Remembering David Band and his time at UCSD

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Introduction

I became ill yesterday (July 9, 2009) and am unable to attend the Symposium. I want to thank the symposium organizers for making my presentation. To keep this simple, I will not use any graphics.

David worked on the BASTE project for 9 years at UCSD, and made many important scientific discoveries. My presentation will address this period of David's career.

Hiring David

I hired David in the fall of 1990 to join our group at UCSD and play a lead role in our work on the BATSE project. David worked in our group for 9 years until the BATSE funding wound down. Then he accepted a position at Los Alamos and his family moved to Santa Fe.

Before I hired David, our group led the BATSE Spectroscopy Detector design and built all the BATSE detectors. Now that the CGRO was about to be launched, it was time to staff up for mission operations and data analysis. Preparations for these had proceeded for several years, with the BATSE team members at MSFC and GSFC leading the work. I needed an energetic scientist that would hit the ground running, quickly absorb what had been done at MSFC and GSFC, and dive into the data when it became available.

I was impressed by the broad range of topics of David's papers and how he interpreted data from a variety of instruments, such as radio and optical telescopes, to try to figure out the fundamentals of various objects in astrophysics, without a particular bias toward any class of objects. It seemed to me that David was a very curious guy that knew how to get pretty much all there was to get out of the data. Plus, he had an engaging personality and liked discussions on almost any matter, so he would be a good addition to a university-based research group like ours.

Summary of David's time at UCSD

We offered David a postdoctoral position, which he accepted and then proceeded to work on getting his family moved to the vicinity of UCSD. I remember that getting approval for more moving expenses than UCSD offered was a bit tricky. I think David may have received some advice on how to work the system from his father who was university professor. They quickly bought a house near UCSD where they lived their entire time in San Diego. This was a big change from Berkeley where they had lived, and I recall Debbie inquiring about Berkeley-like culture, such as ethnic restaurants and funky shops. Alas, there wasn't much of this near UCSD. I think Debbie eventually fond a way to cope. Plus she was occupied with two young sons, her business, and a community of friends. There were numerous parties at their home that I fondly remember.

At UCSD, David quickly became an integral part of our group and our research department, the Center for Astrophysics and Space Sciences or CASS. For many years, David ran our group's Thursday Research Discussion. This was a forum for scientists at all levels, from graduate students to distinguished visitors. Under David's leadership, this became attended by all of CASS, not just our research group. Then it expanded to become CASS's regular Wednesday research discussion, which contributes significantly to the intellectual vitality of CASS.

David supervised Lyle Ford, one of our graduate students. Lyle received his PhD in 1996 with work on burst spectra. Lyle was one of our best graduate students and had a faculty position shortly after receiving his degree.

David's curiosity and engaging manner led to him being involved in a number of research activities with scientists outside the BATSE team. Plus, he published and presented frequently, so he became one of the go-to guys if you wanted to try out your pet theory on the latest BATSE data. Here are some of the "non-BATSE" people he worked with.

Dieter Hartmann on flash photo-ionization and the "no host galaxy" problem

Anthony Crider and Edison Liang on burst spectral evolution

Hye-Sook Park and many others on optical counterparts

Ehud Cohen, Jonathan Katz, Tsvi Piran, Re'em Sari on relativistic shocks

David also did original, sole-author work on burst repetition, V/Vmax, cosmological time dilation, and spectral evolution.

Large body of work

Throughout his career, David produced a very large body of published work. During his 9 years at UCSD he had 32 refereed papers, and he was the lead author on 17 of them. Due to this high level of achievement, David rapidly rose up the ladder in the "Research Series", UCSD's analog of "Research Professor". In 1991 he became an Assistant Research Physicist, and then moved to the Associate level in 1994 and to Full Research Physicist in 1998.

David's work on BATSE

Now I will move to David's work on the BATSE project.

Our primary BATSE scientific objective was to use the Spectroscopy Detectors for studies of time-resolved spectra of bursts from ~10 keV to the highest energies we could measure, about 50 MeV. Time scales down to tenths of seconds would provide good data when the fluxes were high. The detectors were designed for very good sensitivity and energy resolution. Individual photons were analyzed into energy channels that preserved the resolution from ~10 keV to several MeV. This allowed searches for spectral lines at any energy as well as testing theoretical models' spectra against to the data. For example, to check for low-energy cutoffs, high-energy breaks or hard components, and how they vary. As it turned out, the data were well ahead of the theories, and so many of the results were not compared with theoretical predictions. However, the measured time-resolved relationships of intensity to spectral shape revealed fundamental information on the emission processes and geometries of the emitting regions -- observational results that successful future theories will have to predict.

During the previous decade or so, "cyclotron lines" at 20 - 60 keV and spectral variability up to ~1 MeV on ~ 1 second time scales had been reported and these results looked convincing. It was widely appreciated that they provide basic insight into the physics of the burst processes and the host objects. So, thanks to the CGRO and BATSE, we were about to move into a data goldmine, where the great results could be anticipated with confidence.

Well, it wasn't quite this way. As it turned out, cyclotron lines were not real, but merely statistical blips in data. On the other hand, the spectral variability was a fertile area of work. I will discuss some of David's work on these topics and then close.

The search for cyclotron lines and the "SLED"

As I said earlier, cyclotron lines had been reported in bursts at 20 - 60 keV. These were interpreted as due to magnetic resonance scattering in the extreme magnetic fields of neutron stars, several times 10 to the 12th gauss. Such lines had been seen in X-ray spectra from objects known to be galactic neutron stars; Hercules X-1 is the prototype example. Indeed, since about 1980 our group had done some of the definitive observations of the line and how it varied with the neutron star's rotation. This neutron star interpretation of the Her X-1 line had become widely accepted, and thus it is not surprising that burst lines were added to the neutron star band wagon. Before the CGRO launch, my analysis in one of our proposals showed that the previous instruments could not have seen weaker lines than they reported, but that with the improved capabilities of the Spectroscopy Detectors we should see lines in many more bursts. Maybe they were ubiquitous I wondered.

This was the environment David was moving into.

I think the psychology of what happened next is interesting, and David was heavily involved, so I'll share some of the inside story you may not have heard.

In mid-1991 the data started coming in. Right away there was a spectacular intense burst with excellent time-resolved spectroscopy. We knew we'd find lines soon, maybe within a month. Spectra came to us quickly and frequently, but David didn't see any lines. Nor did anyone one else in the team. Lines should have been jumping out at us in about 20% of the bursts. This wasn't happening. Every few days we'd look at the latest strong burst. No lines! David's office was beginning to bulge with thousands of individual time-resolved spectra, but no lines.

David and others from the team would go to scientific meetings with nothing to show. Everyone was getting involved: Bonnard Teegarden, David Palmer, Michael Briggs, Rob Preece. Brad Schafer, maybe Geoff Pendleton and Bill Paciesas, and several programmers, and perhaps a few others I forgot. And coming up with nothing.

By now it had become sort of a scandal. What's wrong with BATSE? What's wrong with those Spectroscopy Detector guys?

I remember one meeting where Alan Bunner button holed Michael Briggs and me at our poster and said, "Where are the cyclotron lines? What's wrong with you guys?" Maybe the second part is not quite right, but the message was clear. I'd put it this way, "We at headquarters want to hear about those lines you are supposed to be discovering".

To make matters worse, the original analysis software was turning out to be difficult to use in the search for lines. So UCSD and MSFC each went their own way for a while to try to develop something simple and easy to understand that could be used to say "yes" or "no" to the existence of a line, any line. Then we'd work out the details on statistical significance and equivalent width later. These efforts were soon merged into a joint development by Briggs, Preece, Band and Lyle Ford, a UCSD graduate student.

Early in the mission David started looking closely into a glitch or feature in the spectra in the lowest part of the energy range, just below where the cyclotron lines should have been. There was a deficit of counts with a peculiar structure, sort of like a line. Right away we determined it was an electronic effect. It altered the detector response kernel from what we expected and made spectral fitting difficult. Although David was not a lab guy, he developed a good understanding of how the detector and electronics work and figured out the cause of the effect. He named the problem the "SLED" for "Spectroscopy Detector Low Energy Distortion" in a seminal paper in 1992 that laid out all the gritty details. The SLED produced a dip in the spectrum that looked a lot like a cyclotron line. After that time we operated the detectors in higher gain so the SLED feature was at too low an energy to masquerade as a line or to have much effect on spectral fitting used to search for lines.

It was sinking in on us all that the lines were just not there and the earlier results were wrong. Maybe lines were rare, but how rare. Also around this time BATSE's Large Area Detectors were producing directions and logN-logS results that were putting bursts all over the sky and outside the galaxy. The galactic magnetic neutron star hypothesis we had been working with was inconsistent with this scenario.

The coin had flipped and now it wasn't much fun anymore. Instead of going to meetings with lots of interesting lines, our job had morphed into placing a limit on <u>how rare lines are</u>. Since lines can be at any energy, any equivalent width, or any time-slice during a burst, the tests of all the possibilities was a massive undertaking. And it had to be done for every bright burst. As I recall, this work was shared by David, David Palmer, Michael Briggs, and Rob Preece. Apologies to anyone I skipped.

Within the team, there were long reports and long telecons about critical analytical minutiae, f-tests, and rejection at a part in a million and whether this was good enough since we were looking in a parameter space that had millions of cells. The one spectrum in a thousand that had a dip that might be a line was subjected to unrelenting scrutiny. The dilution effect of having so many candidate time slices and so on was profound. It was killing the few cases that looked good.

Somehow David kept his head through all this, as did the others as far as I know. David decided to veer off into Bayesian statistics to really nail the problem from a statistical analysis perspective. Now he was beginning to be out there on his own, because this is such an arcane world that few of his colleagues, or peers for that matter, probably really understood his papers. But, it was getting published in refereed journals and presented at many conferences, so I wasn't worried that it would eventually go down in flames.

Around this time I came to appreciate something fundamentally good about David. He really did understand all this stuff, and wasn't intimidated by numerous intricate details. Complex ideas and equations that ran half a page didn't bother him. This is true about the equations -- look at some of his Bayesian papers.

Eventually I said to David. Look, you are the guy who really knows how rare lines are. So write it down. Say to people, "The probability that lines exist is less than 1% with 90% confidence, or whatever the numbers are." It's interesting that he resisted doing this. Maybe by then he was such a Bayesian that such a statement did not compute for him. But, if a convincing line did eventually show up, he wouldn't have been wrong. I think he felt that such a pronouncement would be taken by the community to mean "BATSE has shown that there is no such thing as lines in bursts". I don't think he wanted to wear this crown. Rather, over time the lack of lines would become the accepted reality. One of his papers did have a cumulative probability distribution figure that one could look at and obtain the less than 1% probability number from. This was the last figure of the paper.

Spectral variability among bursts

Now I will shift to overall spectra of bursts.

When David began analyzing the overall spectra of many bursts he was impressed by the wide range of spectral slopes and energies where the luminosity peaked. Although earlier measurements, for example the work by Jay Norris, had shown this effect, the BATSE Spectroscopy Detectors were writing the book on it. David and I discussed what spectral shape to fit to the spectra and what fitting parameters to extract. He was frustrated that there were no theoretical shapes to use. I suggested that he come up with something and not worry about the theory. At this time, a power law with an exponential roll over at higher energies was often used for energies from about 10 keV to several hundred keV. But there were examples of bursts with power law extensions to may 10's of MeV that were well above an exponential.

David ginned up a simple mathematical formula that covered these cases. It consisted of two components: at low energies it is a power law times an exponential, and at high energies it is just a power law, but with a different slope. The shape is described by three parameters, the two power law slopes and the exponential. The fourth parameter is simply the normalization. This has the desirable properties of describing optically thin thermal bremsstrahlung as well as a broken power law with a continuous transition. Thus, it is based on physically meaningful parameters. But it can adapt to other shapes. A critical point is that from the fitted parameters one may easily calculate the gamma-ray energy where the luminosity peaks. This is the peak a nu-Fnu plot. David called this "hardness". His 1993 paper on "Spectral Diversity" presents this function and tabulates fitting results for 54 strong bursts.

David's results on total burst spectra showed that the spectral break or transition varied by more than ten times, from <100 keV to ~ 1 MeV. I thought it was particularly significant that the hardness was observed to be 2 MeV in one case, and it exceeds 200 keV in half the cases. This work showed that longer bursts tend to have harder spectra.

Spectral variability within bursts

Up to this point David's work did not look into time-resolved spectra. This was pursued next, with the involvement of Lyle Ford, a UCSD graduate student.

Earlier I said that right after data started coming in there was a spectacular intense burst with excellent time-resolved spectroscopy. This occurred on May 3, 1991, and was burst 141, I believe, in the running series we were using at that time. It had about 6 peaks separated by about 1 second , and the spectroscopic data resolved the bright peaks into about 8 intervals. BATSE's "time-to-spill" data accumulation mode worked beautifully. We could obtain high-quality broad-band spectral fits to each interval, which allowed us to really see what was going on. This was a moment some of us had been looking forward to for years.

During the first second the hardness was about 1 MeV, but the total counts were low. Thus, most of the emission was at very high energies. After about 1 second the lower energy emission became stronger, the hardness had decreased to 700 keV, and the total counts were at a peak. Thus the higher energies broke out first. In subsequent peaks a similar behavior occurred, but with lower hardness. Between peaks the hardness dropped to 200 keV. This is a lot of phenomenology to describe here, but the data and David's analysis provided the first clear example of it. So it's worth going over it. Successful models of bursts will have to explain this type of behavior.

David and Lyle's analysis of this and 37 other bright, long bursts was published in 1995. There are plots of the light curves and time-resolved hardness for each, along with correlations of various parameters. Key findings were that generally the hardness varies from hard to soft during a burst, and also from hard to soft within intensity peaks of a burst. Successive peaks tend to have lower hardness. Within a burst hardness can vary by a factor of ten.

The "Band function"

I will close by making note of the "Band function".

After David's 1993 paper someone coined the name "Band Function" for his spectrum formula. This became widely used. The name has stuck and continues to be used. Google it and you get tons of hits, and it's even in wikipedia.

I'm saddened that David is no longer with us. But he will live on in our memories. And his legacy of the "Band function" will endure for a long time.