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Thomas Ypsilantis—The early years

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Thomas Ypsilantis (about 1955).

1. Introduction

Let me begin by thanking the organizers for inviting me here to talk about Tom, who was a not only a good friend but also one of the most imaginative physicists of his generation. I will talk mainly about the early days in Berkeley. Others will tell you about his work in Europe, and particularly his role in developing the RICH technology.

I knew Tom for almost half a century. We first met in 1952 as graduate students in the Berkeley Physics Department, where we both chose to do our thesis research under the direction of Emilio Segrè. After receiving our Ph.D. degrees in 1955/1956 we stayed on, first as post-docs and then as members of the Berkeley physics faculty. We shared an office at LBL for 15 years, and after Tom moved to Europe we stayed in touch by exchanging preprints and getting together whenever one or the other traveled across the Atlantic.

Tom was a very special person. From the very beginning he was imbued with an unquenchable optimism and a fertile imagination that he used to generate new ideas and tackle forefront problems that were outside the scope of the thinking of most of his colleagues. The word “impossible” entered Tom’s mind only when the laws of physics were violated, but never when technological or financial obstacles reared their ugly heads. In Tom’s view such obstacles had to be overcome and such problems had to be solved if the frontiers of physics were to be pushed back. He was never satisfied with just continuing along a well-defined path, but instead he always looked for new directions, new fundamental issues to explore, and new techniques to develop. His forte was in generating ideas and fully exploring their consequences, and he was always clever enough to ally himself with technical wizards like Clyde Wiegand and Jacques Seguinot who could help him turn his dreams into reality.

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In a very real sense Tom's story is the story of the evolution of particle physics. That story started just about the time Tom entered Berkeley. Prior to 1950 particle physics was nuclear physics, and it was the development of high-energy accelerators after World War II that opened the door to that fascinating zoo of particles and their interactions that we have been exploring ever since. Tom influenced the development of particle physics, and at the same time he was very much influenced by it.

When it came to actually doing physics Tom was well organized, and was able to focus his very considerable intellectual talents on whatever problem he was pursuing. His notebooks are full of neat entries that show that he was willing to work out the nitty-gritty details of the complex issues that had to be resolved. He was systematic and tenacious in pursuit of his goals.

My sources for this talk come from personal recollections, from Tom's CV, from his publications, and from comments by Tom's friends and students.

2. Origins

The name Ypsilantis has a long and glorious history in Greece. Perhaps the most notable member of this clan was Alexander Ypsilantis (1792–1828), who was a general in the Tsar's army in Russia, and who led 4500 young Greeks in an unsuccessful war of independence against the Ottoman Turks in 1821. In his CV Tom makes brief reference to his year at the Democritus Institute in Greece in 1966–1967 and writes: "The warm welcome I received I owe in part to the homonymy with General Alexander Ypsilantis who fought the Turks in Greece between 1821 and 1830." Thus it seems that Tom was probably not a direct descendant of the General, but may well have had a more distant connection with him. In any case Tom's immediate ancestors, his father and his mother, emigrated from Greece to the United States in 1924. They settled in Utah, where they had three children, Mary, Tom, and John. Tom's father, John, died unexpectedly when Tom was only $2\frac{1}{2}$ years old, and Tom's mother had the daunting task of supporting her family during the depression. Tom excelled in school, and was also a good athlete. He graduated from the University of Utah with a B.S. degree in Chemistry in 1949. His exposure to the Mormon culture of Utah left no lasting scars.

3. Research

In his CV Tom divides his research activities into three periods: "The Berkeley Years" (1949–1969), "Seven Years of Reflection" (1962–1969), and "Europe" (1969–). I will focus here mainly on the "Berkeley Years", say just a little about "The Seven Years of Reflection", but leave it to others to tell you about his work in Europe.

3.1. Period 1—The Berkeley years

Tom entered the Berkeley Physics Department as a graduate student in 1949. Like most incoming students he spent most of his time during the first two years taking advanced courses and attempting to pass various examinations. He did, however, find time to engage in research, and in fact his first published paper: I. Perlman and T.J. Ypsilantis "Consistency of Nuclear Radii of Even-Even Nuclei form Alpha Decay Theory" Physical Review 79, 30–34 (1950) stems from that time. The next year he and John Reynolds, then a young professor doing mass spectroscopy, co-authored an abstract to the January 1953 Meeting of the American Physical Society at Harvard on "Techniques in Isotopic Abundance Measurements on Elements in group IV" Physical Review 90, 378 (1953). (I found it interesting to note that at this same meeting Enrico Fermi was elected as APS President and Hans Bethe APS Vice-President.)

3.1.1. Experiments with polarized nucleons

In 1952 Tom joined the group of Professor Emilio Segrè, which also included Professor Owen Chamberlain and Dr. Clyde Wiegand. A central theme of the research at that time was the nucleon–nucleon interaction, and the Segrè group used the newly completed 184" synchrocyclotron to engage in a program of nucleon–nucleon scattering experiments. By today's standards the experiments were extremely simple—a beam, a target, a few scintillation counters and associated electronics, some binary scalers, and a log book. It became obvious, even before Tom joined the group that measurements of differential cross sections, even if done very carefully, were insufficient to pin down the relevant amplitudes. It was simply a case of not having enough observables to determine all of the parameters (e.g., phase shifts). Tom and Bob Tripp began looking into the possibility of making and then using polarized beams at the 184" synchrocyclotron to pin down the scattering amplitudes. In 1953, while Segrè and Chamberlain were attending a Conference on the East Coast, they and Clyde Wiegand worked out a scheme for polarizing the protons at the cyclotron by scattering them from an internal target. In his autobiography "A Mind Always in Motion" Segrè writes:

> "Meanwhile, Clyde Wiegand and some of my students continued our experiments at Berkeley. Among the students was Tom Ypsilantis, who had studied chemistry, but had recently come to me because he wanted to change to physics. I soon recognized his human qualities as well as his uncommon scientific ability. During my absence Tom and Clyde succeeded in polarizing the proton beam at the synchrocyclotron by collision. The method was not new; it had been theoretically predicted and experimentally demonstrated at Rochester, New York, but Ypsilantis succeeded in obtaining superior results and started the exploitation of polarized protons, opening new possibilities to the study of nucleon–nucleon collisions. The success obtained and Ypsilantis' spirit of initiative impressed me, and I proposed a faculty appointment for him. He was one of the most promising young physicists at Berkeley, where he continued to do brilliant work for several years."

For the next five years the double and triple scattering experiments initiated by Tom became a veritable Ph.D. thesis factory, and led to more than a dozen significant publications including his own Ph.D. thesis: "Experiments on Polarization in Nucleon–Nucleon Scattering at 310 MeV" (June 1955). Although these experiments were straightforward it was always a challenge to have sufficient beam intensity and to maintain the tight alignment tolerances that were required for these measurements. Even some Greek culture was infused into this work, not so much by Tom, but by Segrè whose extensive knowledge of ancient Greek was used to name some of the critical components. For example, the scattering arm was named "*LEUKATERATOS*". Whenever triple scattering experiments were discussed, either in private or in more public presentations, the speaker would often end up in one of many irreproducible contorted positions as he tried to indicate the various scattering directions. To this end Tom and Segrè hit on the brilliant idea of using a tinker-toy model, that not only served as a useful prop in talks but soon found its way into the illustrations used in the various publications.

As the data accumulated Tom and Henry Stapp started a phase shift analysis. The computers of that time were primitive, to say the least. In our daily work we used slide rules and hand-punched mechanical calculators. Only the major weapons laboratories had computers, and they employed vacuum tubes with high rates of failure. Initially Tom and Stapp worked at Livermore at night, but computer time was hard to get so they joined forces with Nicholas Metropolis on the MANIAC computer at Los Alamos, where time was made available. The data had to be entered with holes punched into paper tapes and the calculations were programmed in machine language. They worked day and night for over a month. Every hour or so vacuum tubes had to be replaced, but in the end they came up with the first comprehensive phase shift analysis of proton–proton scattering at 310 MeV.

Stapp, who was a close friend of Tom's, related a few of his experiences with him. For example, in the middle fifties Tom asked Stapp if he could move into his apartment with him. Stapp agreed and for the next year Tom lived there, but Tom's presence was hardly noticeable. Stapp is an early riser, whereas Tom was a night person. They did actually meet a few times as Stapp went to work at 6 or 7 o'clock in the morning, and Tom would be coming home. My experiences in this regard were similar. We shared an office for 15 years. Tom would seldom show up before noon, but then he would stay late into the night. After all that he would jump into his little Alfa Romeo convertible and pass the wee hours of the morning bar hopping with various friends at the "in places" like the "Blind Lemon" or the "Steppenwolf". Tom worked hard, but he also played hard.

The Alfa Romeo also figured in a story recently related to me by Bill Johnson, who was a former student of Tom. Bill writes:

> "Tom and Hank Stapp were commissioned to pick up a keg of beer for the annual Physics Department picnic (it might even have been the one when Pauli was in Berkeley, since that one stands out clearly in memory). They picked up the keg in Tom's Alfa Romeo and were heading for the picnic when a policeman pulled them over with every intention of citing Tom for speeding. Tom, always a voluble talker, pleaded his innocence and escaped without a ticket by convincing the cop that "these Alfas don't really go so fast, they just look like they're going fast ...."

In a more serious vein Bill added:

> "He is perhaps the first person I ever encountered who truly 'thought outside the box'. No idea was too incredible to stop him from pursuing it to its logical conclusion. If there was ever something profound I learned from him, it was not to be afraid of pursuing far out ideas."

To end this description of Tom's polarization activities let me add this brief footnote. On a visit to Chicago in early 1954 Segrè told Fermi about the high asymmetries observed by Tripp and Ypsilantis in the scattering of polarized protons from complex nuclei. Fermi immediately sat down and proposed an optical model for nucleon–nucleus scattering to explain these results. It was his last contribution to physics.

3.1.2. Discovery of the antiproton and first measurements of the cross sections for scattering, annihilation and charge exchange

In 1954 the Bevatron was nearing completion, and various groups were interested in using it to look for antiprotons. There was a sense of competition between these groups and an uncharacteristic sense of secrecy entered the scene. Tom, though still a student, joined Segrè, Chamberlain, and Wiegand in making plans for this experiment. It was an interesting mix of physicists. Segrè as the senior member and group leader looked out for the interests of the group. He also had an uncanny ability to go to the heart of a problem. Chamberlain was the one we would go to whenever there were unresolved physics issues or questions to be addressed. Wiegand with his superb technical skills was the one who made the experiments work, and Tom with his unbounded imagination was the idea man. It was a potent mix of talents. Their method of choice was a large-acceptance spectrometer to form an intense beam of negatively charged particles, and to use time-of-flight and Cherenkov counter techniques to identify the rare antiprotons among the much more copious pions. One of Tom's main contributions, done in collaboration with Clyde Wiegand, was to design and then test the quadrupole and dipole magnets that were used in this experiment. It is worth keeping in mind that focusing magnets for use in transporting energetic charged particle beams had been developed only a few years earlier by Courant and Snyder at Brookhaven, and that the beam used in this experiment was one of the first modern secondary particle beams.

It was also at this time, that Tom had his first direct exposure to ring imaging Cherenkov counters. Wiegand and Chamberlain had designed a narrow-band velocity-selecting Cherenkov detector designed to detect particles having $\beta = 0.75 \pm 0.014$. A schematic drawing of this counter is shown in Fig. 1, and the resulting response is indicated in Fig. 2.

It may well be that Tom's later interest in ring imaging, and in fact the very existence of this Conference, can be traced back to this formative experience. The experiment itself started in the fall

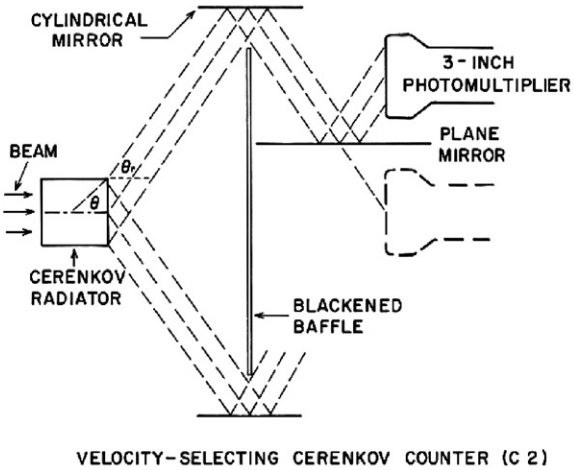

Fig. 1. Schematic diagram of the velocity-selecting Cherenkov counter.

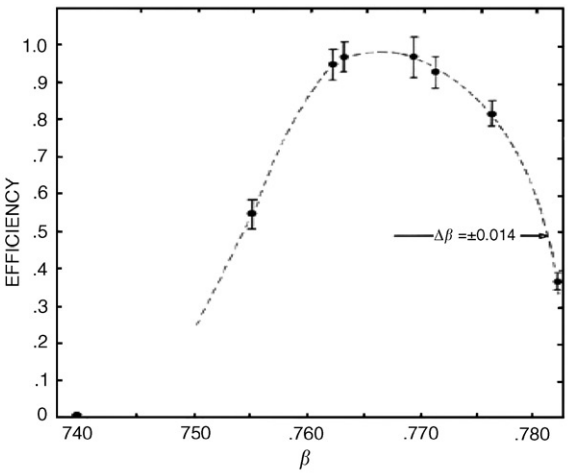

Fig. 2. Response of the velocity-selecting Cherenkov counter.

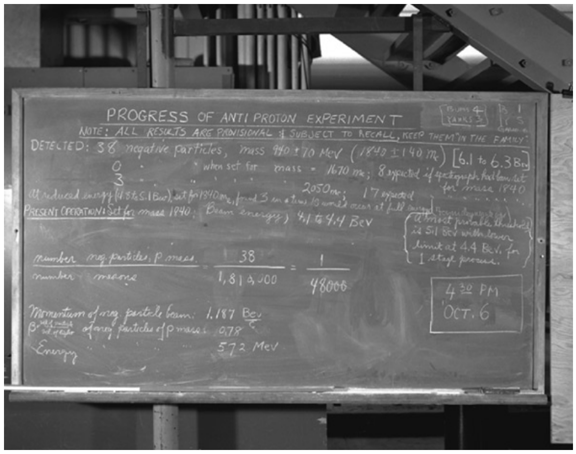

Fig. 3. Real time progress report of the anti-proton experiment.

Fig. 4. The authors of the anti-proton paper: E. Segrè, C. Wiegand, O. Chamberlain, T. Ypsilantis, with E. Lofgren (center).

of 1955, and by early October antiprotons had been clearly seen (Figs. 3 and 4).

Tom's contributions to this experiment were significant, and it was a shame that he and Wiegand did not share the 1959 Nobel Prize that was awarded to Segrè and Chamberlain for this discovery. The experiment did, however, help to establish him as one of the brightest young stars in the field.

In 1956 he was appointed as an Assistant Professor in the Berkeley Physics Department. He was a popular teacher, and soon he began attracting graduate students. For the next few years all of us in the group worked closely together in measuring antiproton cross sections. For example, we were the first to find that that the total cross sections for the antiprotons produced at the Bevatron were several times larger than those for protons. Toward the end of this period, and with the contribution of W. Powell and W. Fowler, we placed a propane bubble chamber in the antiproton beam. We were thus able to make the first measurements of 4π and 6π annihilation cross sections, and to observe an antineutron annihilation star that demonstrated the charge exchange reaction.

3.1.3. Polarization of the recoil proton in pion–nucleon elastic scattering

In 1957 Tom returned briefly to the cyclotron to join in an experiment to measure the polarization of the recoil proton in elastic pion–nucleon scattering at 310 MeV. This was a few years after Fermi had observed the now-famous (3,3) resonance in a cross-section measurement at Chicago. The objective here was to get additional information and then use it to make a detailed phase shift analysis of πN scattering.

3.1.4. The Dish

Up to this point Tom and the rest of us had been doing what I would call the easy experiments. These are the obvious ones where an existing beam hits an existing target and the scattered particles are detected by rather standard means. To be sure these experiments were done carefully, and yielded interesting results, but they did not give Tom the opportunity he always sought to venture into new territory. The break with the past came in 1959 when Chew and Low published a paper describing a method ("The Chew-Low Extrapolation Method") to study the $\pi-\pi$ interaction. The basic idea is to study the reaction $\pi^{\pm}+p\rightarrow\pi^{\pm}+\pi^{+}+n$, and then extrapolate the data to the pion pole in the non-physical region. With his typical enthusiasm Tom mobilized Clyde Wiegand and several students, and together they built "The Dish", an array of scintillation counters to detect the final state neutrons and pions (Fig. 5).

Fig. 5. The "Dish"–A scintillation counter array used to study the π-π interaction.

At the time this device was a quantum leap beyond the traditional scintillation counters that had been in use up to that point. Tom thought big and stimulated those around him to join him in realizing his visions. By the time "The Dish" became operational at the Bevatron in 1960 direct measurements of the $\pi-\pi$ interaction in hydrogen bubble chambers had overtaken his experiment, and in fact the Chew-Low extrapolation was never used to establish any of the many resonant states. Nevertheless, it was an innovative attempt to come to grips with a fundamental problem of great interest, and it did provide thesis fodder for three very good students. One of these students, Joseph Lach, recently wrote me about his recollections of Tom. He says:

> "Tom was one of the first people I got to know when I joined the Segrè group (1957?). I had screwed up my courage and talked to Owen about taking me on but he said he would be off to Harvard on a sabbatical. But he said that Segrè might be willing to take me on which he did. So as a newcomer I wandered around and naturally hung out with the younger faculty types like you and Tom. Looking back on that time, I am still impressed with how open and unassuming all of you were. Not only did that include you and Tom but went all the way up to Segrè. I still recall us heading to the cafeteria to eat outside with the great Berkeley bay view. I can still hear Segrè's occasional comment about cautioning others about sitting with that German (you) who wants to be in the sun! One of my earliest impressions of Nordic vs. Mediterranean differences. This was later further reinforced by my Russian colleagues who would take every possible opportunity to put themselves in the sun.
>
> But I should be more to the point. This is Tom's story. He made a big impression on me. I

remember him as a person just brimming over with ideas. He was happy to share them with who ever he ran into even new grad students. This was invigorating but could also be exhausting. Coming in the lab in the evening and finding Tom (you always had a better chance of finding him at night) and you would immediately get asked what you were doing. Usually his ideas would get translated into more calculations and work for you. That was the down side. He had more ideas that any one I had known. He also had the most crazy ideas of any one I knew. But he had so many ideas that it still translated to the most good ideas of anyone I had known.

There was also a wonderful irreverence about him. He questioned everything and expected you to do the same. There was always a lot of laughter when he was around. I remembered that when I passed through CERN years later, I would also go find Tom and would usually get into an hour or more conversation usually on Cherenkov detectors or some such gadgets that he loved. Also exchanged gossip on our old Berkeley friends. He liked people. I miss him."

3.1.5. Sabbatical in Rome and pion β-decay

In 1959/1960 Tom spent a year in the group of Eduardo Amaldi at the University of Rome. He worked in the same laboratory where almost thirty years earlier Amaldi and Segrè had collaborated with Fermi on slow neutrons. Tom worked with Raoul Gatto on weak interactions. Upon his return to Berkeley he set out to measure the β-decay of the pion, i.e., the decay $\pi^+ \to \pi^0 + e^+ + \nu$. This is a pure vector interaction with a branching ratio that could be accurately predicted to be 1.03×10^{-8}, and thus served as a clean test of weak interaction theory. It is a very difficult and delicate experiment that provided just the kind of challenge that Tom relished. Tom, together with Clyde Wiegand and his students, carried out this experiment at the 184" synchrocyclotron and ended up with 11 events. The result, although statistically limited, agreed with theory, and at the time was comparable in significance to a similar experiment at CERN.

3.2. Period 2—"Seven years of reflection"

At the end of 1962 Tom interrupted his research activities at LBL; in fact, he never resumed them there, although he remained a member of the Berkeley Physics faculty for another 7 years.

3.2.1. Brookhaven (1962–1963)

Tom spent the year 1962–1963 at Brookhaven National Laboratory, where he assumed leadership of the group of Rodney Cool, while Cool was spending a sabbatical year at CERN. This group of seven physicists was engaged in high precision measurements of hadron–hadron total cross sections ($\pi^\pm$, $K^\pm$, $p^\pm$ on protons). They also were involved with B. Barish and A. Tollestrop in an experiment to study antiproton–proton annihilations into two bodies; e.g., $pp^- \to e^+e^-$, $\pi^+\pi^-$, and K^+K^-. This experiment was only completed after Tom had left. Another initiative of Tom's while he was at BNL was based on a suggestion by G. Feldman and J. Bernstein to search for W^- bosons in the reaction $\pi^- p \to pW^- \to pe^- \nu$. As Tom says in his CV: "It was undoubtedly a quarter century too early."

3.2.2. Return to Berkeley (1963–1967)

Upon returning to Berkeley Tom split his time between his teaching duties and thinking about future activities. He followed a course on General Relativity taught by S. Weinberg because he was interested in exploring possible experiments on gravitation. No such experiments materialized, so he once again left Berkeley, this time for Athens.

3.2.3. Institute Democritus in Athens (1966–1967)

In 1966 Tom accepted an invitation by T. Kanellopoulos, Director of the Institute Democritos, to set up a research program there. He worked with five Greek physicists to set up a bubble chamber analysis system. He managed to persuade the Greek government to fund this program at the level of $100,000 per year. He thought that his success in cajoling this money out of the Greek government agencies may well have been due at least in part to the name he shared with the illustrious General. Tom and his group then entered into a collaboration with H. Muirhead of

Liverpool to study p interactions at 7 GeV. While he was in Greece he also served as a member of the Greek Atomic Energy Commission.

3.2.4. Breaking the knot (1967–1968)

At the end of 1967 Tom returned again to Berkeley, and a few months later he resigned his position as a tenured professor in the Physics Department. Many of his friends and colleagues urged him to stay, but in the end he decided to leave. Then for another year he traveled without any precise scientific goal. In his CV he says

> "I needed these seven years and this voyage to sever the tightly knotted bonds that linked me to Berkeley for twenty years. This period, which corresponds to a blank on the chronological list of my publications, has been nevertheless the richest, if not the most peaceful, in my professional (and personal) career."

4. Concluding remarks

Tom left Berkeley, but as you know that is not the end of the story. Greater things were still to come. I will leave it to others to tell you about Tom's years in Europe, and especially about his crucial contributions to ring imaging. It is important to keep in mind that the RICH work, and for that matter all of Tom's scientific activities, were motivated by his desire to find answers to the important questions in our field. I look back in awe at many of Tom's "crazy" ideas—multi-ton Xenon detectors, Hellaz, Aqua-Rich to name just a few—and note how many of them no longer seem quite as crazy. Tom's influence continues apace, and it is almost inevitable that his grandiose schemes may well be realized in the not-too-distant future.

I have talked here mainly about Tom the physicist—the man of imagination, ideas and optimism. But there is another Tom, and that is Tom the human being. Tom was a warm and caring person, whose easy charm and wit endeared him to all of us. His sense of humor was infectious, and he had a way of lightening the mood of any gathering. Another student of Tom's, Rudy Larsen, recently wrote me to say:

> "He was comfortable to be with…no pretense no guile, no unkind words for any."

Tom was proud of his Greek roots. He even purchased a small plot of land on one of the islands, and sent redwood lumber from California to build a house there. I don't know what became of these plans, but I do know that he would have been proud and pleased about the honor that is being bestowed on him today.

Acknowledgements

I would like to thank Bob Tripp, Henry Stapp, Joe Lach, Rudi Larsen, and Bill Johnson for sharing some of their experiences involving Tom with me.