SAND2007-4603C

Recalling the origins of DLTS

David V. Lang[*]

Columbia University, New York, NY 10027

Sandia National Laboratories, Albuquerque, NM 87185

Elsevier use only: Received date here; revised date here; accepted date here

---

Abstract

This paper recalls the events leading up to the author's 1973 discovery of Deep Level Transient Spectroscopy (DLTS). It discusses the status of junction capacitance techniques in the late 1960's and points out why the typical capacitance instrumentation of that era would not have lead the author to the DLTS discovery. This discovery is discussed in the context of the novel NMR-inspired instrumentation used by the author to study fast capacitance transients of the ZnO center in GaP LEDs. Finally, the author makes some general comments about the innovation process.

Keywords: DLTS; defects in semiconductors; history of science; transient capacitance.

---

1. Introduction

The measurement technique of Deep Level Transient Spectroscopy (DLTS) is widely used to study defects in semiconductors.[1] Since the original paper was first published in 1974, it has been cited nearly 2300 times, and is still actively cited today. The story of the origins of this technique is an interesting case study in the history of science and in the process of innovation. In this paper I will discuss these origins by first reviewing the status of defect measurement techniques in the late 1960's and then recalling the events that led to my experiments in the early 1970's that made the "accidental" discovery of DLTS possible.

2. Short History of Capacitance Measurements

It is well known today that monitoring the rf capacitance of a Schottky barrier or pn junction is a convenient and highly sensitive technique to study defects in semiconductors. In the late 1960's, however, such methods were just beginning to be used. My colleague and mentor at Bell Telephone Laboratories, C. H. Henry, first became aware of these techniques at this time from the work of C. T. Sah's group in Electrical Engineering at the University of Illinois.[2,3] Henry and I both had strong ties to the Illinois Physics Department -- I was a post doc with C. P. Slichter from 1969 to 1972 and he was a former Slichter Ph.D. student who was by then the Bell Labs Ph.D. recruiter at the university. Henry and his post doc, H. Kukimoto, had set up a photocapacitance experiment at Bell Labs to study oxygen-doped GaP LEDs, and this was the opportunity that eventually lured me to Bell.

We did not know it at the time, but junction capacitance techniques actually began a few years earlier at RCA Laboratories in Princeton. Williams was the first to use transient junction capacitance, studying what we now know to be the EL2 center in GaAs.[4] Goldstein and Perlman reported what appears to be the first photocapacitance experiment, studying defects in GaP.[5] Both of these results were a natural consequence of the leading role that RCA played in studying space-charge limited current and photoconductivity of wide-gap semiconductors.

---

[*] Corresponding author. Tel.: 1-212-496-6433; fax: 1-212-496-6433; e-mail: dvl2101@columbia.edu.

Capacitance work on Cu-doped GaP was also being done at this time by H. C. Grimmeiss' group at Lund.[6]

All of the capacitance measurements being done in the late 1960's were on a slow time scale, i.e., steady-state or slow transients that could be captured on a strip-chart recorder. As we will discuss in the next section, this is a key difference that distinguished my early work on fast capacitance transients at Bell Labs from others in the field and lead me directly to DLTS.

3. Fast Capacitance Transients

In 1972, I joined C. H. Henry's Department at Bell Labs and begin a basic research project to understand nonradiative recombination in GaP LEDs. This was an important topic at the time due to the low efficiency of such LEDs and the presumption that most of the energy was being lost due to nonradiative channels. Kukimoto had just returned to Japan and I inherited his laboratory and experimental setup. My background was in NMR studies of metals and alkali halides, so to learn more about semiconductors I began by repeating many of the capacitance measurements on GaP LEDs previously done in this laboratory. The GaP:O system was ideal for the slow measurement systems of the day, since the 0.78eV deep state for the first electron on oxygen had a thermal emission transient of nearly an hour at 100C.[7,8] I was also interested in detecting by capacitance methods the relatively shallow ZnO center (E_c-$0.29eV$), which was known to be present in the same diodes and was responsible for the red photo- and electro-luminescence. The thermal emission rate of this defect was known from the thermal quenching of the red photoluminescence, so I knew I only needed to go to about 150K to see this transient on a strip-chart recorder. However, I found no evidence of a ZnO capacitance signal at any temperature! After some consultations, I concluded that the problem was due to temperature-independent tunneling from the ZnO level in the high electric field of the *pn* junction of the LED, which was doped at $4x10^{17}\ cm^{-3}$. Thus, the only way I was going to be able to measure ZnO was to go to a much shorter time scale to observe the tunneling transient.

This was a problem. The Boonton Capacitance meter used by Kukimoto for the photocapacitance experiments had a response time of several tens of milliseconds following a voltage pulse applied to the bias using the transformer coupling scheme developed by G. L. Miller for the GaP:O capture cross section measurements.[8] I could not find a faster method to measure a capacitance transient following a voltage pulse, so I fell back on my NMR experience and set up a modification of the so-called "magic-tee" bridge I had used at Illinois.[9] I reasoned that since this bridge detected a small imbalance in the inductance of a circuit matched to 100 ohms, I should be able detect a small change in capacitance using a similar approach. The Bell Labs "experts" were universally negative towards this approach, since my circuit idea had lots of impedance mismatches and other rf problems. I proceeded in spite of the criticism, however, and made a *20-MHz* bridge circuit that could detect capacitance transients with a recovery time in the microsecond range.[10] Using this fast capacitance bridge, I could easily detect the transient from the ZnO center.

The ZnO capacitance transients could be seen on an oscilloscope, but were too fast to record on a strip-chart or XY recorder. This is where the boxcar amplifier (that turned out to be the key to DLTS) came in. In those days, fast transients were typically recorded using a boxcar amplifier, also known as a gated integrator, since fast digital multichannel recorders were very expensive and not widely available. I had considerable experience using a boxcar for pulsed NMR experiments, so this was a natural thing for me to do. I borrowed a single-channel scanning-gate boxcar and began a detailed study of the ZnO transients. One of the interesting observations came from manually plotting the difference between transients for bias pulses of various heights. This is essentially the same as what later became known as Double-DLTS; it allowed me to measure the electric-field-induced tunneling rate as a function of distance inside the depletion region. The rate depended exponentially on electric field, as might be expected for a tunneling process.[10]

This sets the stage for DLTS. I had all of the pieces in place, but I actually discovered DLTS by accident. The ideas of "time-domain filtering" or "Laplace-transform spectroscopy" or the "rate window" concept came *after* the discovery.

4. The Discovery of DLTS

In those days, my daily routine was to cool the sample to some temperature between 150K and 200K, set up the bias pulse sequence to see the repetitive capacitance transients on the scope, and then make detailed records of the transients using the scanning-gate boxcar connected to an XY recorder. This was a perfect analog world, before the HP35 hand calculator and many years before PCs. My "hard-memory" consisted of notebooks filled with XY recorder output and plots of manual data analyses. In retrospect, this analog world was essential to the discovery of DLTS. If I had being doing these same experiments with today's suite of digital instruments controlled by a PC on an IEEE-488 bus, I might never have noticed the remarkable event that lead to DLTS.

The discovery was so dramatic that I still remember it vividly -- 34 years later. I had just begun my daily routine of shutting down the system. I turned off the cryostat and allowed the sample to begin its slow return to room temperature. However, on this day I was distracted for some reason and didn't immediately turn off the pulse generators or the boxcar, leaving the gate at a fixed delay relative to the bias pulse. A few minutes later, out of the corner of my eye, I could see the analog boxcar output meter swinging wildly. I was initially concerned that something had broken and tried to find the cause of the malfunction. I started by returning the system to the operating temperature and noticed the same wild swing of the meter as I cooled back down! This all seemed very reproducible, not like a malfunction. Before I warmed up again, I connected the thermocouple to the x-axis of the XY recorded to better see what was happening. This turned out to be the very first DLTS spectrum.

One might ask why a single-gate boxcar gave a DLTS signal, since such a signal is the difference between two boxcar gates. The answer is that I had been using the boxcar with an ac-coupled input to avoid the dc offset of the steady-state capacitance. The ac coupling essentially plays the role of the second gate in today's typical DLTS measurement system. Within days I was able to secure a dual-gated boxcar and within weeks had worked out the entire methodology and formalism of DLTS. I was amazed by the resolution, sensitivity, and reproducibility of the technique – as yet unnamed. In fact, I was initially concerned that I was overlooking some flaw; that it could not be as powerful and easy to use as it turned out to be. Because of this, I spent many months using the technique on various semiconductor systems before I felt confident in publishing a paper. The first scientific breakthrough using the method was my work with L. C. Kimerling on radiation damage in GaAs that lead to our discovery of recombination-enhanced defect reactions.[11] We actually submitted this paper before the DLTS paper was published.

Perhaps the biggest uncertainly with the original DLTS paper was choosing a name for the new technique. By the time I was writing the paper, it was clear that the new technique would become very popular. Therefore, I wanted a name that was easy to say, both in full and as an acronym. After considering many alternatives, I concluded that "Deep Level Transient Spectroscopy" and "DLTS" were easy to say without twisting one's tongue. Many years later, some of my colleagues began saying that I chose the name because the first two letters of DLTS coincided with my initials, as in "Dave Lang's Transient Spectroscopy." I was flattered by this suggestion, but can't claim the foresight to have planned it this way.

5. Conclusions

What lessons can be learned from this personal history? I believe there are three. First, it is a good idea to occasionally change one's field of research. This is difficult to do, since it moves one from their "comfort zone" of having success and recognition in a field to being a beginner again. However, it is clear from my experience with DLTS that I would never have considered the instrumentation I used for fast transients if I had not been familiar with similar uses in the totally different field of NMR. Second, one must never take the criticism of "experts" too seriously. My proposal for the fast capacitance bridge was indeed flawed from the serious electrical engineering perspective, but it worked well enough for me to do my experiments. In the end, this was all that was important. Therefore, I learned that if one has good reasons to disagree with the "experts", one should not be dissuaded from trying an unpopular approach. Third, one must carefully watch every aspect of one's experiments – "keep your eyes open." This is why I spent my career at Bell Labs, where I had the time and support to run my own experiments. I could see and feel every bump and wiggle of the incoming data. I doubt that I would have discovered DLTS if I were not personally involved in running my lab.

Sandia National Laboratories is a multiprogram laboratory operated by Sandia Corporation, a Lockheed Martin Company, for the Department of Energy under Contract No. DE-AC04094AL85000.

References

[1] D. V. Lang, *J. Appl. Phys.* **45**, 3023 (1974).
[2] C. T. Sah, L. L. Rosier and L. Forbes, *Appl. Phys. Lett.* **15**, 161 (1969).
[3] C. T. Sah, L. L. Rosier and L. Forbes, *Appl. Phys. Lett.* **15**, 316 (1969).
[4] R. Williams, *J. Appl. Phys.* **37**, 3411 (1966).
[5] B. Goldstein and S. S. Perlman, *Phys. Rev.* **148**, 715 (1966).
[6] H. G. Grimmeiss and G. Olofsson, *J. Appl. Phys.* **40**, 2526 (1969).
[7] H. Kukimoto, C. H. Henry and F. R. Merritt, *Phys. Rev. B* **7**, 2486 (1973).
[8] C. H. Henry, H. Kukimoto, G. L. Miller and F. R. Merritt, *Phys. Rev. B* **7**, 2499 (1973).
[9] D. C. Lo, D. V. Lang, J. B. Boyce and C. P. Slichter, *Phys. Rev. B* **8**, 973 (1973).
[10] D. V. Lang, *J. Appl. Phys.* **45**, 3014 (1974).
[11] D. V. Lang and L. C. Kimerling, *Phys. Rev. Lett.* **33**, 489 (1974).