Remembering John Stallings

Danny Calegari and Benson Farb, coordinating editors

Introduction/Authors' Note

This document collects reminiscences of John Stallings by some of us who knew him. The contributions have been minimally edited and are supplemented by Stallings's own "Items from my unknown autobiography". Photo credits are given where known.

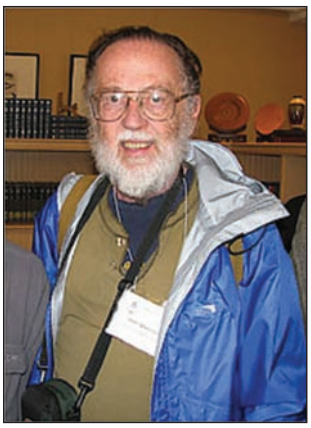


Photo credit: Enrico Le Donne, 2006.

John Robert Stallings Jr.

Danny Calegari

John Robert Stallings Jr. was born in Morrilton (which John pronounced "Marrrlton", rolling the r's), Arkansas, on the 22nd of July 1935, and died at his home in Berkeley, California, on the 24th of November 2008. He achieved early fame as a postdoc with his alternative proof of the Poincaré Conjecture in high dimensions (obtained only days after Smale's breakthrough) using an original method known as engulfing. John then switched his attention to 3-manifold topology and geometric group theory, and transformed both. He brought combinatorial methods from group theory to 3-manifold theory, which at the time was preoccupied with the point-set methods of Bing and Wilder — giving new and cleaner proofs of Papakyriakopoulos's fundamental discoveries, characterizing fibered 3-manifolds by using the fundamental group, and introducing important new methods (e.g., plumbing) in knot theory. Conversely, he brought geometry and topology back to group theory, reinvigorating a field that had forgotten the perspective of Dehn and Nielsen, and laid the groundwork for the later radical innovations of Thurston and Gromov: characterizing groups with infinitely many ends (an achievement for which he was recognized with the Cole Prize in 1965), constructing an example of a finitely presented group with infinite-dimensional H_3 (an example which became the cornerstone of Bestvina-Brady's discrete Morse theory), and clarifying the work of Serre on trees, especially as it applied to Haken 3-manifolds. In 1983 he wrote a brief paper, "Topology of finite graphs", which, besides giving astonishingly short and illuminating proofs of notoriously opaque algebraic theorems, anticipated to some extent the theory of automatic groups and structures developed by Cannon, Thurston, Epstein et al. Later work was more sporadic but included such important notions as triangles of groups and angles between subgroups and continued to be guided by the interaction between (especially 3-dimensional) topology and group theory.

John is remembered as much for his anti-establishment attitudes and distinctive personality as for his mathematics (and sometimes for the interaction between the two, such as in his famous paper "How not to prove the Poincaré Conjecture"); some of this is captured in the reminiscences below. He was always a champion of the originality and brilliance of teenagers and young people, keeping in touch with many young correspondents even in his seventies and taking great delight in the music and antics of modern enfants terrible such as Eminem. Both aspects of his personality are revealed in the following excerpt from his introduction to a talk at Barry Mazur's birthday conference in 1998:

> There are some lessons to be learned from this story. One that appeals to me is that Barry broke the rules; he didn't have a Bachelor's degree, he didn't stay quietly in Princeton for three years. Even Douady broke the unspoken rule that one is supposed to wear clothes under the gown. All those rules, BA diploma, residence in Princeton, wearing shoes, being nice to chairmen and

Danny Calegari is Richard Merkin Professor of Mathematics at Caltech. His email address is dannyc@its.caltech.edu.

deans, being a team player—basically I hate those rules, even though I give in and adjust to them. It makes me feel good to see a rule-breaker succeed!

And, teenagers are creative and amazing. Barry's theorem could be compared to Opus One of some great composer. For instance, Gustav Mahler, at the age of 17, wrote a long poem, and set it to music by the age of 19. This cantata, "Das klagende Lied", is rarely performed in its original form, because Mahler "improved" it later on. I recently heard the original work in Berkeley, played by the Berkeley Symphony Orchestra under Kent Nagano, with a large chorus, six vocal soloists, two of which were from the San Francisco Girls Choir, and an offstage band from Hayward State University that played in a different key and time-signature. It had its adolescent moments, but it is truly a great work of art. Just as Barry's sphere theorem was.

Those of us who are antiestablishmentarians at heart or who met him later in his life probably remember John more for his laconic wit, his predilection for Thai food or for beer and pizza after seminars (at La Val's or Jupiter), his fondness for reading "Zits" in the morning paper (usually in Brewed Awakenings), his impossible turn of speed around about the fourth mile of a hike to Point Reyes, his long gossipy monologues while driving out to a seminar at Stanford, his provocations and his unconventional behavior (tearing up a *Bulletin* article on "Making proofs without modus ponens" with mock rage; speculating on the likelihood that Alex Gottlieb would fall off the balcony outside 1015 Evans) than for his mathematical insights, which were rare in his later years but seemed rarer still because of his modesty and high standards. The truth is, we have internalized his mathematics so that it seems less like a reflection of John and more like a fact of nature. He felt more like a fellow graduate student than one of the exalted faculty. For generations of graduate students at Princeton, Berkeley, and elsewhere, Stallings was larger than life. It is hard to believe that he is gone.

Benson Farb

John Stallings was a one-of-a-kind mathematician. Perhaps this is due to the fact that he came out of a small town in Arkansas. He loved to play up his roots, often portraying himself as a kind of naive country bumpkin. Of course, he ended up getting a Ph.D. from Princeton. His thesis gave a new topological proof of an old important theorem of Grushko in combinatorial group theory. This was the first indication of how Stallings would transform combinatorial group theory by introducing powerful topological methods.

Stallings definitely followed his own path in mathematics. He was known for his great originality; he most often came up with completely original ideas rather than following up on the ideas of others. His work often inspired a great flurry of activity by other mathematicians, who would develop Stallings's methods. There are important cases, though, including his Ph.D. thesis, where Stallings reunderstood someone else's theorem in a completely new way, which would open the door to new developments on an old topic.

Stallings made fundamental contributions to a number of different areas of mathematics, from combinatorial and geometric group theory to knot theory to the theory of 3-dimensional manifolds. Indeed, he was one of the main forces responsible for tying these areas together.

Stallings's ideas were usually remarkably simple but remarkably deep. They seemed to grow out of his incessant doodling. He doodled all the time, especially when he attended talks. One example of a simple but powerful idea is Stallings's beautiful paper "Topology of finite graphs". How many people could write a deep paper with that title? In this paper Stallings took our understanding of free groups to a new level, along the way making older, difficult theorems look almost obvious.

Stallings was greatly respected as a mathematician. One acknowledgment came in the form of his being awarded the Cole Prize for his now famous theorem on ends of groups: a group splits as an amalgamated product over a finite group if and only if it has at least two ends. Here is a paradigmatic example of how fine algebraic structure can be determined by coarse, large-scale information. No wonder this theorem is a cornerstone of geometric group theory.

Everyone loved Stallings. He was always generous with his time and with his ideas. He treated students with the same respect he did colleagues, indeed with more respect. Stallings had a dislike of authority and made a point of playing by his own rules. He often made fun of authority, but not in a mean way. He made fun of himself all the time. Actually, I can't think of anything or any person (including himself) that Stallings took seriously.

Stallings was widely known not just for his genius but for his sense of humor. As one example, he wrote an entire paper in the universal language Interlingua. One of his famous papers on one of the most famous mathematical problems, "How not to prove the Poincaré Conjecture", begins: "I have committed—the sin of falsely proving Poincaré's Conjecture. But that was in another country; and

Benson Farb is professor of mathematics at the University of Chicago. His email address is farb@math.uchicago.edu.

Photo credit: Princeton Mathematics Department, 1958.

Princeton, 1958. Left to right: H. Trotter, Chih-Han Sah, L. Neuwirth, R. Fox, C. Papakyriakopoulos, and J. Stallings.

besides, until now no one has known about it."[1]

Stallings was incredibly generous with his time. He would sit and listen to my mathematical arguments for hours, even though he was working on different things at the time. He would always push for simplicity. While I am a student of Thurston, I am proud that Stallings considered me his "bastard (mathematical) son". I have fond memories of Stallings's seminar, followed by beer at La Val's or dinner at an Indian restaurant, where Stallings would order extra spicy food and proceed to turn bright red and sweat profusely as he ate it.

When I first got to Berkeley I knew no one. I felt alone. It was difficult. Stallings befriended me and showed me great kindness. He treated me, as he did everyone, as an equal and as a mathematical equal, which was truly undeserved. We became close friends, keeping in touch until his death. Stallings was always there when I needed him, whether it be for giving me a ride at an inconvenient time or for comforting me in a time of crisis. I will miss him.

Peter Shalen

John Stallings probably had a bigger influence on my early career than any other mathematician. During my first year in graduate school, studying some of his papers on engulfing gave me my first hint of the rich combinatorial ideas that can come up in the topology of manifolds. In the summer of 1969, after my second year of graduate school, I went to the Georgia topology conference and met Edward M. Brown, from Dartmouth, who told me about Stallings's celebrated paper on the fibration theorem. When I got home I spent several months studying this paper, which packs an extraordinary amount of mathematics into four pages.

In order to understand this paper I had to look at several other sources, including Stallings's beautiful paper on the loop theorem. By the time I was done I had a sense of what 3-manifold theory was about and how closely connected to group theory it was, and I felt that this was my area of research, even though I had not yet made contributions of my own.

At the Georgia conference I also met Mauricio Guttierez, who told me about Stallings's paper on homology and lower central series. I spent a number of years thinking about applying this to 3-manifold theory before using it for the first time in my AMS *Memoir* with Jaco. I still keep finding new applications; every few months I look back at the paper and learn something new.

A little later in my graduate career, Stallings's famous paper on ends of groups appeared. By that time I was in a position to appreciate the importance of the results in the context of the mathematics that was then known, although I don't think anyone at the time realized how broad the impact of the paper on the mathematics of the future would be.

When I finally met John while I was a postdoc at Columbia, I made a mega gaffe, which as you can imagine was disconcerting after all the years I had spent admiring him from afar. I won't give details here, since this is about John and not about me, but I do want to mention John's reaction when I apologized for my gaffe on meeting him for the second time. He just smiled and nodded as if to say, well, the world is full of all kinds of nuts who say all kinds of goofy things. I think he went through life with a kind of amused tolerance for the whole benighted human race.

John was shy and had few really close friends. I never succeeded in getting close to him personally and always had to settle for admiring him from a certain distance. We never had a conversation that lasted for more than a few minutes. I think he was especially shy about talking mathematics. When I brought up connections between his work and mine, he certainly said kind things about my work but never seemed eager to pursue the discussion.

Still, his low-key humor always came through even in a brief chat. The last time I saw him was at a conference, possibly at the University of Arkansas, his alma mater. I saw him when I was on the way back from lunch and started walking with him to try to strike up a conversation. He was walking pretty fast and explained that he was trying to get a beer before the afternoon talks. I don't remember who was talking that afternoon, which is probably just as well.

Hyam Rubinstein

I arrived in Berkeley in the fall of 1970, having never been outside Australia before. It was a

[1] *This is a parody of T. S. Eliot's excerpt from Marlowe's* The Jew of Malta*: "Thou hast committed/Fornication: but that was in another country,/And besides, the wench is dead."*

Peter Shalen is professor of mathematics at the University of Illinois at Chicago and an affiliate professor at the University of Haifa. His email address is shalen@math.uic.edu.

Hyam Rubinstein is professor of mathematics at the University of Melbourne. His email address is rubin@unimelb.edu.au.

fantastic experience being a graduate student there, and I was especially fortunate to have John Stallings as my supervisor.

John had been away in 1971 on leave, and when he returned, I quickly approached him to suggest a thesis problem. I had read a number of his marvelous papers on PL topology, on 3-manifolds, and his recently published book on groups with infinitely many ends. John had a unique writing style—like a great athlete or musical performer, he made mathematics look effortless and graceful. His paper "How not to prove the Poincaré Conjecture" contains some wonderful observations on the traps of research—how one can get so enthusiastic about an idea that self-criticism and skepticism disappear and mistakes can be overlooked. It is still rather mysterious, with the solution of the Poincaré Conjecture by Perelman, that Stallings's algebraic characterization of this by splitting homomorphisms is now established. This is an issue still well worth pursuing.

I felt rather shy around John, but he was a very kind, encouraging supervisor. He suggested a very nice thesis problem arising from Laudenbach's recent paper in the *Annals of Mathematics*, "Sur les 2-spheres d'une variete de dimension 3", and suggested to me that I investigate whether the result could be extended to 2-sided projective planes in nonorientable 3-manifolds. Stallings was a pioneer in the synthesis of ideas between group theory and 3-dimensional topology. In his work on ends, he had proved a deep splitting theorem for groups which can be seen as a version of the sphere and projective plane theorems of Whitehead and Epstein. Laudenbach's result can be interpreted as uniqueness for splitting a 3-manifold fundamental group into a free product. An analogous result for Epstein's projective plane theorem would then be similar to uniqueness for Stallings splittings as an amalgamated free product over a finite group.

Stallings was very encouraging with my rather slow progress on this and was very pleased when I was able to finally come up with a good idea to solve the problem. At that stage he did not have many students, as he had been away, so I had no difficulty in getting to see him whenever I needed to.

I returned to Australia immediately after my Ph.D. Several years later, I invited John out for a visit of several weeks. This was very enjoyable—as well as talking about mathematics, a highlight was a trip to a semiarid area of western Victoria called the Little Desert National Park. Ironically, due to an unusual amount of rain, we were limited to touring the outskirts of the park, as otherwise vehicles were getting bogged! John turned out to have a great knowledge of flowers and had a magnifying glass to observe the beautiful tiny native orchids in the area. I had no idea of their existence, and this was a small instance to me of John's wide range of interests and knowledge.

Over the years I have regularly visited Berkeley, and I always looked forward to dropping in to say hello to John. He will be greatly missed by everyone who knew him.

Mahan Mj

John's style (mathematical or otherwise) was understated and deceptively simple. There was little or no show of power or machinery, and things seemed to be almost childishly simple at times. The first time I had an opportunity to see this in action was in my second week of graduate school at a weekly geometric group theory seminar he ran. He asked the speaker some silly-sounding question on the product of a 2-sphere and a circle, and the speaker said something. John didn't seem too happy about it. "Is this guy really John Stallings?" I asked myself. During the weekend I was thinking about the talk and decided to run through a part of the argument which seemed obvious but not quite clear. It was only then that things started falling in place and John's question made full sense. I felt rather stupid, and the question I had asked myself was answered. Most of the audience—and, I daresay, the speaker too—had overlooked a simple point.

Photo credit: Unknown, 2002.

The next thing I remember about John was doing a reading course with him on the Swiss notes on hyperbolic groups. Repeat performance. I was explaining an argument, and at a certain point he stopped me with "Why?" I tried to hand-wave it away, but I had known John for a while now, and while speaking, I realized there was a gap. Took me all weekend to plug it satisfactorily.

Some people never learn. I was presenting the first result I thought I had proven to John privately. At a certain point he again seemed to find something dubious and made some disconcerting noises. I raced on, and he indulgently allowed me to pass. A month later when I was writing things up, I got stuck. It took a small new idea to plug the gap this time. I went to him and told him about it. He said, "Yeah, I thought something like that would be true."

As John was so easygoing, many of us hung out with him. There were Paul Brown, Bill Grosso, and Noel Brady—his own students—and there were

Mahan Mj is at the School of Mathematical Sciences, RKM Vivekananda University. His email address is mahan.mj@gmail.com.

Photo credit: Stephen Miller, 2004.

Stallings at Brewed Awakenings, one of his favorite haunts.

his "illegitimate children"—Michah Sageev, Benson Farb, me—who, though not his Ph.D. students, nevertheless worked in near-enough areas and were part of the group. Graduate school is hard for many, being a transition from mathematical adolescence to mathematical adulthood. One does not yet have the confidence of the adults, but one has lost the innocence of mathematical childhood in the form of undergraduate study. One is excited one moment and thinking that there is no stupider mortal the next. Being around John was soothing and put things in perspective. He was to me a philosopher and a guide, true, but most of all a friend.

Anandaswarup Gadde

I first met John Stallings in early 1967, when he visited the Tata Institute of Fundamental Research to give a lecture course on polyhedral topology and I was asked to write up his lecture notes. I was a third-year research student at that time, mainly interested in algebraic topology and differential topology, learned through some unpublished notes of Samuel Eilenberg and John Milnor. I knew about John's work on the Poincaré Conjecture and that some papers of William Browder were inspired by John's work. In preparation for his visit, I worked through some papers of J. W. Alexander, J. H. C. Whitehead, E. C. Zeeman's notes from I.H.E.S, and some papers of John on engulfing. For two months I was in constant contact with John discussing his lectures, showing my notes, helping with shopping and a sightseeing trip to Lonawala. He was unassuming, and note taking turned out to be easy except for a couple of appendices, where my reading of Whitehead helped. Most of the time he gave me his own notes, and all I had to do was number the lemmas and add symbols on the stencils. It turned out to be somewhat messy, and some of the elegance of his presentation was lost in misprints. Surprisingly, at the end he offered joint authorship, which I politely declined. He also gave a seminar, which seemed very nice, until a point when he said, "By waving hands twice, we have the result," and this was probably my introduction to combinatorial group theory.... There was some correspondence soon about his lecture notes, his leaving Princeton for Berkeley, and I continued to read his papers. They always seemed very elegant, with neat ideas and easy to read, with unpleasant technical difficulties tucked away in a half page somewhere. Slowly I found that not only were his theorems useful but their generalizations too, and when these did not work, one could often go back to his techniques.

Just before Stallings left Bombay, I asked whether he could suggest some problems for my thesis. I think that I wanted to prove some embedding theorems, following his ideas in the lecture notes, about an alternate argument in the proof of *s*-cobordism theorem avoiding the Whitney trick. But all he said was that somebody named Papakyriakopoulos did some great work. I had never studied anything in 3-dimensional topology before, and after he left, I started reading "On Dehn's lemma and asphericity of knots", the first paper on 3-manifolds which I read. To round it off, I read a few more papers and got stuck in 3-manifolds for a long time. Meanwhile, possibly around the end of 1967, he sent a preprint of "Groups with infinitely many ends", which is probably my main introduction to topological group theory. Though my fascination with higher-dimensional topology continued, I found myself returning to Stallings's papers and ideas. I do not remember any papers of mine not influenced by Stallings, except possibly one on cut points which built on the impressive work of Brian Bowditch. But even here, one of Stallings's students, Bill Grosso, seemed to have many of the basic ideas but not proofs.

Meeting such a first-rate mind at an early part of my career has influenced me, and he kept an affectionate interest in me throughout my career. My acquaintance with Stallings, and miswriting his notes, seems to have made me a member of the Stallings community, and I always felt welcomed by his students and friends.

Koji Fujiwara

I met John for the first time in March 1993 at a conference in Scotland. He happened to be next door in the dorm we all inhabited on campus. I did not know him, but we became friends. I went to Berkeley in September 1993 as a postdoc in

Anandaswarup Gadde is emeritus professor of mathematics at the University of Melbourne. His email address is anandaswarupg@gmail.com

Koji Fujiwara is professor of mathematics at Tohoku University. His email address is fujiwara@math.is.tohoku.ac.jp.

differential geometry. The theme at MSRI that year was differential geometry. I attended his course and seminar at the department and met many people. For example, Sela was giving a series of talks on his work to solve a system of equations on free groups, and Perelman was in the audience. In one year and a half, when I went back to Japan, I was a topologist/geometric group theorist.

In Japan he is known for his contribution to the solution of the Poincaré Conjecture in higher dimensions, and I know he was obsessed by the three-dimensional Poincaré Conjecture his whole life. But to me, he is a hero in geometric group theory who is comparable to Dehn.

Stephen Miller

My Berkeley classmates and I felt saved by our surprise discovery of the relaxed Southerner in the Hawaiian shirt. We kept taking his classes and seminars, and found him to be such a wonderful interface for learning mathematics and what it is like to be a mathematician. John had a very loyal following among students, mainly due to the respect he gave them and his easy accessibility. He was both a front door to the subject and a window into what happens behind the scenes in a big place like Berkeley. I definitely owe him much for his insights on navigating the profession.

I am most grateful to John for our correspondence after I finished Berkeley and began graduate school at Princeton. I didn't write him immediately, but once I did, we were pen pals for the next five years or so. He remotely served as a de facto advisor of what to do during my first year. Once, after he mentioned an interesting talk on zeta functions of groups that I found intriguing, he promptly sent me TeXed lecture notes of it! I am constantly finding things he told me valuable and certainly will continue to realize further nuggets of wisdom he told me.

John was fortunate to have recognized the opportunity the VERIP program gave him to retire early in 1994 at age fifty-eight. He was still extremely active as a graduate advisor after this, yet enjoyed the freedom this gave him. He was an extremely humble man who seemed to notice everything. I once heard him speak in a large auditorium at the University of Utah, when someone asked him to put a formula higher up on the board. He said: "OK, I can put it way up there where the little feet are." And our eyes all drifted up near the ceiling, where, in fact, the chalkboard had faint paint markings of a dozen or so baby footsteps! He had such a wonderful sense of humor this way. I remember once visiting him at Berkeley with my mother and brother and getting into a conversation about bad drivers. He obtained victory by telling us he had recently passed a slow driver who was hunched over, consuming soup.

Despite John's modesty, he was not shy about sharing his ideas and promoting the subjects he was interested in and for which he was a subject builder. His 1960s papers on the 3-dimensional Poincaré Conjecture are both brilliant and hilarious at the same time, as were the many emails he sent to his pen pals across the world. He was also quite broad mathematically: the last time I saw him was at a conference we both attended at AIM on sphere packing and Poisson summation techniques. I went up to Berkeley for the day, met him at Brewed Awakenings, had lunch, and then rode back down to Palo Alto with him. He was very happy to show me how he could unlock his car by walking near it using a proximity lock in his wallet. As we crossed the San Mateo bridge, this great senior personality in mathematics announced to me that it was time to have some music. He cranked up Bon Jovi's "It's My Life". And it was: he really seemed to love his life and gave a tremendous amount to people around him, especially my generation of students. About ten years ago he said if something happened to him, you'd have to write him at `stall@math.heaven.edu` <`stall@heaven.edu`>. Remembering that quip is a nice way to remember him at a sad time like this.

Archives of the Mathematisches Forschungsinstitut Oberwolfach. © George Bergman. With permission.

Stallings at Berkeley: top, 1968; bottom, 1984.

Barry Mazur

On John Stallings's University of California homepage there are two photos of him,[1] one where he is clasping a sheaf of papers among a group of mathematicians, a picture taken during his graduate student days at Princeton; and the other a much more recent one. John asks us on his webpage to compare them.

The older Stallings in a grand white beard is engrossed in something off-camera; his interest in whatever it is sparks a questioning look. There is movement in that still photo, with the hint of a smile coming on. The younger Stallings has the

Stephen Miller is associate professor of mathematics at Rutgers University. His email address is `miller@math.rutgers.edu`.

Barry Mazur is Gerhard Gade University Professor at Harvard University. His email address is `barry.mazur.gmail.com`.

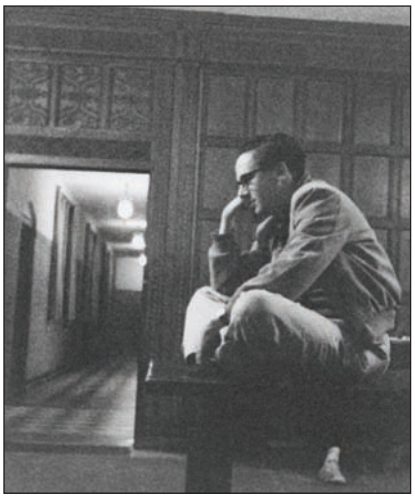
[1] *See photographs on pages 1410 and 1412 of this article.*

more composed smile I remember vividly from our graduate student days: both diffident and at ease.

I remember when we were graduate students at Princeton many occasions when John would stride into a room, intoning something or other with a kind of playful irony in his resonant Arkansas accent. Once, after thinking that he had solved something but finding a hole in his argument, he sang out these lines of Keats:

> And Joy, whose hand is ever at his lips
> Bidding adieu.

His response accomplished, in my eyes, the magical trick of converting the common experience of finding a hole in one's proof—a sure but minor downer, one would think—into a joyous testimonial of the ephemerality of joy. And this was typical, I think, of the grounded, self-ironic, and always amused way he walked through the world (with his successes or his setbacks, with his foibles and his gifts).



Photograph © Jay Goldman, 2000.

Contemplation in the old Fine Hall.

Stallings was the center of a group of graduate students (Jim Stasheff, Han Sah, and I were part of that group) hellbent on piercing the mysteries of a subject that goes nowadays under the not very glorious name of point set topology, but at the time I would often call it pure topology, where the adjective *pure* had for me, I'm now amazed to say, a moral force: all other versions of topology were, I thought, in some sense adulterated—adumbrated by crutches such as *polyhedral* or *smooth* or—heaven forbid—*complex analytic* or *algebraic* structures that would alloy—would sully—the topological essence of the spaces being studied.

I can't imagine that my companions in this group had the same puritanical view as I did, but we all shared the drive to understand what we considered to be the primal objects of topology (notably the real line, the closed interval, the circle, the disk, etc.) and to protect them from the various encroaching monsters and chimeras such as solenoids, Cantorian concoctions, impacted sine curves, and that deep sea serpent, the long line. This latter creature John would always deferentially refer to as "the long long line", alliteratively drawing out the music of those slow syllables in honor of the immensity of the referent. There was something strident in our engagement in this pursuit: we would fall over ourselves dreaming up more and more arcane criteria that distinguished, say, the classical unit interval from one of these behemoths, and when we failed we, joyously, would stomp and proclaim, "We don't even know *the unit interval*!"

But John was at home with, and comprehended, the creations of geometry that struck our fancy in a clearer, more vivid, and more original way than any of us did, whether it was an exhibit from the *cabinet of counterexample wonders* that I described above or one of the even more wonderful constructions of Alexander, Antoine, or Bing. It was an extraordinary experience to watch John talk so slowly and visualize things so fast.

Of course, in the more official world of studies, we were taking in the standard fare of graduate studies in topology, with heavy doses of spectral sequences, H-spaces, and other equipment of the epoch. And surely our professors would have looked askance—or maybe even 180 degrees away— had they known how much time we spent cataloging the ways we "didn't even know the unit interval." But I don't recall that the landscape of my graduate student life was studded, outside of courses, with very many professors. We were largely shaping our own interests—a good thing, after all, for there are ways of becoming educated that are worse than that. It was mainly fellow graduate students—John Stallings and others—along with various visitors to the university and the Institute for Advanced Study that set the tone of graduate studies for me and, vitally important, made it a joy.

John Stallings—Items from My Unknown Autobiography

In order to make my autobiography entirely truthful, I have heard things from other people that I do not remember in the same way. I would love to get suggestions from people telling me what happened in the past that are deeply meaningful. Two examples:

First, there is the baby bottle with the whiskey. As I recall it, one time in the early 1960s, I wandered down Nassau Street in Princeton and found a store that was selling baby bottles with nipples attached. I bought one. Then I took it to Fine Hall for the math tea time. What I seem to recall is that I put some hot chocolate in the bottle and then sat around and sucked on it. But what I have heard from others is that I put whiskey in the bottle and then sucked on it. I seem to recall that it was difficult to suck the hot chocolate, and so it could have happened that I rinsed it out and filled it up with whiskey. I always had a bottle of whiskey in my office in Princeton, and when a speaker came to give a topology seminar, I would try to get the speaker drunk. After all, a drunk speaker is usually more fun than a sober one. Also, there is a problem with seminars, which I call A.S.S. (attention surfeit syndrome), in which a person sits quietly even

when completely bored. This is a serious problem with education in general, and especially in serious seminars and colloquia, where some chairman or other thinks people should shut up and sit still. If the audience drinks too much, they will snore and distract the speaker; but when the speaker is drunk, it all works out pretty well, usually.

Second, there is the "Class Cancelled!" story. Early in my career, when I was starting to teach at Princeton, I got to give some seminar talks on my work on PL manifolds, involving the Engulfing Theorem and high-dimensional Poincaré Conjecture. I did not do a good job of this; there were many details, such as general position, which I did not really know how to do; the typical proof was just by hand-waving. I do recall "great" topologists in the audience, such as Milnor. And I think I called it off eventually, finally getting the proofs of some details in my Tata notes of 1967 or so. But I have heard a slightly different story, which is that I was actually assigned a graduate topology class to teach; I started out by trying to prove some basic lemmas and getting stuck. So, the next meeting of the class, I got stuck at about the same point. In the third meeting of the class, I still couldn't get the details right, and so I looked seriously at the audience and said "Class Cancelled!"—Could that have really happened? I have always believed that young faculty members in the Princeton math department only were allowed to teach those undergraduate classes that were filled with children of rich alumni, and only after many years of this was one allowed to teach a graduate course. But maybe I had the opportunity and flunked it.

Is it allowed nowadays at Princeton to suck on a baby bottle in the common room at tea time? Do seminar organizers try to get the speakers drunk nowadays? Can a teacher get disgusted with a class and cancel it? What else can we do now to get the chairman and the dean embarrassed and irritated and angry???