Semiconducting Microcalorimeter Innovation Pathway

This case describes the development of microcalorimeter detector arrays for applications in X-ray astronomy. The key insight, which initiated the multi-decade (X-ray) microcalorimeter innovation pathway, is generally attributed to a particular Goddard (infrared) astrophysicist; however multiple institutions and individuals have played key roles over the last 30 years. The development of microcalorimeters has been motivated by the need for a high resolution X-ray spectrometer that has high intrinsic quantum efficiency for use in X-ray astronomy. The high quantum efficiency is important because of the very low X-ray fluxes characteristic of most celestial X-ray sources. Although microcalorimeters were first demonstrated in 1984, and selected for flight aboard the Advanced X-ray Astronomical Facility (AXAF) in 1986; due to a series of unfortunate circumstances (AXAF-S split, Astro-E launch failure, Astro-E2 cryogenic system failure, discussed in the previous pathways) the detectors have yet to return data from a space-based platform. This milestone will hopefully be achieved in early-2014 aboard Astro-H. During this 30 year history, three fundamentally different classes of microcalorimeters have emerged – one based on semiconductor thermometers, one based on superconducting transition edge sensors and one based on magnetic resonance. Figure Error**! No text of specified style in document.**-1 illustrates how the best-in-the-world detector performance has improved over time. We consider the three generations here as separate, albeit entangled, innovation pathways. Only the first two histories are presented in detail (as the third is still quite immature).

Gestation period

Although microcalorimeter development explicitly started in 1982, under the context of a call for ideas for the then future AXAF (Advanced X-ray Astronomical Facility – later renamed Chandra), much of the key groundwork was laid several years before that.

The IR astronomer (heretofore referred to as CSA#2) who would eventually propose the X-ray microcalorimeter idea joined Goddard in 1979 for a seemingly unrelated purpose; as an NRC post doc, with a grant to build an infrared spectrometer. At the time, bolometer-type thermal detectors[1] were the device of choice for his application, and he knew that he would need a lot of them. However, with the state of bolometer manufacturing in its infancy as it was – "*people were doing things like soldering wires to pieces of germanium and things like that… and how well things worked depended in detail on how the soldering went*" [I42] – and with university lab "boutique" shops being the primary suppliers, individual detector prices were quite high; much higher than the CSA#2 could afford for his spectrometer project. Faced with a conundrum, the resourceful young scientist decided that he could manufacture the detectors himself.

[1] A Bolometer is a device for measuring the power of incident electromagnetic radiation. They consist of an absorptive element, connected by a weak link to a heat sink. They work by measuring temperature changes in the absorber.

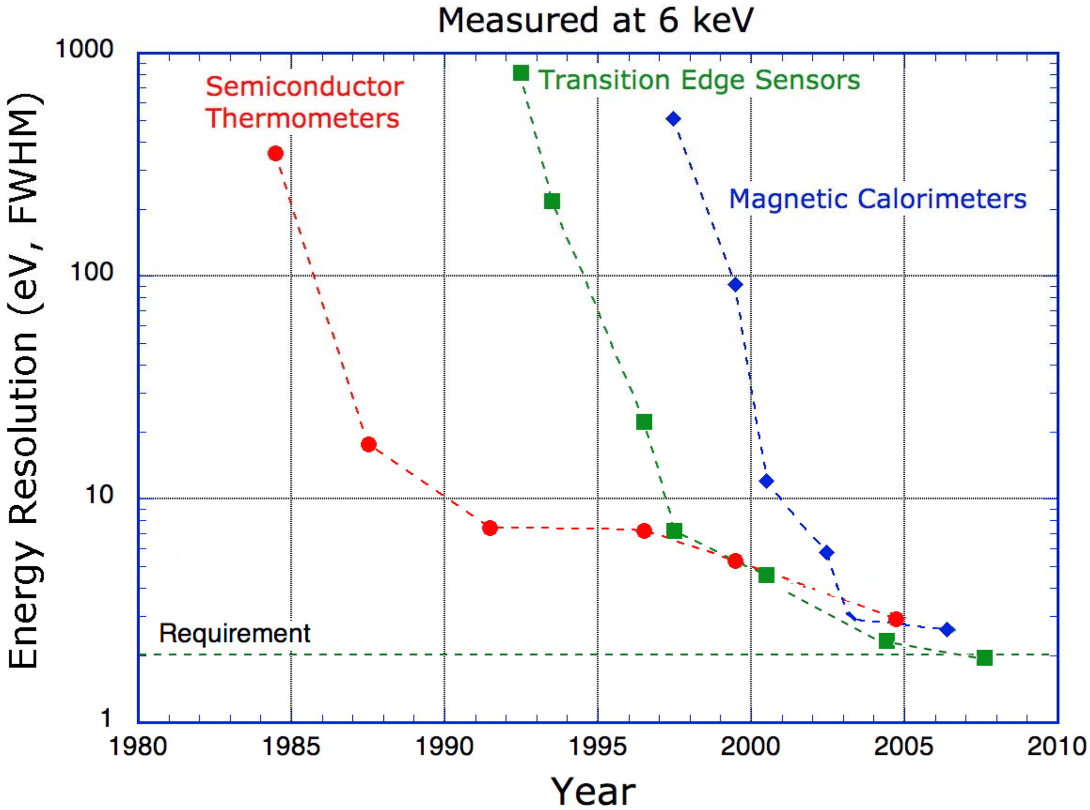

Figure Error! No text of specified style in document.**-1: Performance Improvements of Mircorcalorimeter Devices over Time.**
Each point represents the best-in-the-world, individual detector resolution, measured at 6 keV. This figure was created by the Goddard Calorimeter group.

Although he had no previous device experience, he had become confident in his "*tinkering*" ability during his grad school days,[2] and he recognized that one of Goddard's in-house competences was a relatively well equipped semiconductor processing facility. So, he walked over to talk to "*the guys in the processing lab*," [I42] told them that he was interested in making detectors and that he thought they could do it. They would prove him right; and the close collaboration that was in part forged by this initial bolometer experimentation has persisted through future detector developments (e.g., CZT described above).

In the late 1970s, most bolometer designs employed Germanium thermometers, but CSA#2 was convinced that silicon (Si) could serve the purpose as well. This was important because Si was the material with which the processing lab had experience. CSA#2 did some investigating and found a publication written by a scientist at Texas Instruments (TI), detailing a bolomemter that used silicon thermometers. Excited, he called him (the TI guy) up and asked for details. The strategy seemed promising, so CSA#2 asked if the TI guy had any scraps left over from the development. He had, and was willing to share them. CSA#2 took them to the lab, and with the

[2] CSA#2 explained how a graduate degree in Physics is like an apprenticeship. The student gets real hardware experience by being involved in the full project development cycle at least once. This is different than what aerospace engineers seem (to him) to get, who touch hardware for the first time in the highly regulated flight development environment when they join NASA.

help of the technicians, made his first detector. "*Pretty soon,*" they were making reproducible detectors with good noise performance that was, more importantly, predictable by theory! More than just a good manufacturing characteristic, reproducibly predictable by theory was a critical capability because it meant that volume and operating temperatures could now be tuned to meet particular design specifications and by implication science requirements.

The theory that was being reproduced, and that predicted the relevant scaling laws, was itself quite new [D28]. It came out of some other tangentially related (theoretical) *tinkering*, connected more by the physical proximity of the two tinkerers than by any other attribute of their work. At the time, another Goddard scientist in the observational cosmology group (CSA#3) was working on trying to understand the noise performance of his system for COBE (the results of which would later win him a Nobel Prize). It was through that effort that he developed what would become the widely accepted noise theory for bolometer-type devices. The main relevant contribution of this non-equilibrium thermodynamic noise model was "*that he presented things in such a way that you could see the scaling laws"* [I42]. CSA#3 has often mused, that the resultant paper is by far his most cited work; far more impact (as measured by citation counts) than the work it enabled, for which he received his Nobel prize [I51].

Armed with reproducible bolometers and a theory for how they scaled with different tunable parameters, CSA#2 began thinking about the physical limits of sensitivity that one could achieve with the devices. It became clear that low temperatures held the most promise for increasing sensitivity. So, he invested in a dilution refrigerator for the lab, using funding from the flexible Director's Discretionary Fund (DDF). At the time, the Goddard group was one of the few seriously exploring low temperatures in the world [I42]. However, in hindsight "*it turned out to be an absolutely key thing to have that [dilution fridge]"* [I42] because it gave them the tools with which to experiment. This experimentation occurred largely in CSA#2's free time (he was still working full time on the spectrometer). As he explains: "*that was in the days before full cost accounting and plus that was at the time when I was spending huge amounts of time in the lab, I was here all the time […] I would come late at night after the wife and son had gone to sleep, I'd come back to the lab*" [I42].

In 1981, having established his group as leaders in the domain of IR bolometers, CSA#2 got a call from NIST. The NIST scientist on the line was thinking about using thermal detectors to detect laser pulses, and was wondering if CSA#2 knew the minimum pulse energy that one could detect with a bolometer. Although he didn't have the answer off hand, CSA#2 was confident that he could work it out. So he did. He reported his result to the NIST scientist, and put the calculations in his desk drawer in case it became relevant at some later date [I42]. This was significant because a bolometer that also measures energy (not just power) is essentially a calorimeter.

Initiating collaboration

That "later date" came about 1 year later, when a group of Goddard X-ray astronomers – who happened to sit down the hall from CSA#2, and knew him well from frequent lunch breaks in a shared cafeteria – stopped by to mention that an opportunity to propose a major instrument for

AXAF would be announced in the next few years.[3] They knew that they were going to propose a spectrometer, but they needed a way to distinguish themselves if they were to have a chance of winning. At the time the standard detectors for x-ray spectrometers were silicon diodes, but they weren't nearly good enough for the next generation Grand Observatory. Multiple ideas were batted around the lunch table [I74]. Eventually the X-ray astronomers had come to CSA#2 – an IR astrophysicist[4] - because they'd heard that he'd had some success with a smaller bandgap semi-conductor for IR spectrometers which, if it worked for X-rays, could improve the X-ray resolution as desired.

After they explained the basic principles of x-ray spectrometers to CSA#2 (he had no previous experience in that wavelength), CSA#2 thought for a minute and concluded that InSb (Indium Antimonide – the smaller bandgap semiconductor) wasn't appropriate, but that bolometers were worth exploring. To that end, he pulled out the calculations that he'd originally performed for the NIST scientist; and from that he extrapolated a fundamental limit of 1eV at 6 keV if they could get the devices to 0.1 K [I42, I74]. That was very good; much better than the X-ray astrophysicists had hoped for. So CSA#2 got to work.

Proving theoretical feasibility of the concept

The initial proof-of-concept work was conducted by a core team of CSA#2, a visiting Astrophysics researcher from the University of Wisconsin (UL#3) with a reputation as a "*spectacularly talented experimentalist*" [I88], CSA#3 assisting primarily with the theoretical limits work, and CSA#14 - an Astrophysicist from the Laboratory for High Energy Astrophysics (LHEA) who brought knowledge of the AXAF context.

CSA#2 and UL#3 were not acquainted prior to the collaboration. They were introduced by the X-ray group Branch Head, who had a history of collaboration with the University of Wisconsin, where UL#3 was junior faculty. UL#3 was recruited because the effort needed an X-ray knowledgeable experimentalist – and all the potential internal candidates had either recently been promoted to management positions, or were occupied with another big project. So the Branch Head asked this junior professor to do it. Deciding to participate was a big decision for UL#3. As he remembers, he eventually said yes for a variety of reasons, none of which involved a particular desire to be involved in AXAF:

> *I said, well let me think about it… and I went home and did the calculations… because it kind of seemed like a crazy idea… but the calculations sort of came out... I was in this position of being a fairly new assistant professor and having this sounding rocket program going… and sounding rockets in X-rays were kind of on their last legs because now there were all these satellites up… and it's very hard to compete against a satellite with a sounding rocket… but I was in the business of educating graduate students so I wanted to stay in sounding rockets if I could (and sounding rockets are about the only*

[3] It had been ranked first in the NRC Astrophysics Decadal Survey for the 1980's (released in 1982) among major new programs. The call was for "an AXAF operated as a permanent national observatory in space, to provide x-ray pictures of the Universe comparable in depth and detail with those of the most advanced optical and radio telescopes. Continuing the remarkable development of x-ray technology applied to astronomy during the 1970's, this facility will combine greatly improved angular and spectral resolution with a sensitivity up to one hundred times greater than that of any previous x-ray mission."

[4] With the Astrophysicists discipline X-ray and IR Astronomy are different specialties with essentially no overlap, requiring different expertise.

way for students to actually learn about building hardware) so I looked at it as hey, if this thing works, it's never been on a satellite, so we could put it on a sounding rocket and do some new stuff… keeping my sounding rocket program alive for at least five years (of course 27 years later, my sounding rockets are still the only things that have gotten astronomical data with these detectors)… [I93]

The collaboration worked out better than either could have expected. As recalls CSA#2 "*From the first time I met [UL#3]it was an absolutely perfect collaboration because he was so very smart and knew a lot and had done a lot of things, I mean just perfect teaming… I ended up working with [him] for many years… we probably talk several times a week. Usually arranged marriages, you don't think of as working all that well, but it came off awfully well.*" The feeling was mutual, in UL#3's words "[CSA#2] *is probably the quickest guy I've ever met… just always dripping with ideas.*" [I93]

They got straight to work: "*we [CSA#2 and UL#3] basically spent the whole summer in the lab in 1983*" [I42] funded by a DDF. By the end of that summer, they had obtained the first microcalorimeter spectrum (published in 1984 as the famous [D29 and D30]).[5] Although the performance of that first detector wasn't even as good as the best silicon diode at the time (achieving an energy resolution of ~250 eV FWHM at 6 keV as shown in Figure **Error! No text of specified style in document.**-1), and nowhere near what would be needed on AXAF, it proved the concept [D1]. That was enough to get detector development underway in earnest. The next step was to explore the theorized low temperature limits. Over the next year, they worked with the dilution refrigerator (that had previously been purchased for CSA#2's lab) and began collaborating with the cryogenics branch, who were developing sub-Kelvin refrigerators (ADRs – discussed in detail in the CADR innovation pathway).

Selection for a Flagship-oriented Technology Development Effort

In 1984, an NRA (NASA Research Announcement) was released as expected, requesting instrument proposals to fly on AXAF. Goddard's X-ray astrophysics group, lead by CSA#12, pitched to fly XRS (the X-Ray Spectrometer) based on a microcalorimeter detector plane. Given the extreme novelty of the concept at the time, they proposed to continue developing the incumbent technology in parallel.

> *At the present time, Si(Li) technology represents the most generally useful proven approach to X-ray spectroscopy over the energy range 0.3-10 keV. […] There is no denying that the CCD and calorimeter (discussed in the following section) technologies have great promise, and so we assume that the best approach to developing their enormous potential for AXAF is by their selection for this definition. Until that potential is objectively demonstrated, however, we feel that it is prudent to also include in the definition studies those already-proven technologies which are guaranteed to produce important new results whether or not the newer technologies approach their potential capabilities. (p 29)*

[5] In fact, "*the first experimental development coincidentally began simultaneously on both sides of the Atlantic in 1982. In Milan, Ettore Fiorini had been working on detecting neutrinoless double beta decay and, intrigued by a suggestion in a preprint by Guenakh Mitselmakher that the betas might be detected thermally, went to Niinikoski to investigate the practicality of this idea. They devised an approach that was developed into the first successful physics experiment using thermal spectrometers*" [D25]

> *The simple calculation below indicates, however, that it should be relatively straightforward (at least in principle) to fabricate a device which has FWHM ~1eV, or almost two orders of magnitude better than a state-of-the-art Si(Li) detector. (p.32)* [D1]

Despite its relative immaturity, XRS was selected as a category 3 instrument. The category 3 designation means that their scientific merit was rated extremely high, but the technology was deemed too immature to be incorporated into a flight project as is. The classification as category 3 translated into a year's worth of technology funding and the opportunity to prove that they could be mission ready. At the year-end review, the committee was impressed by their progress, but did not yet consider them mission ready, so they gave them a year extension on the technology development [I42 and I74]. As CSA#2 remembers it: "*They asked me what the remaining hard problems were; so I listed them. The next day, they gave us a list of things to do by next year. It was all the hard problems I'd listed!*" A year later in 1986, hard problems mostly solved, they were selected as the primary spectroscopy instrument for the mission.

A little background…

Each individual microcalorimeter detector has for main components, as shown in Figure **Error! No text of specified style in document.**-2: an absorber, thermometer(s), a weak thermal link, and a heat sink. A microcalorimeter works by measuring temperature changes due to an incident X-ray photon. The incident X-ray is "thermalized" by the absorber, producing a temperature change that is measured by the thermometer. The weak link then provides a conductance path between the absorber and the heat sink, allowing heat to be dissipated (so the absorber can return to its initial temperature). In designing efficient microcalorimeters, the goal is thus to find 1) an absorber material that is both a) opaque to x-rays, and yet has a low heat capacity (so that a single absorbed photon yields a measurable temperature change) and b) thermalizes well;[6] 2) a thermometer that is sensitive; and 3) a thermal link that is weak enough that the time for the base temperature to be restored is the slowest time constant in the system (compared with thermalization and diffusion times), but not so weak that the device is too slow to handle the incident X-ray flux.

Flight instrument detector planes consist of arrays of tightly packed detectors, with the number and size of the individual detector pixels determining the field of view and spatial resolution. At the array architecture level, key challenges relate to ensuring uniformity across the detectors, minimizing "dead zones" (i.e., packaging the detectors so that there are no gaps where incident photons can pass undetected), thermal isolation and reading out the multiple channels. While some of these challenges may seem like generic array packaging issues, characteristics of microcalorimeters create unique challenges for fabrication as will be discussed in the sections that follow.

[6] it must reproducibly and efficiently distribute the energy of the initial photon across a thermal distribution of phonons

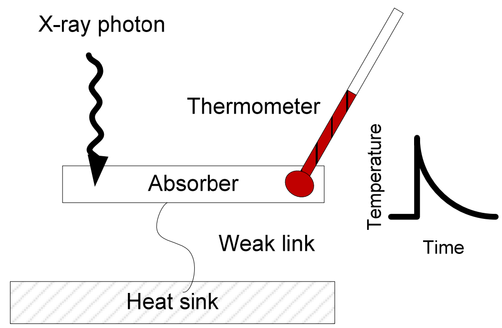

Figure Error! No text of specified style in document.**-2: Cartoon of an Individual Microcalorimeter (adapted from phonon group figure)**

Returning to the development…

The team addressed the detector and array level challenges essentially simultaneously, using mock arrays to test different detector architectures and configurations, and exploring attachment and arraying strategies in the process [I72, I68].

Initial Technology and Architectural Exploration: Making it work (1984 through late 1980s)

As the mission prospects for a microcalorimeter instrument materialized (in 1984), and a steady stream of funding from HQ seemed ensured, the team began expanding. CSA#8, who would become a key member of Goddard's microcalorimeter group, and CSA#14, joined the team as NRC post docs; in addition St#1, then a recently graduated undergrad taking a "break" before starting grad school, came to work on the project for a few years. They worked hard because *"it was exciting in the sense that it was all pioneering*" [I68]. Although much of the groundwork had already been laid, there were still many open research questions for the young researchers to tackle. As recalls CSA#8, "*when I got involved, it was like now let's think about how to actually make these things"* [I68]. "Making it work" proceeded along several component and architectural dimensions simultaneously, with each decision impacting several other factors. The below discussion is divided in terms of development streams; components first, architectural considerations later. All the streams were proceeding in parallel, but for clarity, they are described from start to finish independently, within the period under question (e.g., initial exploration).

Thermometers: Achieving the desired resistance in a repeatable way

By 1984, the first key decision, to use ion-implanted Si thermometer, had already been made based on a combination of material constraints and practical considerations. As explained in the 1984 NRA proposal:

> *We have baselined ion-implanted Si for this proposal because it has high enough Debye temperature to give a predicted FWHM which can be as low as 1 eV, and the implantation techniques required for the formation of the thermistor are well understood. […] Another possible material is diamond, which has a very high value of Debye temperature, but the required X-ray absorption […] demands a very thick detector, and the implantation of a thermistor is problematic…* (p. 33) [D1]

From the perspective of the science team, the thermometer question was settled in 1984. However, having chosen to ion-implant Si with (P and B), the team was confronted with a critical practical challenge which would occupy the Detector Development Lab (DDL) technologists for many years to come. The extant theory states that implantation dose determines the shape of the resistance-temperature curve of the base material. Thus, given a certain operating temperature, one should be able to control the resistance (and consequently measurement sensitivity) by means of the implantation density (also known as dose). However, in practice, resistance of the Si is extremely sensitive to the implantation dose, and the fabrication equipment at the time did not permit a high level of repeatability. As a result, the thermometer yield was extremely low [I83, 84].

To make useable thermometers at all, the DDL technologists employed a brute force strategy: They would put many wafers into the ion implanter and implant the full range of doses, and screen them later. In this way, the hope was that at least one of the wafers would have the desired electrical properties. As recalls the microcalorimeter fabrication lead: "*we were using this old ion implanter… we revised the process as much as we could within the hardware that we had, but it was still very variable from run to run*" [I83]. With the old tool, even implanting over the full range wouldn't guarantee a single useful wafer. "*What we ended up doing was buying this new ion implanter for a few million dollars. Its repeatability was much much better. Now you could at least get some wafer in the range that had the right dose*" [I83]. The strategy was far from efficient but it did guarantee a non-zero yield.

The decision to buy a new few million dollar ion implanter was a big one at the time. To put it into perspective, the cost of the ion implanter was equivalent to the two year budget of the entire microcalorimeter development effort at the time, and it is one of the biggest pieces of equipment that the fabrication laboratory has ever purchased. However the investment was made at a time when the lab was expanding – Goddard was developing microelectronics and detector fabrication as a core competency – and the microcalorimeter development effort had the support of more than one branch. The alignment of multiple (science needs) was key:

> *We got together, both my management and the management from quality assurance, got together and convinced the center that we needed [the ion implanter and a better clean room] With the combination of those, institutional money got put on it. That sort of argument goes around every year… there's multi-project support money… various parts of the institutions make their budgets and claims and you know lobby for this and you get your scientists to say, oh yeah, we need this to enable the next missions. In those days, there seemed to be more money than there is today* [I83].

Despite the equipment acquisition the ion-implanting strategy remained extremely inefficient (in terms of yield) and was subject to multiple sources of measurement noise; namely, "1/f noise" due to variability in the depth across the implanted dopant and Johnson noise produced by ohmic heating.

One parallel technology path that was pursued for a short time involved an effort to eliminate the Johnson noise term (which is endemic to resistive measurements). CSA#8 began investigating kinetic inductance thermometers, which measured changes in inductance (instead of changes in resistance) and therefore didn't dissipate any energy. As explained in the 1993 research proposal: "*The energy resolution of such a device could be an order of magnitude better than a calorimeter with a resistive thermometer with the same total heat capacity, and it should in principle be possible…*" [D2, p. 1]. However, flight project pressures in the late 1990s forced the team to focus on maturing the method that was already proven. "*Based on the limited success of kinetic-inductance sensors in the past several years, we suggest shifting the resources toward more promising alternatives [TES].*" *(*Reviewers comments re: NRA 95-OSS-17, D5) The Kinetic Inductance approach was never revisited, even when exploration resources became available later, because by that time, the TES approach had indeed demonstrated more promise [I84].

Absorber Material: Finding a material that would thermalize X-rays

The choice of ion-implanted silicon had implications for absorber design as well. The relatively low sensitivity of silicon thermometers imposed a requirement for a low heat capacity absorber. Coupled with the need for high stopping power (at the energies of interest) and good thermalization (consistent temperature response to incident photons), the candidate material choices were quite limited [I84, I72]. The earliest microcalorimeters employed silicon absorbers, partially because of the team's prior experience with the material, but also because of silicon's relatively low heat capacity. However, as test devices were constructed, they discovered a large additional noise term not predicted by theory, and it was immediately clear that a separate absorber would be needed [I88]. They believed that this was due to incomplete (and variable) thermalization of the incident photon. So, they began searching for higher efficiency thermalizing absorbers "*by attaching small samples of materials with low heat capacity and small or nonexistent band-gaps (e.g., metals, semimetals, narrow-gap semiconductors, and superconductors) to working detectors*" [D33].

As recalls CSA#8 "*it was a real challenge to find a material that would thermalize x-rays*" [I68]. They explored a wide range of semiconductor materials, and in this respect, the interdisciplinary nature of the team proved advantageous. Recall that the concept originator (CSA#2) was an IR Astronomer by training. "*The key thing about this whole process was this confluence of the right backgrounds […] [CSA#2] had a tremendous amount of knowledge of solid state materials that could be used as thermal sensors, so that's where it started*" [I68]. The alternatives they turned to were the materials that one would expect to find lying around an infrared lab [I88]. In addition, they drew on expertise from colleagues in the international community as well as in other groups at Goddard. "*We weren't really going through yellow pages to find material scientists, but we were talking to our colleagues in the field […] discussing things and trying things out*" [I68]. They tested InSb, Bi, HgCdTe, HgTe, and sapphire, eventually settling on a mercury telluride (HgTe) absorber [D33]. HgTe enabled an order of magnitude better energy resolution than the original Si (see 17 eV breakthrough in 1987 – Figure **Error! No text of specified style in document.**-1). As explains CSA#10:

> *HgTe turns out to be pretty much the only absorber you can use with the Si thermistors, and it's a compromise. It's got high Z, high-atomic-number constituents, it's a semi-metal so it doesn't have a gap […] it's Debye temperature (lattice specific heat) isn't particularly wonderful, but it's acceptable… But it's hard to grow which means it needs*

> *to be grown separately from the detector and be attached with epoxy one detector at a time…* [I72]

CSA#2 framed the materials choice slightly differently. In his mind, there may well be better materials, but HgTe works well enough that it was no longer worth looking for a better alternative:

> *We don't have any illusions that what is being used now is in any sense optimal. It's something that works. And materials problems are something that are typically very difficult so no one wants to actually go off on the "great quest" unless you really have to do it.* [I88]

Although the absorber material question was effectively put to rest after the 17 eV HgTe result, some parallel investigation of superconducting absorbers continued into the late 1980s. The idea was to leverage the fact that certain superconducting materials have i) very good stopping power and ii) very very low heat capacity below their transition temperature, so, in terms of stopping power per unit of heat capacity (the relevant figure of merit) they're spectacular. This property is particularly relevant at higher energies, and that's where it was first investigated seriously [I72]. The problem (in the 1-10keV range) was that superconductors don't thermalize X-rays quickly enough to be practical [I89].

This issue of thermalization rate led to another development branch. Also in the late 80s, one member of the team began working on superconducting tunnel junctions. Tunnel junctions are another form of non-dispersive spectrometers. Instead of measuring energy as heat, they measure energy as charge. The main potential advantage of this approach was the extremely high photon counts that it theoretically enables. However, in practice, collecting every electron-hole pair created by the incident X-ray proved at least as difficult as the complete thermalization problem. And, as with the superconducting absorbers, and kinetic inductance thermometers, the technology branch was pruned before its feasibility had a chance to be established due to program pressures to focus [I84]. In fact, other researchers in the community did pursue this path; although the concept never did yield fruit.

Absorber Attachment: Achieving array-wide uniformity

The choice of mercury telluride (HgTe) for the absorber material meant that each absorber needed to be attached individually post-fabrication... by hand. This literally entailed "*physically glue[ing] each of the absorbers onto the pixel with epoxy… and if the epoxy ran, it created a non-uniform attachment point… and unpredictable thermal performance*" [I84] with a range that extended outside acceptable limits for instrument performance. The necessity of by-hand attachment was "*painstaking*" and limited the scalability of array sizes to the number of individual absorbers that could be realistically attached by hand. For AXAF the array was only 6 x 6 (i.e., 36 pixels), but for future missions, the science demanded a much larger field of view. The out-of-limits thermal variability posed a near-term and more serious problem. The *unpredictable* degraded energy resolution couldn't just be calibrated out, and thus necessitated post-production screening.

The "*big breakthrough*" was a clever, albeit crude, "*engineering solution*" that came from the scientists [I84]. They solved the epoxy running problem with a silicon spacer – glued between the thermometer and absorber, it served to manage the surface tension of the epoxy and maintain a constant bond joint from pixel to pixel. The attachments still needed to be made by hand, but at least now the interface was thermally consistent. This enabled good enough "array resolution" to meet AXAF's requirements.

Thermal Isolation: The weak link and crazy legs

The microcalorimeter cartoon in Figure **Error! No text of specified style in document.**-2 represents the "weak link" as a thin squiggly line between the absorber/thermometer and a heat sink. In the real system, this thermal isolation was achieved by etching all but very thin silicon beams which maintained a "weak" attachment between the thermometer and the array structure. The key design parameter for the weak link is a time constant - the thermal decay time constant needs to be slow enough for complete thermalization, but faster than the flux of incident X-rays. Achieving the necessary thermal isolation in practice presented one of the most difficult fabrication challenges [I83].

In the late 1980s, Goddard's DDL (and the semiconductor industry at large) was primarily using wet chemistry techniques for micromachining silicon. This limited the patterns that could be etched. Specifically, lines tended to be straight, with right angles, following the crystal structure of the material [I83]. From the perspective of thermal isolation, this presented a problem because "*if you had straight legs with regular cross-sections, the surfaces look like mirrors to the phonons… they don't scatter… makes it a very good thermal connection*" [I83]. The normal strategies of making the legs longer or thinner didn't solve the problem, because they were still "regular cross-sections." So, the technologists tried various approaches, including what they called "*crazy legs*" (i.e., physically wiggling the legs "*so there's no line of sight*"), or leaving extra nubs of silicon [I83]. Eventually, the technologists became aware of a silicon texturing technique developed by a small company in California, whom they had met at a conference. As recalls CSE#6 "*that gave us something we could engineer*" [I83]. What he meant by that statement, was the texturing approach gave them a set of parameters that could be optimally tuned to solve the problem at hand.

Arraying Microcalorimeters:

Nonetheless, the challenge with all three of these approaches was that it added bulk to the support structure with implications for the array architecture. An important detector plane design parameter is the fill factor (the percentage of the field of view that is active). The goal is to position the individual pixels as close together as possible. This becomes challenging, when the detectors need to be thermally isolated, or read-out with detailed spatial resolution. Ideally, the support structure can be hidden behind the active part of the detector, but with the requirement for bulky, wiggly legs this proved practically infeasible with the existing technology.

As a result, the first generation of microcalorimeter arrays were one dimensional, with twelve pixels positioned side-by-side in a row [I83, I88]. This left room for the beams to protrude from either side. However, the science requirements demanded a wider detector plane, so the team explored multiple strategies for producing the equivalent of a two dimensional array. Among other approaches, they tried vertically stacking multiple 1D arrays so that they would appear 2D from above. Eventually, however, they realized that they could meet the AXAF science requirements with a so-called bi-linear array. This solution involved positioning two 1D arrays in opposite directions, so that all the support structure protrudes outside the field of view.

Shifting context: AXAF Split

During the mid to late 1980s, the team enjoyed a fair amount of autonomy, charged to solve some extremely difficult technical challenges. However, the level of political exposure that produced this shelter can also cause major shifts in the project development context. In the case of AXAF, the development context was abruptly changed in 1992 when, faced with budget constraints, the project was split in two. The first mission, dubbed "AXAF-I" would fly the imaging (I) instruments; the second mission, "AXAF-S" would be the spectrometer (S) (and host the microcalorimeter array). Although the AXAF-S mission idea persisted for another year, no one was terribly surprised when in 1993 AXAF-S was "*sacrificed*" in the budget. However, the language in the budget that year left the door open for XRS to be recast as an international collaboration [I74]. Shortly thereafter NASA signed an agreement with ISAS (the Japanese agency responsible for astrophysics before JAXA was formed) to this effect. Now, instead of flying AXAF-S as a stand-alone mission, NASA would provide the XRS payload to the Japanese as the primary instrument aboard the 5^{th} Japanese X-ray Astronomy satellite, Astro-E [D26].

Ironically, the main impact of the transition from AXAF-S to Astro-E was to raise the intensity of the schedule pressure. As explains CSA#8, "*there was now a very certain launch date because where on the US-side, the AXAF program was being delayed year for year for a long time and then got split off into AXAF... The partners in Japan had a good reputation for once they chose a launch date, they really made it, so if anything, going to Astro-E just made people really realize ok, we have to make things work now*" [I84].

In addition, the shift in mission management to Japan actually gave more control to the science team. This statement may initially seem counterintuitive since the added layer of international collaboration typically rigidizes relationships; however, in practice, two elements combined to produce the stated result. First, the prevailing culture at ISAS pays deference to scientific judgment (compared to at NASA, where project managers typically come from engineering backgrounds). As a result, the Japanese managers were relatively more receptive to scientific rationales, and trusting of science-team derived schedules. Second since the instrument was formally managed by a Japanese project manager, the in house Goddard team was "*weaker*" than it might have otherwise been. Coupled with the fact that the original XRS project scientist was also Goddard's Director of Space Sciences at the time, the balance of power was shifted to science compared to other projects. The result was that instrument technology development continued much longer into the formal project than is typical [I74].

Evolving a Project-specific Development into a Research "Group" (early- to mid-90s)

Through the mid 1990s with the development of the flight detector array for Astro-E well underway, the team experienced a "changing of the guard" while also expanding, in terms of both personnel and technical approaches. This was partially an organic evolution and partially an active decision on the part of Goddard management to ensure a solid team of "in-house" experimentalists (to avoid a repeat of the need to sub-contract with a University Professor re: AXAF) [I88].

Through the first decade of microcalorimeter development CSA#2 had stayed intimately involved with the project. However in the early 90s two things happened which forced him to reduce his involvement; and consequently make room for the "new blood." First, the COBE satellite had been launched and was returning data; and the analysis work that entailed was keeping the IR Astronomers busy (recall that COBE was the application context for which the original thermodynamic noise theory was developed). Second, the head of CSA#2's group was promoted and as a result, the larger entity that he now managed was moved to a new building. The result was much less frequent interactions. As CSA#2 explains: "s*urprisingly, or maybe not surprisingly, a quarter mile of distance puts a fairly high potential barrier…*" [I88] This physical move corresponded with the first PI (CSA#12) taking over as Director of Space Sciences and handing off the project to CSA#8 (one of the post docs who had been hired in '84). While CSA#2 and 8 were close colleagues, their relationship was different than what existed between CSA#12 and the rest of the "*old guard*." The combination of factors resulted in CSA#2 spending less time in the X-ray lab. "*It was probably all for the best that I wasn't there as much because it created a little more space for the new generation… and they were good. I tried to keep my fingers in the stuff… but I wasn't working day to day"* [I88].

It was also during this period that the Goddard microcalorimeter group was formed. It's difficult to pinpoint precisely when the group became a group, but "*by 1995 we're certainly a group in that we have multiple [dedicated] people in our team, they're working on different things – we started to split up and specialize"* [I84]. In addition to the characteristics of more than one person, with different specialties, the microcalorimeter group began applying for and sharing resources to pursue new developments not explicitly related to the project that had originally brought them together [D2-24].

The Goddard team also became an acknowledged leader in their field. In the 1996 NRA, the evaluator noted that "*the proposing team [is] an experienced group with a proven track record…*" [D5] The team believes that being known made Goddard an attractive place to be: "*it helps define a group if you're known and there are people seeking you out in terms of job applications and post docs"* [I88]. During this period, two new full time scientists were hired as well as a compliment of engineers and technicians. The first scientist formally joined the group in 1992 as a post doc, although she had already been part of the "*inner microcalorimeter circle"* for several years. She had become connected with UL#3 and CSA#2 through her dissertation work on Compton Scattering. In fact, it was her need for higher energy microcalorimeters that had led the team to explore superconducting absorbers in the 1980s [I89]. She also brought a connection to Stanford University, which would become important in the next generation microcalorimeters. The second scientist had also taken a circuitous path to Goddard, and his past connections would also provide a link to the third generation of microcalorimeter approach (of lesser focus in this research).

The team branched out in a number of parallel technical development trajectories as well. They continued development of semiconductor thermometers for both hard X-ray spectroscopy as well as soft X-ray (suborbital sounding rocket, XQC) cosmic background scans. In addition, just as it seemed that a practical performance plateau had been reached on the semiconductor thermometer technology trajectory, the team became aware of a promising new approach based on superconducting transition edge sensors (TES).

In order to maintain the flow of the narrative, the semiconductor thermister pathway is told in its entirety first, with the TES story following.

Silicon Thermisters in the late '90s: prepping for flight

By the late 1990s, most of the team's silicon thermister microcalorimeter efforts were devoted to developing flight hardware for Astro-E. "*My recollection is that we were struggling to build something up until the last moment; trying to see what's the best we could fly*" [I90]. Their R&D efforts, on the other hand, had shifted almost completely to the new class of TES calorimeters.

> *We feel that calorimeters using transition edge sensors (TES) offer the most promise of meeting these requirements [for future missions], so we propose to concentrate our development efforts on such detectors* [D9].

However, at the turn of the millennium, two unrelated events – a launch failure and a decadal survey ranking – fundamentally changed the microcalorimeter development context, and gave the incumbent technology new life.

Launch Failure of Astro-E (2000)

In 2000, Astro-E should have been the first flight demonstration of the microcalorimeter spectrometer, and the last use of the silicon thermister microcalorimeters. However, the ill fated Astro-E spacecraft was destroyed 42 seconds after launch, due to a booster failure. The team spent less than a day "*mourning the loss"* [I69] before they began franticly scraping together a proposal for a redo. Fortunately, and unfortunately, proposals for the next SMEX call were due a week later. Miraculously, they made the deadline and received funding as a "mission of opportunity" (MoO). MoOs are designed to support payload's being developed for missions lead by other countries. In this case, the XRS team was bidding to be a NASA furnished instrument on an ISAS follow-on to Astro-E (if there was to be one). The Japanese simultaneously worked to convince their government that they needed to try again. Both were successful – there would be an Astro-E2 and NASA would provide the spectrometer; and that spectrometer would employ silicon thermister microcalorimeters [I68, I69, D26].

Constellation-X Ranked Second (2001)

Early in 2001, the 2000s decadal survey "Astronomy and Astrophysics in the New Millennium" was released and Constellation-X was ranked second (among major space-based initiatives), after the James Webb Space Telescope (JWST). Based on these recommendations, NASA initiated JWST as a formal program, and directed some money to Goddard for Con-X "pre-Phase A" technology development and mission studies. While the directed study provided a strong motivation to continue TES development, it imposed much less certainty than a formal program would have (leaving further room for a future resurgence of silicon thermister R&D efforts).

If Astro-E hadn't ended up in the ocean, things might have been different. If Con-X had been selected first in the 2000s Decadal Survey, things might have been different. But, Astro-E was destroyed on launch and Con-X was ranked second, so instead of demarking a clean shift from a silicon thermisters legacy to a focused, funded TES microcalorimeter future, the new millennium saw parallel, albeit mutually dependent, development paths for the two technologies.

A New Opportunity for Semiconductor Thermometer Exploration

The SMEX proposal that the team submitted in February of 2000 defined identical detectors to those that had been delivered for Astro-E. Given that the design had incorporated all of the latest development, there was no need to update [I84]. However, the project approval didn't happen overnight. It wasn't until 2002 that all of the funding had been approved, and the international agreement with Japan had been put in place. During the intervening time, "*we were busy trying to improve the state-of-the-art of these detectors*" [I84]. The team was funded by the same sequence of ROSES/NRA grants that had sustained them through the 90s. During the 2000-2002 window, they made three major improvements, bolstered by progress in related domains and a new team member with complementary experience.

Now more than a decade-and-a-half since the beginning of the microcalorimeter development, the fabrication lead CSE#6, had been promoted into a branch head position and been encouraged to take on a wider range of projects [I83]. His career had progressed as is common for engineering professionals, out of the technical into the managerial. His replacement, CSE#7 was already quite an experienced member of the DDL, and had been previously involved in the development of bolometers (the basis for CSA#2's original microcalorimeter idea). She immediately brought to bear some insights from that parallel development, as well as novel semiconductor fabrication techniques that had evolved in the industry at large [I87].

Thermometers: Eliminating 1/f noise and improving yield

Recall that the first generation of microcalormiter thermometers were made by ion-implanting silicon wafers as uniformly as possible. And, that although the desired resistance R(T) could be achieved by implanting the full range of doses and post-screening the resultant wafers (which was inefficient in terms of yield), for any given resistance, variability in the depth distribution of the dopant created a so-called "1/f noise" term (reducing the sensitivity of the measurements).

During the second window of exploration, several attempts to address the noise challenge were made. One was conducted as "the deep doping DDF." The goal was to achieve the desired depth distribution uniformity by incorporating the doping into the growing process itself. They worked on it for a year, but were never successful. At the time, the process of growing silicon wasn't well enough controlled to "*count the right number of atoms going in as they did the growth*" (which would have been required) [I87].

The approach that eventually became standard involved the adoption of a new wafer material; it solved both the 1/f noise and yield issues simultaneously. The material was called Silicon on Insulator (SOI) and consists of a thin layer (1µm) of Silicon separated from a bulk layer of Silcon by a thin oxide layer [I87]. The idea came from a group in France, as will be discussed in more detail below (in the section on mechanisms of adoption) [I88]. The oxide layer served as a diffusion stop. Now, when the implanted silicon was annealed, the dopant diffused uniformly within the limit provided by the oxide layer. This eliminated the 1/f noise term almost completely, effectively "*putting us on a different curve*" [I88] with respect to noise reduction.[7]

With the new material, the yield jumped from ~3% to nearly 100% almost overnight [I84, I87]. They were still implanting over the full range, but now, nearly all the devices that you would expect to work did, so the screening was easy [I87]. Combined with the fabrication improvements discussed below, Astro-E2's energy resolution was twice as good as the array flown on Astro-E (6 eV compared to 12 eV) [I88, D43].

Detector attachment: crazy legs revisited

In the same way that SOI wafers had a built in diffusion stop, the oxide layer also served as an etch stop. Coupled with the semiconductor industry-wide transition from wet etching (and the corresponding need for straight lines with right corners) to DRIE "deep reactive ion etching" (which enable complete pattern flexibility), many of the challenges faced during the Astro-E development were now easily overcome.

From a fabrication perspective, as recalls CSE#7:

> *When I took it [Astro-E2 detector fab] over, the design looked very much like it would have looked for the bulk silicon devices. There were straight legs, square corners, all sorts of things that you have to have for bulk silicon devices… and I said, ok, but we have lots of things we can do with SOI and with dry etch techniques that we've learned with microbolometers… we can make curved structures, we can make strain relievers… we can do all kinds of things. We can fix problems that had come up during Astro-E…* [I87]

From the perspective of the science team, each step in the transition for straight lines with square corners had a purpose, and performance needed to be carefully characterized before the move to crazy legs could be made. As explained by CSA#10:

> *The set of test devices made to first characterize the SOI technique for x-ray devices was also designed to help us quantify thermal conductance in the very thin silicon... the series of devices with straight legs of different widths and lengths was to help us characterize the scaling of thermal conductance…The move to bent legs also had to be accompanied by mechanical analysis (to make sure the resonant frequencies of the structures were acceptably high.)*

In the end, the new techniques allowed for significantly more compact support structures and essentially eliminated the array "fill factor" problem discussed above. The weak link could now be hidden completely behind the absorber material, with the detectors packed as tightly as desired in a real 2D array.

Absorber attachment: a slightly less painstaking process

[7] The team had previously been stuck in a "*square root regime*" (i.e., if you wanted to make the devices quieter, they needed to be bigger, which, in a small area meant thicker. But, when you increased the volume, you increased heat capacity, which reduced potential energy resolution (nearly defeating the purpose of the volume increase) [I84, I88].)

Of course the need to attach each absorber by hand persisted, and continued to limit feasible array size. However, even though the new techniques couldn't eliminate the manual post processing step, they could at least incorporate the need for a spacer into the silicon design (see **Error! Reference source not found.**).

> *Once they told me how they have to glue the absorber onto the calorimeter and they told me they had to put like a little silicon chunk in there to stand it off from the thing, I said well wait a minute… I've got a way to do that much better, make your little spacer out of this material I've been working with.* [I87]

At first the scientists were skeptical about making a design change that didn't directly improve performance. In fact, they carried the two development paths for some time. It may have partially been a conscious, or subconscious, boundary contest about where scientists or technologists should have relative design authority. Either way, once the scientists had the opportunity to verify the performance of the embedded spacers "*they found that the SU8 spacers were much better than having the silicon spacer, they adopted that as well.*" "*There were numerous changes that were adopted from things that I'd learned working on other projects.*" [I87]

Mechanisms of adoption, on the process side

Between Astro-E and Astro-E2, the team achieved a resolution improvement of a factor of 2. These improvements were due to process improvements in the way they manufactured the device (not what they manufactured – i.e., they were no longer in a "*technique limited regime*") and were enabled by innovations in the related semiconductor industry, imported to the microcalorimeter development as part of the tool kit of a new addition to the team. In fairness, Goddard (and the previous technologist) had already begun to make the transition to DRIE etching techniques, but they were using them to produce better versions of the same long straight legs, rather than exploring the full range of new microstructures that could now be achieved.

The idea to use SOI wafers for microcalorimeters was initially suggested to the X-ray group by CSE#7, and came indirectly from CSA#2 and the parallel bolometer work. He had become aware of the relevant material and technique as a "silver lining" to a failed bolometer instrument proposal bid. During the revue, CSA#2 noticed that the winning detector concept, developed by a group in France, was achieving better performance than should have been possible. He knew their volume, and given Goddard's scaling laws, they should have been too noisy. Yet, the results clearly showed that they weren't. "*They were doing better than they had any right to… that led me to believe that they had a fundamental advantage.*" [I88] Upon deeper inspection, the "fundamental advantage" was a far better implantation technique; they were using the diffusion in SOI technique described above.

It's worth noting that the technique wasn't a secret because of any active IP (intellectual property) protection on the part of the French company. It just so happened that "*the head engineer was a Frenchman who didn't speak any English, so he wasn't going to present anything, and the higher ups who spoke English didn't have the detailed expertise to know the most important part of the design… so the information just wasn't coming out.*" [I88]

The proposal competition enabled CSE#2 to see the results first hand, and he brought the technique back to Goddard. As he puts it: "*I've always been a big fan of copying anything that looks like it's going to work; because we don't need to, or want to, invent these parts. If you can get it from somewhere else you should get it from somewhere else.*"[I88] Funded by a DDF "the ultra thin bolometer array DDF," CSE#7 (the Astro-E2 fabrication lead, who at the time was CSE#2's lead technologist on the IR microbolometer project) began investigating the application SOI, and in particular, the diffusion techniques. The original intent was to make bolometers on a nitride membrane. As recalls CSE#7: *"I was supposed to back-etch the silicon and leave the free-standing nitride structure. But I didn't do what I was told to do… I removed the oxide and nitride and left the free-standing silicon…and I said look at this stuff this is beautiful material"* [I87] The ultra thin, flexible yet strong, single crystal, completely planer structure that remained was exactly what they needed. CSE#2 agreed. "*That little DDF many years ago enabled all of this. All of the bolometer arrays and the first microcalorimeter arrays that were done with ion implanted SOI membrane."* [I87]

Throughout her time on the microcalorimeter project, CSE#7 continued to work on the microbolometer development as well. Architecturally, IR microbolometers and X-ray microcalorimeters are very similar, so from a detector fabrication perspective, there is a lot of synergy. However, since they serve distinct science communities, cross-pollination of solutions is not always achieved as often (or as soon) as one might hope. As fabrication got more sophisticated, particularly as both microbolometers and calorimeters transitioned to TES, a group called the "TES roundup" was established to facilitate knowledge transfer. The idea was to bring all the scientists and technologists together on a monthly basis to discuss common challenges. During the early stages of the development, attendance at these meetings was high, but as the crunch of flight projects hit, the meetings occur much less frequently and attendance is quite poor. Told from the perspective of the technologist:

> *[W]hen these things were in their early stage of development, those meetings were a very useful tool to get the scientists in the same room, talking to each other… Now the only way they talk to each other is if they have a problem and you convince them… 'maybe the X-ray group has seen something similar, you could call them?'* [I87]

Implicit in the above quote is an expertise hierarchy between the science and technology communities at Goddard. While the scientists certainly have a lot of respect for the technologists' expertise in fabrication, that expertise is somehow seen as a lower art. Technologists make design suggestions, and Scientists make decisions. Technologists make the devices, Scientists tests and verify them to make sure they meet the science goals (although this is not the case for all instrument developments). In addition to the above quotes, the below illustrate this relationship:

> Technologist: *"We're the machine shop. The X-ray group are experts in their field, world leaders and all that, and so they have a very clear idea of what they want and how to get it"*

> Scientist: *"I became aware that there was concern on the detector systems branch, that they weren't getting as much credit … so we started making sure that they were always on our papers."*
>
> Technologist: *"We're technical partners in this. They're the scientists and we're the technology people."*
>
> Scientist: *"It's a long standing collaboration [with the DDL]that's been very fruitful… we're working together on figuring out how to make these things work, not either saying these are the science requirements, now you design it, nor this is the design, now you make it… they're part of the group, they're part of the effort"*
>
> Technologist: "*The way things work here at Goddard, even for technology development you really need a scientific justification for it, so you always want to have a scientist on board with your proposals if you expect them to win. We've got a lot of great ideas, but if it doesn't have some kind of bearing on the science community then it's not going to win.*"

Con-X Delays: A Third Opportunity for Semiconductor Microcalorimeter Exploration (2003/4)

Although Astro-E2 (like Astro-E before) had been intended as the last flight of silicon thermistor microcalorimeter technology, funding uncertainties in the Con-X program gave the semiconductor development program a (second) new life. In 2003/4, NASA-level funding "reallocation" prompted the team to hitch their technology program to a nearer term opportunity, while diversifying their development efforts. This logic was articulated in the 2004 proposal excerpt below:

> *When we wrote the predecessor to this proposal in 2001, our primary goal was to implement TES microcalorimeters in the format required for Constellation-X. While we have made impressive progress towards that goal, **the goal itself is shifting**. With the extension of the Constellation-X time line due to reallocation of the NASA budget, changes to the reference design are already being discussed. **The delay to the Constellation-X program** (first launch now projected for 2017, while once we were working towards 2010) **make NASA participation in an intermediate term x-ray spectroscopy mission, such as the Japanese New X-ray Telescope (NeXT)** [Tawara et al., 2003], **a strategic investment.** We propose to continue to develop arrays of x-ray microcalorimeters, no longer towards a single, specific spaceflight implementation, but rather towards several different strategic optimizations. These include the original Constellation-X baseline, arrays of the larger but slower pixels needed for NeXT, optimization of arrays with more imaging elements as needed for some reconfigurations of Constellation-X or vision missions such as Generation-X and MAXIM, and optimization for the soft x-ray band. The end product of our research will be a robust technology that can be readily optimized to address a variety of questions about the origin and evolution of the complex structures of the Universe.* (emphasis added) [D14]

Compared to Astro-E and E2, the NeXT science objectives would require a much larger detector plane. This forced the team to begin addressing the "by-hand" scalability limitation of the HgTe absorber attachment (even though this was no longer a long term concern with the TES approach). Increasing the number of detectors also resurfaced some of the heat dissipation issues discussed earlier. A number of technology development efforts were initiated in preparation for a NeXT bid.

Automated Absorber Attachment

Recall that the state-of-the-art in absorber attachment involved carefully dabbing four dots of epoxy, under a microscope, onto four attachment points on each detector and then carefully positioning the HgTe absorber. For the 6x6 Astro-E/E2 detector arrays, this process was tedious. For the 16x16 array that would be needed for NeXT, it verged on impractical. For the 1000+ pixel array planned for Constellation-X, the idea seemed insane. So, the DDL began investigating methods to automate the attachment process. Within a year, they settled on a stamping approach. "[W]*e found a way to automate it with stamping. We'd stamp into the glue and then stamp into the absorbers; let it cure; pick them up; and then the absorbers were stuck to the detector*." [I87] Now instead of the number of post fabrication steps scaling with the number of pixels in the array, the extra steps were fixed at one. However, this technology was never used, due to another context change (described below). Nonetheless, the capability to attach large numbers of "things" to arrays of "things" is a valuable, generally applicable, tool that will certainly be used in the future [I87].

Low temperature, thermally isolated electronics

A fundamental challenge of implementing low temperature detectors is that the detector needs to be kept cold (in this case <70 mK), while being readout by electronics that both produce heat and function poorly at cryogenic temperatures. Specifically, as the number of pixels in an array grows, so do the number of readout channels, and by proxy, the magnitude of the heat produced by the electronics. The cooling challenge can be addressed both by improving the cooling power of the cryogenic system (see CADR case) and improving thermal isolation of the electronics.

Both semiconducting bolometers and microcalorimeters use Junction Field Effect Transistors (JFET's) as the first elements of the detector readout chain. The challenge is explained clearly in D20:

> *For the Japanese New X-ray Telescope (NeXT), the pixel count will be increased by more than a factor of 10, and yet the array and readout circuitry must still fit within roughly the same envelope as XRS. Since the JFET's operate at temperatures substantially warmer than the detectors, complicated mechanical methods of thermal isolation (tensioned Kevlar and hand-soldered 0.5 mil wire) have been necessary to operate the transistors without loading the detectors with heat or dumping too much power to cryogenic systems. The existing transistor circuit packages will be too large to scale up to 400 detectors, so we need a way to make JFET assemblies that also incorporate thermal isolation as well as overall miniaturization and large-scale monolithic fabrication. By making devices with inherent thermal isolation, we stand to gain large savings in volume and power dissipation, thereby making much larger arrays of*

semiconductor bolometer and x-ray microcalorimeters possible, and with high uniformity and reliability.

Funded by three years of DDF (2001-2003 and 2005), low-noise JFET module-on-a-chip's were fabricated on SOI wafers. However, the project was abandoned before it reached maturity due the following context change.

Another Failure

The second re-birth of silicon thermistors was further crystallized by another failure. In 2005, the second opportunity to obtain high resolution high throughput X-ray spectroscopy also ended in disappointment. Although Astro-E2 launched successfully, the cooling system failed 2 months into the on-orbit check-out, rendering the cryogenic microcalorimeter system useless [D44]. They just couldn't catch a break. And this time a follow-on opportunity was not immediately available.

Playing it a little more conservatively: Astro-H instead of NeXT

When the 2004 ROSES proposal (D14) was written, NeXT was evoked as a potential near term application; an R&D justification for diversifying the group's technology development efforts to mitigate against the uncertainty associated with delays in the Con-X program. After the 2005 Astro-E2 failure, the nature of NeXT as a flight opportunity changed in several ways. First, it seemed more certain that NeXT would fly in some form, since both governments would want to capitalize on all the previous investment. At the same time, mission success became even more critical; the third time had to be the charm. Second, it reduced the threshold of what would constitute sufficiently new science. All the arguments for why Astro-E and E2 could answer important science questions still applied, and now NeXT could answer them for the first time, rather than needing to build on those discoveries. Combined, this meant that for the post-Astro-E2 failure NeXT, hitting a low price point was more important than advancing the frontier of the possible. In practice, this translated into reducing the active pixel count to 32 and reusing hardware that was originally developed for Astro-E2.

From the perspective of the scientists, this was a strategic decision:

> *When we wrote our NeXT proposal… there was always all kinds of strategy type things going on, about how much can we afford to ask for and still be competitive… and the thinking was that we should try to keep our budget to about half of the total budget that was available so that NASA will be able to pick more than one Mission of Opportunity (MoO). If we tried to grab all of the money, that might be too much of an all or nothing. So we were trying to keep our proposal to a certain level… once we did that it precluded the larger array.* [I84]

From the perspective of the technologists, it was demoralizing to say the least. As expressed with varying levels of political correctness[8]:

[8] These quotes are not attributed at the request of the informants.

They won the proposal, but then it appeared that they had to back off on the number of pixels that they were going to be able to fly, so all those new technologies that we developed weren't going to be used.

I think that our branch would have been happier, after all the work that we did, if we'd have been included in the flight proposal. If you go back through the development work, and the strong arguments that lead to them winning the proposal, much of that work was done here. And then to have us be the only ones at Goddard to be cut out of it, at Goddard?… I think that was a little bit short sited. I know why they did it, and I know that they were instructed to do so by higher ups, and I know that their main driver was to win the darn proposal, but…

It was a big hit to the Branch to lose this valuable flight work. But then TIRS came along and the Branch focused our resources on that, so we managed to stay employed!

The strategy was successful. Three years after the Astro-E2 failure, in 2008, the calorimeter group won their second MoO to develop SXS (Soft X-ray Spectrometer) to fly aboard Japan's 6^{th} X-ray astronomy mission – Astro-H. The launch is scheduled for 2014, 30 years after this innovation pathway was initiated.

Concluding comments

Assuming that Astro-H returns data successfully, and now that TES is fairly mature and is markedly better for larger field of view applications, it is unlikely that semiconductor-based microcalorimeters will be used again. Nonetheless, it is interesting to note the extent of the performance improvement that occurred between the AXAF baseline and what will eventually fly on Astro-H. Given that TES were invented, and projected to be vastly superior as early as 1996, had political and technical context changes not perpetuated the old technical approach it is unlikely that these process-enabled performance improvements would have been realized. Today, the performance differences between Thermistors and TES (at the individual detector level) are negligible. The key difference is one of scalability – TES absorbers can be fabricated monolithically, and the readout electronics are lower power with better isolation. However, had the tradeoff between improving the semiconductor approach vs investing heavily in the TES approach been articulated in the above terms, the conclusion to transition would likely have been different. This is not to say that they made the wrong decision, rather to point out the extent of performance improvements that were achieved through process refinement, after the first flight instrument.