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Authorship in Great Pulsar Discoveries

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Abstract. This is a slightly embellished "transcript" of the after-dinner speech presented at the January 2004 Aspen meeting on binary pulsars which should really have been called the meeting on the double pulsar. Here I recall my experiences in the discovery of some of the great pulsars of our time, including the double pulsar, which I am sure will rank as one of the most important discoveries in pulsar astronomy.

1. Introduction

Having agreed to give after-dinner speeches in Amsterdam and Crete, my initial response when Fred (Rasio) asked me if I would speak at the conference dinner was to decline, on the grounds that I had run out of material. There were, of course, the secret stories, those brought up at 1 am after one too many drinks, usually concerning authorship and who discovered what when, or how it was relayed to others. But there was my life to consider, and the safety of my children.

I told Fred I needed a theme or topic. His response was: "How about authorship in great pulsar discoveries?" I was nervous. Is there a secret "code of conduct" among pulsar observers that "what happens in the collaboration stays in the collaboration", that hastily-composed emails and fights should never be publicly disclosed? Could I give an honest recount of some of the greatest pulsar discoveries of our time without my own personal bias creeping in? Do we all tend to use an inverse square law to weight our own contributions? What chance would I have of getting peer-reviewed publications and telescope time in the future?

But, there was a story or two to tell. I wavered.

Fred added that he'd throw in $100. I thought about it for a while, and thought, "what the heck!" I said I only wanted the $100 if it came out OK, so he agreed that there would be a ceremonial hand-over of the check upon conclusion of the "paper" if it met with his seal of approval. I knew no alcohol should pass my lips that night.

2. The Nervous Start

My students tell me that my cycling tights were not really appropriate attire for the slightly formal evening, but when in Aspen, skiing must take priority! As I approached the podium there was an uneasy silence at several tables around the room. The smell of fear rose in my nostrils. What had I got myself into? I needed to put the room at rest, after all this was *supposed* to be funny! But as I scanned the tables I could tell this was potentially a *huge* mistake. I couldn't make eye contact with the chief protagonists that were to feature in my stories. The first joke fell flat, my heart quickened. I announced the topic, "Authorship in great pulsar discoveries." Some laughed, others froze and I decided I needed an out clause. But there was no going back.

3. The Speech

I considered carefully whether to accept Fred's invitation to give the after dinner speech tonight, and had told him that I needed a topic. Fred suggested, "How about *authorship in great pulsar discoveries*?" And so that is the topic of my talk tonight.

Oohs and aahs from the audience.

Before I begin let me say that whatever you are going to think of us at the end of this speech, let me just say that whatever we are doing, it is working. This meeting has been the celebration of a great discovery built upon great discoveries over the past 30 years. This is a field that refuses to die, and gathers together brilliant scientists from all over the world to fantastic locations like this for exciting intellectual discussion. Oh, and I am not going to mention "*the planet*."

Enormous relief from what might be described as a "senior" table.

I want to take you back in time to a time when there were only five binary pulsars, when pulsar magnetic fields decayed with a timescale of 5 Myrs (didn't they Andrew), when certain Indian theorists referred to the apparent floor to the magnetic field strengths of millisecond pulsars as "saturation," when Shri Kulkarni was like a bright young postdoc who sat in the front row at pulsar conferences, when Jim Cordes used scintillation velocities to show that pulsars were only travelling at 90 km s^{-1}, when the luminosity of a pulsar was still proportional to B^2, when the electron layer had infinite extent in the most popular model, when "radicals" (and there have been other descriptions) like Wolfgang Kundt refused to believe that PSR B1913+16 was recycled, and when Ed van den Heuvel looked exactly like he does today. The time was May 1986, and I was in the first few weeks of my PhD eager to meet the *Pulsar Gods* and their followers who had assembled in Nanking, China for an IAU symposium.

As an undergraduate, I had been given a paper on the binary pulsar for my Honours thesis, and was keen to talk to some of the names I had encountered in the literature. Chief among my heroes was Ed van den Heuvel. After floundering in what might be termed my "introductory reading" on the topic of binary

pulsars, case A mass transfer, Roche lobes, supernova, asymmetric kicks, Kelvin-Helmholtz timescales, I elevated Ed to "*pulsar God*" status for his beautiful cartoons that illustrated how you actually get a binary pulsar! Without these cartoons, which have been cut and pasted into 100 papers, I think I never would have completed my undergraduate education.

Anyone who was anyone appeared to be at this meeting. I had a great time putting faces to the names of the people involved in the great discoveries and theories I'd been studying: Backer, Taylor, Lyne, Srinivasan and Radhakrishnan to name but a few. I soon learned that first-authorship on a great discovery was worthy of some merit and could open career paths to those fortunate enough to achieve the coveted "gold medal" position.

I also learned that not everyone in pulsar astronomy was that keen to talk to your average enthusiastic first-year PhD student. When I approached the great Indian pulsar astronomer "Rad," I told him that I wanted to talk to him about pulsar velocities. His first response to me was, "A lot of people think they know a lot about this subject, but they don't. Let me give you an example, and if you can answer this question I will talk to you, otherwise I won't. If a $1.4\,M_\odot$ neutron star is orbiting a $4.2\,M_\odot$ pre-supernova star, and the $4.2\,M_\odot$ star explodes symmetrically and leaves behind a $1.4\,M_\odot$ neutron star, the two stars fly apart. In the original rest frame what is the angle between the final velocity vectors of the two stars?" After much fumbling and coaching from the author of the famous "Radhakrishnan and Shukre" paper on pulsar velocities, I reached the answer. Today Rad and I are good friends. It was just his way of finding out whether you were worth talking to.

Collaboration is the key to success in many areas of pulsar astronomy, and in my experience this works well with a small number of people. Once the number exceeds some critical threshold, and crosses international boundaries and time zones, the collaboration goes from being one of helpful hints and scientific exchanges to an "every man/woman for himself/herself" situation once the scent of a great discovery is in the air. The minute something wonderous appears, everyone becomes keen to prove their input was either pivotal for the discovery through some brilliant insight or the sheer number of hours worked. Depending upon seniority, one might hear the phrase "culmination of years of planning," or "without my X this would not have been possible." Although sometimes cast as the villains, senior members of collaborations have the joy of ordering their ambitious underlings as they vie for their position on the discovery paper, which I suspect is a pain that exceeds many a long night at the telescope, the construction of pivotal pieces of software, or hours in front of the computer.

It soon became clear to me that the way to fame and fortune was to get involved in pulsar surveys and timing. My now good friend Simon Johnston was painstakingly surveying the Southern Galactic plane during my PhD. After my second two-week stint at "helping out with observing"—which meant observing for 16 hours a day with a skeleton crew—I dared venture the suggestion that some lowly position on some of the results might be a worthy reward for my time. I was told that I would need to do more than "a bit of observing" to warrant authorship. OK, I was learning the rules.

Rule # 1 Four weeks at a telescope is not sufficient for authorship if you are a student.

Around about this time, the so-called 47 Tuc A and B pulsars were revealed at a colloquium at the Australia Telescope by Jon Ables. 47 Tuc A was particularly exciting, with an orbital period of just 32 minutes and a spin period of 4.48 ms. The publication of these "pulsars" came about because the *might of the Jodrell Bank Filterbanks* was about to descend on the Parkes radio telescope. This was the genesis of what has become the most prolific collaboration of modern times, the Jodrell/ATNF/Bologna effort (now expanded into more institutions than I care to list, and often affectionately referred to as "the mega-collaboration" because of its size and power).

At this time in history, a collaborative effort existed between the Tasmanian pulsar group and Jon Ables, which had a "friendly rivalry" with Manchester and his collaborators. The ATNF/Tasmanian block thought they were sitting on the most exciting pulsar of all time (47 Tuc A), but, unfortunately for them, it was rarely visible at a high level of significance. Knowing that the filterbanks from the North were imminent, they were faced with an agonising decision: do we go public or chance Lyne & Manchester finding the wonder-source? At this point they used an old trick later perfected by the Caltech group: they placed an IAU telegram announcing the discovery! In most areas of astronomy, this means "Please observe this source as soon and as often as possible." However, in pulsar astronomy, it means "We discovered this source and we want the rest of you to know that, so piss off and leave it alone." It is also customary to give just enough information to make your claim without adding anything really that useful, e.g., only give the period to a few decimal places. (This technique backfired horribly once when the Ter 5A pulsar was published in a telegram only for people to actually start observing it, publishing a Nature paper prior to its actual discoverers).

Rule #2 When in danger of losing your discovery, put out an IAU telegram. There are no referees!

Since my supervisor, Dick Manchester, was from a rival group from the discoverers of 47 Tuc A, he failed to even mention it to me when we next met. The pulsars had a dispersion measure of about 65 pc cm^{-3}, and when I was leaving Australia, the Parkes dish was spending a lot of time on 47 Tuc. Jon Ables had proudly told me that they had essentially asked for every hour available on 47 Tuc that term. Just prior to my departure Ron Ekers had organised a workshop on the pulsar, and Dick and I soon found ourselves involved in a vigourous debate concerning the pulsar's validity with the discoverers.

Sitting at my terminal one day at Goddard, I received an intriguing email from Simon Johnston listing every gory detail of the discovery of 47 Tuc C (at a dispersion measure of 24.5 pc cm^{-3}!!) by Dick Manchester. Simon recounted that he felt privileged to see Dick so happy sitting in his office holding the "period-dispersion measure" (pdm) plot in his hand.

When I arrived at Jodrell Bank, my first task was to port the search code used on the Convex to work on the Alliant supercomputer at Jodrell Bank. Having completed this, a young student "Clive" had the task of searching the vast

amounts of filterbank data for additional pulsars in, amongst other clusters, 47 Tuc. With two pulsars at a DM of 65, and only one at 25, there remained the possibility that 47 Tuc C was a foreground object. In Australia from time to time we have an expression "man's man," which we usually reserve for men that are 6 foot 4 inches tall, responsible, handsome, natural leaders and fearless. The best way of describing Clive is to say that he was kind-of the opposite. His great love was ABBA music, he was terrified of Australian native fauna, could not climb the ladders at Parkes and loved to giggle. Nevertheless Clive possessed a great quality, and that was for discovering faint pulsars in 47 Tucanae through mind-numbing examination of pulsar "candidates." In fact Clive was so good at sniffing out pulsars that it wasn't long before 47 Tuc D was discovered at the same DM as 47 Tuc C. The fate of 47 Tuc A and B appeared to be sealed, but soon a much more serious battle was looming, and that was about the authorship list on the 47 Tuc D paper.

Now, sensing that if you could find C and D, why not E, F and G? Soon long hours at the computer on both sides of the world were in place searching for more 47 Tuc pulsars as every member of the collaboration was keen to ensure their "contribution" was noted. Clive proved to have a keen nose for pulsars, claiming several, but rather than let our young gun have it all his own way, soon frantic emails started crossing the globe with the Jodrell and ATNF groups each vying to have the claim on discovering the most. By the time 47 Tuc "M" was claimed, it was time to publish. Clive thought he had a good chance at "the silver," but he managed a credible bronze. I soon witnessed how people could work when something big was on. Sadly, in our haste, it was not realised by anyone that 47 Tuc K was in fact the third harmonic of D. This was discovered by Clive some time after the publication appeared, and in fact was revealed to me in the same week that a certain planet disappeared from view for the last time.

One sunny afternoon I came to the Parkes telescope having been soundly asleep after being on night duty. I was observing with my good friend Simon Johnston and student Jon Bell. As was customary, we were taking data on a large number of projects. I came to the control room and asked what was happening. Jon, in his ever-pleasant manner informed me that as I had slept Simon had just discovered a binary pulsar in the SMC. Before us was a plot showing period as a function of MJD, and although Simon's periodicity was plausible, it didn't look quite right to me. Simon and Jon headed off to lunch and I lunged at the data. As fate would have it, I had just completed a new dazzling orbit-fitting GUI to an old Jodrell standby, "fitorbit." I quickly recognised the $\tan(\theta)$ signature of an eccentric binary, and before they returned from lunch I had become the first person to know that PSR J0045−7319 was an eccentric binary with e near 0.8!

At this point nothing too sinister was going on. The telescope was trained on the SMC at the first available opportunity, and via telephone we had turned "Luch" Nicastro into a virtual tape robot back at Epping. New arrival times were popping out as the backlog of old data were reprocessed and it wasn't long before we had the emphatic signature of an eccentric binary before us that was correctly predicting the period changes we were witnessing. We were however involved in what might be termed "minimalist information exchange" to our

future co-authors. This is a common practice when one stumbles on a discovery. It is incredible fun to be the first person on the Earth to know something unique. For many of us the lure of the science outweighs sinister objectives, but before long one realises that revelation of the discovery can lead to a sudden surge of interest from otherwise bored collaborators, all keen to maximise their share of the glory. By maximising one's input before the others know what is happening, when it comes time to decide the authorship list, one might naively think that you have a competitive advantage over your colleagues in the collaboration, thus ensuring a large slice of the glory. This ignores the potential backlash that can ensue.

The follow-up to PSR J0045−7319's discovery was swift with everyone who was unfortunate enough to have been absent for the discovery keen to "contribute." Dick had the fun task of deciding authorship, and our "burst of research" and "inspirational discovery" status was factored in and balanced against our radio silence which might have cost one of us the "gold."

My student Jon Bell was the only one of us with sufficient optical expertise to try and locate the companion, which we knew had to be several solar masses. There were only two possibilities: a B star or a black hole! Jon's initial reports were intriguing. He claimed there was only one star that had any chance of being the companion, and it appeared much too faint. None of us bothered to check his calculations, and before long the draft Nature letter announcing the discovery of the first black hole–pulsar binary was being feverishly written. We had a single night on the AAT to get a spectrum of the potential optical candidate, and we decided that if we were rained out the Nature letter would go ahead anyway. Fortunately for us, the skies above Siding Spring cleared and a B star was revealed at the location of PSR J0045−7319. Had the letter gone in, being left off it might have been a great blessing!

Late in the Parkes 70 cm survey the eclipsing binary pulsar PSR J2051−0827 was discovered. I had the honour of attending the first session after its discovery where the orbit was to be determined. All we knew from the original 2.5-minute observation was that it was very short-period. Early data revealed an eclipse but was also well fit by an eccentric orbit of only 2.4 hours in duration. This millisecond pulsar was looking like the most exciting thing for years! There was no shortage of volunteers for the late shift that evening. Vicky Kaspi and Nichi D'Amico trained the dish and started taking data. The potential glory of an eccentric pulsar made it difficult for me to sleep. At 4am I rocked up with Duncan Lorimer at the telescope ready to take over from the rather exhausted crew. Such was the excitement that the initial e$\sim$0.1 solution had cultivated that the eccentricity was no longer a free parameter! With fresh eyes I looked at what appeared to me to be a perfect sinusoid with a rather ill-fitted eccentric orbit going through it. So tired were the "up until 4am" crew—no doubt because they'd been fitting orbits all night without success—that Nichi said to me, "go on Matthew put us out of our misery." I set e to zero, hit the fit button, and it was all over. The lesson here was that it is easier to see glory where it isn't than vice versa.

My tale ends with a somewhat more recent discovery that I was fortunate enough to be a close witness to. On a recent trip up to Parkes I found myself on the

Newell highway on a rather smelly bus with a mobile phone and too much time on my hands. I knew my old Masters student and friend Duncan Lorimer and his wife Maura were at Parkes, so I decided to give them a ring to see if someone was going to pick me up from the bus and just see how things were going.

Duncan answered the phone. Yes, someone was picking me up. He soon cut the small-talk. "I've made a bit of a discovery," he said in his normal laid-back, understated manner. "I've been testing some search code that I've been writing to barycentre binaries, and I wanted some short-period binary to test it on." There was a lot of umming and aahing. Each word was measured. I could tell he didn't want to incriminate himself. "As we'd taken some timing data for Marta (Burgay) I thought I'd see if my code worked on 0737," he said. Then, a pause. "So it boomed in and I had this thought that I'd just set $a \sin i$ to $-a \sin i$ and see what happened," he said. I pondered the enormity of this statement while Duncan paused.

I think it is fair to say that Dunc's popularity wasn't at an all time high after the discovery of 0737. An expert on pulsar statistics, he'd already calculated the number of 0737s in the Galaxy with his collaborators in the US prior to publication, even though "in theory" he was not part of the discovery team. With the original discovery paper arriving at a referee who was collaborating with Dunc it all got messy when she announced that she and Dunc had their own Nature paper in preparation. With Nature insisting that there could be only one paper on 0737 collaborations were born that might not have otherwise existed! Dunc found himself on the discovery paper after all but not without some loss in popularity.

He continued, "So I looked at the candidates and this 2.7 s pulsar was there!" I gasped. "That is HUUUUUGE!" I exclaimed. "That's amazing. This is one of the greatest pulsar discoveries of all time! You're an absolute legend!" After drawing breath I continued, knowing the recent history, "And, you are in all sorts of trouble! What on Earth were you thinking?", I said. We pondered the enormity of his discovery. Before long I lost the signal on my mobile. Every time I came back into range I rang back until my batteries were dead—just as well the Nokia 8250 has a lot of talk-time! My mind was racing with plans, but not only was I not even remotely part of the discovery team, the discoverer wasn't either! At that point there were only two people in the world who knew the first double-pulsar had been discovered, and by the time I arrived at the telescope Maura had had the secret shared with her. Our problem was that in theory Dunc was not supposed to be accessing any of the data. The ATNF doesn't allow people to just point the dish at random objects, and given his history on this object, he was in an awkward position. He was basically doing the 0737 discovery team a favour by observing for them, but somehow I didn't think that they were going to be eternally grateful when he told his story. It wasn't long before Dunc and Maura had a timing solution for the B pulsar, and had the mass ratio etc. There was no doubt this was a big discovery! Commemorative photos were taken. I felt privileged to be present. Although I'd done almost nothing I was very happy to see my former student make such a pivotal discovery.

We talked through his options:

In option #1 we could just do nothing and wait to see how long others might take to discover the second pulsar. It wasn't particularly faint, although it was only visible for a fraction of the orbit. We might conceivably go to the grave with the secret! This had the advantage that nobody would want to kill him.

In option #2 we could tell no-one but get access to some data for some other (admittedly bogus) reason, find the B pulsar in it before anyone else and be famous on a cosy few-author publication!

In option #3 Dunc could just confess.

I advised him that honesty was probably the best policy. He emailed his boss and informed him of his discovery. Instructions came back that he should "tell absolutely no-one."

Early the next morning Andrew Lyne rang from Manchester. We exchanged small-talk for about 15 minutes before I said, "Andrew, I know your dark secret." He asked me to tell no-one about the B pulsar, which I managed to do for at least a month.

At this point, not even the first Nature paper was published. Was it going to be pulled, or was this a good opportunity to get two publications for the price of one?

Dunc, Maura and I pondered where Dunc might end up on the "B-pulsar" discovery paper. Would he be unexpectedly rewarded with the gold medal position as a token of undying gratitude by the 0737 team? Was the bronze a fairer outcome? Or, would he be expelled from the University of Manchester and forever barred from the Parkes telescope? Would people descend upon Parkes from all continents to steer the dish at 0737 to stake their claim on follow-up observations?

As an interested observer to this process, I could see that none of these questions were black and white. Was Dunc just an innocent scientist who "just happened to set $a \sin i$ to $-a \sin i$?" Or, in the back of his mind was the temptation of potentially finding the "B" pulsar when sitting in front of the raw data with newly-crafted software too much to resist? How would have I reacted if presented with such a dilemma? Had I not campaigned hard for a place in history on the ill-fated planet pulsar paper? I was glad not to have been faced with the temptations before Dunc with the addition of a single minus sign. After all it was only one ASCII character!

Fortunately I was not involved in any of the subsequent discussions about authorship. I can only imagine the email quantity. Within days of the Nature publication appearing, the GBT was besieged with applications to observe the double pulsar, which we saw at this meeting. More moral dilemmas arose. Was this a legitimate use of the rapid response science program? I myself was very glad that these observations all took place. Without them this meeting would not have been able to marvel at the scintillation or polarimetric results, and this meeting would not have been as interesting. I congratulate all of those involved in the discovery and follow-up of this amazing double-pulsar system, especially Marta and Duncan. May there be many more great discoveries in this exciting field!

4. Epilogue

Some time after we left Parkes I realised how Dunc could have ensured his place in history. He could have issued an IAU telegram that just explained what happened:

IAU telegram #0737. Lorimer, D., Mc Laughlin, M. [and Bailes?!]

A 2.7 s pulsar companion has been discovered orbiting the binary pulsar PSR J0737−3039 during an innocent software test at the Parkes 64 m telescope. The 2.7 s pulsar shows curious behaviour as a function of orbital phase.