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Chasing Tom Phillips

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Abstract. This paper gives an informal and personal view of the major impact that Tom Phillips has had on the author's scientific career and field of research.

1 Introduction

I have spent much of my career chasing down a scientific and technical path that was pioneered by Tom Phillips. Indeed, I have benefitted in so many ways from Tom and his work, directly and indirectly, that it is almost hopeless to try to write it all down. Nonetheless, I will try to give a brief sketch.

2 Caltech, 1977–81

It is amazing to reflect on events that were happening all around me when I started at Caltech as a freshman in the fall of 1977. These events would later have a defining impact on my life and career, but at the time I was oblivious to most of them. I signed up for a course (Ph 10) that Bob Leighton taught for freshmen interested in special topics beyond those covered in the standard freshman physics course (Ph 1). In retrospect, it seems that Leighton's goal was to cover those topics that he had found most useful over the course of his career: we studied the calculus of variations, complex analysis, analytic and numerical solutions to the Laplace equation, fluid flow, and the mechanics of solids (tensors, stress and strain, etc.). Of course, at the time Leighton had just finished building the first 10-meter telescope at Owens Valley, so he also included some lectures on radio astronomy, interferometry, etc. Had I managed to learn everything, I probably could have skipped the rest of my coursework and jumped straight into research. Unfortunately, the impedance match between the level and pace of Bob's lectures and my ability to absorb them was quite poor; it would take me another decade or so to finally learn some of the things that Bob was trying to teach us.

By the time I completed final exams in December 1977, JPL's Robert Powell and Albert Hibbs had published a paper (Powell & Hibbs 1977) describing several potential applications for large antennas in space, in anticipation of the vast cargo capabilities of the Space Shuttle. Hibbs was definitely an interesting character: he was one of Feynman's very few Ph.D. students and book co-authors (Feynman & Hibbs 1965), and played a central role in initiating JPL's planetary exploration program in the early 1960's (Hibbs 1961). However, Powell and

Hibbs's 1977 attempt to sketch out the future was not entirely successful. Instead of their anticipated acre-sized geostationary antennas, global mobile telephony was ultimately accomplished using multiple satellites in low-earth orbit (e.g., Iridium and Globalstar). Another still unfulfilled concept described by Powell and Hibbs was a 10 m deployable submillimeter space telescope. Leighton's work was clearly a primary source of inspiration, and a photograph of his first OVRO dish was included in the 1977 paper to illustrate that the necessary precision was attainable. Leighton was certainly well known at JPL given his previous involvement in the imaging experiments on the Mariner Mars missions in the mid-1960s. Powell and Hibbs did not generate the idea of a submillimeter space telescope; rather, their paper summarized the progress of a study that was already initiated at JPL in 1977, starting with a workshop and carried forward by a team led by Sam Gulkis, Tom Kuiper, and Paul Swanson (Swanson et al. 1982). These were some of the first steps that ultimately led to the inclusion of the Large Deployable Reflector (LDR) in the 1982 Field committee decadal survey report, a pivotal event for the young field of submillimeter astronomy. Indeed, it was the Field report recommendation that prompted NASA to start investing in the submillimeter technology development, which ultimately led to NASA participation in Herschel and Planck. Of course, Tom Phillips played a critical role in all of this, especially the Field report recommendation, and the LDR instrumentation concept that he and Dan Watson produced (Phillips & Watson 1984) along with the associated technology development plan.

In the summer of 1978, after my freshman year, I managed to find a job working in the Space Radiation Laboratory (SRL) group at Caltech led by Ed Stone and Robbie Vogt. I stayed in the group for the rest of my undergraduate career and ended up finding a nice project assisting graduate student Neil Gehrels (now at GSFC) with the calibration of an electron spectrometer flown on the Voyager missions. The instrument, called TET, was designed a few years before by Stan Whitcomb (LIGO Chief Scientist, Caltech) while he was an SRL undergraduate at Caltech. Stan had gone on to Chicago and was a graduate student in Roger Hildebrand's group along with Jocelyn Keene. During those years, I had occasional interactions with Robbie Vogt, both formal and informal. Robbie was serving as Division Chair for Physics, Mathematics, and Astronomy, and apparently with some help from Bob Leighton and Gill Knapp, was responsible for bringing Tom Phillips to Caltech from Bell Labs. One afternoon, as I was sitting at my desk working, Robbie stopped by and started chatting to me about Tom coming to Caltech and mentioned that Tom was going to build a 10 m submillimeter telescope on Mauna Kea — the "high dish." Again, my impedance match was poor and this useful information was largely reflected. However, in the summer of 1981 after I had graduated and was preparing to leave for Berkeley, Tom's name came up again, but this time in a more persistent way: Geoff and Karen Blake had arrived from Duke at the beginning of the summer so that Geoff could start his graduate student career in Tom's group. Geoff and I had actually met four years earlier when we were both freshmen at Caltech, but Geoff was smarter and left for Duke after one year. Spending the summer of 1981 together in Pasadena gave us a chance to renew our friendship, and gave me a little exposure to Geoff and Tom's area of research.

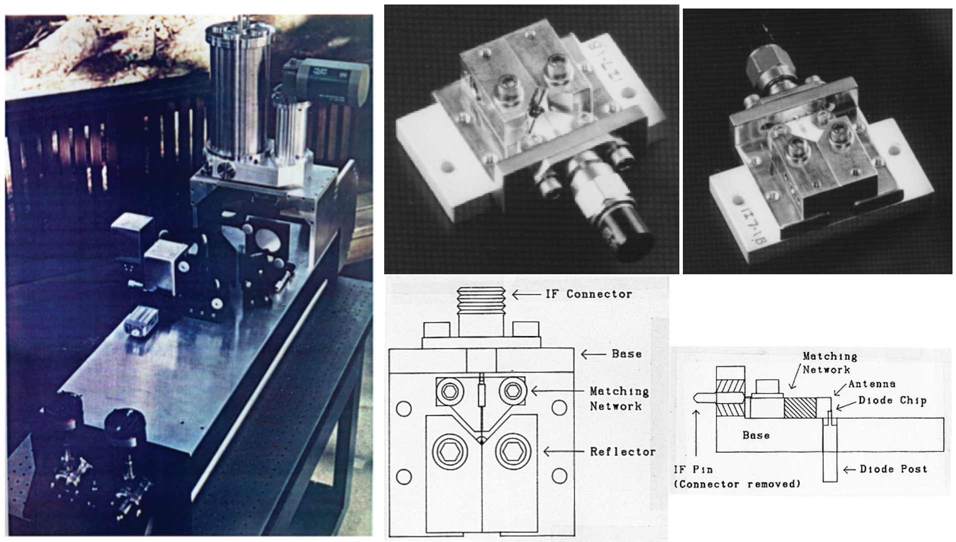

Figure 1. Left: The Berkeley airborne far-infrared laser heterodyne spectrometer in the early stages of assembly (Betz & Zmuidzinas 1984; Zmuidzinas 1987). Right: Corner-reflector mounts used for the whisker-contacted Schottky diode mixers.

3 Berkeley, 1981–87

Despite these early interactions at Caltech, when I arrived at Berkeley in the fall of 1981 I still had a vague notion of pursuing theoretical physics, having been inspired in this direction after taking Feynman's general relativity course in my senior year at Caltech. Fortunately, that didn't last long: the head TA for the course that I was also assigned to help teach was a very sharp sixth-year theory student still in search of an adviser. Another experienced student, Andrew Lange, gave a talk to the first-year students in which he suggested that we forget theory and take the machine shop class instead. That was very good advice. The Townes/Genzel group seemed to have a wide range of interesting projects, and they steered me to Al Betz, who at the time was working with Genzel on starting a new effort in submillimeter astronomy, following the pioneering work of Fetterman et al. (1981). It seemed like a good opportunity to learn about a wide range of experimental techniques — lasers, spectroscopy, optics, microwaves, electronics, cryogenics, etc., and Al was an accomplished experimentalist, so I joined. Two versions of the instrument (see Fig. 1) were to be built: a ground-based version, which ended up being Andy Harris's thesis project (Harris 1986), and a more compact version (Betz & Zmuidzinas 1984) for the NASA Kuiper Airborne Observatory (KAO; see Fig. 1). This turned out to be a very timely project, especially given the Field report recommendations.

I had a minor role in the construction of the ground based instrument: I was responsible for building the 6.4 GHz cooled GaAs FET IF amplifier. I carefully studied the reports and papers produced by Tap Lum and Dave Williams at Berkeley and Sandy Weinreb at NRAO, learned something about impedance and

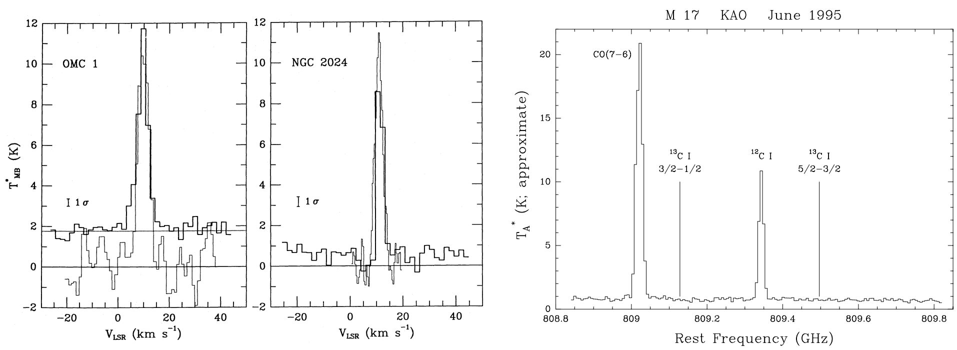

Figure 2. *Left*: The thicker lines show the 1986 observations of 809 GHz $^3P_2 \rightarrow {}^3P_1$ atomic carbon emission using the Berkeley airborne laser/Schottky heterodyne spectrometer (Zmuidzinas 1987); thin lines represent 492 GHz spectra obtained on the KAO with the Caltech InSb receiver (Phillips et al. 1980; Phillips & Huggins 1981). *Right*: The first 809 GHz spectrum obtained with an SIS receiver, taken in 1995 using the Caltech instrument on the KAO (Zmuidzinas et al. 1995a), shortly before the KAO was decommissioned.

noise matching, and managed to produce a mediocre amplifier, just good enough to allow detection of the 809 GHz $^3P_2 \rightarrow {}^3P_1$ line of neutral carbon using the UH 88-inch telescope (Jaffe et al. 1985). The ground-based work quickly moved to observing the brighter CO $J = 6 \rightarrow 5$ and $J = 7 \rightarrow 6$ lines, leaving further study of the 809 GHz carbon line for the airborne instrument (see Fig. 2). The goal was to compare the intensity of the 809 GHz line with that of the 492 GHz $^3P_1 \rightarrow {}^3P_0$ line that Tom Phillips and his co-workers had detected (Phillips et al. 1980; Phillips & Huggins 1981) using Tom's InSb receiver on the KAO. In his 1981 paper (Phillips & Huggins 1981), Tom suggested that the 492 GHz line might be optically thick, and therefore the atomic carbon abundance could be even higher than the already surprisingly large values obtained under the assumption that the lines were optically thin. By comparing line intensities, we hoped to be able to judge whether the lines were optically thick or not. However, the comparison would need to be done quite carefully in order to obtain a meaningful result, and airborne observations would have an edge in sensitivity and calibration. That was how I really became familiar with Tom's work: I read his papers numerous times, and when I needed additional help I would call Jocelyn Keene, who by then was a postdoc in Tom's group at Caltech.

As would eventually be done a decade later at the CSO (Keene et al. 1998), a better method for gauging the optical depth of the atomic carbon lines would be to observe the ^{13}C isotope in the $^3P_2 \rightarrow {}^3P_1$ transition, taking advantage of the hyperfine splitting (see Fig. 2). However, the receiver sensitivity would need to be improved by at least a factor of 10. One promising approach was to push the frequency range of superconducting tunnel junction (SIS) mixers that Tom had co-invented to 810 GHz, and indeed this was a goal that Tom had set for his group, as Mike Wengler's influential 1985 paper (Wengler et al. 1985) on the first quasioptical SIS mixer made clear. Mike's paper described how to couple submillimeter radiation into an SIS junction over a wide bandwidth using

Figure 3. Champaign-Urbana in December 1987, the view from our front door shortly after our arrival.

a planar bow-tie antenna on a hyperhemispherical lens, a technique pioneered by David Rutledge and his group at Caltech (Rutledge & Muha 1982; Compton et al. 1987). I first heard about this work at NASA/Ames, when Tom and his group were wrapping up a flight series on the KAO and we were preparing for one. Geoff Blake showed me the beautiful, high-sensitivity line survey data that he had obtained using the 230 GHz SIS receiver at OVRO (Blake et al. 1987), and explained to me how Wengler's work would extend this capability to higher frequencies. I was hooked.

While it seemed clear that SIS was the future of submillimeter astronomy, I needed to finish my thesis. The obvious next step for the laser heterodyne instrument was to push to higher frequencies, to the bright C^+ line at 1900 GHz. We had competition — Dan Watson and Erich Grossman were working in Tom's group developing a higher-frequency analog to the Caltech InSb receiver, but one that used a Ge:Ga photoconductive mixer and a tunable local oscillator based on a fixed-frequency laser coupled to a Schottky sideband generator (Grossman 1987). In fact, during my oral Ph.D. candidacy exam, Paul Richards asked me to calculate the expected sensitivity of that system, and it was depressingly good! But our system was simpler, and by reversing the usual direction of the diode whisker so that it bent away from the corner reflector (Zmuidzinas, Betz, & Boreiko 1989; see Fig. 1), we were able to insert an IF impedance-matching circuit that resulted in decent performance at the relatively high IF frequency of 9.3 GHz dictated by the frequency offset of the C^+ transition from the difluoromethane laser line. With this setup, we were able to obtain the first resolved spectra of C^+ (Boreiko, Betz, & Zmuidzinas 1988; Zmuidzinas 1987; Betz, Boreiko, & Zmuidzinas 1988; Boreiko, Betz, & Zmuidzinas 1990), which revealed interesting features such as reversed line profiles and absorption by foreground gas, a topic which will undoubtedly be revisited with Herschel/HIFI.

4 Illinois, 1988–89

The next step was obvious — I needed to learn something about SIS mixers. I met Fred Lo at the 1986 AAS meeting in Pasadena and learned that he was interested in developing an SIS effort at the University of Illinois, in part because the newly formed Berkeley-Illinois-Maryland Array (BIMA) collaboration would need SIS receivers. Illinois had a top-notch reputation in solid-state physics, John Bardeen had brought John Tucker to Illinois, and Dale Van Harlingen's group in the physics department was actually producing superconducting tunnel junctions and devices, so I accepted Fred's offer of a postdoctoral position, and with relatively few distractions (see Fig. 3) apart from the occasional tractor pull or antique farm equipment show with Lew Snyder, got to work.

Early on, Fred organized an SIS workshop that was very helpful. John Tucker gave a nice lecture about his SIS theory (Tucker 1979; Tucker & Feldman 1985) and Tony Kerr and Tony Stark relayed their experiences building mixers and receivers at 100 GHz. By then, Tom's next student on the high-frequency SIS project, Thomas Büttgenbach, had published an impressive paper (Büttgenbach et al. 1988) in which he described the use of a spiral antenna in place of the bow-tie; the new antenna was needed because Rick Compton's thesis work (Compton et al. 1987) with Rutledge had shown that the bow-tie had an undesirable antenna pattern. Although the spiral antenna design seemed attractive, there was another problem: impedance matching to the SIS junction would be very poor in a submillimeter mixer due to the junction capacitance. This problem had been tackled at millimeter wavelengths through the use of integrated tuning inductors (D'Addario 1984; Räisänen et al. 1985; Kerr et al. 1988) but the Caltech mixers still used the lead-alloy devices produced by Ron Miller at Bell Labs using the angle evaporation technique pioneered by Gerry Dolan (Dolan 1977) which was capable of very small junction areas but was not compatible with additional tuning circuitry. In addition, the lead junctions were not very robust, as Woody describes in this volume (Woody 2009). It was clear that a more sophisticated fabrication method would be needed, such as the Nb/Al-oxide/Nb trilayer process that had been developed by Gurvitch, Washington, & Huggins (1983) at Bell Labs or the new NbN/MgO/NbN junctions developed at JPL (LeDuc et al. 1987). Although NbN could potentially go to higher frequencies, the niobium process was simpler and Kerr and his NRAO colleagues were having good success at 100 GHz using Nb junctions from both Hypres and U. Virginia (Lichtenberger et al. 1989) so we started to work on Nb junction fabrication at Illinois.

Dale Van Harlingen's group was very active and was firmly embedded in the solid-state physics environment at Illinois. High-temperature superconductivity had just been discovered, and Dale's student Gene Hilton (now at NIST) was busy making high-T_c films using laser ablation. Another student, Fred Sharifi, spent a lot of his time introducing me to the intricacies of device fabrication, and together we constructed a sputtering deposition system and an etching system, mostly out of old surplus vacuum components. Despite the crude nature of our tools, they were successful in producing junctions that looked suitable for submillimeter SIS mixers, as shown in Figure 4.

The next step was to design a mixer — another problem in impedance matching. Initially, I thought about using a tapered transmission line to adia-

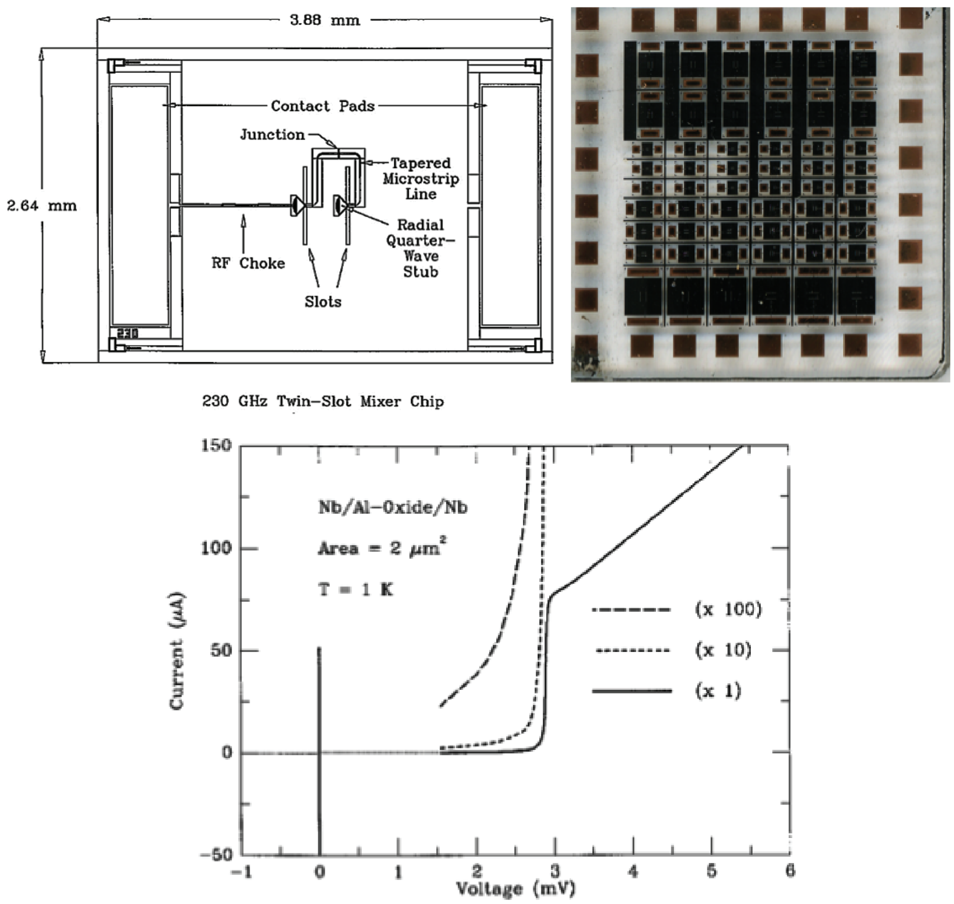

Figure 4. *Top Left*: Design of the twin-slot quasioptical mixer chip developed at U. Illinois in 1988–89. The 230 GHz version is shown; scaled versions for 350, 500, and 800 GHz operation were also developed. *Top Right*: Photograph of the first twin-slot devices fabricated at U. Illinois in late 1989. *Bottom*: *I-V* curve of a niobium SIS junction produced at U. Illinois in late 1988.

batically bring the 90 Ω impedance of Büttgenbach's spiral antenna down to the very low (∼ 4 Ω), primarily capacitive impedance of the SIS junction at 500 GHz. This seemed like the best approach at the time since we knew very little about the properties of the materials we were using, and a resonant design seemed risky. Adrian Webster visited Illinois around that time, and suggested using a slot antenna since it might have a lower impedance, making the matching problem easier. That turned out to be true, provided that one used full-wave rather than half-wave slots. Also, two slots were better since they could produce a symmetric beam pattern that could be efficiently illuminated with a hemispherical lens. Best of all, the properties of the antenna were calculable (Kominami, Pozar, & Schaubert 1985; Zmuidzinas & LeDuc 1992). In fact, twin-slot antennas had been used at millimeter wavelengths by Kerr (Kerr, Siegel, & Mattauch 1977) and Neikirk (Heston et al. 1991), but with significant differences in the design. With a mixer design in hand, we went to work producing the devices (see Fig. 4).

By the summer of 1989, we had made enough progress that Fred Lo and John Tucker started to talk to me about my next career step. In fact, it was then that John Tucker took me to a superconductivity conference that was held in Huntsville, Alabama, and we were treated to an hour-long discourse, entirely from memory, on a theory of high-T_c superconductivity by none other than eighty-year-old Edward Teller, who afterwards treated us to a Mozart piano concerto at the conference reception. Nonetheless, I very much enjoyed that trip, and especially the opportunity to get to know John a bit better. So things were looking good at Illinois, and then my phone rang — it was Tom Phillips calling from Caltech.

5 Caltech, 1990–present

Caltech was irresistible. Not only had the CSO recently been completed, but thanks in large part to Tom's efforts in getting NASA to invest in submillimeter technology development, there was a beautiful new facility at JPL — the Microdevices Laboratory (MDL) — outfitted with state-of-the-art equipment for SIS junction fabrication. I was given a laboratory at Caltech — I believe Bob Leighton had previously used it — and proceeded to transform it from a random collection of old unwanted equipment into a receiver testing laboratory. While a few of the twin-slot devices that were fabricated at Illinois showed SIS *I*-*V* curves, the yield was very low. So I started visiting Rick LeDuc at JPL/MDL, learning from Rick how to use the MDL cleanroom equipment for SIS fabrication as practiced at JPL. By early 1991, about a year after my arrival at Caltech, Rick and I produced the first set of usable twin-slot devices, and the resulting receiver tests at 500 GHz were encouraging (Zmuidzinas & LeDuc 1991, 1992). The environment at Caltech was terrific — apart from Tom, I could turn to Geoff Blake, Thomas Büttgenbach, John Carlstrom, Todd Groesbeck, Jocelyn Keene, Jacob Kooi, Darek Lis, Pat Schaeffer, Rob Schoelkopf, Peter Schilke, Gene Serabyn, Jeff Stern, Taco, and many others for help.

With the mixers working, the next step was to build a receiver for the KAO (Zmuidzinas et al. 1995a). The new receiver would operate at similar frequencies to Tom's InSb receiver, and in fact would have similar noise temperatures — the

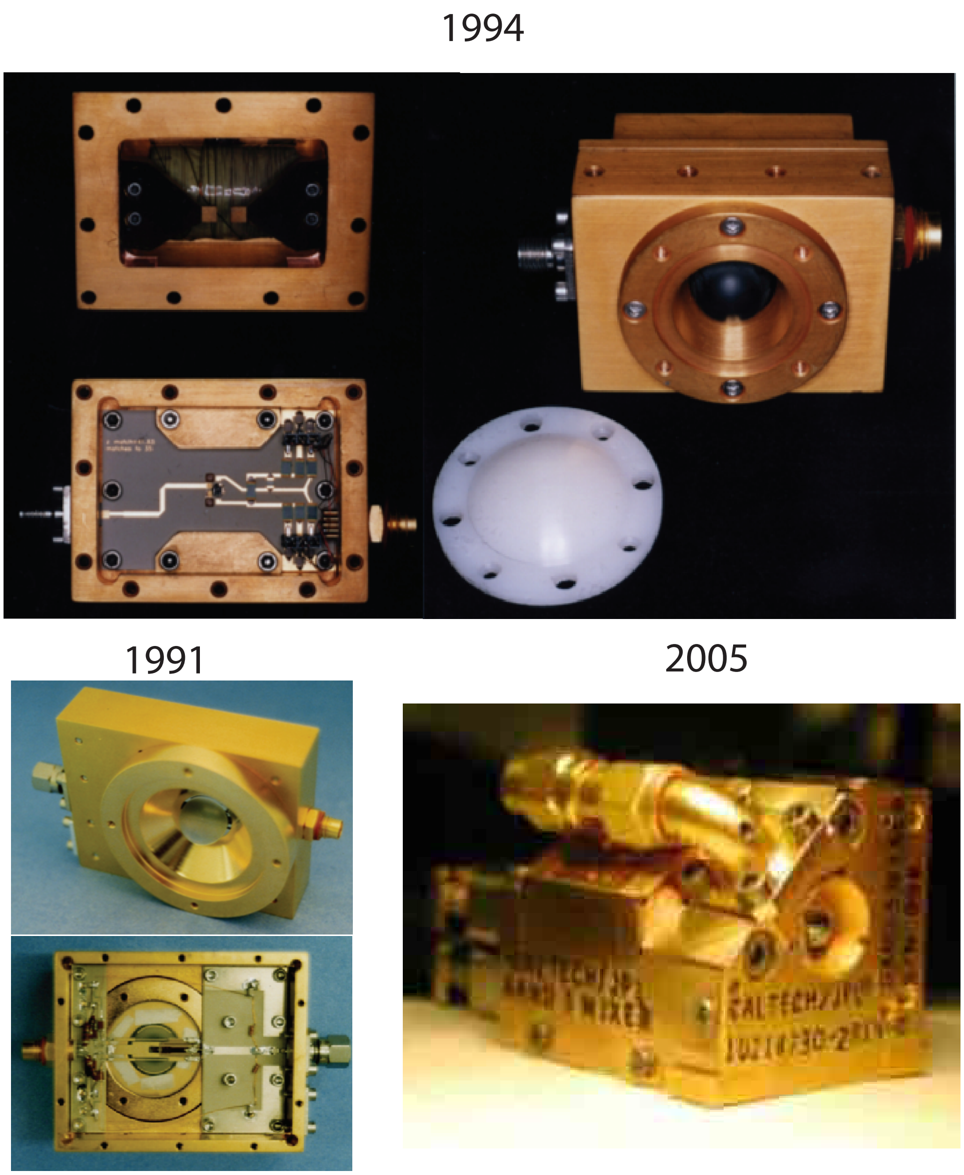

Figure 5. Twin-slot mixer evolution. *Bottom Left*: The first version, developed at U. Illinois in 1989 and tested at 500 GHz at Caltech in 1991. *Top*: The second version, flown on the KAO in 1993–95 for observations in the 500–800 GHz range. *Bottom Right*: The latest version, designed by A. Karpov at Caltech for use on Herschel/HIFI at 1.2 THz (Karpov et al. 2004).

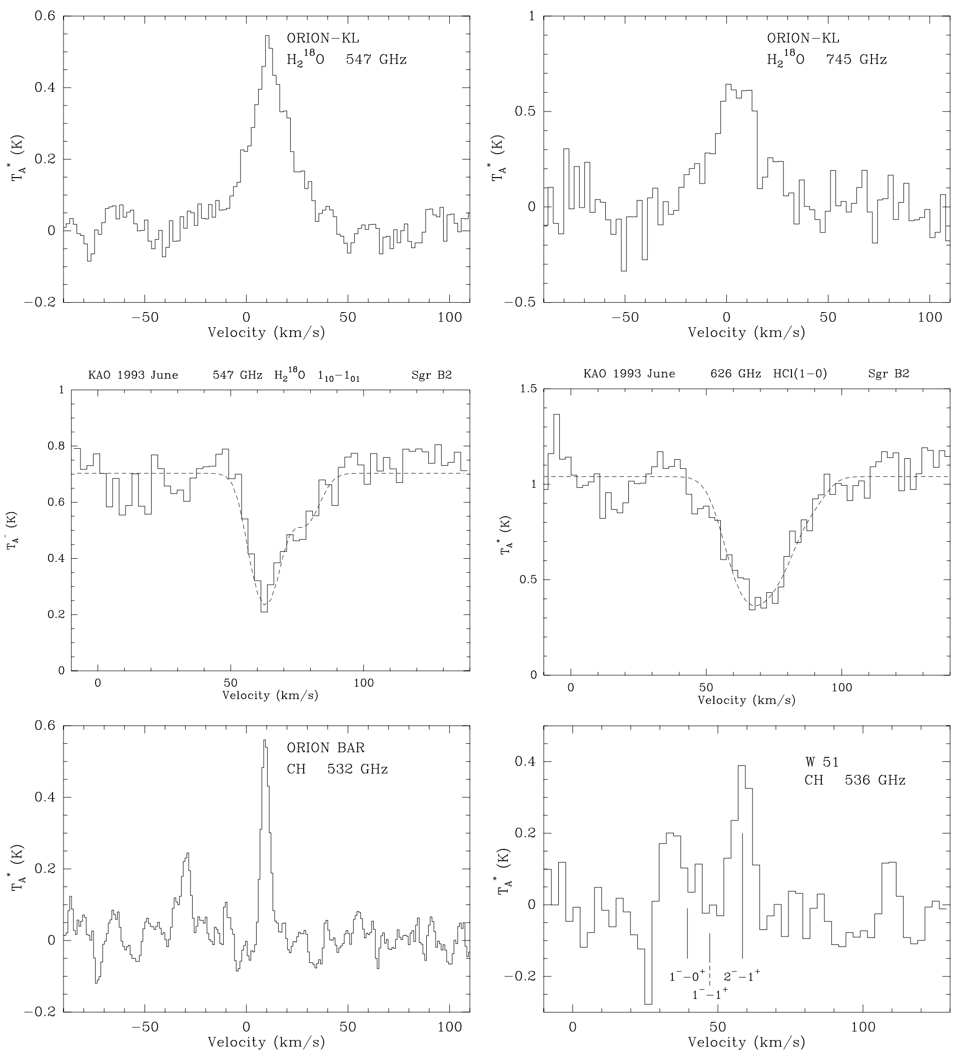

Figure 6. Submillimeter spectra obtained with the Caltech SIS receiver on the KAO, 1992–95.

real advantage of the SIS version was an IF bandwidth of $\sim 1\,\mathrm{GHz}$ instead of a few MHz. With this receiver, we were able to revisit the spectral territory that Tom had pioneered nearly two decades earlier, making a few new detections in the process (Zmuidzinas et al. 1995b; see Fig. 6). The backend spectrometer was a digital correlator that used a custom chip developed by Brian Von Herzen at the CSO — yet another way that we were riding on Tom's coattails.

With the shutdown of the KAO in 1995 to make way for SOFIA, my focus shifted to the CSO, Herschel/HIFI, SOFIA, and by 1999 to the development of a new type of superconducting detector for submillimeter imaging — the microwave kinetic inductance detector, or MKID, and its future use on the 25 m CCAT. So it is really only over the past decade that it has finally been possible for me to stop chasing Tom's pioneering footsteps, and to start heading into new territory!

Acknowledgments. As I hope has been made clear, I owe an enormous debt to Tom Phillips — for pioneering the field of submillimeter astronomy and getting it funded, for inventing the SIS mixer, for detecting neutral carbon, for helping get the MDL built, for getting the U.S. involved in Herschel, etc. — but especially for the opportunity for me to return to Caltech. Thanks, Tom!

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