The Superconducting Transition Edge Sensor (TES) Microcalorimeter Innovation Pathway

Gestation (pre-1996)

In a sense, all of the previous decade of semiconductor microcalorimeter development played an important role in the gestation of the TES microcalorimeters. By the mid 1990s, the Goddard calorimeter group had emerged as a recognized leader in the field, and as a result, had continued to attract top talent from, and established strong connections with, most of the leading institutions and groups around the country. In addition, an enabling infrastructure of fabrication and test facilities was already well developed and staffed within the DDL. However, while this network and these complimentary assets were important enablers of the TES development, in many respects, the new TES technology, which was invented outside of the Goddard microcalorimeter community, was competence destroying.

Goddard was made aware of the promise of the new TES technology during a colleagues' seminar, given at Goddard in 1995; the speaker was a professor that CSA#10 had known during grad school. "*Everything is connections. Because he knew me and [CSA#2] he visited Goddard and wanted to catch us up on the latest*" [I72] He shared recent results from one of his grad students' work on TES that looked very promising. At least in part because of their shared pasts, CSA#10 took the claims seriously on their face and began investigating the new area. The results did indeed look promising. Although the theoretical limits of TES sensitivity[1] were similar to those of the ion-implanted silicon thermometers, the new approach had two key practical implications that made the prospects of reaching the professed theoretical limits more reasonable.[2]

Superconducting and semiconducting microcalorimeters both perform non-dispersive spectroscopy by measuring temperature changes in an absorber, due to the thermalization of X-ray photons, as changes in resistance. The main difference is that superconducting thermometers permit a higher heat capacity budget since they are extremely sensitive thermometers in their transition regions. Firstly, this meant that the low heat capacity constraint that had driven the selection of HgTe as the absorber material, no longer applied. Now normal metals could be considered, which thermalize deposited energy quickly and efficiently. This also had huge implications for manufacturability and consequently scalability of the array field of view. Where each HgTe absorber needed to be attached by hand, now absorbers could conceivably by deposited as part of the fabrication process. The second key implication became more relevant later in the development. Where ion-implanted thermisters were readout using JFETs (which had heat dissipation issues and didn't multiplex well [D20]), TES devices used SQUID (superconducting quantum interference device) readouts, for which these challenges were less of an issue. "*So we started getting into the field.*" [I72] In fact, TES was already called out as an important area for future investigation in the 1996 X-ray calorimeter NRA [D5].

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[1] As part of his dissertation, the grad student extended the 3M calorimeter theory to include superconducting devices and predicted a theoretical limit of ~1eV energy resolution [D38].

[2] *At the time, there was a bigger difference in the resolution expected for TESs vs. Silicon… they hadn't quite converged as much as they have now* [72].

By 1996, the grad student mentioned above (heretofore referred to as NL#1) had graduated and joined the National Institute of Standards and Technology (NIST), to apply the TES technology, that he had invented, to semiconductor fabrication. However, thanks to the presentation at Goddard, one of his early projects involved a collaboration with CSA#2, to develop arrays of "pop-up"[3] TES microbolometers [D46]. The work was funded, for three years, culminating in a failed proposal for the ESA-led Plank mission [I90]. Although they didn't win the bid, that three years of work laid the foundation for much of what would follow. Firstly, the proposal allowed NL#1's to hire his first post doc (who would later become CSE#8). CSE#8 spent 3 years, from 1997-2000 at NIST developing SQUID multiplexers and TES bolometers. When his post doc ended, CSA#2 suggested that CSE#8 join Goddard and found him a job in the DDL. The move guaranteed that a strong relationship would be maintained with NIST[4], as well as CSE#8's tacit knowledge of their processes and technologies. Second, it justified Goddard's investment in the deposition equipment (in 1997, ~$100K) that would enable future in-house fabrication. Third, it likely lowered the barriers to entry for the X-ray group, since the DDL had already begun ramping up the infrastructure to support the IR group (and their TES bolometer efforts). Finally, it matured the SQUID multiplexing technology to a point where the readout didn't limit device performance. In the words of CSA#10: "*That was a breakthrough that was a necessary development for TESs to get where they've gone. If SQUID arrays had not already been invented, TESs would have needed them to be invented.*" [I89]

Early developments (96-99)

Although Goddard began exploring TES bolometers and microcalorimeters in 1996/7 it took several years before the X-ray group became competitive in this new realm. Their leadership in silicon thermistor microcalorimeters didn't translate directly to the new TES devices. The team didn't emerge as "*real players*" until 1999/2000. *Up until that point, we were just considered people who were dabbling in TESs. Suddenly we were on the map."* [I89] The 1999 NRA reflectively attributes the early challenges to a combination of competing priorities, a steep learning curve and an enabling breakthrough:

> *In the last three years, while completing the XRS instrument and flying XQC, the Goddard calorimeter group has been acquiring expertise in TES technology specifically and superconducting electronics in general. This has been achieved both through acquiring personnel and through the lessons of practical experience. Hard lessons learned with the Al/Ag system led to the development of Mo/Au TES bilayers.* [D9]

One of the "acquired personnel" was CS#3 who, at the time, was working on tunnel junctions as part of a post doc in Europe. He knew CSA#9 (the second scientist to join the microcalorimeter group in the mid 90s) from related work during CSA#9's post doc. When it became clear that the team needed someone with prior cryogenic superconductor expertise and a propensity for fabrication work, CSA#9 suggested CS#3; called him up and invited him to apply to Goddard. CS#3 accepted the invitation, and following a open competition, begun work on bilayers in 1996 [I94].

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[3] The term "pop-up detector" is used to describe a strategy for tightly packing arrays, which involves folding bolometer the legs (weak link) so that they are hidden behind the pixel area.

[4] Incidentally, CSE#8's educational background was a PhD in thin-film physics from Brown.

A New Materials System: Switching to Molybdenum in the Bilayer

The Early NIST TES devices used an Aluminum-Silver (Al/Ag) system, and produced pretty good results. However, despite complete knowledge sharing (CSA#10 even spent a "sabbatical" at NIST as a visiting scientist) and significant effort, neither Al/Ag nor Al/Au TESs were ever made to work at Goddard. NIST's fabrication "*was complicated and required some specialized shadow masks to do the films in-situ without any etching involved.*" [I91] Goddard wanted to leverage the more sophisticated photolithography techniques employed in the DDL, but the devices kept corroding [I94]. At the time, CSA#10 and CS#3 believed that the problems with the shadow masks may have been due to differences in humidity in Colorado vs. Greenbelt. They later realized that aluminum bases systems were incompatible with the chemistry of photolithography, and began looking for a new material system. Even if the shadow masks approach had been made to work at Goddard, the strategy isn't suitable for close packed arrays with precisely defined features, so they would have had to switch to photolithography eventually anyway [I89. I91].

By 1998, the Goddard team had abandoned the NIST material system in favor of Mo/Au TES bilayers. "*Once we made that jump, then it was almost like it was a matter of time before we'd start getting good results.*" [I89] This was right around the time that other TES groups started doing the same. There is some disagreement among the Goddard team about where the idea came from. CSA#10 remembers sitting down with CS#3, who had

> *pulled together a bunch of alloy tables and recommended that we use a Molybdenum-based system… I don't think that we were ever copying. I think that this is actually a case where if you sit down and look at the chemistry and you know you need a superconductor with a T_c in a certain range, and a good robustness to diffusion and intermetalics… I think you end up with a molly-based system. There are some people who think that it may have filtered into the discussion through some side channels that I wasn't aware of.* [I89]

According to CS#3, the choice of Molybdenum was strongly influenced by his past experience working with "*Tantalum from when I was doing tunnel junctions.*" That led him to consider "*refractory metal (it's much more robust)*" and of those "*the closest T_c I found was actually molybdenum…*" [I94]. He began testing the molybdenum with the equipment within the DDL and by '98 had produced Goddard's first Mo/Au bilayer.

Based on similar rationale, NIST chose Mo/Cu (copper) and Goddard chose Mo/Au (gold), which they both still use today. There are pros and cons of each. Mo/Cu oxidizes when exposed to air, and the Mo/Au can create an intermetalic that is self-superconducting at certain temperatures. The former can be mitigated through surface treatment. The latter hasn't caused problems in the relevant operating range. Goddard's detectors are currently considered superior in the context of X-ray Astrophysics, but the distinguishing characteristics are not directly attributable to the choice of Mo/Cu.

Learning to Use Superconducting Electronics

It took another couple of years between Goddard's switch to Mo/Au and their first good test result. While working with any new material requires some amount of getting-up-to-speed time, the real hurdle involved the transition to superconducting electronics. The first TES paper had stated that the devices would need to be read out SQUIDs, and so that's what they did. However, unbeknown to them until around 1999, the commercial devices they had been using weren't capable of measuring a good resolution even if they had had a good detector. It turns out that the conventional "flux-lock-loop" available at the time just wasn't, and never would be, able to handle the fast slew rate characteristic of the fast TES transitions [I89].

In 1999 Goddard began purchasing custom TESs directly from NIST. Once they had the NIST SQUID series arrays, they converted the old dilution refrigerator (the one that CSA#2 bought in 1979, that had done all of the silicon thermister bolometer and calorimeter work) to work with SQUIDs. "*When we got that up and running we had a good test platform for our TES calorimeters… In 1999 we made even better devices and had the set-up to actually test them… I remember it was in the spring 2000 because we were doing the testing right after Astro-E ended up in the Ocean…it was kind of consolation*" [I89]

These "December '99 Devices," tested in 2000 and published as [D36], put the Goddard calorimeter group on the TES map. The results also legitimized the funding stream within Goddard. Where the '96 NRA reviewers indicated that "*the scope of the program as described in the proposal does not warrant such a large increase…*" in reference to the TES branching out, and the '99 NRA was only funded for two years because it was deemed overly optimistic and unfocused, by 2001 (after the strong results) the TES program was seen as worthwhile in its own right, and has been separately funded at ~$1-3M on a yearly basis since [D2-24].

A Next Big Mission on the Horizon (97-2000)

In discussing the TES microcalorimeter innovation pathway, it is impossible to divorce the technology from the future program concepts that were being floated around Goddard at the time of its inception. The (still) future X-ray Observatory, currently known as the International X-ray Observatory (IXO) evolved as a merger of the U.S. Constellation-X (Con-X) and the European X-Ray Evolving Universe Spectroscopy (XEUS) mission in 2008 [D45]. Con-X's initial call for instrument concepts was formally released in 1998 in preparation for the 2000s decadal survey, but the initial formulation took place informally a couple of years before that; around the time that the TES concept was initially being explored.

As CSA#10 remembers it "*[a colleague] who at the time was trying to get a mission concept together, said we need something with a spectral resolution about 2eV and collection area of […] can your calorimeter do that?*" [I89] Her initial response was a non-committal "*maybe? Theoretically it could…*" [I89] Recall that the theoretical limit worked out in 1982 by CSA#2 et al. was 1eV; however at the time, the best single-pixel resolution that had been achieved with any calorimeter was ~7eV. CSA#10 recalls that the discussion evolved through a series of informal gatherings, discussions in the conference room about what was possible and what it would take to get there. They concluded that 2eV was attainable, but would require a major investment in the new technology. "*Although we were already looking at the TESs as something that we should be developing, we hadn't gotten very far with them in those early days.*" [I89]

When it came time to submit a proposal for the Con-X spectrometer in 1998, Goddard submitted "A Comprehensive Approach to Developing a 2 eV Calorimeter Spectrometer for Constellation-X." [D6] As described in the proposal:

> *The scale of this array, coupled with [the technical requirements] represents a substantial extension of the current state of the art in x-ray detectors. To accomplish this goal with a maximum likelihood of success, we have assembled a team composed of three leading groups in x-ray microcalorimeters. […] We propose to follow parallel paths of investigation and to develop several technologies critical for the support of all or most of these paths.*

Two points in the above quote merit further discussion. First, the qualitative density of the microcalorimeter network is noteworthy. The three groups on the proposal are Goddard/University of Wisconsin-Madison (UL#3), the National Institute of Standards and Technology (NIST) (where the Stanford grad student moved upon graduation NL#1), and SAO/LBNL/Brown University (CSA#9's and CSE#8's alma mater). Second, the difference in assumed credibility between the group that proposed microcalorimeters in 1984 for AXAF, and the one that proposed TES for Con-X in 1998, is remarkable. Where the 1984 proposal emphasizes the mathematical rigor of the calculations and suggests that maintaining a backup plan is a wise choice, the 1998 proposal focuses on the proven track record of key individuals. While parallel paths are laid out, they are presented as an appropriate research risk diversification strategy, not a back up (in case we fail to improve the state-of-the-art by a factor of 3).

The ambitious program of technology development was approved in support of Constellation-X.

> *At the time of the selection of technologies to develop for Constellation-X, however, resistor-based calorimeters were the clear front-runners, with ion-implanted Si, neutron transmutation doped (NTD) Ge, and superconducting transition-edge calorimeters each having attained resolution of about 7 eV at 6 keV. The nearest contender, the electron tunneling normal-insulating-superconductor (NIS) calorimeters developed at NIST, had achieved at best 22 eV at 6 keV. The long term potential of the other calorimeter technologies ought not to be overlooked, however. The paramagnetic calorimeters, in particular, may deserve a later second look.* [D35]

The technologies were selected for funding, by an independent peer review at headquarters. In the years leading up to the 2000s decadal survey, multiple technologies were carried, with the goal of making as much progress as possible before a down-selection was required. Although the funding was supposed to be for three years, "*we [the project team] funded them for a period of time, probably never at the level that we said we would because we never got that level of funding [from HQ] because it's always gotten cut…*" Within a year *"some of the areas kind of dropped out, and some of the areas were kind of getting funding from elsewhere, or weren't as critical."* [I57]

Nonetheless, the three groups – Goddard/UW, Lawrence and NIST – formed an integrated product team and created their own project stability. They outlined a clear roadmap for achieving the required 2eV energy resolution [D35].

Constellation-X: The Impact of Not Being First (2001)

However, Con-X was not ranked first, as they had hoped. 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 money to Goddard for Con-X "pre-Phase A" technology development and mission studies. During the decade of the 2000s, Con-X was funded between $4M and $12M per year, most often in the $6M-$10M range. Of that 70-80% has been dedicated to technology development, but most of that went to the mirror group. While this may seem like a substantial investment, it hasn't felt that way for the people involved. Con-X is an extremely ambitious mission, with breakthrough performance improvements required across multiple technology areas. In addition to the technology development resources having been spread thinly, significant budgetary uncertainly has severely limited project management's ability to plan. As described by a manager on the project:

> *We've had fits and starts a) because of the money being erratic (for several years we've had midyear budget cuts that are really drastic… and then b) you have to reduce your staff… and it's hard to build it back up again. An example is that we had this really good mechanical team working on the mirror* [mounting… But he] *got called off because we're low priority…* […, but] *technology requires the right kind of person, and it just takes time to get that person up to speed. It's not like we're just designing [*something that we've done before*]… so we got another mechanical engineer, and he wasn't a good fit… then we got another one and they worked together, but still not a good fit.. and then we finally got someone else… and we were progressing, but really slowly… eventually we realized that we just had to get the right person, but it was really like 2 or 3 years that we made no appreciable progress on the mechanical mounting aspect.* [I57]

The established stability of the calorimeter group served to mitigate the effects of budgetary uncertainty on the TES development. While the effort was initially funded through the Con-X project, it also received significant and steady resources from APRA (Astronomy and Physics Research and Analysis – the ROSES program element dedicated to Astrophysics technology development), in three year tranches. During the decade of the 2000s, the calorimeter group never got more than 50% of their TES development funding from the Con-X project "*mostly it was more like 10%"* but their status as "*recognized experts*" developing a "*key enabling technology for the next grand x-ray observatory*" certainly factored into the attractiveness of their sequence of APRA proposals. Although the Con-X project team had no formal influence over the peer-reviewed APRA funding allocation, there was some level of informal coordination:

> *The people at headquarters [who make the funding decisions] full well knew that this was the prime candidate technology. We would kind of exchange with HQ at the beginning of the fiscal year… they would tell me how much funding was going to applicable technologies so that we could take that into consideration in our funding levels, and obviously we told them what we were funding… everybody understood all around that the funding for the calorimeters would come from multiple sources.* [I57]

This limited soft-power influence over Con-X-specific technology development funding allocation created an unusual management problem for the Project Manager. Despite being responsible for maturing the technologies that would be required to make her mission feasible, PM#2 only had direct influence over a small amount of the actual budget devoted to that end. The true responsibility for R&D management fell to the scientists for all intents and purposes. In the case of the calorimeter, she believes that scientists managed their efforts like managers (unlike some of the other technologies). What this meant for her:

> *With the calorimeter, in this particular instance, they seem to be very focused toward developing the technology toward IXO, well in tuned with what the needs are… I mean frankly, some of the other areas of technology, even when we were funding them, they wouldn't write their APRAs applicable to Con-X. Our TES guys proposed totally in line with what Con-X needed, and then they pooled their resources, and were strategic… A lot of it stems back to how the people propose.*
>
> *That's the route, in NASA, I think HQ knows that, that's part of what APRA is there for… to develop the technology for these upcoming missions that may or may not have the money to support that technology!* [I57]

On the part of the microcalorimeter team, they didn't just seem strategic; in fact, they set out a long range, broad spectrum, plan in 1998 and have, for the most part, followed it since. In addition to the Con-X support, the team received ~$1M per year from APRA as well as variable increments from IRAD ranging from ~50K to ~$400K, with support for 1-5.5 FTEs [D17-24]. In terms of technology strategy, one of the lessons learned from the silicon thermometer microcalorimeter development was the importance of arraying issues. Although solving practical problems wasn't scientifically "sexy," the credibility that the group had previously established allowed them to take the long term view expressed in the following. The new lead scientist (CSE#10) decided that:

> *demonstrating arrays, and reading out arrays, would really show that the technology is ready. With what's happening on IXO right now, I feel a bit vindicated. There was a period of time when we were slogging… we weren't making splashy announcements about breakthrough resolutions because we were solving arraying issues... but it was worth it because we are getting very good resolution (among the best in the world) in arrays… and so having a mind to 'this needs to be demonstrated for a mission'… having that mindset in an early phase even when that missions is really far off, I think that mindset is really important to making technologies ready for missions and I think this has benefited us and that's why the US technology, the NIST multiplexers and the Goddard arrays is the baseline technology for the IXO calorimeter right now. We pushed them to a higher demonstration level.* [I69]

TES Exploration: Inventing to Meet a Specification (the 2000s)

An important byproduct of the TES development being formulated in the context of a next big mission, which was itself, a "next mission" in a string of Grand X-ray Observatories, was the specificity of the early requirements. As recalls CSE#8 (the technologist who had post-doced at NIST):

> *As early as 1998 or 9, they had a pixel design that they were working towards… because the astronomers need [a particular functionality]… here's the [requirement], so we worked for a decade basically with that target in mind… we slowly brought [resolution] down to the Con-X and then IXO spec… and the science guys were bringing the readout [capability] up so that it could interface with this pixel…* [I90]

This specificity was a mixed blessing. On the one hand, it constrained their ability to experiment; on the other hand, it provided an unusual level of stability both in terms of funding and personnel.

> *It was sort of frustrating at times because you wanted to try something really different, but you had to make the same stupid pixel over and over again that you knew wasn't going to work… but the mission forced us into that mold and now we have a very successful design… [because of that] we were able to get funding and maintain the size of team necessary to solve the problems.* [I90]

From a technical point of view, the first half of 2000s decade was a time of getting settled. The relevant equipment had been procured in the late 90s (where the silicon thermisters require a high energy implant, TES needs a very different piece of equipment to deposit thin metal films) but it took until 2004/5 before the process really stabilized. CSA#2 calls this era "*technique limited"* meaning that while the scientists may have a pretty good idea of what needed to be done in theory, practical implementation issues on the fabrication-side constrained what could be done. Of course, the act of solving practical issues (and the corresponding fundamental understanding that was entailed) had an important impact on what to try next.

Basic Production Issues: Galvanic Corrosion and Leads That Won't Stick (Stabilized ~ 2004)

During the early stages of detector development, scientists and technologists focus on different aspects of the concept of "demonstrated." As explained by CSE#8, a fabrication guy, "*they just want devices that work and if you make one good one, that's great, they'll just test that one for a long time."* [I90] However, from his technologist perspective, getting one to work means little; developing a repeatable process capable of producing the 1000s of identical detectors has to be the goal:

> *We have these huge wafers and the device is only 100 µm on a side – from a 4 inch wafer you can get lots and lots of devices… and you only need one to work, and they'll test it and publish the results and everything is great, but when you need 3000, production issues are difficult problems, and they don't care if it's solved during the development phase…* [I90]

The above quote probably overstates the difference in perspective. It's not that scientists aren't concerned with repeatability; they absolutely are. The difference is in what they perceive as a fundamental problem. Not being intimately involved in the fabrication, it is impossible to differentiate between what is reported as a "*process mistake rather than a more fundamental problem.*" [e94] The implication of this distinction is partially illustrate below, and revisited with respect to electroplating in the next section.

In this case, the early production challenges largely involved materials processing questions. The TES itself is a bilayer (thin layers of Molybdenum and Gold). When interfacing two thin metal films, a voltage is produced between them and that often causes one of the metals to corrode. Since putting unlike metals together is an intrinsic characteristic of TESs, it couldn't be avoided, so overcoming this challenge was a matter of "*learning the chemistry*" and not exposing the device to non-neutral PH solutions while yielding that particular step. In fact, some of the corrosion "accidents" inspired the vacuum gap absorber solution described below.

The other basic production challenge involved attaching leads to gold (similar to CZT case). Gold is "*one of those blessing and a curse type material.*" [I90] It has great electrical properties and doesn't oxidize, making it stable over long periods of time, but not much sticks to it, and most of the materials that do "*are kind of gooey*" and hard to work with [I90]. They eventually found that if the lead was attached to both the Mo layer and the Au layer, the interface was robust. Nonetheless, the mitigating the "curse" side of the gold continues to an area of R&D.

A related challenge to the method of lead attachments was the related question of where to attach them. As array size increased, wiring leads to each individual pixel became impractical: "*there just wasn't enough real estate*" [I94]. So the team began investigating efficient attachment schemes. One promising approach was superconducting through wafer micro-vias. The effort was funded by DDF for two and a half years before Con-X funding cuts caused all non-essential development paths to be put on hold. Seven years later, this strategy is being revisited in the context of microbolometers [I94].

Noise Reduction #1: Strange Geometries and Metal Fingers (Breakthrough ~ 2003)

While the technologists were focused on getting the process under control, the science team was trying to understand the performance of the successful devices that had been produced. In the same way that unexplained noise terms had plagued the original semiconductor microcalorimeters, so too was it an issue for the TES development. Echoing the 1/f catch-22, the team wanted to operate lower in the superconducting transition to improve sensitivity, but found that a mysterious noise term (which got worse lower in the transition) negated any gains. There was considerable disagreement in the TES community – which largely included Goddard, NIST and a Dutch group working on calorimeters for Xeus – about the underlying mechanism [I89].

This community discussion was structured by a series of focused conferences. The group would meet on a bi-yearly basis at the "Low Temperature Detectors (LTD)" conference. In 2001, it was decided that a smaller informal "TES" meeting would also be held in the off years. NIST hosted the first one, and this unexplained noise was a major topic of conversation. The American groups believed that it was a voltage noise, while the European's claimed that their (same) noise was due to thermal fluctuations. It's possible that the conflicting explanations stemmed from differences in their approaches. Where the American's both used Molybdenum-based materials systems, the Dutch used Titanium/Gold, which they operated at a higher resistance. As CSA#10 explains "*people tend to get settled into what first works.*" She believes that molly-based systems are more robust, but acknowledges that there are some operational advantages to higher resistance systems. Regardless, the same fix, improved the underlying noise problem in both devices [I89].

By the time CSE#8 joined Goddard in 2000, everyone knew that square TESs were noisy, but no one knew what would work. "E*veryone was trying different configurations… I'm going to make mine circular; I'm going to make mine a diamond;* brute empiricism [I90]. Just as CSE#8 was leaving, NIST wrote a theory paper, arguing that putting metal on top of the device would suppress the order parameter. The paper suggested a pattern of metal stripes just along the edge. In fact, they also have an all-encompassing patent [D37] "*and probably completely un-enforceable*" [I90] for putting metal stripes on TESs. "*It's interesting how ideas can spread like wildfire;*" [I89] when word got out that NIST was actually getting noise reduction with these metal stripes, everyone started trying it. At Goddard, the IR group started doing it first (CSE#8 was initially working on bolometers), then the X-ray group followed. By LTD 2003, "*we all showed up with stripes on our devices. Even the Dutch, but they said oh, that helps us cool the interior… we spent that conference arguing about why the stripes were working.*" [I89] But the underlying mechanisms didn't really matter, empirically it worked, and some configurations seemed to work better than others.

As CSE#8 remembers it, all CSA#2 saw at the meeting was "*garbage, garbage and more garbage, except for [the Dutch] talk on perpendicular stripes.*" [I90] Their insight was that the lowest noise configuration was when the stripes were placed perpendicular to the current flow; but that insight was incomplete. As presented, full length perpendicular stripes could cause a normal metal series resistance and either make the detector unstable or significantly reduce its sensitivity. So, CSE#7 (who was working on TES bolometers in parallel with the silicon thermisters) drew "*this snaking thing, where the perpendicular stripe didn't go all the way across.*" [I90] It worked well, and that's the design that the community has settled on. In CSE#8's view:

> *It was [a] case of an active community that were all really focused on the problem [working together] ….groups outside of Goddard proposed solutions and [we built on them] then in our group internal to Goddard, it was science and engineering working together to come up with a solution…when you're fighting this invisible monster kind of thing, you need a lot of ideas on the table…* [I90]

And so the invisible noise monster was slain by multiple empirical swords, in some wavebands anyway.

Noise Reduction #2: Electroplating and Vacuum Gaps Absorbers (Breakthrough ~2005)

The "fingers," or "stripes," took care of the noise term in the IR bands (satisfying the bolometer team), but while it certainly helped with the X-ray microcalorimeters, they were still plagued by inconsistently noisy devices. They were getting wide variations in resolution, and even the best devices weren't good enough (~4 eV compared to the required 2). They believed that the problem was in the material science of the Bismuth absorber. The single biggest breakthrough in the TES development came with the transition to electroplating (as a deposition technique) and separating the semi-metal absorber from the superconducting TES by a "vacuum gap." This insight solved several interrelated challenges [I89].

First, recall that one of the key advantages of the TES is that it's extremely sensitive thermometer allows the use of normal metal absorbers (and their correspondingly high heat capacity, but good thermalization). However, putting a normal metal in direct contact with a superconductor kills its superconductivity. They hoped that by using Bismuth (a semi-metal with very low electron density) they could avoid this problem by putting down an absorber layer that wouldn't affect the TES; but, "*we just found that if we didn't get interaction it was accidental… [for example] if there was some contamination, it provided a nice little barrier layer, but that was accidental and we couldn't count on it [happening].*" [I89]

The solution emerged organically from a series of group discussions, making it is difficult to reconstruct exactly who thought of what, but the discussions were internal to Goddard. Because of the contamination accidents, they started brainstorming ways to limit the contact. At the time they were already using a "mushroom absorber" (big top on a thin stem) to hide the electronics, so they started thinking about how far they could extend the cantilever. This line of reasoning lead to the notion that additional supports could be built outside the TES to support an extreme cantilever. Around the same time, they were also considering spacer materials (barriers like the accidental contamination). In the end, the two concepts merged when they realized that empty space was the best barrier of all; hence the vacuum gap [I89].

Second, Bismuth is a very rough material; rough to the point that the Bismuth layer affected the deposition of the normal metal layer. To fix this problem, CSE#8 suggested that they try depositing the Bismuth using electrolysis instead of evaporation, as they'd been doing. He believed that electrolysis was a better approach in general because (1) evaporating wastes a lot of material (so it would be cheaper) and (2) the material quality should in theory be better too [I89]. However, developing a Bismuth electroplating technique proved quite challenging. Although electroplating is a fairly standard and well established semiconductor fabrication technique, no one does it with Bismuth - "*bismuth is weird*." CSE#8 hired a post doc (who stayed on as CSE#9), with experience in electroplating to "*develop the recipe.*" [I90]

During the post doc, CSE#9 worked with some colleagues in a basic research – thin-film physics – lab at John's Hopkins. He also explored some different configurations of the absorber, as another angle of attack for reducing noise. Many of the ideas were farfetched. For example, he found a way to "*get bismuth to just below the melting point to see if he could re-crystallize it into a big chunk… that and things like that were doomed… so he had a lot of garbage ideas going on and the electroplating thing… and that one turned out really well.*" [I90] It didn't take long from idea to proof that electroplating is actually a much better, much more reliable way of making high quality absorbers. However in hindsight that was only partially true.

Initially electroplating did work extremely well. As CSA#10 remembers it: "*We had a series of great runs with the electroplated absorbers before we started seeing the limit of that technique's reproducibility. For a while there, it seemed like we couldn't make a bad device.*" And, when problems started to show up, the technologists initially attributed them to process error [e94], so the science team discounted their significance appropriately. In the end, it turned out that the challenges weren't human error *per se*, but neither did they affect (and negate) the importance of the electroplating breakthrough. In the words of CSE#8:

> *I think TES has been blessed several times with [things working] the first time and you didn't know why, but you were able to capitalize on the big success and then it would be several years later and several failed devices later before you figured out why that first one worked so well. That was true of electroplating… [CSE#9] made some really nice films during his post doc, but he wasn't exactly monitoring everything he could have…*
>
> *We've lost the recipe several times since then and gone through periods of making bad devices…*
>
> *Electroplating is like the chemical nightmare, there's all these different buffers and things in the solution, there's four or five different components…they're actually helping the bismuth precipitate in uniform ways, [whereas] sometimes it makes clumps and sometimes it makes dust… we eventually learned that you were balancing the chemical environment with the buffers.. .things that a chemist might just know … but you put a bunch of physicists and electrical engineers on it and it takes a while…* [I90]

The above quotes capture two capture important aspects of maturity. Looking back, the technologists emphasize the challenges of scaling up production; which occupied them for several years after the big success. The scientists on the other hand, focus on the proof of feasibility which changed the shape of a development trajectory.

In the late 1990s, NIST had been the only group that had working devices. When Goddard came online in 2000, the leadership was less clear. NIST was focused on lower energies (where the photon can be absorbed by the TES directly) and higher energies (where a huge absorber was necessary). Since Goddard had been focused on Con-X/IXO requirements from the beginning, arraying issues had led them to develop detectors optimized for X-ray astronomy. The vacuum gap breakthrough was motivated by the needs of this particular waveband; its invention served as a signal that 1-10 keV was Goddard territory.

With electroplating and vacuum gap absorbers, Goddard was now making detectors with sub 3 eV energy resolution at 6 keV. Ten years into the development efforts, the Con-X goal was within reach.

<u>*Scaling up SQUID Readouts, an ongoing process*</u>

One area that NIST continued to dominate was in the SQUID arrays, and later multiplexers. This was partially a strategic decision on the part of NASA HQ, as will be discussed more below. Recall that the original TES paper had identified SQUIDs as the way to read them out. No one really considered using anything else, but different multiplexing strategies, including "time division," "frequency division" and "resonators," were explored and eventually combined [I90].

The original SQUID series arrays were developed in CSE#8's group at NIST during the 90s. Series array SQUID amplifiers can handle much higher slew rates than single SQUIDs because the larger scale of their output (from the series combination of 100 SQUIDs) permits reduction of the strength of the inductive coupling of the input. Multiplexing became important as increasing sensor array sizes required larger numbers of readout channels. The group created a multiplexer by putting a single input SQUID into each TES bias circuit, and then switching those

SQUIDs on and off so that only one at a time communicated with a shared series-array SQUID. Around the same time other groups were exploring "frequency division" since they needed to maintain an AC bias, and others still were developing multiplexing via resonators. Each strategy had relative advantages and disadvantages. Current devices have evolved as a time, frequency, resonator hybrid, combining the best of all alternatives. As array architectures have become more ambitious, the readout burden has increased exponentially, and the readouts have evolved incredibly in the last 15 years to keep pace [I90].

Goddard's involvement in this aspect of the evolution has been primarily as a customer. Despite submitting several proposals starting as early as 2000, neither the Goddard TES bolometer nor microcalorimeter teams have ever been funded to develop an in-house SQUID capability. Several rationales for this strategic HQ decision have been offered. The Jet Propulsion Lab (another NASA-center) was already very strong in Niobium tri-layer fabrication, "*so it could be that they [HQ] felt like they'd paid for that [capability] already.*" [I90] Another explanation traces back to the relative priority given to science R&D versus cross-cutting capabilities. Within NASA's selection framework, improving a particular mission performance (e.g., achieving higher energy resolution in an IXO-like detector) is valued higher than improving broadly relevant enabling infrastructure (e.g., breakthroughs in channel reduction are a necessary precursor for the large format arrays planned for next generation IR and X-ray observatories). Fortunately, in the case of SQUIDs, there was a national lab and university community that was more than happy to focus on developing SQUIDs.

It's difficult to assess, even *ex post*, whether the decision not to develop SQUIDs in-house resulted in cost savings. Through the 2000s, HQ via Goddard has paid the NIST group millions of dollars to produce SQUIDs. Of course, had they developed their own capability, "*HQ would be paying us millions of dollars too I suppose… to buy all kinds of dedicated equipment, staff it, maintain it on a yearly basis, all the materials… it would have been a substantial expansion of our group.*" [I90] Ignoring the question of whether Goddard or NIST was in a better position to make progress on the technology for the moment, from a purely "make vs. buy" comparison, it's worth noting that although Goddard chose to "buy" it was from a government lab serving essentially the same tiny market (there are very few customers in need of readouts for thousand element superconducting TES arrays). This co-specialization arrangement with NIST has worked well because of the strong relationship that continues to exist. In turn, the NIST group "*have developed relationships with the Berkley [frequency] and JPL [resonator] groups to make sure that everybody is benefiting from the breakthroughs in technology. There isn't any cut-throat competition issue where you hide what you're doing to make your instrument slightly better…*" [I90]

<u>Theory and Empiricism: Branching Out and Stepping Back (2006-present)</u>

Once an R&D program is folded into a project, understanding the physics of one's devices becomes a luxury; and TES had spent its entire history as part of a project. The project didn't care why the metal comb-like fingers suppressed noise; just that they did. But there is a limit to how much one can proceed without the other. As CSA#10 describes it: "*once we got devices that worked well enough, we definitely focused on doing integration and demonstration… but we never let go of trying to understand some of the more theoretical aspects… we would also theoretically try to engage outside scientists… e.g., talk to folks at the University of Maryland* *(down the street) and get them to work on our problems."* [I89] In the mean time, the brute-force experimentation was guided by intuition. For example, the noise suppressing shapes and structures that they tried, were selected based on potential mechanisms "*… well maybe it's due to flux flow noise… and if that's true, then how do we create nucleation sites for the flux flow to only move in certain areas? … so it's never a "this looks pretty, let's try that…" it's always more of a if this is what's going on, then making something like this could affect it."* CSA#10 still considers this brute empiricism because the trials weren't designed to rule out competing explanations (i.e., "*if it does improve it, it doesn't prove anything since there are multiple affects going on*). If it did work, they just incorporated it and moved on [I89].

It wasn't until years later that they seized an opportunity to re-engage with the theory and finally start to understand some of the fundamentals of the devices they'd been building for almost a decade. Around the 2006/7 timeframe, one of the scientists in the calorimeter group began looking for resources to apply the calorimeter concept to the domain of Solar Physics. The original intention was to advance the emerging magnetic calorimeter technology, and at the same time tap into a new potential patron mission-area. All his proposals were rejected, because the technology was considered too immature. So, he tried again; this time proposing Solar X-ray TESs. This time, they won a 3 year APRA grant 2007-10 which has been renewed in 2010 for another three years [I89].

Solar X-rays require much smaller, much faster TESs compared to Astrophysics. Being forced to play in a very different length scale has "*helped us put together effects that we'd noticed before – that these devices are acting coherently over very long length scales – as you shrink the devices down, you could clearly see that we would get transition temperature of the device to scale with the separation of the leads… even with the bilayer being exactly the same."* [I89] While they had intuitively known for many years that all of the superconducting-normal metal interfaces should have some impact, this insight allowed them to identify so-called weak-link superconductivity [I89]. They now had a theoretical explanation for why the lateral proximity effect is as important in describing the behavior, as the vertical proximity of the layers. From a practical point of view, a good theory saves you money. Being able to do the device optimization on paper saves a lot of expensive experimentation (in both time and materials).

In this case, branching out into a second line of research allowed the team to step back while still moving forward:

> *[It] kept our more fundamental investigations going... so we could keep one line of devices relatively static (while we worked on systems issues) while this other line is where we did all the experimentation to understand length scales and other effects and that ended up working out very well for us…* [I89]

Another Chapter in the IXO Saga

2010 marked the release of another Decadal Survey. IXO was not recommended for development as a formal program since several of the enabling technologies were still deemed too immature. It was however recommended that technology development continue, for reassessment in 2020. There is a provision in the Decadal for reevaluation should the European Cosmic Vision process rank IXO more highly. Realistically though, with continued cost increases on the JWST project (which is now estimated at ~$6B, compared to the $700M originally projected) it is unlikely that there will be any Astrophysics money left over for other projects anyway [I93]. Nonetheless, TES development continues. The team won 2010 APRA grants for both the Solar and IXO efforts. After all, once a 2 eV at 6 keV mission is flown, it will revolutionize our understanding of the universe. With technology that enables this level of breakthrough, it's just a matter of time.