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Reminiscences of a Graduate Student at Princeton in the Late 50s

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It's a pleasure to be back at Temple for a Philadelphia area seminar, although it says something about my age that I'm now talking about the history of mathematics where 30 years ago I would've been addressing the Philadelphia area Topology seminar.

I'll talk today about what it was like to be a grad student at Princeton in the late 50's, the hey day of algebraic topology when Princeton (University andInstitute) was one of the most important centers of algebraic topology. I'll also talk about some of the consequences of those experiences.

But first of all, a little background. From 52-56, I was an undergrad at the University of Michigan. As a freshman, I had Honors Analytic Geometry and Calculus from Moise, who soon offered a fifth hour for those interested; there we delved into the foundations of the calculus: ϵs and δs and on to Dedekind cuts. The fifth hour turnout was small and dwindled further - one of us went on the become chair of classics at Michigan, one went on in logic, one I think became an Episcopal priest. By the time I was the only one remaining, Moise had me in a modified RL Moore method, essentially an introduction to point-set topology.

As a sophomore, I had Honors Calculus from Leveque. As a junior, it was on to Advanced Calculus with a choice of a section in LSA or in Engineering. Since I already had so much theory, I opted for the Engineering section to acquire more computational or problem solving skills, but the section as taught be Meyers was quite theoretical, most of which I already had. I also took courses in Non-Euclidean Geometry and Projective Geometry - which were meant for prospective math teachers! In my senior year after Math Club one evening, I mentionned that it was too bad there were no honors courses for upper classmen. Leveque looked at me in disbelief and asked if I were joking. I wasn't. Turned out I was supposed to be advised by an honors advisor who would see to it I took the appropriate advanced courses, prepartoory to graduate work, but I had gone to an ordinary advisor who was all too willing to approve any of my choices since I showed initiative. After that, the Chair asked Moise who was supposed to be my advisor, why he

had approved my selections. Moise couldn't remember why, never realizing he had not been involved as official advisor!

Fortunately, Projective Geometry had been taught by Rainich who managed to get me to go deeper than the course required and quite turned me on to the relation between geometry and algebra and especially to associativity as an *option*, which played a major role in my future research. In fact, Desargues' theorem as relating associativity and projective space provided an intersting contrast to my dissertation work on homotopy associativity and projective spaces.

As a senior, I took his 'Theory of Invariants' which turned out to emphasize Maxwell's equations as a major example, the other two students in the course being physics majors. The invariants here were those of classical algebra, but he included secondary invariants, those that are defined invariantly only when the primary invariants vanish. This stood me in good sted when I encountered secondary cohomology operations.

Separate from that course, Rainich offered an extracurricular introduction to reading mathematics in Russian. Since familiarity with the Cyrillic alphabet is crucial, Rainich explained the not quite bijection between the two alphabets and the sounds of the letters, then handed out a dittoed sheet and told us to start reading. The page turned out to be Little Red Ridinghood but with a heavy Russian accent, approximately Leetl Rad Raydinghoood.

During the summer before my senior year, I picked up Moise's semi-RL Moore program again and consulted with him enough that he suggested I take his graduate topology course. This was another major influence on my future career. The course went from point-set topology through the Jordan curve theorem and into homology which Moise wrote with the older convention: H^* for *homology.*

I was originally wait listed by Princeton but later admitted, coincidently? soon after Moise had a trip to Princeton? And so I arrived at Princeton in Fall 1956. While I was schlepping my footlocker from the train station to the Grad College, Bott happened to drive by on his way to the Institute and offered me a lift. I had never had Bott as a teacher but he shared an office with Moise and Samelson (!) and I had babysat his children!

Steenrod was the advisor to the entering grad class. He informed us that the usual was to signup for three courses, work really hard at one and attend at least a second. At that time, the department offered no standard first year courses; many of my fellow students had a year of graduate work already and

one tried to put me in my place by claiming topology wasn't mathematics!

The lowest level courses available in the traditional trivium were:

Steenrod: Algebraic topology but now H^* meant *COhomology* which moreover was a ring and we (he) soon moved on to cohomology operations.

Bochner: Real Analysis but taught as a 3 year cycle so day one had Theorem 212 or some such number!

Kaplansky: Algebra meaning C^*-algebras

I experienced quite a shock. Somehow we learned that class work was not where it was at. We were supposed to be working on our own or in student seminars; that's where I learned about Lie groups and a little about those new gadgets: sheaves. Sometimes we had our own seminars there int he Graduate College and sometimes in the evening we would walk over to the Institute and use their lecture room (they had only one back then). In those days, we had no teaching duties nor even assisting.

For a topologist - nascent or established - Thursday's were a delight. There was a topology seminar at the Institute in the morning organized by Montgomery and another at the University in the afternoon. In addition to the several topologists at the University (Steenrod,Moore, Milnor, Fox as well as instructors like Peterson), there were several superstars at the Institute: Dold, Thom, Serre, Bott... I can still remember arriving at the seminar early and overhearing some of the discussion of the controversy about one of the entries in Toda's table of calculated homotopy groups. Once that was resolved, the periodicity pattern stared out and Bott was off and running, soon to have his proof of the periodicity of the homtopy groups of the stable unitary group.

In the Spring of 1957, that first year, Milnor offered a course on Characteristic Classes of fiber bundles. On the first day, he asked for volunteers to take notes. Since I was an inveterate notetaker anyway, I volunteered. The quality of his lectures was such that note taking was easy. Whenever I ran into trouble with filling in some details, Milnor would supply them, a pleasant contrast to a friend at another institution who was left essentially on his own to fill in the details. There is also the story of Jerry Levine's taking notes on lectures of Thom. Levine went to Thom afterwards to inquire about something Thom claimed was easy to see but Levine had difficulty writing up. Thom's response: very easy to see, very difficult to prove!

A major contribution of Milnor was the translation of Thom's cobordism theory - not from French to English but from Thomism to easily compre-

hensible mathematics. The lectures were also outstanding in developing the various kinds of characteristic classes, those of Stiefel-Whitney, of Chern and of Pontrjagin, each by using a different method. More about the course and subsequent book later.

Milnor was on leave my second year so I chose to work with John Moore. This turned out to be fortunate since Milnor had very few successful students while Moore had many. The joke was that Milnor suggested only those problems he hadn't been able to do himself, but it was really more a contrast in style of interaction. Milnor gave polished lectures that were easy to follow but was rather shy one-on-one. Moore was sometimes rather muddled as a lecturer, sometimes seeming to be working through some new mathematics as he lectured, but was marvelous one-on-one, drawing out from the student knowledge the student didn't know he had. Milnor and Moore had only one formal collaboration: the famous Hopf algebras paper which went through many versions, increasingly in the direction of the abstraction Moore favored. When I discussed the paper in my presentation on Milnor's work in Algebraic Topology for his 60th birthday fest, he thanked me for explaining what was in it!

On the semester break, I was back in Ann Arbor and Bott was gave me the preprint of his periodicity paper. I was rash enough to offer a student seminar based on it. I knew little of the classical Morse theory on which it was based, which might have been ok if the audience consisted of fellow students but the public notice attracted more participants from the Institute, including Jimmy Jenkins and Ted Frenkel.

That second year Frank Adams was at the Institute solving the Hopf invariant one problem: The spheres S^n for $N = 0, 1, 3, 7$ admit multiplications with unit regarded as the unit spheres in the real, complex, quaternion and Cayley numbers respectively. Is that all?? By then it was known that n would have to be of the form $n = 2^k - 1$ and $n = 15$ had been eliminated; Adans showed indeed the classical examples were all there were. We had developed a slight friendship, so I was privileged to read and comment on a draft of the paper. In fact, I worked on it on a trip down here to Philadelphia while courting my future wife Ann!

At Princeton, the qualifying exams for the Ph. D. were called 'Generals' and were strictly oral. We had the feeling the faculty pretty well knew our abilities before the formal exams, so they weren't too traumatic. After they were over, there was of course a party with faculty and other grad

4

students. One of my classmates was Ray Smullyan ('What is the Name of (T)his Book?'), who had worked as a professional magician before coming to Princeton, so of course he entertained us with card tricks. Since the eminent probabilist Feller was there, Ray would ask Feller to predict the probablility of a certain outcome involving a deck of cards. Of course, in Smullyan's hands the probablity was always 1 and Feller was a marvelous straightman - feigning total astonishment!

By the end of my second year, I was well into what became my thesis but chose to move on to Oxford on a Marshall Scholarship. The faculty there (J H C Whitehead, James and Barratt who I had known at Princeton) treated me more as a colleague than a grad student, though I hung out with the latter. Officially all topology grad students were assigned to Whitehead, but I was really working with Barratt so I was officially transferred to him. Then he was called to Manchester so Ioan James took me on.

In my second year at Oxford, there was an option to apply for a third but by then Ann and I knew our son was on the way so we opted to return to the US. Then I discovered that the Marshall would pay for the way home IFF I took an Oxford degree. I felt I had enough invested in Princeton that I would like to have their Ph.D. so I decided to split the thesis in two - topology and algebra. Knowing the strict rules for submission in the US - size of margins, weight of paper, etc. - I asked my then supervisor Ioan James what the rules were at Oxford. His reply: Well, if you are going to submit a sonnet, it should have 14 lines. Turned out the thesis had to be submitted already bound. I discovered that Oxford had a special rule: no material submitted elsewhere for a degree could be included, even with attribution! So I submitted Part II to Oxford reviewing Part I (which would *later* be submitted to Princeton) as needed.

So that's why I have a Ph. D. and a D. Phil.

In those days, getting a job was a lot easier, even a job in the US when I was still in England. Ann and I decided MIT in Boston was nearer families than the West coast and, strangely, Yale in those days had a discouraging reputation in topology - to say nothing of a better title for less pay.

Now back to the history of Characteristic Classes.

While still at Princeton in the Spring of 1957, I wrote up the notes and went over them with Milnor. Princeton (both University and Institute) had incredibly good technical typists and the notes were reproduced by a process some of you may never have heard of - I think it was called Ozalid. For

several years, requests for copies kept coming to Princeton. When Xerox came along, they spread by 'samizdat' but soon there were copies of copies of.... While at Princeton in 1969-70, I mentionned to Milnor the continued interest and asked if it was time for it to be published. He told me he had begun a revision, providing more background, but had since moved on to other things; moreover, the notes were no longer up to date. I suggested I do some surgery to make a transition between the revision and the old version and write an 'epilogue' to at least mention many of the developments in the intervening 13 years. He agreed and generously made me a coauthor, rather than just the scribe. We also added the appendices. To this day, I meet other mathematicians and even physicists who recognize me as part of Milnor-Stasheff.

The appendix in terms of the classical Chern-Weil theory using differential forms was particularly fortuitous (13 years earlier differential geometry was represented at Princeton only by an undergrad course and that due to Milnor). de Rham cohomology was totally out of the picture when I was a grad student, having peaked around 1950 (cf. the Colloque de Bruxelles) but being set aside in favor of singular (co)homology thanks to Serre's thesis and his sequel. It took foliation theory to produce choux de Bruxelles. By the early 80's, problems in quantum physics involved *anomalies* which turned out to be directly related to characteristic classes in terms of differential forms. A young post-doc in the UNC physics department, Tom Kephart, was working on anomaly calculations and asked if I were the Stasheff of Milnor and Stasheff. Fortunately Tom was willing to meet me half way in our discussions. In those days, there was a real language barrier between physics and math - different words for the same concept (e.g. the big group and the little group for them meant a transformation group and an isotropy subgroup, gauge potentials turned out to be connections on a fibre bundle, etc.) and even the notation was sometimes opposed: their arrow went from the big group to the subgroup since they were really referring to the representations. Also many physicists used representation meaning irreducible reps (irreps) and were on such intimate terms that they would refer to tehm by number, e.g. the 16 or 16 bar of SU(2).

Discussions with Kephart led me back into mathematical physics, which I had not touched since my undergrad days, and, as they say, the rest is history. Oh, that's right! History is what I'm supposed to talk about today!

Another important aspect of collaboration with Kephart was contact with

one of his mentors Henk van Dam (though Tom was officially Paul Frampton's post-doc). van Dam had been trained in the Netherlands, even was an undergrad math major there, so he was bilingual and able to translate from physspeak into mathspeak. He introduced me to Poisson algebra and Dirac's first class constraints. This enabled me to complete the translation of the Batalin-Fradkin-Vilkovisky machine for reduction of constrained Hamiltonian systems into pure mathematics, in fact, a new kind of homological algebra. Henk also influenced me to attend the Symposium on Anomalies Geometry Topology at Argonne in 1985 wherre I met the mathematical physicist Cotta-Ramusino with whom I subsequently collaborated - again based on Characteristic Classes.

But nothing of Characteristic Classes appeared in my theses. Rather they were concerned with the topology and algebra of homotopy associativity and related higher order homotopies. Recall that in defining the fundamental group following Poincaré, we observe that path addition is associative onoly up to homotopy. From a homotopy point of view, it was natural to ask: what happens with 4 variables? There are 5 ways of associating 4 variables and a given homotopy for associativity relates them as follows:

For 5 variables, the relevant picture is that of a convex polytope and there are convex polytopes of dimension $n - 2$ with vertices labelled by all ways of associating n variables. These are now known as the associahedra or the Stasheff polytopes. There are corressponding algebraic structures with underlying differential graded modules known as A_∞-algebras.

Remarkably, somewhat later, such structures and their Lie analogs showed up in mathematical physics, initially in Batalin-Fradkin-Vilkovisky and in string field theory and recently in deformation quantization. iAn intermediate version shows up in representation theory in the guise of the Biedenharn-Elliott identities for the 6j-symbols and in Drinfel'd's quasi-hopf algebras. Talking to Biedenharn, I found one barrier to communication was that his representations had preferred bases while I had been very thoroughly trained as an undergraduate that a vector space was an entity in itself and a choice of basis was indeed a choice. Rainich in his book on Relativity had done the same for vectors and even tensors, going so far as to say something like: the last thing you want to do is to write them down in terms of a basis.

So much for the referee of the first submission of the theses for publication who rejected the papers as being entirely self-contained and of no interest for the rest of mathematics.