CELEBRATIO MATHEMATICA

Saunders Mac Lane

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This brief biography is based mostly on Mac Lane's own *A mathematical autobiography* [2005], a manuscript "mostly completed" by Saunders himself, but "extensively developed" by Janet Beissenger in conversations with Saunders in his last years, as well as some other mathematicians (see [2005], acknowledgments). It is also written from the perspective of a topologist; for a different view see the excellent articles by Colin McLarty on Saunders' philosophic work [McLarty 2005; 2006; 2007]. I have added relatively little, based on my limited personal knowledge of Saunders and on some information from other mathematicians, e.g., David Eisenbud. The latter wrote a preface to the autobiography which is reproduced in this volume.

The McLean clan came from the Highlands of Scotland, near Castle Duart overlooking the Straits of Mull. The clan was defeated by the British in 1746 in the Battle of Culloden (the last pitched battle fought on British soil), and eventually Saunders' ancestors came to western Pennsylvania and Ohio in the early 1800s. Saunders' grandfather, William Ward McLane, born in 1846, became a Presbyterian minister, and then was charged with heresy due to preaching about Charles Darwin. He escaped to New Haven, Connecticut, and became pastor of a Presbyterian church.

Saunders' father, Donald McLane, born 1882, studied at Yale and the Union Theological Seminary in New York, and became a Congregationalist minister. He married Winifred Saunders in 1908, and Leslie Saunders MacLane was born on 4 August 1909.

His first name, Leslie, was quickly dropped and, from one month of age, Saunders had only two names. McLean had morphed into MacLane a generation earlier in order to not sound Irish, and MacLane got a space, Mac Lane, when Saunders' wife Dorothy, who typed his PhD thesis and later papers, found it easier to type Mac Lane with a space.

Except for a few years in Boston around age seven, Saunders grew up in small towns, which he preferred. He started high school in Utica, New York, where his father fell ill (possibly due to the influenza epidemic of 1919), and eventually died when Saunders was 14. The family then lived with Saunders' widowed grandfather where Saunders got to know his uncles, one of whom decided to send Saunders to Yale and fund him at the princely amount of $1,200 per year. (Incidentally, I had a scholarship for the same $1,200 per year, nearly 30 years later at the University of Chicago, which still covered most of my expenses.)

Entering Yale in 1926, Saunders thought about his financial future and figured he needed to save $100,000, providing $4,000 per year for old age — a sufficient sum, he thought. He wanted an intellectual career, and chose chemistry for its uses in industry. But then he inquired about a career in math and was told that one could become an actuary, so he switched his major to math. A career in mathematical research had not yet occurred to him.

A young Yale professor, Filmer Northrop, who had just studied with Alfred North Whitehead at Harvard, brought Russell and Whitehead's three-volume *Principia Mathematica* to Saunders' attention. He read and annotated the first volume, and a life-long interest in logic, foundations and some philosophy was born.

Saunders was still of a mind to acquire knowledge, not to create it. But in his senior year, Øystein Ore came to Yale bringing notions from Emmy Noether's school of abstract algebra in Göttingen. Ore's lectures and the reading of Otto Haupt's very abstract text in algebra, led Saunders to shift to the world of discovering knowledge, as opposed to just acquiring knowledge that others had discovered.

Saunders had been an excellent student at Yale, apparently having the highest GPA ever. He received an award at an event in 1929 at which Robert Maynard Hutchins, just appointed President of the University of Chicago in 1929 (at the age of 30 after being Dean at the Yale Law School for two years), also received an award. When Hutchins met Saunders, he promptly recruited him to start graduate school at Chicago with a $1,000 fellowship. Ore was not pleased to hear that Saunders chose Chicago, pointing out that Harvard or Princeton (or presumably Yale) were better choices than Chicago.

Nonetheless, Saunders went to Chicago, discovering upon arrival that he had not been admitted because Hutchins had not informed the math department of his decision. But admission was quickly arranged.

Saunders was disappointed in his first year at Chicago, where he perceived too much emphasis on pushing students to complete a thesis as a one-time piece of research before going out in the real world, rather than as a preparation for future research.

However there were positive aspects to his year at Chicago: study with E. H. Moore leading to a seminar talk on the Zermelo–Fraenkel axioms for set theory; reading the most accessible available source for topology, Veblen's *Analysis situs*, where he learned about Betti numbers, but not that there were concepts like homology groups; a Master's thesis in abstract algebra where he tried to axiomatize exponentials; and most importantly, meeting Dorothy Jones whom he later married in 1933. Saunders saw little opportunity to write a PhD thesis in logic at Chicago, so he applied for, and received, a fellowship to study in Göttingen.

Saunders spent the academic years 1931–32 and 1932–33 at the University of Göttingen. He experienced Göttingen in its full glory, and then in its dénouement in 1933.

David Hilbert, although retired, lectured once a week during term; Edmund Landau gave careful, precise, but unmotivated lectures on Dirichlet series; Emmy Noether influenced Saunders greatly with her axiomatic view of algebra which fitted his interested in logic and foundations; Paul Bernays became Saunders's thesis adviser; Richard Courant was the administrative head of the Mathematics Institute; Herglotz held the chair in applied mathematics.

In addition to these tenured mathematicians there were many assistants and graduate students such as Hans Lewy, Franz Rellich, Ernst Witt, Oswald Teichmüller and Fritz John.

The Nazis came to power in February 1933, and Hitler became Chancellor. On 7 April 1933, a law was passed (Law for the Restoration of the Professional Civil Service) that all Jewish faculty members at universities (all state supported) be dismissed immediately, except for a few with either 25 years of service or who had fought in the German army in World War I.

Courant was immediately fired. Landau, having served in World War I, was spared, but left within months after his classes were boycotted by Nazi students. Noether went to Bryn Mawr College. Bernays went to Eidgenössische Technische Hochschule in Zürich. Weyl left for the Institute for Advanced Study (he had turned them down a year earlier but they renewed the offer). Levy and many younger mathematicians left. The only full professor remaining was Herglotz.

This is Saunders' description of this dénouement:

> Prior to 1933, the atmosphere at Göttingen crackled with enthusiasm for mathematical ideas and discoveries. It was a remarkable exhibit of the university as an amalgam of research, teaching, and inspiration. But the glory of mathematics at Göttingen came to an abrupt end when Hitler came to power. The Nazi government wrecked what had been the most prominent and influential center of mathematics in the world.

Saunders had another year on his Humboldt fellowship, but now rushed to finish his thesis. Bernays, his real adviser, had left so it was Weyl who had to approve the thesis. He did so, but gave it the lowest passing grade. Two days after Saunders had passed (very well) his major and two minor doctoral exams, he and Dorothy were wed in a quiet ceremony at City Hall (21 July 1933). (Coincidentally, at City Hall they met Fritz John and his non-Jewish girlfriend who were getting married hastily before such marriages were outlawed; Saunders and Dorothy were witnesses and were sworn to secrecy, and the Johns then left and traveled separately to New York.)

After Göttingen, Saunders bounced around, first at Yale as a postdoc (1933–34), then Harvard (1934–36), Cornell (1936–37), and finally Chicago (1937–38). During those years, Saunders' research seems, retrospectively, to be somewhat unfocused. There was some logic; some algebra, partly under the influence of Øystein Ore at Yale; and some topology. The latter had begun with the study of Hausdorff's book *Mengenlehre* as a Yale undergraduate, then, with the study of *Analysis situs* at Chicago, next with Hassler Whitney (who was working on the 4-color problem and graphs) at Harvard, and then with V. W. Adkisson at the University of Arkansas (where Saunders and Dorothy were visiting her relatives). Adkisson was a student of J. R. Kline, and with Saunders studied whether a homeomorphism of a graph G in $\mathbb{S}^2$ could be extended to a homeomorphism of $\mathbb{S}^2$. These bits of topology were a far cry from Saunders' later work in topology with Eilenberg, but they served as a warm-up.

Saunders' daughter Gretchen was born during his year at Chicago. Then the lure of an assistant professorship brought the family to Harvard in 1938. Right away Saunders and Garrett Birkhoff began offering an undergraduate course in algebra; Birkhoff's course emphasized algebras, lattices and groups, Saunders' course emphasized group theory, Galois theory and an axiomatic treatment of vector spaces. Soon they combined their notes, and in 1941 *Survey of modern algebra* was published. It was the first American text to use the ideas of Emmy Noether, and eventually spread beyond the elite departments.

During his Harvard years, Saunders spent some time studying group extensions and crossed-product algebras. In 1941, he gave the Ziwet lectures at the University of Michigan, speaking about group extensions. In the audience was Samuel Eilenberg, and in conversation they realized that group extensions were related to the Steenrod homology of the p-adic solenoid; thus had their famous collaboration begun, resulting eventually in 15 joint papers.

The war years intervened. Saunders' second daughter, Cynthia, arrived in 1941. Saunders spent time at Columbia both working on war-related applied math problems, and talking with Eilenberg who was now at Columbia.

They introduced the notion of *category* in a paper titled "General theory of natural equivalences" [Eilenberg and MacLane 1945]. Saunders referred to it later as "off beat" and "far out," and when I was a student at Chicago the subject was sometimes referred to as "generalized abstract nonsense" by both its aficionados and detractors. The original idea of categories with objects and morphisms, and natural transformations between them, has blossomed almost beyond recognition. But it was their papers on cohomology of groups and Eilenberg–MacLane spaces that made their reputations, at least among topologists.

Here is a quote from Alex Heller writing about Eilenberg in the *Notices of the AMS* [Bass et al. 1998; 2000]:

> With Mac Lane he developed the theory of cohomology of groups, thus providing a proper setting for the remarkable theorem of Hopf on the homology of highly connected spaces. This led them to the study of the Eilenberg–Mac Lane spaces and thus to a deeper understanding of the relations between homotopy and homology. Their most fateful invention perhaps was that of category theory, responding, no doubt, to the exigencies of algebraic topology, but destined to radiate across most of mathematics.

And here [Mac Lane 2002] is Saunders himself writing about the genesis of his collaboration with Eilenberg. The setting is 1941 at the University of Michigan, where Saunders is giving the Ziwet Lectures.

> At that time, I had been fascinated with the description of group extensions and the corresponding crossed product algebras, which had entered into my research with O. F. G. Schilling on class field theory. So group extensions became the topic of my Ziwet lectures. I set out the description of a group extension by means of factor sets and computed the group of such extensions for the case of an interesting abelian factor group defined for any prime p and given be generators a_n with $pa_{n+1} = a_n$ for all n. When I presented this result in my lecture, Sammy immediately pointed out that I had found Steenrod's calculation of the homology group of the p-adic solenoid. This solenoid, already studied by Sammy in Poland, can be described thus: Inside a torus T_1, wind another torus T_2 p-times, then another torus T_3 p-times inside T_2, and so on. What is the homology of the final intersection? Sammy observed the the Ext group I had calculated gave exactly Steenrod's calculation of the homology of the solenoid! The coincidence was highly mysterious. Why in the world did a group of abelian group extensions come up in homology? We stayed up all night trying to find out "why." Sammy wanted to get to the bottom of this coincidence.
>
> It finally turned out that the answer involved the relation between the (integral) homology groups $H_n(X)$ of a space X with the cohomology groups $H^n(X, G)$ of the same space, with

coefficients in an abelian group G. It was then known that there was an isomorphism Θ,

$$\Theta : H^n(X, G) \to \mathrm{Hom}(H_n(X), G),$$

where the right hand group is that of all homomorphisms of $H_n(X)$ into G. But we found that this map Θ had a kernel which was exactly my group of abelian group extensions, $\mathrm{Ext}(H_{n-1}(X), G)$. In other words, we found and described a short exact sequence

$$0 \to \mathrm{Ext}(H_{n-1}(X), G) \to H^n(X, G) \to \mathrm{Hom}(H_n(X), G) \to 0.$$

In effect, this "determines" the cohomology groups in terms of the integral homology groups, and this explains why the algebraically introduced groups Ext have a topological use. This exact sequence is now known as the "universal coefficient theorem."

The definition and construction of what became known as Eilenberg–Mac Lane spaces, $K(G, n)$, occurred in the mid 1940s. The homotopy groups of a topological space X, $\pi_n(X)$, are defined by the homotopy classes of continuous maps of the n-sphere into X, a much simpler definition than that of homology groups $H_n(X; \mathbb{Z})$ (which nonetheless had been defined much earlier in the work of Poincaré and were easier to compute). The fundamental group $\pi_1(X)$ may be non-Abelian, but the higher homotopy groups were Abelian. Hopf showed that $\pi_3(\mathbb{S}^2) = \mathbb{Z}$ in contrast to $H_3(\mathbb{S}^2, \mathbb{Z}) = 0$. So homotopy groups were different than homology groups and, as it turned out, were much harder to compute. To this day not all of the higher homotopy groups of even the 2-sphere are known.

Eilenberg and Mac Lane stepped in and constructed, for any finitely presented Abelian group G, a space $K(G, n)$ whose n-th homotopy group is G and all other homotopy groups are zero. These spaces form essential building blocks for more complicated spaces. There are only a few simple examples, namely, $K(Z, 1) = \mathbb{S}^1$, the circle; $K(\mathbb{Z}/2, 1) = \mathbb{R}P^\infty$, the infinite-dimensional real projective space; and $K(\mathbb{Z}, 2) = \mathbb{C}P^\infty$, the infinite-dimensional complex projective space. Other examples can be obtained from the Dold–Thom theorem: if the only non-zero homology of X is G in dimension n, then the infinite symmetric product of X is a $K(G, n)$; for example, take $X = \mathbb{S}^n$.

Saunders spent 1938–47 at Harvard (Irving Kaplansky and Roger Lyndon were the best known of his Harvard students), but then was lured back to Chicago in 1949 by Hutchins (who remained Chancellor until 1952) and the new chair Marshall Harvey Stone. He spent the intervening year touring Europe, and in particular talking with J. H. C. Whitehead in Oxford. They sorted out the first case of what later became Postnikov systems, defining $k^3 \in H^3(\pi_1, \pi_2)$.

Saunders stayed at Chicago for the rest of his mathematical life. He has often recounted the *Stone Age* [Mac Lane 1989] at Chicago (1947–59), when Chicago was arguably the best department in the world. The senior faculty were Adrian Albert, S.-S. Chern, Stone, André Weil, Antoni Zygmund and of course Mac Lane; the junior faculty included Paul Halmos, Kaplansky, Irving Segal and Edwin Spanier, plus many eventual stars among the graduate students.

Stone retired, and it all came apart in the late 1950s when Weil went to the Institute for Advanced Study, Chern and Spanier opted for Berkeley in 1958, and Halmos and Segal left. Still, it had been a great time, and many of the stars remained.

I was an undergraduate and a graduate student at Chicago (1954–1964), but I was pretty much oblivious to the great mathematicians of the Stone Age, preferring to concentrate on lesser games than math. But I dimly recall Saunders as a lively teacher. While teaching an algebra course, probably in the winter quarter of 1958, a new grad student entered the class in about the fifth week, and promptly started correcting Saunders and engaging him in class. It was Robert Solovay.

Another time in 1959, Saunders announced with obvious pleasure that his good friend Henry Whitehead would give a guest lecture, and he said with a grin that J. H. C. stood for "Jesus, he's confusing," or sometimes "Jesus, he's crazy." This was the same time that Whitehead was visiting Michigan and heard of Mort Brown's proof of the topological Schoenflies conjecture.

Saunders was an ebullient man, and often spoke and lectured with a not-quite-suppressed smile. As I look at the photo from the cover of his book, I see cheeks which have smiled and laughed a lot; at least that's how I knew him.

I got a teaching job at Roosevelt University in 1960, mainly on the basis of a letter of recommendation from Saunders, and this supported me for the next four years. Later in spring 1962 when I finally passed my PhD qualifying exam, I asked Saunders about working with him. He asked what I was interested in, and I mentioned group theory, homological algebra, and topological manifolds. He wisely told me to spend the summer thinking about these topics and then talk with him when I returned in the fall. I only thought about topological manifolds, and thus never became Saunders' student (perhaps fortunately for me, as Saunders expected his students to come by once a week to talk about what they had accomplished, a requirement beyond me).

At one point he heard I was working on the Annulus Conjecture, and he said that it was a rather hard problem for a PhD thesis. (It was, and it only paid off several years later.)

These small interactions with Saunders were nonetheless important to me, and I always felt warmly towards him.

In 1949 at the relatively early age of 39, Saunders was elected to the National Academy of Sciences. His most influential work was that with Eilenberg on group extensions, cohomology of groups and Eilenberg–Mac Lane spaces and, maybe, category theory. Yet Eilenberg was not elected for another 10 years; it may be that it was Saunders' abilities outside research (plus being highly accepted in the "old boys" network that still existed in those days), that helped account for his early election.

In 1959 Saunders became chair of the editorial board of the *Proceedings of the National Academy of Sciences*, and served for eight years. At that time, *PNAS* had no system of refereeing, and accepted nearly all papers submitted by members or communicated by members. As might be expected, the quality was mixed. Until that time, math papers formed the largest proportion of *PNAS*, again with mixed quality.

Since those days the right of a member to publish or communicate became an embarrassment, and this right has gradually been whittled away. Members still retain a small edge, but since I joined the editorial board in 2002, that edge has dwindled and will surely disappear. The argument on behalf of an extra privilege for an Academy member is that a member may author or see a paper, rejected by the mainstream, which the member believes is fresh and original and therefore needs the *PNAS* imprimatur to gain the attention of the world. It's not a good argument in math, and doubtful elsewhere.

While Saunders was chair, the treasurer of NAS, worried about the finances of *PNAS*, instituted page charges. Without big grants, mathematicians stopped publishing in *PNAS*, nearly disappearing from its pages. These days *PNAS* will not charge mathematicians, and is trying hard to increase the representation of math in its pages.

Saunders was elected President of the AMS in 1972, at a time of some tumult due to Viet Nam and the emerging women's movement in mathematics. Chicago had always admitted women and had produced quite a few female PhDs over the years, a fact that Saunders seemed to be proud of. However I recall that he showed up at a meeting of the Council of the AMS wearing a tie with a bunch of little white piglets and a bunch of MCP's (male chauvinist pig). I'd guess he did it to tease the more ardent feminists, but I'm not sure it went over well. About the same time he nominated and helped elect Julia Robinson to the NAS, the first female member in mathematics (she shortly went on to be elected President of the AMS, an honor which at that time was traditionally reserved for members of the NAS). I believe that he also helped get Stephen Kleene elected at the same time, for Kleene had had a long and distinguished career in logic, and was equally deserving of being elected.

While President, Saunders encouraged the AMS to be more proactive in government affairs, establishing with SIAM and the MAA the Joint Projects Board in Mathematics to influence public policy.

Saunders continued to be active in mathematics and mathematical governance nearly to the end of his life. I recall him lecturing with vigor on something categorical in the opening term of the Newton Institute in Cambridge in 1992.

Saunders wife Dorothy had been infected with a variant of encephalitis during the war, and, as it progressed along with Parkinson's and arthritis, it became more and more difficult for them to travel together. They celebrated their 50th wedding anniversary in 1983, and on 3 February 1985 Dorothy died. Saunders writes very warmly of Dorothy and their marriage. Saunders remarried in 1986, to Osa Skotting (ex-wife of Irving Segal), and they enjoyed his remaining years together.

Saunders had a fondness for clever verse, often about mathematics or mathematicians, and the best way to end this essay is with some examples that he often quoted or sang.

Where are the zeros of zeta of s?

(Sung to the tune of "Sweet Betsy from Pike"; words by Tom Apostol.)

> Where are the zeros of zeta of s?
> G. F. B. Riemann has made a good guess,
> They're all on the critical line, said he,
> And their density's one over 2pi log t.
>
> This statement of Riemann's has been like trigger
> And many good men, with vim and with vigor,
> Have attempted to find, with mathematical rigor,
> What happens to zeta as mod t gets bigger.

The efforts of Landau and Bohr and Cramer,
And Littlewood, Hardy and Titchmarsh are there,
In spite of their efforts and skill and finesse,
(In) locating the zeros there's been no success.

In 1914 G. H. Hardy did find,
An infinite number that lay on the line,
His theorem however won't rule out the case,
There might be a zero at some other place.

Let P be the function pi minus li,
The order of P is not known for x high,
If square root of x times $\log x$ we could show,
Then Riemann's conjecture would surely be so.
Related to this is another enigma,
Concerning the Lindelöf function mu(sigma)
Which measures the growth in the critical strip,
On the number of zeros it gives us a grip.

But nobody knows how this function behaves,
Convexity tells us it can have no waves,
Lindelöf said that the shape of its graph,
Is constant when sigma is more than one-half.

Oh, where are the zeros of zeta of s?
We must know exactly, we cannot just guess,
In order to strengthen the prime number theorem,
The integral's contour must not get too near 'em.

Upon hearing of the zeta function song, Saunders composed lyrics (to the same music) for

Simple groups

What are the orders of all simple groups?
I speak of the honest ones, not of the loops
It seems that old Burnside the orders has guessed
Except for the cyclic ones, even the rest.

Groups made up with the permutes will produce some more
For A_n is simple if n exceeds 4
There is Sir Mathieu who came into view
Exhibiting groups of an order quite new.

Still others have come on to study this thing
Of Artin and Chevalley now we shall sing
With matrices finite they made quite a list
The question is: Could there be others they've missed?

Suzuki and Ree then maintained it's the case
That these methods had not reached the end of the chase
They wrote down some matrices just four by four
That made up a simple group; why not make more?

And then came the opus of Thompson and Feit
Which shed on the problem remarkable light
A group when the order won't factor by two
Is cyclic or solvable. That's what is true.

Suzuki and Ree had caused eyebrows to raise,
But the theoreticians they just couldn't faze.
Their groups weren't new; if you added a twist,
You could get them from old ones with a flick of the wrist.

Still some hardy souls felt a thorn in their side,
For the five groups of Mathieu all reason defied;
Not A_n, not twisted, and not Chevalley,
They called them sporadic and filed them away.

Are Mathieu groups creatures of Heaven or Hell?
Zvonimir Janko determined to tell.
He found out what nobody wanted to know:
The masters had missed 1 7 5 5 6 0.

The floodgates were opened, new groups were the rage,
And twelve or more sprouted to greet the new age;
By Janko, and Conway, and Fischer, and Held,
McLaughlin, Suzuki and Higman and Sims.

You probably noticed the last lines don't rhyme.
Well, that is quite simply a sign of the time;
There's chaos, nor order, among simple groups,
And maybe we'd better go back to the loops.

And two more:

Here's to Marston, Mickey Morse,
A man experienced in divorce.
His opinion of himself, we charge
Like nose and book is in the large.

Here's to Lefschetz, Solomon L.,
Irrepressible as hell.
When he's at last beneath the sod,
He'll then begin to heckle God.

Finally, Steve Awodey [2007] wrote the following ditty to commemorate the verses that Saunders enjoyed:

To Saunders Mac Lane, of the plaid sport coat,
with his thingamajigs and the Homs he wrote,
in mathematics he stood alone,
in a category of his own!

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