in its earliest version, at the ninth Texas Symposium on Relativistic Astrophysics in Munich in 1978 (Taylor and McCulloch 1980). The Texas series of meetings had a tradition of announcements of important observational results. The first Texas meeting had been held in response to the growing interest in quasars as new objects discovered by radio astronomers in the late fifties (Robinson et al. 1965). Taylor’s talk in Munich is one of the more celebrated of the announcements made at this series of meetings (interview, 2nd May, 2008).

Well, I’ll tell you when I first even knew that there was any debate, was at the Texas Symposium in Munich.¹ And so somebody asked me a question, well let me back up just a little bit. I was scheduled to give a paper there on something like the second or third day of the conference, and Jürgen Ehlers, who was one of the conference organizers, recognized that somehow not getting to this until nearly the last day of the conference was not a good idea. So he asked me to get up and say just a few words about it in a session on the first day so that at least people would know what I looked like and we could talk in the halls, and so forth, afterwards. So I did that and I basically gave the result² and said I’ll give all the details at the scheduled time the day after tomorrow, or something like that. Somebody then in the audience asked a question, I don’t remember who it was, ‘when you say that you have seen the period decay and it agrees with the prediction, what prediction are you using?’ And I sort of was blind-sided by that. I just thought that everyone knew how to calculate this, except maybe me. And so I think I must have stood there wondering how to answer for a minute and Tommy Gold, who happened to be the session chairman, whispered in my ear, ‘Landau and Lifshitz’, so I said it’s given in Landau and Lifshitz. So that more or less is what transpired. I mean, I remember having conversations later with people about it and I began to realize that, of course, that was just sort of an heuristic formula and the calculation wasn’t even derived, I guess, in Landau and Lifshitz, it was given as an exercise for the student to do.

It is humorous to note that Gold, the session chairman, had been, with his collaborator Bondi, one of the early skeptics concerning whether binary stars could emit gravitational radiation. Although Gold would certainly have been very familiar with Landau and Lifshitz’ treatment, he might also have been inclined to agree with Bondi’s comment (to the present author, quoted in Kennefick 2007), that it was very “glib”.

So once Taylor was apprised of the existence of the controversy, what was his reaction (interview, 2nd May, 2008)?

So ok, so I was aware then that there was a controversy about it. Whenever I quizzed theorists, that I knew pretty well, about it, they tended to be people like Kip Thorne, for example. Kip always said, ‘oh yes, you know, we’re still worrying about the mathematical details, but we know it’s right.’ And my impression was that, I think pretty much I gained the impression that you convey to a large extent in your book as well³, that the more mathematically oriented physicists, and particularly those who had been doing relativity in mathematics

¹ At this point on the interview recording, the author can hear himself say ‘Really.’

² As quoted in Weisberg and Taylor 1981, the binary pulsar decayed at a rate of $(-2.5 \pm 0.3) \times 10^{-12}$, compared to a value predicted from the quadrupole formula of $(-2.38 \pm 0.02) \times 10^{-12}$.

³ A reference to Kennefick 2007, illustrating one of the problems faced by an oral historian who wishes to write books and continue doing oral histories!

departments, were still concerned about the lack of rigor and the full mathematical beauty, but the physicists like Thorne and Feynman and others just had little patience with that kind of concern and wanted to get on with it and see what you could do with it. And they more or less told me ‘don’t worry about it.’

So communication between theorists and experimenters contained this interesting feature, that a reasonably lively controversy amongst the theorists could be completely invisible to the experimenters. Obviously the controversy was not one which consumed the total energy of theorists in the field, but it still involved a good deal of back and forth and even a dedicated workshop, during the period in question, and yet no mention was made of its existence within Taylor’s hearing. Partly, as Taylor says, this was because of the kind of theorists he was talking to. In the field of relativistic astrophysics, there were people close to the astrophysics end of the spectrum, and people closer to the relativity end, and Taylor, as an astrophysicist, was naturally more likely to talk to those on the astrophysics end. Since those theorists were less likely to be skeptical of the quadrupole formula, they naturally chose not to bring up any caveats about the derivations which they felt were unlikely ever to have a bearing on the observations underway. Furthermore, and this bears on a point I will try to bring out at the end of the paper, they may have felt some slight embarrassment that there existed theorists in their field who still doubted the canonical understanding of gravitational radiation in general relativity.

4 Trading zones and pidgins

In his book *Image and Logic* Peter Galison (1997), one of the pioneers of the careful micro-study of physicists in action, argues that different groups of scientists, in particular experimental and theoretical physicists often speak different technical languages and encounter difficulty in communicating with each other. He argues that, in such situations, physicists find it useful to develop a pidgin, a term used to describe a secondary language, formed usually from a mishmash of other languages, used to facilitate trade between different peoples. Galison describes the conceptual space between different groups of physicists as a trading zone and discusses the use of pidgins, which in his usage may refer to particular mathematical constructs designed to permit experimenters and theoreticians (let’s say) to discuss and compare the predictions of the latter with the results of the former.

The binary pulsar is an interesting case to observe the possible need for trading zones, since it was a discovery by radio astronomers who had, otherwise, relatively little contact with relativists interested in gravitational waves. At the same time their field had arisen alongside the broader culture of relativistic astrophysics, which was formed by a first contact between radio astronomers and relativists after the discovery of quasars. To what extent do we observe the need for a trading zone between experimenters and theorists in our particular story? Certainly there seem to be areas of physics in which theorists and experimenters talk to each other regularly and apparently freely, and it is certainly also true that when physicists, even from very different subject areas, converse, they speak a recognizable technical language which seems to be quite unconscious of boundaries. Indeed, for the physicist, the international, inter subject quality of physics speech is one of the defining experiences of being a physicist (no doubt the same may be true for scholars in other disciplines). Nevertheless there is some evidence, in the case of the binary pulsar story, supporting the model put forward by Galison. One promising way to understand how scientists deal with trading zones, when and if they occur, is through the notion of *interactional expertise*, a concept which describes the ability of someone to talk intelligibly and usefully

to an expert about their field, even if they are not (yet) capable of working in that field, which would be full expertise (Collins et al. 2007). It may be that, even where physicists lack direct expertise to work in a neighboring field, they at least possess interactional expertise to talk with their fellow physicists in that field.

Let us begin with the discovery of the binary pulsar in 1974. The two astronomers involved, Joseph Taylor and Russell Hulse, both received educations fairly typical of astronomers of their generation in that they were educated primarily in physics (in fact Hulse was still a graduate student when he discovered the binary pulsar). In this context, particularly as the two men were working in radio astronomy, astronomy is conceived of as being more or less a sub-discipline of physics, albeit an unusually ancient one which still maintained a certain level of institutional independence. As such they took courses in general relativity, a subject within physics which was typically considered an optional higher level course, but one which might be especially relevant to those planning to specialize in astronomy. As radio astronomers interested in pulsars, relativity theory was clearly relevant to an understanding of the source of the signals they planned to study, but not nearly as relevant and routine as the physics of the electromagnetically based detectors and instruments they operated.

Accordingly Joe Taylor describes one of his first actions on discovering that he had a binary pulsar with a uniquely close orbit involving unprecedentedly intense gravitational interaction between the two components (interview, 2nd May, 2008).

We'd both taken the obligatory, or almost obligatory, relativity course in University, as part of our physics training, but neither one of us was very deeply into relativity. My wife was much amused when one day, this was when I was at the University of Massachusetts, of course, I said I don't have to teach today, I'm going to drive into Boston and visit the Tech Coop. And I spent the day in the MIT bookstore and came back with a pile of books, Weinberg, and Misner, Thorne and Wheeler and all the other ones that you would imagine. She was much amused that I spent the next few months deeply engrossed in these books.

So certainly the astronomers felt a need to get up to speed with the elements of relativistic orbital motion (the books referenced are Weinberg 1972 and Misner et al. 1973). To what extent was there a language gap between them and the practitioners of this discipline? Partly the gap was a social gap. Neither Taylor nor Hulse habituated amongst relativists and therefore did not partake in their discourse. So Taylor went unaware of the ongoing quadrupole formula controversy, throughout the time when, as we would be tempted to say today, he was determining the outcome of this controversy.

But leaving aside this question of discourse, when Taylor and his collaborators did speak to relativists, could they make themselves understood and be understood? Clearly they could, for the most part. But some obstacles were encountered. By the time Taylor and company were dealing with the orbital decay of the binary pulsar, Hulse had finished his doctorate and moved on. A collaborator with whom Taylor published many of the early papers announcing and discussing the orbital decay was Joel Weisberg. Weisberg does recall language difficulty playing some modest role in talking to theorists, before they found a long term collaborator in a talented young French relativist, Thibault Damour (interview conducted by the author, by phone, on 24th February, 2000).

It's interesting, we had a failed attempt to work with one person. And I think the problem was he couldn't talk well enough to experimentalists. He couldn't give us results that were easily interpretable by us, whereas Thibault could. It was quite interesting.

Weisberg describes the kind of theorist that would be helpful in the process of theory testing using the binary pulsar data, saying "it had to be people who could talk

a language I could understand.” Regarding the one failed effort mentioned above, the problem had a very practical aspect, “he [the theorist] couldn’t give us specific things to test.” At the same time he emphasizes that their eventual collaborator, Damour was “brilliant” and “made fundamental progress”, so “it wasn’t just a language thing.” He adds (in a private communication) that the “theorist ‘speaking the right language’ was not, by itself, enough for a successful collaboration.”

Nevertheless, to examine the “language thing,” I suspect it is fair to say that, in the absence of a relativity community, Taylor and Weisberg would have been capable of performing calculations to establish the predictions of certain theories (though their case was a particularly difficult one, given the strong fields associated with neutron stars, so whether they could have carried on the calculations while pursuing their observational program is certainly open to doubt). In fact, as we shall see, they did contribute original work on the theory side. The problem seems to me to be legitimately a question of language and society, in the sense that Taylor and Weisberg’s problem was not primarily that they lacked the expertise to do the calculations. That much they could have acquired, and did acquire, with time and effort (but again, the kinds of calculations which would have satisfied skeptics like Ehlers would have been especially challenging). What they lacked was fluency in the language spoken by theorists, and social standing within the discourse of theory. The existence of theories to test is inextricably linked with the existence of theorists who developed them, who have a vested interest in the testing. Since the theorists are the experts, it is understandable that the astronomers, like Taylor and Weisberg, would feel distinctly hesitant about publicly putting forth calculations in an area that was not their own realm of expertise. We get a sense of this in their 1981 paper announcing the orbital decay result, where they cagily refuse to be drawn into the controversy over the validity of the quadrupole formula.

We are also aware that some relativists hold Einstein’s quadrupole formula, which underlies the calculation of energy loss rates in [this paper], to be invalid for gravitationally free-falling systems [such as binary stars]. Obviously the dispute about what the theory actually predicts must be resolved, but the present experimental situation does not by itself seem to demand any changes.

It is also worth noting that there was one other advantageous aspect to Taylor and Weisberg’s eventual collaborator Damour, in addition to his ability to speak their language and his “brilliance.” Damour’s views on the quadrupole formula controversy were similar to those of skeptics like Ehlers, and therefore his calculations went to great lengths to address many of their stated concerns. To quote from his 1983 review paper

In 1979 Taylor, Fowler and McCulloch reported the observations of a secular acceleration of the mean orbital longitude of the binary pulsar PSR 1913+16: i.e. in other words, a secular diminution of the time of return of the periastron ... While this effect had been qualitatively and quantitatively predicted on the basis of the above-mentioned heuristic argument, it had not been validly demonstrated to be a consequence of Einstein’s theory; on one hand because the detailed calculations were not complete enough to control all the terms of the equations of motion and were plagued by mathematical inconsistencies, and, on the other hand, because the methods of calculation did not apply to a system, like the binary pulsar, containing “compact” objects ... with very strong self gravitational fields.

Since, as we have seen, experimenters have better things to do with their time than to be drawn into arcane theoretical debates, it is important that the calculations which are done by theorists are not black boxes whose inner workings are totally

opaque to the experimenters. It is important that the results of these calculations can be couched in a form which deals with observables pertinent to the actual measurements being made. The need for what Galison would describe as a pidgin helped to produce the parametrized post-Newtonian (PPN) framework as a tool to mediate the theory testing process. The PPN framework is a way of expressing results from the Newtonian theory with correction terms based upon the post-Newtonian approximation of General Relativity, with the addition to each term of a parameter. Each parameter can be defined differently if one is using a different modern theory of gravity. Thus a theory can be expressed in terms of these ten parameters which will permit anyone engaged in a weak-field (solar-system) test of gravity to quickly determine which theory makes what prediction about that quantity.

The PPN formalism is important because the theory-testing process requires an alliance of theorists and experimenters. Theorists made predictions based on their calculations. Experimenters made measurements which were then compared to the results of the calculations. But some theories have very few published results which experimenters can test. The PPN framework demands only that the parameters have been worked out for a given theory for experimenters to be able to determine what the prediction for that theory would be in the case of the particular test they have in mind. This PPN framework had been widely used during solar system tests of general relativity, but was ill-adapted to the binary pulsar case because it presumed that the gravitational fields involved were very weak. Nevertheless a somewhat similar, but much less general (focusing as it did upon the case of gravitational radiation emission) parametrization was established which facilitated the theory testing aspect of Weisberg and Taylor's 1981 paper. To quote from Clifford Will's paper on the subject (1977)

Because of the complexity of many alternative theories of gravitation beyond the post-Newtonian approximation, we have not attempted to devise a general formulation analogous to the PPN framework beyond writing equation (2) with arbitrary parameters. However, we can provide a general description of the method used to arrive at equation (2), emphasizing those features that are common to the theories being studied.

So given the existence of a pidgin to create a trading zone between astronomers (and others) interested in doing theory testing and gravitational theorists, why did the astronomers shrink from commenting directly on the quadrupole formula itself? One obvious answer is that the pidgin was not designed to facilitate such a conversation. It permitted comparisons between calculations derived from different theories. It was not designed for the more complex and open-ended task of critiquing subtle details of such calculations. Another answer is that the barriers were as much social as linguistic (the two must obviously be linked). The astronomers felt they lacked the social standing to weigh in on a question which obviously fell within the purview of the theorists. Because the controversy over which calculation within a given theory was the correct one depended on subtle judgments, it naturally required the expertise of the practicing theorists. This is precisely the meaning of the Theoreticians' Regress, that it depends on subtleties of expert judgment and not on some closed algorithmic model of how to perform a calculation.

5 Skeptics' dilemma

I have argued that the closing of debate in the quadrupole formula controversy occurred at least partly because of the quickening effect caused by the binary pulsar increasing the importance of the controversy. At the same time, the lifetime of the controversy, once the binary pulsar data became available, was greatly constrained

by the existence of experimental data which bore directly on the topic at issue. For the theoretical controversy to continue indefinitely, there would have to have been a significant effort to contest either the experimental evidence or the interpretation of it. The fact that there was no such significant attack on the ruling interpretation of the binary pulsar data certainly limited the lifetime of the controversy, so it is interesting to look at the reaction of the skeptics to the work of Taylor and his collaborators.

In any problem of orbital mechanics there are many mechanisms which might account for all or part of an observed change in orbital period. That even the most famous agreements between theory and observation can be challenged in this way is shown by the saga of Robert Dicke's efforts to measure the oblateness of the Sun (the degree to which its shape departs from a perfect sphere). Dicke had pointed out that if the Solar oblateness turned out to be significantly different from zero, its gravitational influence on the orbit of Mercury would throw out the close agreement between the prediction of General Relativity and the observed perihelion advance of the planet Mercury (Dicke and Goldenberg 1967). As with the case of the Mercury Perihelion, the binary pulsar data seemed particularly impressive because it agreed with the prediction of the quadrupole formula with little or no need to take into account of other factors. The interpretation was that the system was very "clean." The corollary to this, naturally, is that any evidence that the system was not so clean would throw out the agreement. Given this opening to challenge the *interpretation* of the binary pulsar data, it is interesting that the gravitational wave skeptics were not involved in proposing alternative mechanisms.

Certainly there were those who considered it, amongst them Peter Havas and, very likely, his former student Arnold Rosenblum. They were to the fore in demanding that the observations not be accounted a successful test of general relativity given that (in their opinion) the quadrupole formula had not been shown to be a valid prediction of that theory. Joe Taylor recalls that certain people were particular about this question of terminology (interview, 2nd May, 2008).

Well, let me think, the people who kept bugging me about it, so to speak, were Peter Havas, Fred Cooperstock and Arnold Rosenblum. Arnold bugged me about it a lot. Anyway, they just kept saying 'Look, even though you have an experimental number now, we're not even sure what the theoretical number is and you can't go around saying that you've confirmed something.' So I tried to remain outside of the argument, letting the theorists fight it out until they all . . . persuaded one another. So that seemed to be the best thing for me to do and we were simply concerned with getting an experimental result that we were happy with.

The alternative scenarios to the gravitational wave interpretation were actually put forward in print, but generally not by the skeptics. This may have been because the skeptics found themselves in a similar position to the experimenters. They had a vested interest in the debate, but lacked the special expertise which would have permitted them to comment. Likely dissipative mechanisms (or even non-dissipative ones) fell within the purview of astrophysics rather than relativity, and were explored and commented upon by astrophysicists rather than relativists.

The most important issues which had to be dealt with in demonstrating that the observed decay agreed with the quadrupole formula prediction was the nature of the unseen companion in the system, and the relative acceleration of the binary pulsar to our solar system. If the unseen companion was a sufficiently compact object, like another neutron star (which is now firmly believed to be the case) then it would undergo little deformation as a result of the visible pulsar's tidal effect. But if it was a normal star, it would develop a marked oblateness which would in turn create a perturbation

in the orbit of the pulsar (a tidal friction-like effect) which would be difficult, except over longer timescales, to distinguish from the orbital decay due to radiation damping. Effects of this type would, however, have affected other measurements made in the system, and with time the experimenters became convinced that the system was extraordinarily clean. As Taylor and McCulloch (1980) stated in their paper from the Texas Symposium

If one were given the task of designing an ideal machine for testing gravitation theories, the result might be a system rather similar to PSR1913+16; an accurate clock of large mass and small size, moving at high speed in an eccentric orbit around a similar object located in otherwise empty space. To be sure, one would place the system somewhat closer to the Earth than ~ 5 kpc, or would arrange for a more powerful transmitter to convey the clock pulses to terrestrial telescopes; but we cannot expect Nature to be concerned with the inadequacies of our instrumentation!

This sense of wonder at the sheer serendipity of coming across such a system (many relativity theorists had sworn for decades that no system would ever be found in which gravitational wave effects would be measurable) was brought into focus for me after the more recent discovery of the “double pulsar” a system with an even closer orbit than the original binary pulsar, in which both pulsars are visible from Earth. I have heard this system referred to as “a relativistic astrophysicist’s wet dream.”

Taylor and McCulloch’s comment illustrates the three main technical challenges in creating a match between theory and experiment for this system. First, the system must be in empty space. The presence of interstellar gas, for instance, would certainly alter the orbit of the system with time, as a result of dynamical friction. A related issue would be if the pulsars themselves were blowing off material at a significant rate, in which case the mass loss would affect the orbital motion. Secondly, as we have seen, both objects must be compact objects, such as neutron stars, so that perturbations due to the failure of the bodies to behave as point sources can be ignored. As a corollary to this, if the system contained a third massive object, this would obviously also affect the orbit of the two known components. Finally, the object should be close to us, not only for reasons of detection, but because a more distant object is in a more different orbit around the center of the galaxy and would be accelerating more strongly with respect to us here on Earth (for a list of references and discussion of a number of these issues, see Damour and Taylor 1991).

It is a well known result of special relativity that systems which are in inertial motion with respect to each other have clocks which run at different rates. If the systems are accelerating with respect to each other, then their respective clocks will alter, with time, in their relative rates of running. Since the solar system and the binary pulsar system are in different orbits around the galactic center they are not in the same inertial frame with each other. Accordingly the sensitive timing which is required to measure the orbital damping effect is also capable of measuring the relative accelerations of these two systems. In so far as doubt persisted about the validity of the quadrupole formula, this was a bad thing. Indeed, at one point during the 1980s, it did happen that the analysis of measurements of the binary pulsar did fall out of agreement with the quadrupole formula, by a much smaller amount than had been at issue in the earlier theoretical debate (in so far as that debate had ever been completely quantified). A close analysis of the relativistic theory of timing between the two systems, carried out by Taylor in collaboration with Thibault Damour, showed that the discrepancy could be explained on the basis of fully accounting for the timing issues (Damour and Taylor 1991).

Ultimately, as Taylor recalls, the situation reached the point where, if one *assumed* the validity of the quadrupole formula, one could make an accurate determination

of the position of the binary pulsar in the galaxy, based on its relative acceleration. This measurement was more accurate than was possible by other methods at that time. This makes as good a moment as any to mark the end of the quadrupole formula controversy. When a prediction turns from a thing to be tested to a tool to be used, the debate is surely closed (and this, of course, goes some way to explain the impatience of non-skeptics to achieve that moment of closure). It is a mark of the importance of the controversy that the measurement of the distance to the galactic center which could have been provided by the binary pulsar data never became a canonical one, though it is in agreement with subsequent measurements using other techniques.

As Damour and Taylor put it in 1991

If we assume that the standard general relativistic framework . . . is valid we see that, in a few years, the measurement of \dot{P}_b^{obs} (the rate of decay of the binary pulsar's orbit) can be turned into a measurement of . . . the galactic constants R_o (the distance from the Solar System to the Galactic center) and v_o (the speed of galactic rotation at about the center at the position of the solar system) (especially v_o , which presently contributes the biggest uncertainty). Such a "pulsar timing" measurement of v_o would be free from many of the astrophysical uncertainties that have plagued other determinations.

Since the Taylor-Hulse discovery, subsequent binary pulsars have been found where the relative acceleration of the two systems does not permit a particularly accurate determination of the rate of orbital damping. Had the controversy persisted so far this might have provided some opening for skeptics. However the discovery of the double pulsar in 2003, a system in which both pulsars are oriented so that both their radio beams are visible from the Earth, has provided a system with even stronger orbital damping than the original binary pulsars, whose results are in agreement with it.

How much interpretive flexibility was there for skeptics to continue the controversy? This has been a bone of contention in the field of science studies, where gravitational wave physics has been the subject of long term sociological analysis by Harry Collins (Collins 1994, 2004), some of whose conclusions have been challenged by the philosopher Allan Franklin (Franklin 1994). Did the skeptics largely abandon the fight because, as Franklin would have it, they were rational actors or, as Collins would have it, they had run out of sociological space in which to continue the argument? I suspect both considerations played a role. A rational actor will certainly take sociological considerations into account when determining whether to continue a debate. Most physicists do not wish to face social ostracism, even in a cause they believe to be right. At the same time any social constructivist will agree that the ruling out of certain arguments as work in the field progresses, the limitations placed on interpretative flexibility in the ebb and flow of debate, can tax the ingenuity of even the most stubborn skeptics to the point at which they give up the struggle. The social struggle can become unequal in a double sense, in that skeptics are both outnumbered and outmaneuvered by their opponents. Whether the maneuvering was all in vain, given the inevitable verdict of nature is, of course, an interesting question, but not one that is trivial to answer by the historian's method.

That skeptics *considered* continuing the battle is clear enough. Although Fred Cooperstock did retire from the fray for a decade or so after the mid-eighties, he subsequently put forward a new argument that gravitational waves would not propagate energy through empty space. The failure, for a numbers of years, of the new generation of gravitational wave detectors like LIGO, to detect gravitational waves passing by the Earth, provided some opening for skeptics like Cooperstock. He and others put forward arguments that the existing theory is correct for *sources* like the binary pulsar, but fails for *detectors* like LIGO, thus explaining why evidence existed for gravitational waves binary neutron star systems, even though as we have not,

as yet, detected them⁴. The specifics of these new skeptical arguments vary widely⁵. It is important to distinguish between the scepticism of professional physicists like Cooperstock, and the irreconcilable objections of amateurs who focus on the sheer expense of the detectors which, they claim, can never succeed in detecting anything. These amateurs are prone to claim that the recent detections must be fraudulent in some way. Their arguments are not engaged in any way by the professionals in the field.

Peter Havas, when I interviewed him in 1995, certainly spoke of the openings he believed had existed, at least for a time, for an attack on the standard interpretation of the pulsar timing results. He still entertained significant doubts about the consensus which had emerged at that time. Joe Taylor reports that Havas, and his student Arnold Rosenblum, did ask to see some of the data and that he sent them a magnetic tape containing some (private communication). When he asked them a year later whether they had made progress they indicated that they had been distracted by other problems. Nevertheless, a search for Arnold Rosenblum's papers on the SAO/NASA Astrophysics Data System server shows that, from the mid-eighties, after several years spent on his calculations of gravitational wave emission that did not agree with the quadrupole formula, he then devoted a number of papers to the problems of relativistic timing in orbital and binary systems. Although none of this series of papers referred directly to the binary pulsar, they are strongly suggestive that he had spent a considerable amount of time thinking about this issue, leading him into that field⁶.

Therefore we can say that the skeptics considered a foray against the conventional interpretation of the binary pulsar data, but decided against it. One can say that the physics of the situation obliged them to react this way, in that they felt they could not overturn the hard empirical evidence provided by the binary pulsar data. But one can also say there were sociological reasons. They were not in a position to do their own experiment to challenge the data, because they lacked the standing in that field which would have permitted them to enter it with any hope of success. For starters they would never have been granted time on a radio telescope to do their own measurements of this system (one group of astronomers did do some independent timing measurements of the binary pulsar, guided by data supplied by Taylor, and concluded that Taylor and his collaborators were correct in their results on the orbital decay, see Boriakoff et al. 1982). Even worse, in so far as the interpretation of the data could be challenged by theorists, it was by astrophysicists with experience in the study of stellar binaries and pulsars, not by relativists experienced in gravitational waves. Thus from a professional point of view the skeptics were in a double bind which, combined with their increasing isolation within their own community, as the debate moved towards a final resolution, prevented any kind of continuation of the public debate. Whatever private doubts were held by a few theorists about the reliability of the existing calculations, the empirical result was regarded as beyond dispute. The final option open to the skeptics, arguing that Taylor had simply got it wrong, was undoubtedly not entertained because of the outstanding reputation which Taylor enjoyed within the astrophysics community for his careful and painstaking work.

In the case of the binary pulsar replication demanded access to radio telescope time to look at the same system or, better, the discovery of an independent system.

⁴ We cannot hope, with current technology, to detect the gravitational waves emitted by the known binary pulsar systems. It is only when such systems reach their terminal point and spiral into each other and merge that Earth-based detectors can hope to observe them.

⁵ A sample of modern gravitational wave skepticism is given by the following references: Cooperstock 1992, Bel 1996 and Aldrovandi et al. 2008.

⁶ Arnold Rosenblum died tragically young in 1991 (Cohen et al. 1991).

But, as we have seen, subsequent systems were often not as ideal for this experiment as the original. Not until the discovery of the double pulsar can we be said to have a fully comparable replication of the original, so one can certainly speculate that there may have been some scope for further controversy in the decades between 1980 and the early years of the twentieth century, had there been sufficient sociological space to support such a debate. But while logical space for disputation may have remained, the skeptics had run out of sociological space. Indeed, there is every reason to believe that the field of gravitational wave physics could ill afford to permit such a controversy to linger for that amount of time, lest it put its own disciplinary standing at risk.

6 Conclusions

We have seen how the measurement of orbital decay in the binary pulsar helped to convince physicists and others that gravitational waves were real and possibly detectable. The fact that the results vindicated long-standing predictions of the theory was also vital in establishing confidence in the theory underpinning planned detectors such as LIGO. Even though social boundaries and possible differences of language prevented the observers, Taylor and his collaborators, from directly declaring that the skeptics were wrong, their experimental work did have the effect of successively reducing the social space in which they might have continued their objections to the use of the quadrupole formula. However, as I show elsewhere (Kennefick 2007), this closing down of the contested terrain was accomplished not just by the increasingly accurate experimental work but by increasingly sophisticated theoretical work. Additionally close collaboration between the observers (Taylor and Weisberg) and one of the theorists (Damour) played a role in bringing the controversy to a close. As we have seen, a good boundary point marking the shift from controversy to post-controversy was the moment when one might make important discoveries (the distance from Earth to the center of the galaxy) by assuming that the quadrupole formula was true. When a contested result becomes, in its turn, a tool for research, then any lingering skepticism receives short shrift from most workers in the field.

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