

# The Cadmium Zinc Telluride (CZT) Detector Array Innovation Pathway

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This pathway describes the development of soft gamma-ray/hard x-ray detector arrays at NASA's Goddard Space Flight Center (GSFC, or Goddard), through a collaboration between the gamma-ray spectroscopy group and the detector branch. The Cadmium Zinc Telluride (CdZnTe, or CZT) detectors which resulted from this decade-long development were first flown on the wildly successful SWIFT mission in 2004, and have since enabled a whole class of imaging applications in the hard X-ray/soft gamma-ray range. The CZT detectors filled a long recognized need for a room temperature semiconductor capable of high resolution imaging and spectroscopy in this particular waveband.

## **Gestation Period (pre-1993 collaboration)**

Although the Goddard gamma-ray spectroscopy group and the detector branch didn't begin collaborating on a CZT development program until 1993, relevant roots of the innovation pathway trace much farther back. In the early 1990s, there were very few instruments covering the hard x-ray / low-energy gamma-ray spectral band (5-500 keV) in existence. This gap was due more to the technical challenges associated with making detectors in this range, than a lack of scientific interest; however challenges of the former limited numbers of the latter [I73]. The Goddard scientists were hoping to fill that gap by branching out into this new energy band, which they believed held the key to understanding such diverse phenomena as:

- *element creation, explosion dynamics and event rates for supernovae*
- *the origin of gamma-ray bursts through sensitive searches for cyclotron and other lines*
- *physical conditions in the vicinity of neutron star surfaces through observations of cyclotron and other lines in x-ray pulsars*
- *physical conditions in the central engines of AGN [D1]*

At the time, the default hard X-ray spectroscopy detectors were Ge:Li (Germanium doped with Lithium). These detectors had relatively low  $Z$  (which meant that the detectors would need to be very thick to create sufficient stopping power at the relevant energy band) and needed to be cryogenically cooled (requiring the accompaniment of the mass and complexity associated with advanced cooling apparatus, effectively rendering explorer class missions out of reach) [I52, 53]. Recognizing that suitable detector technology was a prerequisite to achieving their objectives, the group had been “*on the lookout*” [I73] for a suitable detector for some time. Being “*on the lookout*” in this context involved regular attendance at the conferences where researchers presented new developments in the realm of high  $Z$ , room-temperature solid state detectors [I73].

By the late 1980s, the scientists began to hear about two new semiconductor compounds – Mercuric Iodide ( $\text{Hg I}_2$ ) and Cadmium Telluride ( $\text{CdTe}$ ) – which, following nearly two decades of research since their discovery in 1970/1971 were beginning to show promise as radiation detectors [D37, I80]. The former development had been heavily funded by the Department of Energy in the context of nuclear monitoring [D1]. The latter was under development by smaller companies interested in applications to medical imaging [I80]. Both materials offered the promise of room-temperature semiconductor gamma-ray detection, but each presented

significant technical challenges which limited their practical utility. By the late 80s, when the scientists were monitoring developments, HgI<sub>2</sub> seemed the leading alternative [D8, D1].

Thus, in 1991, the scientists put in an SR&T (Sustaining Research and Technology, now called NASA Research Activity or NRA) proposal to study the application of HgI<sub>2</sub> devices at NASA. They won the SR&T bid as well as a follow-on grant to investigate the relative detector performance characteristics of different state-of-the-art detectors using balloon-based platforms (including the cryo-cooled baseline Ge:Li, room temperature HgI<sub>2</sub> and alloys of CdTe and Zn) [D1, I52]. For the second grant, a young astrophysicist (CSA#4) was hired as an NRC post doc, specifically to investigate room temperature gamma-ray detectors. It was during preparation of that proposal that the Director of Space Sciences (CSA#12) recommended that the scientists collaborate with Goddard's detector branch and made the introductions. The director had learned the value of this type of interdisciplinary collaboration through prior work with the detector branch head (primarily in the context of microcalorimeter development for AXAF (Advanced X-ray Astrophysics Facility) – see microcalorimeter case).

### **Project Initiation (A phone call and an executive decision)**

In a sense, NASA's decade-long development of CdZnTe gamma-ray detector technology was initiated by a phone call from above. One afternoon in 1993, Goddard's detector branch head got an unusual call. It was from the Director of Space Science at Goddard. His request was for the detector branch to support the high energy physics group in developing room temperature gamma-ray detectors, an area of science he believed would be critically important in the future. Based on preliminary investigation by Goddard's science community, two potential candidate detector materials seemed promising: HgI<sub>2</sub> and CdZnTe. The director expressed no preference between them. As recalls the branch head (BH#2), *"this was not a guy you said no to."* So, following the conversation, the branch head returned to his desk and *"did his homework"* on the options [I59].

Interestingly, the Director of Space Science doesn't remember that conversation as having been particularly unusual or important [I74]. While it wasn't common for a science director to call an engineering branch head, it was an important part of what the director saw as his job. As he viewed the situation: *"either I could give equal, uniform blanket approval to everything that came out of my organization; or I could demonstrate some preference for some things that I thought were more promising."* He chose the latter approach realizing that *"I could be much more effective if I occasionally (not all the time) demonstrated passion (or anger)... this was one case where [BH#2] was being pulled [in other directions] so I thought it was important to let him know how important I thought this was"* [I74]. Although the director had no formal influence over the engineering directorate, he found that these *"demonstrated preferences"* served to guide the level of effort, provided by engineering to support proposals, and projects.

This case was no different. Within a couple of weeks of *"homework"*, the branch head had (independently) come to the conclusion that CZT was the most viable path forward [I59]. In the time since the scientists' assessment of the state of the field, a new material growing process had been developed for undoped CdTe as well as the invention of a new semiconductor compound CdZnTe [D9, D10]. These two advances changed the relative assessment of the device options. Based on the updated information, the branch head chose CZT for two reasons. First, HgI<sub>2</sub> is a

“*miserable material to work with,*” [I59] particularly from a health and safety point of view. To work with it in their fabrication lab, many expensive changes would have had to have been made. CZT didn’t pose these concerns. Second, the main group working on HgI<sub>2</sub> was located in Israel. While this did not preclude collaboration, it certainly would have made it more challenging. Expertises in CZT processing were domestic. Combined, these factors made the branch head’s decision to pursue CZT detectors a straightforward one [I59]. Thus, a few short months into NASA’s development, a major potential technological trajectory had already been pruned. HgI<sub>2</sub> was pursued in the broader community and was later found to have other performance-oriented challenges compared to CZT/CdTe (which have since emerged as the dominant technologies) [I48].

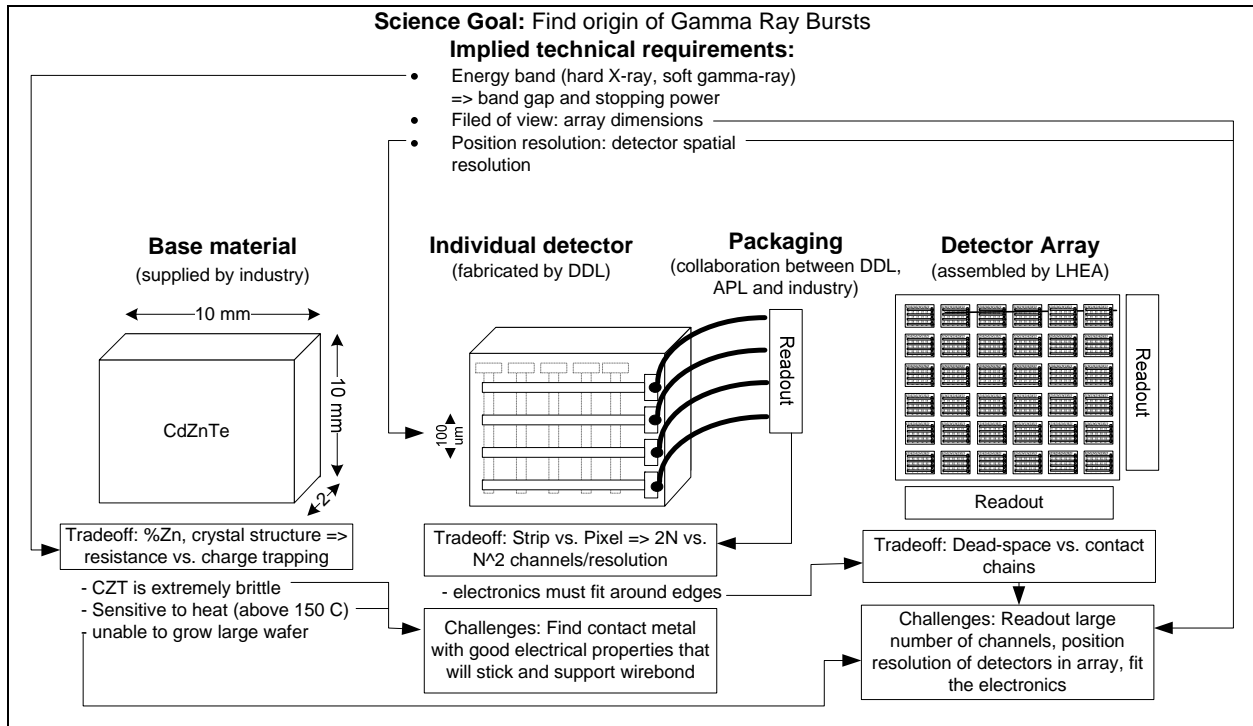
### **Goal Guided Exploration**

Almost from the beginning, the science goal of finding the origin of Gamma Ray Bursts guided the technology development. GRBs are the most luminous electromagnetic events occurring in the universe, and for several decades represented a major mystery in the field of high energy astrophysics. Originally discovered by a satellite designed to monitor for covert nuclear weapons tests in space in the 1960s,<sup>1</sup> and declassified in 1973, these gamma-ray bursts of “cosmic origin” became a topic of much scientific speculation. No one knew what they were or where they were coming from. Even accurately positioning these short-lived (~1 second) high energy bursts, which seemed to crop up at random locations, posed a major challenge: Since, unlike light at less-energetic wave-lengths, gamma-rays cannot be bent (and focused), the detector area needs to be as large as the desired imaging area [D39]. Also, since scientists were unable to predict where the next burst would come from, this imaging area needed to be wide [D29].

This science need translated into a requirement for a wide area detector plane with sub 100 μm position resolution [D13]. The intention was to procure CZT wafers from industry; have the detector branch pattern fine contact structures, wire bond leads and package the detectors as a suitably large array, per the design of the science team, as shown in Figure **Error! No text of specified style in document.**-1. However, in order to accomplish this, an interrelated set of major technical challenges needed to be overcome at each level of manufacturing and integration.

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<sup>1</sup> For a complete history, see: <http://heasarc.gsfc.nasa.gov/docs/history/>



**Figure Error! No text of specified style in document.-1: Flow of Design Tradeoffs in the CZT Innovation Pathway.**

Acronyms defined: DDL (Detector Development Lab); APL (Applied Physics Lab); LHEA (Laboratory for High Energy Astrophysics).

Although the semiconductor compound CdTe was invented in 1970 as a room-temperature radiation detector, it wasn't used for that purpose until two decades later. At the time, various doping compounds were explored, but a formula for a practical detector base material eluded the investigators. In developing detector materials, a key tradeoff is between electrical resistance (low dark current) and charge trapping (efficient charge collection); introducing impurities increases both. Although CdTe compounds of the 70s and 80s made poor detectors, they made great substrates for HgCdTe (Mercury Cadmium Telluride), a popular IR detector material [I48, I52, I55]. As a result, bolstered by heavy DoD investment in HgCdTe and the supporting development infrastructure, a small CdZnTe substrate industry had emerged. As a substrate industry, value was placed on an ability to grow large uniform crystals, and that's were development efforts had been focused [I80].

However, in late 1980s and early 1990s two related breakthroughs reignited interest in producing cadmium telluride compounds as detectors in their own right [D8, D9, I48]. A new growing process for CdTe doped with Zn was developed, as well as a method for growing undoped CdTe wafers. Each approach had its limitation. Mechanically, both CZT and CdTe were brittle and temperature sensitive, but comparatively CdTe could be grown with a larger area of single crystal grains compared to CZT. Electrically, CZT had the advantage of not polarizing and had lower dark current. However unlike in previous generations, these limitations were seen as challenges, not show stoppers. CZT was pursued primarily in North America, while Japan and Europe invested in CdTe development [I80, D40].

By 1994, when the Goddard detector branch had begun its development in earnest, the state-of-the-art was to grow 10mm square wafers and deposit a single contact of electroless gold with an eye dropper. Even at that size, the yield was less than 3% and, although the electroless gold contacts had good electrical properties, the method was too crude to produce the required spatial resolution [I80]. In addition, the contact was too thin and porous to withstand the wire bonding needed to attach leads. To address these limitations, the Goddard team began investing and experimenting along several dimensions of the problem. On the materials front, knowing that they would be constrained by the quality of materials that could be procured, the Goddard team began collaborating closely with the two leading CZT producers in an effort to broaden the supplier base. They entered into a sequence of SBIR (Small Business Innovation Research) contracts (in 94 and 95 [D32]) with the small business that had pioneered the new growth techniques in 88-92 while simultaneously investing “*heavily*” in materials with the more established substrate growing firm [I59]. In both cases, the contracts provided materials for the in-house detector development program as well as resources for the companies to improve their own processing techniques.

The decision to work with two materials growers simultaneously was based on a hunch, and sheds some important light on the way these types of decisions were taken at the time. When the relevant detector branch engineer (CSE#4) was assigned to lead the CZT development, collaborating with the inventors of the enabling techniques was only natural. However, doing his due diligence, the engineer also reached out to his HgCdTe colleagues (who were more familiar with the lay of the substrate industry) for advice [I82]. One trusted colleague suggested he visit a company called II-VI, the established substrate grower mentioned above. During the visit, the Goddard team was impressed by the professionalism of II-VI and learned that they had recently spun-off a group to compete in the emerging CZT sector; it turned out that one of the inventors of the new technique had moved over from the initiating small business. The “*feeling*” that the engineer “*got from the visit,*” convinced him to diversify the Goddard investment [I82]. History has shown him to have been correct; the original small business never succeeded in the CZT detector business; in the mid 90s, the company took a new name and has since focused on medical imaging. The substrate spin-off (although it struggled itself) remains a major player today.

On the detector side, the Goddard engineers experimented with combinations of surface preparation chemicals, contact deposition techniques and contact materials [I48]. They leveraged lessons learned from past in-house detector developments for other flight projects as well as standard practices from the semiconductor industry at-large [I48, I59]. This past experience defined the strategies and materials with which they experimented, but within that space it came down to a painstaking process of trial and error. As recalls CSE#3: “*it was painful!*” “*Nothing would stick to the damn thing!*” [I31] The technologists spent hours and hours in the lab trying different approaches. As viewed by the scientists, including CSA#4: “*it wasn’t quite random trial and error – he was an expert in this and had lots of tricks - but sometimes it seemed that way*” [I52]. After more than a year of systematic trial-and-error, they finally identified a combination of metals that would 1) stick to the base material, 2) make good electrical contacts, and 3) survive the wire bonding process. The branch head, BH#2, remembers the excitement the

first day the technologist found a suitable contact; “*that was the big breakthrough,*” now you could have a device [I59].

Once a suitable material had been found, “*depositing the microstructures was easy*” [I48] using standard fabrication procedures; however, choosing what to pattern represented a key design tradeoff that crossed disciplinary boundaries. Although fine resolution detectors were common place, large arrays of fine detectors were not. The challenge was in scaling up the number of output channels that needed to be processed by the readout electronics or ASICs (Application Specific Integrated Circuits). To achieve x-y position resolution with a pixel detector requires a lead attached to every pixel (i.e.,  $N \times N$  channels). This number can be reduced to  $2N$  by employing crossed-strip contacts (as illustrated in Figure **Error! No text of specified style in document.**-1) with a single lead for every strip [I53]. The tradeoff was one of efficiency vs. maturity. While strip detectors minimized the burden on the ASIC, strip detectors were less used (so the corresponding packaging was less mature). The decision to use strip detectors meant that readout leads would have to be individually wirebonded to each of the strips and all of the electronics infrastructure would have to be placed on the side, with implications for the array architecture [I80]. These types of trade-offs were resolved in real time by an *ad hoc* interdisciplinary team. A strong working relationship was forged by at least weekly meetings and often long hours, working side-by-side in the lab [I52]. This close collaboration of scientists and engineers was relatively unusual; as recalls one of the scientists: “*it was kind of interesting; they were all surprised to see a scientist over there all the time. I don’t know if I annoyed them or not [the Branch Head] would say oh no, an invader! But meeting weekly really helped*” [I52].

This work was carried out in parallel with several other flight and development projects in the DLL (Detector Development Laboratory), which enabled cross-pollination of ideas and techniques as well as book keeping financial flexibility [I48, I59]. To the extent that detector branch labor was book-kept to this development effort, the funds came from an *ad hoc* “*retainer*” that was paid by the Gamma-ray spectroscopy group to the detector group on a yearly basis; since the early 1990’s they have contributed an average of \$100K per year to the detector branch’s operating budget [I48, I59, I73]. This retainer nominally covered the labor of approximately one person-year. However, it is generally understood that this “*came nowhere near to covering their costs [during the CZT development]*” [I48]. The branch head saw this as a joint investment in Goddard’s future, and covered the remaining costs with monies from a series of on-going RTOPS (Research and Technology Objectives and Plans Summary) grants [I59]. RTOPS were a historical form of directed technology funding to NASA Centers that no longer exists. As recounted by BH#2, this level of flexibility was possible because “*it was a different time. Back then the guidelines were very loose. [Grants read] something like we entrust you to go and understand the state-of-the-art in this area*” [I59]. Initial materials purchases also came through these RTOPS accounts; later though, materials procurements were made directly by the science team. All of the science funding came from three parallel sources. The team received relatively stable yearly installments of \$100K per year (won with annual proposals) from a DDF (director’s discretionary fund) [D3-5] grant and a sequence of three-year, \$200K per year, SR&T awards [D1, 2]. The third funding stream came as support for the groups’ on-going balloon campaigns (discussed below).

## **A Focusing (yet unsuccessful) Sequence of Mission Opportunities**

By 1995, the Goddard group was the only one in the world able to produce CZT strip detectors with sub 100 micron position resolution [D22]; however an individual detector was a far cry from the array that would be required to implement their BASIS (Burst and All Sky Imaging Survey) GRB finding mission concept [D13]. In 1995, the team won a study contract as part of the “New Mission Concepts for Astrophysics” call. In this context, and in preparation for an imminent explorer call, they began mocking-up a prototype array as a proof-of-concept. Although the 6x6 “engineering model” was only mechanical (i.e., they couldn’t actually read out an image) it served several purposes. First, it convinced future reviewers that the idea was feasible. More importantly though, it identified and forced a solution to several unexpected challenges. The act of placing detectors in the custom-made, plastic, egg-carton-like support structure taught the team how to handle the detectors; how fragile they were; how to glue them in; how to position them. Out of this effort, Metrology (i.e., accurately knowing the position of the strips within the array) was identified as an important design challenge. While this effort was lead by the scientist, the detector branch participated heavily [I80].

The next year, in August 1996, a real flight opportunity presented itself in the form of a NASA call for a MidEX (medium explorer) class mission [D29]. The team pitched an extremely ambitious device concept, called BASIS (The Burst and All Sky Imaging Survey), which included a 6x6 array of 36 <100 $\mu$ m pitch resolution CZT strip detectors. Looking back “*we were crazy to think this was obtainable*” [I52]. The review committee agreed with that assessment, and the proposal was not selected, due to serious concerns about technical feasibility. Despite the rejection, the team persevered, continuing to improve the capability along the dimensions of material quality and detector architectures, “*very dejected*” but confident that the science was sufficiently worthwhile that a new opportunity would arise [I85].

A year later, another explorer class call was released, this time for a SMEX (small explorer) mission. They re-pitched the BASIS concept. Not surprisingly, what had been “extremely ambitious” for a MidEX, was no less risky for the smaller (cheaper) SMEX opportunity. Again they were rejected. However, the exercise of proposing was productive for several reasons. First, it maintained a mission focus to the development; and the excitement that embodied. Second, it communicated continued scientific interest in GRB positioning to the HQ funders as well as Goddard management. This latter point merits further explanation. Recall from above, how the director of space science believed that in a resource constrained environment, it was his responsibility to focus available resources on particular projects. In this context, it meant coordinating a pre-screening when an explorer call came out, and allocating engineering support resources to some proposals and not others [I74]. Although he had no formal authority over the engineering directorate, his soft influence was significant. Thus, an important part of proposing early (perhaps before the technology was mature enough) was to signal that this was an important area for future calls.

## **Exploration at the Architecture Level**

Not having been selected for mission development, while continuing to have access to resources from the ongoing SR&T and RTOPs grants, gave the team time to continue developing the technology. Through the close collaboration with the CZT materials industry, enabled by SBIRs, a significant amount of technical information had been shared [D38]. By 1995, the original small

business, now operating under a new name, was producing more sophisticated detectors as well as growing materials. The larger company had invested heavily in a semiconductor fabrication facility and was employing similar techniques to Goddard for depositing fine contact patterns [180, 182]. However, the base material yield was still quite low. It became apparent that as future missions would require very large detector arrays (i.e., many detectors), low yields and the correspondingly elevated costs, would become a constraining factor.

While searching for a way to screen the procured materials pre-processing, the technologists serendipitously expanded the team to include an in-house expert in Non Destructive Evaluation (NDE). As recalled by the NDE expert, “*they had heard that I had an IR camera, so they came knocking on my door*” [158]. The CZT team got more than a camera. Together with the NDE expert, they developed a set of materials screening processes that correlated observable defects (in the IR) to detector performance [148, 158]. What this meant was that large CZT wafers could be grown, and screened so that the largest useful pieces (i.e., without defects) could be identified and harvested. This technique improved the effective yield significantly, and has since become an industry standard. In fact, the NDE expert became so intimately familiar with the device fabrication techniques that he took over as the tech lead after the original technologist moved into management. This partnership also created an additional source of funding; the NDE expert was able to secure \$60-70K per year, over 4 years from a dedicated non-destructive evaluation funding pot [158].

Packaging the detectors (i.e., connecting the contacts to readout electronics) became an area of focus during this second round of exploration as well. Most of the standard techniques required the addition of significant heat; however one of the challenging characteristics of CZT is its sensitivity to temperature. Over 150 C, its electrical properties degrade. While they were trying to solve this problem, the team became aware of a small business doing work on cold soldering, when they attended a presentation given on-site at Goddard [182]. After the presentation, they approached the presenters and began discussing modes of collaboration. As a small business, developing an SBIR proposal seemed like the path of least resistance. This company won two rounds of SBIR contracts in 1997 and 1998 [D32] to investigate the application of the low temperature packaging techniques to CZT. As with the materials companies, Goddard shared their patterning technology with the company (since it was relevant to their packaging solution). While the technology developed through these SBIRs was not implemented directly on SWIFT, the capabilities that were developed have been leveraged on future efforts, including the CalTech-led NuSTAR mission. And, the company has since developed expertise in detector development as well as packaging and in effect transitioned the competency away from Goddard [180, 182].

Another important collaboration in the area of packaging was formed with APL (the Applied Physics Laboratory). Having worked together before, with APL “*just down the road,*” and with more sophisticated packaging facilities, it was only natural to leverage their expertise as the packaging became more complex [148]. Where the small company described above had expertise in soldering, APL helped more with array design and construction; particularly through the use of their automatic wire bonding machine [182]. The work with APL was managed through a standard fee for service contracting vehicle. To this end, the team also worked closely with, and learned a lot from, scientists and engineers in Europe who had been developing a similarly large



array of CdTe for INTEGRAL. Although SWIFT and INTEGRAL were conceived around the same time, the European team managed to get approval several years earlier. As remembers one of the Goddard technologists: “*We learned a lot from each other. We even went over to France to see what they were doing. Information sharing was very free*” [I48].

### **Balloon program**

A parallel balloon program, which was run out of the same gamma-ray spectroscopy group, provided several opportunities to “fly” CZT detectors. While the scientific focus of the balloon programs imposed drastically different detector requirements (making the engineering cross-over somewhat limited), the context did provide some level of excitement for the team, some stable funding (balloon campaigns are funded out of the same program as other NASA Research Activities (NRA)) and motivation to experiment with different types of patterns and packaging to suit the various objectives [I86].

Well before the CZT development had been initiated, the gamma-ray spectroscopy group had been maintaining a fairly active balloon program called GRIS (the Gamma Ray Imaging Spectrometer). During its two decade history, GRIS made nine flights, carrying cryogenically cooled germanium detectors, measuring the high energy background. As discussed above, the NRC post doc who was initially brought on to compare various detector approaches, did so with a piggy-back instrument on GRIS [D33].

PorTIA, the Piggyback Room Temperature Instrument for Astronomy, flew an inch square (single pixel) CZT detector three times in 1995. These flights served to characterize the detector background; an important precursor for future CZT instruments. From a detector perspective, although the PorTIA detectors weren’t new per se, “*we were learning how to fabricate, test, and package CZT detectors in general so this provided us another challenge*” [I86, D41].

In 2001, a follow-on balloon-born gamma-ray program called InFOCuS (International Focusing Optics Collaboration for  $\mu$ Crab Sensitivity) was initiated. InFOCuS pairs newly developed focusing X-ray optics with a CZT pixel detector focal plane (because of the international collaboration, CdTe detectors were also flown, as will be discussed later) [D42]. From the detector developers perspective, being involved in the InFOCuS program drove them to develop pixel detectors (since the focal plane is relatively small, minimizing channels with strip detectors was not a driver as it was for BASIS) and enabled a close working relationship with CdTe developers around the world including with respect to the array architecture collaboration discussed above [I80].

### **Changing context leads to feasible design.**

Shortly after the second BASIS rejection, the technical constraints of the problem were fundamentally changed by a scientific discovery. In 1998, the European BeppoSAX mission observed a GRB “afterglow” in the X-ray and later UV/optical ranges. It was important enough in the field, that the Goddard lead scientist remembers it vividly:

*I can tell you exactly where I was when I learned about the BeppoSAX result. I was at a conference in Japan. [gamma ray bursts was one part of it]. Pirro was at that conference, and he was the project scientist for BeppoSAX. On March 1<sup>st</sup>, he came into*

*the conference with a plot in his hand... I can still remember him walking into the room because he was a little bit late and we were about to start... he said 'we have a new result! We have a new result! He's waving this plot and we all gathered around his paper and it was the afterglow discovery from February 28<sup>th</sup>. So we knew about it the day afterwards; that's not normal in Astrophysics, but it is part of the culture of the GRB community. [I73]*

The result was significant for several reasons. First, the major driver of the early developments was the need for a wide field of view with extremely high spatial resolution, in a wave band with difficult optical properties. Now, only the presence and approximate position of the short-lived gamma-ray bursts needed to be detected; the accurate positioning could be done by a separate X-ray telescope, with a narrower field of view, slewed to the approximate location after the observation. This drastically reduced the technical constraints on the CZT arrays, to within the capabilities of the day.

The whole community recognized that there was an opportunity here. Within NASA, there was a sense that the Agency had pretty much decided that the next step in gamma-ray bursts, was an explorer class question and that they weren't going to devote a major mission for it. In fact, in that year's Administrator's address to the American Astronomical Society annual meeting, Dan Goldin mentioned (as recalls the scientist):

*a) gamma-ray bursts; b) how exciting the BeppoSAX finding was; c) how NASA really wanted to make the next step following on the Italian lead; and d) that this was the kind of problem that he thought was perfectly suited for the explore program [I73].*

That was an unusual stance for an administrator to take, and it had a significant impact on the next explorer call [I73]. The reason it's unusual is because the explorer program is a competitive program serving all fields, and so usually NASA is very hands off about saying what they want to do – they just leave it up to the competitors and the peer review process to select what science to fly. The community interpreted Goldin's statements as “*essentially saying that NASA was going to select a GRB mission in the next explorer round*” [I73]. Not surprisingly, five GRB mission concepts were submitted in response to the next call.

### **SWIFT Mission Development**

The call came in 1999 with the announcement of a next MidEX (mid size Explorer). The team proposed the much simpler (from a detector point of view) SWIFT concept, which incorporated the large area, coarse resolution CZT array and slewing X-ray telescope described above [D31]. The detectors were non-patterned CZT, supplied by industry (the substrate company). The proposal was successful [D35] and resulted in the launch of SWIFT in 2004.

Although none of the strip deposition and wirebonding techniques that had been developed to support the BASIS concept ended up being used directly on SWIFT, the lessons learned through the development process and other complementary capabilities played an important role in SWIFT's success [I48, I73, I81]. For example, it is unlikely that the CZT materials company would have had the capacity to produce the required 32000 detectors for SWIFT's detector array without NASA's earlier investment [D34, I80]; neither would the NASA team have had the

experience to specify “contractable” requirements nor been able to evaluate material quality on the flight procurement without the 10 years of prior experience.

However, since the detector design for SWIFT was so much simpler than what had been developed for BASIS, and the materials growing companies had improved their detectors fabrication practices through close collaboration with Goddard’s detector branch, suitable detectors could now be procured from industry directly. This effectively eliminated the need for detector branch involvement in the flight development. In fact, the scientists decided to handle the procurement themselves. As a result, all the engineers moved on to management or other projects, and Goddard has since lost its lead in CZT detector fabrication [I80].

### **CdZnTe after SWIFT in the US**

The science team has moved on from CZT detectors as well; though in a different way than the engineers. SWIFT is still flying; returning data; making discoveries [D43]. It is considered a wildly successful mission and the data and analysis continues to keep the science team busy [I73]. As a result, NASA’s gamma-ray spectroscopy group has not pitched a follow-on mission that would leverage all the CZT strip and pixel detector development. New detectors have been procured for the InFOCuS balloon program, but funding for balloon flights has been intermittent at best [I80].

The next American hard X-ray mission will be NuSTAR. It is a NASA Small Explorer mission led by CalTech, managed by JPL, and implemented by an international team of scientists and engineers that is scheduled for launch in 2011 [D44]. Goddard is participating in a technology consulting capacity [I80, I82]. It will fly a high resolution CZT pixel array behind focusing optics (so the detector plane is small). The detectors are being provided by the CZT material grower that got its start developing substrates for HgCdTe, packaged by the company that applied its cold soldering techniques to CZT under a Goddard COTRed SBIR.

### **Hard X-ray Direct Semiconductor Detectors Internationally**

As described above, teams in Europe and Japan chose to address the electrical stability concerns of CdTe rather than the manufacturing challenges of CdZnTe that occupied the Americans. Both strategies have yielded successful detector technology, as evidenced by SWIFT in the US, INTEGRAL in Europe and Astro-E2 in Japan. Through the 1990s, JAXA set up a similar close relationship with their local material growers and detector manufacturers as did NASA. Interestingly though, where the success of SWIFT in some sense ended the development efforts at NASA, a series of failures in JAXA’s Astro program have kept development efforts alive. In fact, if the International X-ray Observatory (IXO) moves forward, the Hard X-ray back plane will be a Japanese contribution, leveraging CdTe technology. This is not to suggest that history has proven CdTe to be the superior technical approach – that Japan will likely contribute the hard X-ray telescope is driven as much by politics as any other factor. Nor does it suggest that failures of Astro-E/E2 have enabled to the success of CdTe technology; although the prolonged sequence of similar mission opportunities has provided stable funding and context for continued development. However, if the failures allowed development to continue and become a core competency, while the success of SWIFT terminated the in-house detector program, and if CdTe’s status as a core competency is motivating JAXA’s claim to the IXO hard X-ray telescope, then the importance of mission sequencing may merit further exploration.

