REMEMBERING MARK MAHOWALD
1931–2013

DOUGLAS C. RAVENEL
WITH CONTRIBUTIONS BY MARTIN C. TANGORA, STEWART B. PRIDDY,
DONALD M. DAVIS, MARK J. BEHRENS AND WEN-HSIUNG LIN
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Mark Mahowald was a dominant figure in algebraic topology for decades. After being trained as an analyst he became a self trained homotopy theorist in the early 1960s. His 1967 AMS Memoir *The metastable homotopy of* S^n, with its dozens of charts packed with arcane information, established him as a master of computations related to the homotopy groups of spheres. It came to be known in the field simply as "the red book." It was the unanimous opinion of experts that he knew this subject far better than anyone else. His insight and intuition were legendary. Countless coworkers benefited from his ideas and advice.

He was born on December 1, 1931, in Albany, Minnesota, a very small and mostly German Catholic community. He was one of seven children. His father was a family physician who ran his practice out of his home. There was an enyclopedia in the house which Mark would consult whenever he was curious about something. He was largely self taught. He also spent a lot of time on the nearby farm that his father owned. Among his six siblings, two would become academics, one a medical doctor and one a Jesuit priest.

Mark first attended college at Carnegie Tech (now Carnegie Mellon University) in Pittsburgh and planned to major in chemical engineering. After two years, knowing that his father could not afford to support him in any longer, he joined the naval ROTC so he could support himself with the four year scholarship that they offered. At the same time he transferred to the University of Minnesota and changed his major to mathematics "because I could get a degree faster that way."

Indeed he got his Bachelors, Masters and Ph.D. (1955) degrees in three successive years, all paid for by the ROTC. He decided that the optimum approach to the prelims in graduate school would be to take them as soon as he had a good chance of a minimal pass. He calculated this with such precision that after he took the exam, they told him that they had to give him a pass, but that they didn't recommend that he continue. He then wrote a thesis, *Measure in Groups*, in a few months. It was so good that the approach he used on the problem was later adopted by Bourbaki.

In July 1954 he married Zoemarie Graham. After getting his degree, he fulfilled his military obligation by serving in the U. S. Marines. He trained at Quantico and was stationed at a base in California. After two years he received a hardship discharge related to Zoe's health problems. The family then moved to Cincinnati. There he worked for General Electric and taught night classes (sometimes as many as five at once) at Xavier University.

In 1959 he was offered a more attractive position at Syracuse. He switched from analysis to algebraic topology because he found topologists more congenial to work with. Over the years he would himself become a major factor in this congeniality, helping create an exceptionally friendly community within mathematics. In his career he had 50 coauthors.

In 1963 Northwestern hired him as a full professor at the age of 31. He had been hoping to reach that milestone by 30. By then he and Zoe had five children. The university helped him finance the purchase of a house in Wilmette that was big enough for his family. Within a decade Northwestern hired four more algebraic topologists and became one of the leading centers of homotopy theory in the world. He was the prime mover in the creation of the Midwest Topology Seminar, which meets three weekends a year to this day.

He had a lifelong passion for sailing. He built himself a small sailboat as a teenager. After moving to Chicago he became involved in racing on Lake Michigan. He named three succesive sailboats *Thetajay*, a reference to the notation θ_j for certain hypothetical elements in the stable homotopy groups of spheres.

He officially retired in 1997 but remained mathematically active until his death. He died of a rare lung disease at the age of 81 on July 20, 2013. He knew he was dying in May and had time to say goodbye to all of his loved ones, but did not suffer inordinately. Upon his death he donated his body to science. A few months later the *Chicago Tribune* ran a story about how it was being used as a cadaver by students at Northwestern's medical school. Mark would have been pleased.

Recollections of students and colleagues

Tangora

When Mark Mahowald came to Northwestern in 1963, I was very discouraged. It was not at all clear how I could finish my PhD before the time limit ran out. I was in my 8th year of graduate work in 1964-65 and was sitting in on a seminar on cohomology operations, taught by a new young hire named Bob Mosher.

At the advice of the wonderful H. C. Wang, I had a talk with Bob Williams, who gave me an exercise on the Steenrod algebra, and when I killed that exercise, he and Mahowald discussed what should be done with me. That winter I tried to attend a seminar that Mahowald was giving, and in my diary I called it "the snow job of all time" and said that Mahowald was a "genius."

Meanwhile Williams had urged me to master the material in Mosher's seminar, and, desperate as I was, I did so. In May, Mahowald made an error in his seminar, about a sub-algebra of the Steenrod algebra, and I was the only one who saw it and could correct it. Around that time Peter May came to the Chicago area to talk about his thesis, and Mahowald and Williams told me that if I could find a way to use a computer to push the computations in May's thesis, I could have a thesis of my own.

The topic was perfect for me, and pretty soon I was extending the computations of Ext (by hand), and Mark was phoning me at night to ask me questions. One got the impression that Mark was always working, from before breakfast until late at night. And one quickly got the impression that he expected, or hoped for, the same diligence from everyone else. Before long, whenever we saw each other in the hallway, he would ask me, "Any new theorems?" He asked this with a smile, but it did put some pressure on. In August my diary says, "I never get any ideas – he gets so many!"

It was a very productive summer. Every day I would push Ext a little further, and then drop in on Mark in his office, where he would show me stuff that I could not hope to understand. It was to be his famous Memoir. But he was also writing our first paper on differentials in the Adams spectral sequence, and in his bibliography he cited my thesis, although I had not even begun to write it, much less know that it would be accepted.

Already in June, I had a dream, where we had gone to the University of Chicago for a seminar, and when we came out of the talk, Mark said to me, "Let me show you what I've got here." He pulled a crystal ball out of his pocket, and started to pull from it long chains of brightly colored beads and flowers.

One day in November he gave me a problem at 10 a.m., and when he saw me again at 2 p.m., he asked me, "Any progress?" In the meantime I had taught two classes and gone to lunch. This kind of friendly pressure was wonderful but harrowing.

One of the best reasons to become a mathematician is that it gives you the opportunity to come in close contact with great minds. There was a result in my thesis that I tried very hard to prove, and I thought I was being very imaginative about it, but got nowhere. As a post-doc at the University of Chicago, I got to know Saunders Mac Lane, and one day I asked him for an hour of his time, and presented the problem. He was very nice, told me that important results cannot be expected to be easy, and made several suggestions, all of which I had already tried without success. A couple of days later I went to Evanston and presented the problem to Mark. Of course he already knew about it. He threw five different approaches to the problem onto the chalkboard. I could not see how any of them were connected to the problem. Others have talked about how differently his mind worked from most mathematicians, and that's my experience of it.

Mark knew that what he considered a proof was not always accepted as such by other mathematicians. I can still hear Frank Adams' loud protests coming from behind the closed door of Mark's office when Frank was visiting.

It should be said that, while Mark Mahowald and Frank Adams were very different men and mathematicians, they did have enormous respect and admiration for each other. At the Edinburgh conference in April 2011, which was in part a celebration of Mark's work, Michael Atiyah made a comment in his talk that some took to be a suggestion as to how Adams' immortal Hopf Invariant One paper should have been written. When Mark spoke, he said, "If Adams had written Hopf Invariant One the way Michael suggests, I would never have become an algebraic topologist." In fact Mark jumped into algebraic topology by reading and mastering that paper; it was his bible. Adams (like Saunders MacLane) was very good at bringing in new ideas and then working very carefully to get them into a clear framework. Mark had a wealth of new ideas too, but clarity was not his strength. As early as 1964, young topologists were being told that they could make a good living by "explaining Mahowald."

Once Mark said, in a talk, "Is this obvious to everybody?" (pause) "It's probably true!" Often he would start to give a proof, and then founder, and look around at his puzzled audience, and then say, "Well, how about this?" and try a completely different argument. There was also a time when someone asked him about a statement that he had just made in a talk, and Mark said, "Well, that's trivial. And if you give me a minute, I'll try to think why it's trivial."

Once a colleague who tended to use lots of different alphabets on the chalkboard was giving a talk, and stopped to look in the lectern desk to see if there was any colored chalk. Mark said, "If you have colored chalk, you can use the same letter for everything!"

Mark was not strong on the Greek alphabet. He sometimes would write a squiggle on the board, and if asked, would say, "That's the universal Greek letter." (He meant either lower-case zeta or xi.) Once he asked what an upper case Xi would look like, and when someone showed him, he said, "That's perfect! That's the next best thing to not giving it a name at all."

Priddy

As a fresh Ph.D. contemplating a job interview at Northwestern University, I had some questions: "What sort of man is Mark Mahowald?" "What about his mathematics?" Answers from my mentors: "Well, he is a big man; a former Marine; a force from the North" "He is a deep mathematician but his arguments are often difficult to follow." I decided to consult his 1967 AMS Memoir. From this it was clear he was adept at computing homotopy groups of spheres, a notoriously complicated, difficult subject. It appeared that a profound but elemental force was at work here. This and all I had been told turned out to be true, in spades!

At this time, 1968, Mark was building a group in homotopy theory. In addition to Dan Kahn, soon joined by Michael Barratt and later by Eric Friedlander, there were also several young people in residence: H. Margolis, D.R. Anderson, A. Lawrence, and R. Patterson. It was an exciting time with lots of visitors from all over. Almost every algebraic topologist passing through Chicago came to lecture at the weekly seminar led by Mark who absolutely insisted on a lecture every week without fail. Hiroshi Toda of Kyoto came for a year. Watching them draw curly-cue cell diagrams was awesomely mystifying.

Mark was a leading force in the Midwest Topology Seminar, a quarterly Saturday meeting in the Chicago area with a yearly meeting at other universities in the Midwest. For a couple of decades Mark was the coordinator for this series which contributed immensely to activity in the field. It was always de rigueur to invite an outside speaker with a hot new result in topology. During those decades (1970-2000) Northwestern had a series of Emphasis Years in various subjects rotating every four years or so. There were long-term senior visitors and two junior positions reserved for the year. Because of Mark's vigorous leadership, topology got more than its share of these resources. His strategy was to keep an excellent candidate in the wings in case there was an extra slot available. He was such a dominant player in the department that he often prevailed. Associated with these years was an international conference where many of the seminal results in the field were exposed for the first time. Gunnar Carlsson described his solution to the Segal Conjecture; Haynes Miller explained his proof of the Sullivan Conjecture; the always-formidable Frank Adams gave his view of Mahowald's new infinite family of elements in the homotopy groups of spheres. All this was terribly exciting and as one of my former MIT instructors said those conferences helped put Northwestern mathematics on the map. Central to all this activity was Mark's energetic leadership.

Many of our most talented students were attracted to the depth of Mark's lectures. One was W.H. Lin whose solution to the first case of the Segal Conjecture started an extended industry engaging many others and culminating with Carlsson's work. Another was Michael Hopkins who won a Rhodes Scholarship then returned to get a second Ph.D. with Mark; it was difficult to escape Mark's spell. Young people from the University of Chicago, Paul Goerss, Mark Behrens, and others, were often found in his office. During the heyday of homotopy theory at Northwestern, Mark was one of the most important people in shaping careers. He continued to be active and influential until the last weeks of his life.

Davis

I was very fortunate to have a 40-year collaboration with Mark Mahowald, resulting in 35 published papers, with publication dates extending from 1975 to 2014. He taught me more about homotopy theory than I will ever know.

My Stanford thesis advisor, Jim Milgram, knew that Mark and I would be a good pair, and they arranged for my postdoctoral position at Northwestern in 1972-4. My thesis was related to the problem of finding the smallest Euclidean space in which real projective n-space could be immersed, and Mark was a top expert on this question.

In my first meeting with Mark, he asked me what I was working on. I explained a certain approach to the immersion problem (that never amounted to much) and he suggested a totally different approach, which I immediately began to study. It involved an inductive argument, largely numerical but with a lot of underlying topology to justify the induction. One part of it resulted in our 1975 paper ([DM75]), from which we were able to deduce many immersion results in subsequent papers. We were never able to prove the part that he really wanted, one with which he was quite obsessed over the years. The best we were able to do was in a 2008 paper ([DDGM08]); with some very hard work we found a tiny improvement on all known results. In the end, we needed a compatibility condition that he felt should be satisfied, but he could never convince me that it was provable.

This was very typical of our work. He had enormous insight, but needed people like me to pin it down.

One time I didn't do a good enough job. In a 1982 paper [DM82], we proved that there could not be a spectrum with certain cohomology groups. In the late 1990's Haynes Miller and Mike Hopkins introduced some new spectra and, together with Mark, discovered that one of them had precisely the cohomology that we had shown to be not realizable. With great effort, Mark eventually realized that the mistake in our work had come from an error in the published information about the homotopy groups of spheres. Ironically, Mark and I were then able to use this new spectrum, tmf, to obtain some new nonimmersion results for real projective spaces [BDM02].

Working with Mark was great fun. We shared a common interest in sports, which we enjoyed discussing. Unfortunately, the first time he took me sailing I got seasick, and never went again. We spent the fall of 1982 at University of Warwick with our wives. I bought a car, and we frequently took the Mahowalds on weekend road trips. This and a 1990 stay together in Oxford were great experiences for me, thanks to Mark. In each case, he was invited and then managed to extend the invitation to include me.

I visited Northwestern many times to work with Mark, usually staying with him and his wife Zoe. I very much appreciated their hospitality. I also spent 1978-9 as Visiting Associate Professor at Northwestern, with my family. That was possibly the coldest snowiest winter ever in Chicago. But a new 20-inch snowfall on top of 20 that had already fallen did not deter Mark from driving me and the rest of the NU topology group to the Midwest Topology Seminar at University of Chicago. We were about the only car on the road. This was Mark; nothing could stop him.

Behrens

I first met Mark Mahowald as a prospective graduate student visiting Northwestern University. As an undergraduate I had been studying homotopy theory with Norihiko Minami, a graduate from the Northwestern school of topology, and Mahowald met with me for a full hour to talk about his current research. I was immediately bewildered with some weird mixture of the Steenrod Algebra, certain polynomial equations over $\mathbb{F}_4$, and the random appearance of the group $SL_2(\mathbb{F}_3)$. Frankly, I had *no idea* what was going on, but courteously pretended that I did.

Only later did I discover that he was trying to tell me about the supersingular elliptic curve at the prime 2, and his latest gadget, Topological Modular Forms (*tmf* for short, but back then it was called eo_2). I remember as a first year graduate student at Peter May's 60th birthday conference joking with my fellow graduate students about the prospects of "eo_2 resolutions" after hearing a similarly incomprehensible talk Mahowald gave at the conference. Little did I know that in a few short years I would be hanging onto every word that this man said, and shaping my career around the stuff he was talking about.

Indeed, I was bitten by the computation bug the following year, and my advisor, Peter May, suggested I go up to Northwestern to talk with Mahowald about possible thesis projects. It was not long until I was commuting up to Northwestern every week to talk to Mark to learn about his unique perspectives on the stable homotopy groups of spheres. Amazingly he would devote his entire afternoons to talking with me. Talking with Mark was like learning a language by the immersion technique. At first, I had no idea what he was trying to tell me. Each week, I would ask him the same questions, and he would politely pretend I didn't ask him the same question the week before, and give a different explanation, until one took. His vision, passion, and mathematics will dominate my own work for the rest of my life.

I would later learn that he regularly took graduate students and postdocs under his gentle wing in this manner, initiating them in an oral tradition of homotopy theory that cannot be learned from any paper or book. Mark was generous with his ideas, and would freely hand off amazing theorems to young mathematicians whose task was to provide a proper proof. Mark was a kind and thoughtful person (a friend of mine once told me that as a graduate student she was devastated by a critical referee's report that she was certain Mark had wrote — soon afterwards her spirits were uplifted by an invitation by Mark to come speak at Northwestern).

Mark's mathematics is best described as non-traditional. Arrows in his diagrams often lacked heads, for he was inevitably going to get the direction wrong. Once when he was talking about duality, I noticed he subconsciously started to write "*od*" instead of "*bo*." Rob Thompson recently told me that Mark said he wasn't concerned that the *bo*-based Adams spectral sequence didn't have an E_2-term expressible as an *Ext* group, because he "never understood homological algebra anyways." Indeed, for Mark everything could be thought of as a CW complex, and drawn with his idiosyncratic "cell diagrams."

Mark's talks were always difficult for folks to understand. A geometer once told me about a talk Mark gave which began "In this talk, all spaces are mod 2..." He told me he didn't understand this, or anything which followed. His writing was equally difficult to grasp at times. Perhaps he was gifted with an extraordinary vision and cursed with an inability to put it into words. I, however, believe that there are no words adequate to describe his incredible world-view of computational homotopy theory.

For Mark, mathematics was not so much about the big theorem as understanding the bigger picture. The homotopy groups of spheres were full of *mysteries*, and he was the oracle. His standards of proof were different than is typical in our field — at times it felt he was more like a physicist with brilliant intuition than an uptight mathematician bound by the restraints of rigor. Mark told me that the Kervaire invariant elements were "simultaneously v_2-periodic, and v_n-periodic for all n" – a paradoxical situation that somehow strengthened Mark's hope to prove they all existed, but as Hill-Hopkins-Ravenel later discovered, forced their eventual nonexistance. For Mark, a theorem was proven by understanding *why* it was true, by placing it in a philosophical setting where its truth was so self-evident, he could come up with a half dozen different arguments for its validity. He always told me that when doing a computation, arrive at the answer through two independent means, so as to make sure you don't make a mistake. I think Adams summed up Mahowald's enigmatic brilliance the best in his review of his paper "The primary v_2-family":

> This is mathematics as it is lived; some of it seems like work in progress.
> May it continue to progress.

LIN

A substantial portion of Mark Mahowald's work in his career as an algebraic topologist is on the connective K-theory *bo* and its applications in homotopy. Involved in this theory is the subalgebra $\mathcal{A}_1$ of the Steenrod algebra $\mathcal{A}$ generated by Sq^1 and Sq^2. In the early 1970's he gave me a supposed-to-be Ph. D. thesis problem which is to study a similar theory associated with the next more complicated subalgebra $\mathcal{A}_2$ of $\mathcal{A}$ generated Sq^1, Sq^2 and Sq^4. What Mark had in mind then is now known as "tmf" worked out by him and M. Hopkins 30 years later. I didn't succeed in this problem of course. Instead, I got my Ph. D. by proving a small result about the Ext groups of $\mathcal{A}_1$ over $\mathcal{A}_2$ which Mark found useful in some other work of his.

Conceiving a preliminary idea of proving a conjecture of Mahowald on the stunted real projective space $\mathrm{P}^{\infty}_{-\infty}$ I returned to Northwestern University (**NU**) in the year 1978-79 to pursue further research with some financial support arranged by Mark. This conjecture later became Lin's theorem. A more easier-to-understand proof, primarily due to Mahowald himself and Don Davis, replaces my original proof of the theorem in publication. This theorem is the simplest case of Segal conjecture. I think Mark's deep observation behind his conjecture on $\mathrm{P}^{\infty}_{-\infty}$ together with his 1976 paper (next paragraph) are the main impetus to the solution of Segal conjecture.

In 1976 Mahowald published a very brilliant paper, the main result being the existence of the η_j family in the stable homotopy groups of spheres π^s_*. The techniques and results in this paper inspire some of younger generations of topologists and make them famous. To say a few, the solutions of "immersion conjecture", "elementary groups cases of Segal conjecture" and "Sullivan conjecture", all connected, directly or indirectly, to the η_j paper in some way. Brown-Gitler spectra are the main ingredient in this paper. Using these spectra and his techniques I proved some other similar results in π^s_* including a paper showing that the Whitehead squares $[\iota_{2^i-1}, \iota_{2^i-1}]$ are projective.

I have been returning to **NU** many times, short terms or long terms, to work with him with intensive frequences from 2003 to 2011. Almost every summer in these years I visited him for a week or a month. The main topics are some of unpublished work of mine about quadratic constructions. Part of our discussions was about the Kervaire invariant problem θ_i which has been intriguing to him for a long time. It is known now that θ_i is non-existent for each $i \geq 7$ by Hill, Hopkins and Ravenel. I remember he once communicated with me sometime before 2008 that he believed θ_6 should exist.

Mark loves to do research in mathematics, especially in algebraic topology. When he visited Tsing Hua University in Taiwan in 1984 I asked him a question: "What would you do if you were a college student again?" He said: "I would go to Princeton to study algebraic topology with Norman Steenrod." Besides math he has some other interests. He is very knowlodgeable of U. S. interstate highway network and U. S. geography. He also likes to watch TV movies. He once told me that he liked to watch the movie "Midway" about WWII between Japan and U. S. in which one of the big movie stars graduated from **NU**. He also knew that a famous female Hollywood movie star came from his home town of Wilmette.

RAVENEL

Mark Mahowald was an inspirational mathematician. I met him in the mid 70s, early in my career. He was hugely encouraging and made me feel like I had just met a rich uncle I never knew I had. Like everyone else, I was impressed by the depth of his intuition and insight. I got the impression he could do the Adams spectral sequence in his sleep.

If you do not know what the Adams spectral sequence is, you will not learn it here. Suffice it to say it is an exquisite mystery that hard core homotopy theorists like Mahowald have a long term love hate relationship with. He could draw charts of it with abandon, explaining patterns known only to him like they were mechanical devices. For him the homotopy groups of spheres were familiar tangible objects, like the furniture in his house.

In 1999 there was a homotopy theory meeting at Oberwolfach which, to my lasting regret, I missed. There was an evening lecture by a young German named Christian Nassau. He had a new computer algorithm for producing charts of the Adams spectral sequence. The talk was technical and possibly boring until the very end.

Then he produced a spectacular slide showing much more of the spectral sequence than anyone had ever seen before. Mahowald jumped out of his chair and ran to the front of the room, talking about newly revealed patterns in the picture and what they meant. It was as if he had been waiting his whole life to see that picture.

Talking to Mark was like riding a comet, a transporting but often bewildering experience. I heard many people say they had a five minute conversation with him that was very profound, and spent the rest of the day trying to figure out what he said.

One of my favorite recollections is of a discussion we had at a conference at Northwestern in 1977. He was very excited about some things he had recently discovered. I could almost follow what he was saying, but it was not easy. I swear to God, he used the term "Hopf invariant" twice in the same sentence *with two different meanings!* When the conference ended, it was Haynes Miller's job to drive me to the airport. Mark gave him directions that involved virtual streets, but I did make my plane.

Listening to him lecture was even more bewildering. He had wonderful things to say, but nearly every line of it was garbled in some way, the terminology not quite right or the arrows going in the wrong direction. Polished elegance was not his style.

Some Mahowaldisms

- Advice on traveling to Europe: "Pack two suitcases very carefully and leave one of them at home."
- When he was department chair, he was asked by the staff if they could throw out some old documents. His answer: "You can throw out anything you want, provided you make a copy of it first."
- When discussing a certain finite abelian 2-group in algebraic topology, he said it was a vector space. When told that it had elements of order 4, he said, "that's what I meant, a $\mathbf{Z}/4$-vector space."
- Referring to the notation in Toda's book on the homotopy groups of spheres, written just before Mark got into the game: "One should call these elements by their first names."
- "The element η is not in the Image of J for personal reasons."

His relationship with Adams

Mark's contemporary Frank Adams was, up until his death in 1989, the preeminent algebraic topologist in the world. His style was quite different from Mahowald's. Unlike Mark, he would never discuss his intuition, only finished mathematics. They knew and respected each other and had a cordial relationship. However there was a certain wariness on both sides that was fun to watch. Adams, who had his own unique sense of humor, wrote a poem about Mahowald.

The school at Northwestern
    is as fertile as manure,
full of deep insights,
    some rather obscure.

Mark loves those damn thetas
    like a sister or brother,
and if you don't like one proof
    he'll give you another.

Mark as a friend

While his mathematics was overwhelming, Mahowald's personality was quite gentle. There was not a trace of arrogance or intimidation. He was easy going, unassuming and very friendly. He never held a grudge. It was impossible not to like him. His 60th birthday celebration in 1991 had the best mathematical roast I have ever been to. There were dozens of stories about his quirky behavior, each told with great affection.

He was a major reason why it is fun to be an algebraic topologist. The subject is not the same without him.

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