Foreword

The 2009 U.S. Workshop on the Physics and Chemistry of II–VI Materials was held in The Embassy Suites Lakefront Downtown Hotel, Chicago, Illinois from October 6 until October 8 2009. This Workshop was the 28th in the series, which began in October 1981 in Minneapolis. The first meeting was dedicated to the scientific and generic technological issues of importance to mercury cadmium telluride and its applications in infrared imaging. Workshop interest has now evolved to include all II–VI materials having application in a variety of emitters and detectors. In 1989 the Workshop expanded its scope to include related II–VI materials such as CdTe and CdZnTe that are used as substrates for epitaxial HgCdTe growth as well as for gamma-ray detectors and solar cells, as well as other electrooptical II–VI materials such as ZnO and ZnMgO.

In 2009, 73 papers were presented at the Workshop, and more than half of them were peer-reviewed and published in this volume. The Workshop co-chairs were Dr. Marion B. Reine of BAE Systems, Lexington, MA, and Prof. Siva Sivananthan of the Department of Physics, University of Illinois at Chicago, IL. Ralph Nadell of Palisades Convention Management, Inc. and Yesim Anter of University of Illinois at Chicago coordinated the Workshop arrangements.

This year's William E. Spicer Award, which used to be known as the "Best Student Paper Award" until 2004, was presented to Mr. Michele Moresco, a Ph.D. candidate in the Department of Electrical and Computer Science, Boston University, Boston, MA. The title of his paper is "Full-Band Monte Carlo Simulation of HgCdTe APDs," and Mr. Moresco is advised by Prof. Enrico Bellotti.

Much of the credit for editing this volume goes to the session organizers for arranging prompt review of the papers, and to the authors for choosing this forum for presenting their results. Keith Lannan, Electro-Optic Technology Division, U.S. Naval Surface Warfare Center, Crane, Indiana, gave a keynote address on "The Navy EO/IR Technology Roadmap." Several interesting papers were presented in subject areas of solar cells, CdZnTe substrates, gamma-ray detectors, type II strained-layer superlattices, devices, processing, characterization, impurity doping, and defects in II–VI materials. In addition to the Workshop papers presented in 2009, this year's foreword includes a special piece on the history of the II–VI Workshop, written by Thomas Casselman of EPIR Technologies, Inc. Mr. Casselman portrays the history of the Workshop in detail from his own experience and perspective, with a hint of humor and his characteristic articulation of events.

The proceedings of the first ten Workshops (from 1981 to 1991) have been published in the *Journal of Vacuum Science and Technology*. Papers from the Workshop from 1992 onward appear in special issues of the *Journal of Electronic Materials*.

The II–VI Workshop Committee would like to thank the following organizations for their continued sponsorship:

- U.S. Army CECOM Night Vision & Electronic Sensors Directorate
- U.S. Army Research Laboratory
- U.S. Army SMDC
- U.S. Navy Electro-Optics Center, Penn State Applied Research Laboratory
- Office of Naval Research
- Air Force Research Laboratory
- The Minerals, Metal, & Materials Society

The American Physical Society has endorsed this Workshop.

In 2010, the Workshop is going to be held in New Orleans, Louisiana from October 26 to 28, and in 2011 and 2013 the Workshop is scheduled to be held in The Embassy Suites Lakefront Downtown Hotel, Chicago, IL.

The workshop website is: <http://www.ii-viworkshop.org/>.

S. Sivananthan
Director, Microphysics Laboratory,
University of Illinois at Chicago,
845 W. Taylor St., Chicago, IL 60607
Special Issue Co-Editor

N.K. Dhar
Defense Advanced Research Projects Agency,
3701 North Fairfax Drive,
Arlington, VA 22203-1714
Special Issue Co-Editor

Y. Anter
Project Coordinator, Microphysics Laboratory, M/C 273,
University of Illinois at Chicago,
845 W. Taylor St., Chicago, IL 60607
Special Issue Editorial Coordinator

THE HISTORY OF THE US WORKSHOP ON THE PHYSICS AND CHEMISTRY OF HgCdTe (NOW THE II-VI WORKSHOP)

This is a reminiscence of how the Workshop came about, its status as of 2009, and where I believe its future lies. A part of this was a talk given at the Workshop in 1997. I have expanded it to bring it up to date. At my age, I do not completely trust my memory, so any errors or omissions are my responsibility and should not be taken as history carved in stone. The history, or better put, the story, of the Workshop is one of connections, accidental or deliberate, that took us to where we are today.

It all started when I first became involved in HgCdTe. In the late 1970s, Paul Petersen of Honeywell Corporate Research Center did the first calculation of the Auger lifetime in HgCdTe for *n*-type material. One day, when we were talking in his office and he was teaching me about this new (for me) material, Paul was sitting at his desk looking at the Kane band structure diagram upside down while I was across from him with the right-side-up view. Paul then noticed that there was an analog to the Auger lifetime-limiting mechanism in *n*-type HgCdTe but involving a Coulomb interaction between heavy holes (rather than between electrons in the conduction band) and, this is most important, that the light-hole band acted as an intermediary band to yield a threshold energy for this Auger process about equal to that for the common *n*-type mechanism (Auger 1).

Up until then, everyone had accepted that the *p*-type Auger recombination lifetime described by John Blakemore in his seminal book *Semiconductor Statistics* would be the only *p*-type Auger mechanism and that it had a threshold energy much larger than that of *n*-type Auger recombination. This implied that its lifetime would be considerably longer. Paul did a literature search and found that Beattie in England had described the ten band-to-band Auger mechanisms in zincblende material and, indeed, there it was—the seventh mechanism listed, which was identical to Petersen's observation—hence we dubbed it Auger 7. Auger 1 was the classic *n*-type mechanism, and Blakemore's *p*-type mechanism was Auger 3.

This was a significant discovery because, if borne out by theory and experiment, we had an inherent lifetime mechanism in *p*-type material as powerful as that in *n*-type material, especially for long-wavelength infrared (IR) detectors.

So, Paul asked me if I would take on the job of quantifying Auger 7, saying he was too busy to attack the problem. This was probably a kindness on his part, because at the time I was searching for a problem to work on. I had never worked in semiconductors before, having had a very eclectic career up to that point, covering a wide area of topics. The common thread through my career was numerical computation—I really am a computational physicist.

So, off I went as was my wont, “a fool rushing in where angels fear to tread,” and proceeded to attack the Auger 7 calculation for HgCdTe. Then, I got a lucky break. Honeywell decided to send me on a summer sabbatical in early summer 1979 to Stanford to work with the Computer Science Department and learn about design automation of silicon-based integrated circuits (ICs). Unfortunately, the department did not have any time for me, so I was basically on my own, and I spent a lot of my time doing a “back-of-the-envelope” calculation of Auger 7. I did not have a background in semiconductor material or device physics. (Jim Phillips, now at Lockheed Martin Missiles and Space, brought me up to speed, pointing out that IR detectors were either resistors or diodes.) Besides, lifetime was a material-physics transport problem, and I did not need to know about the actual devices. Frankly, I did not know spatial band bending from a deep knee bend.

I completed the Auger 7 calculation, approximating my way through until I came up with a rough value for the value of the ratio of intrinsic Auger 7 to intrinsic Auger 1 of roughly 6. That made the values of Auger 7 too close to that of Auger 1 for anyone to be comfortable with the idea that we could beat the Auger lifetime killer in LWIR by turning to *p*-type absorbers.

Returning to Minneapolis, I gave a paper at, I believe, the 1979 IRIS Detector Specialty, and published the results. During the meeting, Dick Reynolds, head of DARLA Science Office, asked why the IR materials community did not have a conference such as those for silicon or the II-V materials.

I had an idea. While talking to Marion Reine in his Honeywell Radiation Center (now BAE North America) office in Lexington, MA, I suggested that we should hold a Workshop on HgCdTe hosted by Honeywell to which we would invite university and government researchers. I, with my aggressive, competitive nature did not want to invite any competitors. At the time, Joe Schmidt and his colleagues at Honeywell had compiled some of the physical properties of HgCdTe in something called the “Gold Book.” I did not feel this “highly proprietary” information should be given to our competitors. Marion, a man of grace and wisdom,

blanched at my stance and gently suggested that this was a bad idea and that industry should be invited. On returning to Minneapolis, I had a conversation with one of my colleagues, Ernie Stelzer, who suggested I talk to Bill Spicer of Stanford about forming a HgCdTe conference. Bill, in turn, who knew Reynolds quite well and is a world-class scientist, was being encouraged by Reynolds to form a HgCdTe conference. So, not having any idea who Bill Spicer was, I called him up, cold, and told him about my idea of a Workshop. I guess I had the nerve of a sore tooth. In Yiddish, we call it *Chutzpah*. Now, if you know Bill, you know he does not have a pretentious bone in his body. This first-rate scientist talked to me as an equal, and we agreed we would co-chair the conference. Now, we had to find the money to support such an effort, hopefully from the government because HgCdTe was primarily a defense material. We tried Dick Reynolds; he pretended he had never heard of HgCdTe and refused us. Besides Dick, there was a contract technical representative from Air Force Office of Scientific Research who was supporting some small HgCdTe programs at Honeywell, including my lifetime calculations. We tried him, but he said it would never work. Finally, Horst Wittman, then at the Army Research Office rode to the rescue and identified funding. I think it was for US \$10k. We were off and running.

Bill Spicer was the key person because he had organized conferences before, and he was responsible for identifying members of the organizing/governing committee. We agreed that university people would outnumber industrial members to avoid competitive industry politics (little did I know that university politics could be the most vicious of them all). And so we had Bill and myself, Paul Raccach (University Of Illinois, Chicago Circle), Roshni Aggarwal (MIT & Lincoln Labs), Arto Nurmikko (Brown University), Tom McGill (Cal Tech), Grady Roberts (Texas Instruments), Derek Cheung (Rockwell Science Center), Horst, Gordon Griffith (Air Force Materials Laboratories at Wright-Pat AFB), and Charles “Bucky” Freeman (Army Night Vision Labs). Yes, I know there is someone missing and that requires an explanation.

One day Bill called me and said that we needed to add another company to our list: Santa Barbara Research Center, a subsidiary of Hughes Aircraft. My reply was, “No, I don’t know why we should—I have never heard of them.” Talk about arrogance. Bill, in his inimitable manner, gently but firmly persuaded me to include SBRC, and we added Kevin Riley, then at SBRC, to the committee.

My job was to organize the conference while Bill got the players, dealt with the contract that came through Stanford, and set up the call for papers. That is, he did the majority of the crucial work. Now, I had never organized anything bigger than a Boy Scout camp out, but I had a tremendous resource at Honeywell: my colleague, Gene O’Neill. He had experience in organizing large events. So, I put Gene in charge of food and venue. I set the calendar and, in a brilliant stroke, I set the first organizing committee meeting for February 1981 in Minneapolis, the first Workshop for Bloomington, MN in late October, 1981, and the paper selection committee meeting in Dallas in July 1981. Now you need to know that, in the Minneapolis area in January and February, there are many days where the maximum temperature does not get above -20°F , and we often got our first frost in September. Also, Dallas could get very warm in July. However, it was my idea that we were not there to “party,” so I figured that the weather was not an important factor.

I picked the conference hotel to be the Radisson in the suburb of Bloomington, MN, out in the boonies. Gene arranged the catering, and I remember we decided on a conference fee of US \$200, including lunches and proceedings. Meanwhile, I had to deal with a very nervous Horst Wittman, who was quite worried about the legality of some aspects of what we were doing under the Stanford contract. Every 2 weeks or so, Horst would call and would open the conversation with, “Tom we are going to prison.” After a few of these calls I relaxed, since I was sure that, under Bill Spicer, every item in that contract would be scrutinized very carefully by him. Needless to say, neither Bill, nor Horst, nor I spent 1 s behind bars.

Everything went quite smoothly and I remained quite calm (unusual for me), not realizing the enormity of the task. Remember, we did not have Palisades take over Workshop management until 1984, I believe. In August, the day before I was to travel to a European conference to give a paper on Auger 7, I was told that there would be a special seminar given by a Jean-Pierre Faurie of CNRS-LETI (France), who would talk about molecular beam epitaxial growth of HgCdTe. I had no idea what this was about, but I heard Jean-Pierre’s talk and got an idea: why not invite him to the Workshop? He was very reluctant, because he had spent months in the USA and did not want to return so soon. I talked him into it, and Bill and I agreed to pay his way. In those days Jean-Pierre was achieving electron mobilities of around 10. So, the first Workshop was a smashing success; we had over 200 attendees. The goals of the Workshop as stated by the committee were:

- (a) Synthesize what is known and understood about the physics and chemistry of HgCdTe
- (b) Identify the fundamental research areas that needed to be pursued
- (c) Connect the fundamental properties of HgCdTe with the materials growth processes
- (d) Bring together workers in fundamental research to exchange ideas, problems, and solutions
- (e) By supporting graduate schools that focused on IR materials, furnish new blood to work at IR companies as well as teach at universities

It should be pointed out that the Workshop focused on material research not on devices. I remember that George Chapman of SBRC and I had a poster paper where we described the properties of a MCT diode made by using silver paint to make contact. George came to me in a panic because Bucky Freeman had told him the paper was a major security breach and that we were in deep trouble. We calmed Bucky down by pointing out that anyone could do the same thing. After that, device physics and even focal-plane arrays were presented at the Workshop.

Jean-Pierre did come, and he and Paul Raccach met and talked about the future. Then Raccach pulled a coup, realizing that Faurie wanted to leave France. He told Dick Reynolds about Faurie and his work. As a result, DARPA bought Faurie a MBE machine and he set up a laboratory, the MicroPhysics Laboratory at the University of Illinois/Chicago, with a full professorship. The Workshop had its first triumph, although all the credit goes to Reynolds, Raccach, and Faurie, but we were the matchmakers. Connections! There is more, as MBE HgCdTe matured right in front of us to a standard technology for IR focal planes.

Over the last 28 years, the Workshop has certainly met all its goals, especially because (1) important HgCdTe research groups from government (NRL, NVEOL, Wright Labs, etc.) became involved in the community, (2) university groups from the USA and abroad were encouraged to pursue topics in HgCdTe, and (3) a worldwide research and development community came into being, and various groups established critical collaborations. Furthermore, techniques were finally shared among a diverse group of scientists.

Of equal importance, from my point of view, the first Workshop had superb food, including an opening cocktail party (sponsored by Honeywell), lunches, and snacks still talked about today by those who were there.

Bill Spicer had said to me in the early days that, in his experience, specialty conferences such as the MCT Workshop peaked after about 5 years and then interest waned and the conference eventually ceased to exist. It is the only time I can remember that Bill was wrong. It is 29 years later and here we are, still going like the Energizer bunny!

There have been some remarkable results throughout the years. The seminal experimental photoemission spectroscopy work by Bill Spicer's group at Stanford, followed by the application of theoretical band-structure calculations made by Arden Sher's group at SRI International, established the first structural model of HgCdTe. The Spicer-Sher-Chen bond stability model described the source of MCT's structural weakness; furthermore, it led Sher to propose HgZnTe as a more structurally robust material.

Based on Peter Bratt work at the Santa Barbara Research Center, device physics models were developed that described the formation of barriers to minority-carrier flow in heterojunction structures. In addition, improvements in passivation were reported with the discovery of CdTe as the "ideal" passivant for HgCdTe devices.

However, drastic changes occurred in Department of Defense (DoD)-funded research in the late 1980s. As the major companies developed product, DoD funding for research began drying up. First, it was not clear what research funding could do to improve the performance of MCT-based IR FPAs. Second, it was not understood by the funding agencies that there was a need for further research. It was thought that the research was basically complete, wrongly so. However, there were exceptions, one of which was Lyn Brown of AFML-Wright-Patterson (AFML-WAFT). She has consistently championed research in the material, including a major effort on defect reduction. Without her efforts in supporting research and getting Air Force funding for the Workshop, I believe that the Workshop would no longer exist today. I would be remiss not to mention the support given to the Workshop by Jim Waterman, Craig Hoffman, and their colleagues at the Naval Research laboratory as well as the Army Research Lab and NVEOL.

Now here we are almost 30 years later, and the Workshop still attracts worldwide members of the IR community. We are indeed an international community, including members from the UK, Israel, Germany, South Korea, Australia, and Norway. In the 1990s, it was decided to

expand the reach of the Workshop to include other II–VI materials, especially the wide-gap materials such as CdTe and ZnTe. These are important materials for a variety of devices, including ionizing-radiation detectors, solar cells, etc. The cross-coupling between the wide bandgap and narrow bandgap has been important, especially in areas such as growth and processing.

Now, devices and even IR FPAs are discussed at the Workshop. People are very comfortable in presenting data on $2k \times 2k$ arrays for astronomy. Another new device structure that takes advantage of specific properties of HgCdTe is the avalanche photodiode. One can trace the improvements in APD performance by reading the proceedings of the Workshop over the past few years. How much longer will the Workshop last? As long as there are problems to be solved for II–VI materials. We are still not where silicon was in the 1970s. We still have not achieved success in a critical area of research: growth of HgCdTe on silicon substrates, which would allow much larger IR FPAs than are currently available on CdZnTe substrates.

To be successful in the future we are going to have to change the requirements paradigm, so that materials requirements are not set by flowing down from system requirements without consideration of their feasibility. Rather, there will have to be a balance, where state-of-the-art materials development flows up to systems people so that they have realistic knowledge of materials capability.

T.N. Casselman, EPIR Technologies, Inc.
590 Territorial Drive, Unit B, Bolingbrook, IL
Email: tcasselman@epir.com;
cassco@c-fourth.com
December 2009