CONVERSATION

Interview with Hyman Bass

Hyman Bass, Lisa Carbone, and Yvonne Lai

Hyman Bass's remarkable and vibrant mathematical life has spanned seven decades. His research centered on algebra and connections to related areas. In the 1990s, he began to investigate mathematical knowledge for teaching. He has supervised 25 mathematics PhD students, with 174 mathematical descendants, and three mathematics education PhD students. Hyman was an inaugural Fellow of the American Mathematical Society in 2012. He has been elected to the National Academy of Sciences, the American Academy of Arts and Sciences, the National Academy of Education, and the Third World Academy of Sciences, and is a Fellow of the American Association for the Advancement of Science. He received the US National Medal of Science in July 2007 from President George W. Bush.

His mathematical writing is well renowned for its systematic elegance. Among other awards, he received the Frank Nelson Cole Prize in Algebra from the AMS.

He has a long and distinguished record of service to the profession. He was vice president of the AMS in 1980–1981, president of the AMS in 2001–2003, and president of the International Commission on Mathematical Instruction in 1998–2006. Hyman was awarded the Mary P. Dolciani Award "for distinguished contributions to the mathematics education of K–16 students" by the MAA in 2013 and the Yueh-Gin Gung and Dr. Charles Y. Hu Award for Distinguished Service to Mathematics by the MAA in 2006. He was chair of the AMS Committee on Education from 1995 to 2000.

This article is an abridged version of the interview. The full version, which also includes more background information, is available at https://doi.org/10.48550/arXiv.2502.09751.

Hyman Bass is a professor of the Marsal Family School of Education and the Samuel Eilenberg Distinguished University Professor of the Department of Mathematics at the University of Michigan. His email address is hybass@umich.edu.

Lisa Carbone is a professor of mathematics at Rutgers University. Her email address is carbonel@math.rutgers.edu.

Yvonne Lai is a professor of mathematics at the University of Nebraska-Lincoln. Her email address is yvonnexlai@unl.edu.

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1. 1930s–1959: Becoming a Mathematician

In the Second World War. *Interviewer: You were born with World War II on your doorstep. How did this shape you?*

Bass: I was the seventh of eight children. The first set of conscious memories of being aware of the larger world came about the time of World War II. My two older brothers went to the Navy. Three of my four sisters went off to Washington.

My father then had no help from my older brothers, so I would leave grade school about noon to help him on the delivery truck. And so, my first experience working was carrying meat into markets to be weighed on scales. And my early education of the "real world" was delivering meat on a pickup truck, and working at a processing house where cattle were slaughtered, dressed, and put in coolers.

My brother Manuel was in the Naval Officer Training Corps, which was basically engineering. When he came home on leave, he would teach my younger brother Isaac and me about the science he was learning. This planted a seed for an interest in science in general. I even did a project, possibly for Westinghouse, with the fruit fly *drosophila melanogaster*. But it didn't take hold the way mathematics eventually did.

Becoming an algebraist in the post-war and Sputnik years. *Interviewer: At what age did you know that you were very dedicated to mathematics?*

Bass: Not until college.

I had applied to Princeton for its reputation as a liberal arts school. They gave a regional scholarship, perhaps for geographical diversity.

I didn't know anything about Princeton, and I inherited the frugality of my parents. When I went from California to Princeton, I hitchhiked. I made several cross-country trips that way, not wanting to be a financial burden. The total expenditure my parents made on my education was about $500. I worked each summer. The summer between

Figure 1. The Bass siblings left-to-right: Leon, Pearl, Manuel, Frances, Hyman, Sylva, Madeline, and Isaac in Los Angeles, 1966, at their father's funeral.

high school and Princeton, I worked in a logging camp in Oregon. It shut down because of fire danger, and then I worked as a forest fire fighter. The summer after freshman year, I worked on a salmon fishing boat near the San Juan Islands in Puget Sound.

When I arrived in Princeton, it was a shock. Coming from California with blue skies and pastel colors, I found earthy colors and leaded glass windows. I found it depressing at first, although eventually I came to like it.

You asked me when I turned toward mathematics. There's a very clear and definitive answer to that: freshman honors calculus, taught by Emil Artin. The teaching assistants included John Tate and Serge Lang.[1]

I loved the kind of mathematical thinking that I found not only in the instruction, but also among the students. They would come up with these ideas, and I would think, *how on earth would anyone think of that?* I guess if you're around that enough and you pay close attention to it, eventually you learn some of it, but not all.

Interviewer: When was it apparent that you wanted to go to graduate school?

Bass: That was taken for granted once I was a major. I loved mathematical thinking, and there was no place else to devote oneself to that, outside of graduate school.

The main application of mathematics came during the Second World War. In fact, Samuel Eilenberg and Saunders MacLane began their collaboration during a war program at Columbia that was devoted to ballistics among other issues.[2]

[1] *Hyman discusses his experience in this course in* [Bas15] *and* [LM99].

[2] *This program was part of the Applied Mathematics Panel, created in 1942 as a division of the National Defense Research Committee within the Office of Scientific Research and Development* [Ree88].

Figure 2. Hyman as a PhD student at the University of Chicago, 1958.

Many of the people like von Neumann and others, who worked on the Manhattan Project development of the atomic bomb, were mostly pure mathematicians, or physicists. Broad, conceptual, and big thinking was important to the work.

One big effect of World War II was that because of the importance of radar and eventually atomic weapons and cryptography, the military came to appreciate that fundamental research has a big role to play in national security. They also understood what was needed to make this happen, and they wanted it to continue after the war.[3]

From 1951 to 1955, I was in Princeton and then in graduate school in Chicago until 1959. In 1957, Sputnik occurred. A lot of people believed that Russia was so backward that they would not be capable of anything like that. So it was a real awakening and shock. The country discovered that they needed to take this seriously and to develop much more capacity and strength in technology and science. They created incentives to support education moving in that direction. So any student who intimated in any way that they were interested in science or math was amply encouraged. In my graduate and postgraduate years, it was easy to get a scholarship or fellowship, because of the support for progress in basic science and mathematics.

2. 1960s–1990s: Mostly Commutative Homological Algebra, Projective Modules, Ubiquity, and Trees

Interviewer: In the 1960s to 1970s, your work was mostly focused on homological algebra and projective modules. What stands out to you in your memory about the work you did in those years?

[3] *This acknowledgment eventually led to the founding of the National Science Foundation; see* `https://www.nsf.gov/about/history/narrative`.

Bass: I do remember that it's always nice to come into an area that's fresh, which homological algebra was at the time. The territory hasn't been completely tilled, and the low hanging fruit hasn't all been picked. It offers many new questions. Homological algebra was modeled on things that were developed in topology—homology and cohomology theories.

Saunders MacLane had this paper about natural transformations which were basically morphisms between functors. For example, a finite-dimensional vector space is isomorphic to its dual. They have the same dimension. But it's isomorphic to its second dual in a natural way, in that you can identify it with its second dual. Among those two isomorphisms, one is canonical; it's *natural*. And so, the notions of category theory began to emerge.

Homological algebra, which contains the seeds of category theory, was just beginning to be developed as a novel theory that axiomatized ideas of algebraic topology. Topology is about complicated, continuous objects. Discrete things are easier to manage. They are more susceptible to calculation, making it easier to detect differences. For example, (co)homology theories attach discrete algebraic objects to spaces. So these theories were powerful tools, because they could algebraicize complicated geometric objects. Homological algebra axiomatized the methods of these theories. And axioms are then open to application to other categories than topological spaces, like rings, groups, Lie algebras, etc.

Descartes built a major bridge between algebra and geometry. This has evolved to the practice of associating a space X with a suitable ring $R(X)$ of functions on X. This supports a partial dictionary for translating many geometric objects related to X into algebraic analogues related to $R(X)$. For example, a vector bundle on X corresponds to a projective $R(X)$-module. And projective modules have purely algebraic meaning for any ring, not necessarily of the form $R(X)$. Thus, projective modules and the homological algebra of rings are purely algebraic theories that have important links to geometry, but also connections, some unanticipated, to classical questions in algebra.

The new homological algebra of rings opened its door for application. The first homological invariant of a ring R is its global dimension. I remember that Eilenberg investigated this for the most highly developed part of ring theory, that was, at the time, finite-dimensional algebras. They have a nilpotent radical, modulo which they are semisimple. It turned out that the global dimension of R is zero if R is semisimple, and infinite otherwise. So, disappointingly, homological algebra illuminated nothing new.

But a more productive site of application was in commutative algebra, which is tied to algebraic geometry. What does homological algebra have to say there? Here, there were huge payoffs. These were the developments of Jean-Pierre Serre, Maurice Auslander, David Buchsbaum, and other algebraists at the time. For example, finiteness of global dimension was equivalent to regularity. Geometrically, it was equivalent to smoothness of the corresponding variety. That was the birth of commutative homological algebra, which remains a vibrant area of research to this day.

Irving Kaplansky was beginning to expose homological methods in commutative algebra in a series of lectures that inspired the direction of my dissertation.[4]

In its early stages, homological algebra brought into the foreground things about projective and injective resolutions, and projective modules in particular. Projective modules are very nice. Free modules are like vector spaces, but with scalars from a ring instead of a field. So then the idea is that you'd like to imitate linear algebra or elementary divisor theory for principal ideal domains, and see to what extent you can generalize that.

In my thesis I was investigating things to do with injective dimension. That was less actively treated at the time, so it was a little-worked territory for me. One reason, I think, that it was not given as much attention is that injective modules tend not to be finitely generated. For instance, for abelian groups, an important injective module is $\mathbb{Q}/\mathbb{Z}$, which is definitely not finitely generated. So, the development of injective modules and injective dimension led me in a direction that seemed like a very natural thing to look at homologically. At the same time, it was not so clear how interesting it was more broadly in mathematics. I didn't worry about that at the time. But my so-called "Ubiquity" paper, that culminated my work on this topic, ended up as one of my most cited papers [Bas63].

Given my approach, it was natural to investigate rings A such that A itself as an A-module has finite injective dimension. I found several conditions equivalent to this, especially when A is commutative Noetherian. When I showed this to Serre, he pointed out that, when A is an affine ring as in algebraic geometry, some of these conditions, related to duality, had been studied by Grothendieck, who called them Gorenstein rings, in reference to Gorenstein's thesis about plane curve singularities. Other kinds of examples were integral group rings of finite abelian groups, relating to my interest in integral representations of finite groups.

The Ubiquity paper was an effort to coherently assemble a host of not obviously related results, before turning my attention to other things. This was a kind of "house cleaning"; my aim was not to solve a big problem. And so, I found it curious that this paper drew so much attention.

[4]*Irving Kaplansky was Hyman's doctoral advisor.*

To bring things up to date, we can look at group actions on trees. This area emerged from work on the congruence subgroup problem, which asks, for an arithmetic group over a global field, to what extent are finite index subgroups "explained" by congruence subgroups. In a split simple algebraic group G, effective results were found when $\mathrm{rank}(G) \geq 2$. It was long known that this fails for rank 1, for example $SL_2(\mathbb{Z})$.

Serre went on to analyze the case of $SL_2(A)$, where A is an S-arithmetic ring, for example $\mathbb{Z}[\frac{1}{p}]$, p prime. He showed that the congruence subgroup property is essentially rescued if the unit group of A is infinite. His methods involved the study of discrete subgroups Γ of $SL_2(F)$, where F is a local field. The one notable result about this was Ihara's Theorem: *If Γ is torsion free, then Γ is a free group.* Serre systematized this study by using the action of Γ on the Bruhat–Tits building, which, in rank 1, is a tree.

In 1968, Serre gave a Collège de France course on these ideas. I was on a year's leave in Paris, and Serre invited me to prepare notes of his lectures. I was happy to do this, though my focus at the time was on algebraic K-theory. Serre initiated a general study of a group Γ acting on a tree X. Beyond recovering Ihara's Theorem, he showed, for example, that if $\Gamma\backslash X$ is an edge, then Γ admits an amalgamated free product decomposition. In a café discussion about the course notes, Serre indicated how these results should be fragments of a general combinatorial theory of group actions on trees, and he proposed that I might work out the details in the notes. I did this, formulating it as a kind of analogue of ramified covering space theory, in which Γ is represented as the "fundamental group" of the quotient "graph of groups" $\Gamma\backslash\backslash X$. This work, now called "Bass–Serre theory," was incorporated into Serre's book, *Trees*, based on the course.

This done, my attention returned to algebraic K-theory, where, at the Institut des Hautes Études Scientifiques, I did some work with Tate on K_2 of global fields. About a decade later, Peter Shalen asked me a specific question about finitely generated subgroups of $GL_2(\mathbb{C})$ that he suspected could be answered using the theory of group actions on trees. I proved a little theorem confirming his suspicion. When Thurston came to Columbia for a colloquium talk, I showed him the theorem, and asked if he knew what Shalen's interest might be. He said that he didn't know, but that the result could help to prove the Smith Conjecture, about the unknottedness of the fixed circle of a periodic diffeomorphism of the three-sphere. Thurston's impulse turned out to be true, but its realization had to mobilize the work of several mathematicians, eventually assembled in the book, *The Smith Conjecture*.

This work led Morgan and Shalen to deepen and generalize the theory of group actions on trees. In particular, they introduced the notion of a Λ-tree, where Λ is any totally ordered abelian group. Combinatorial trees are the case $\Lambda = \mathbb{Z}$. They established the geometric importance of $\mathbb{R}$-trees. These events prompted my reawakened interest in tree actions, including, in collaboration with Roger Alperin, the case when Λ is nonarchimedian. One outcome of this work was my book with Alex Lubotzky, *Tree Lattices*, written with the assistance of my students, Lisa Carbone and Gabriel Rosenberg [BL01]. This book was a product of a long and fruitful collaboration with Lubotzky on geometric methods in group theory. Alex's presence always seemed to energize and animate one's thinking. The work from this time such as by Carbone, Kulkarni, and Rosenberg pretty much saturated what you could say about how tree lattices are analogous to the case of Lie groups [BCR01, CR03, Car01, BK90].

Figure 3. Left-to-right: J.-P. Serre, Alex Lubotzky, and Hyman Bass, Einstein Institute of Mathematics, Hebrew University of Jerusalem, 1998.

Ihara's paper, a key source of all this work, not only showed that a torsion free discrete subgroup Γ of $SL_2(\mathbb{Q}_p)$ is free, but it attached to Γ a zeta function. Though I had not worked much with zeta functions, I decided, for my own edification, to understand that part of Ihara's paper. I generalized Ihara's construction to any uniform lattice, not necessarily torsion free, on a not necessarily regular tree, and used ideas about noncommutative determinants arising from work on projective modules and representation theory, as, for example, in my paper on "Euler characteristics and characters of discrete groups" [Bas76]. Though I treated this paper as an exercise in self-instruction, it became, again surprisingly, one of my most cited papers.

3. Mid-1960s–Mid-1980s: Algebraic K-theory and the Congruence Subgroup Problem

Interviewer: It may be that what you are best known for is work on algebraic K-theory and the congruence subgroup *problem from the 1960s to the 1980s. What do you recollect about this work?*

Bass: It's interesting how things have a continuity to them. I had been thinking a lot about Serre's Conjecture, that projective modules over a polynomial ring are free. The model that I always had in mind as an algebraist was elementary divisor theory, that is, modules over a principal ideal domain or over a Dedekind domain.

The big awakening was Serre's paper in one of the Dubreil–Pisot Seminars, where he proved that a projective module of sufficiently large rank has a free direct summand. This result was unlike anything that I had seen before. So I studied that proof very closely.

His proof made me realize that there was a big wall between two kinds of methods, topological and algebraic. The wall is akin to that between smoothness and analyticity. With analyticity, if you know a germ of a function—if you know it in a small neighborhood of a point—it is determined globally. Two analytic functions that agree in a small open set are the same. If it's only differentiable, even infinitely differentiable, that's no longer true.

This means that with smoothness—and not analyticity—you can make partitions of unity. You can make pieces of a function that behave nicely in different places, and you can splice them together smoothly.

Partitions of unity were the bread and butter of topological methods. In terms of viewing projective modules like vector bundles, getting a free direct summand means you can get a section which is nonzero everywhere, because then it will generate a one-dimensional direct summand.

Serre's result is the algebraic analogue of a simple topological theorem: *If the rank of a vector bundle E exceeds the dimension of the base, then E has a (continuous) nonvanishing section.* So how do you make a section which is nonzero everywhere? You build it up inductively on the skeletons of the base space. Inductively, having a nonvanishing section on the t-skeleton, you want to extend it to the interior of a cell of the $(t+1)$-skeleton. It has been constructed on the boundary, and you want to extend it without zero to the interior. If the bundle rank is r and the dimension of the base space is d, the obstruction to this extension lies—almost tautologically—in $\pi_t(S^{r-1})$, which vanishes if $r-1 > d-1 \geq t$.

Functions in algebra behave like analytic functions. If you have two polynomials, even in many variables, and they agree on an open set, then they're the same. So you can't stitch polynomial functions given locally.

In a projective module P of rank r, Serre sought to build an element ("section") s that is nonzero modulo every maximal ideal. So one is working with the space X of maximal ideals of the ring. The zero locus $Z(s)$ of an element s is closed in X. Choose a section t nonzero on $Z(s)$ and "linearly independent" of all the sections used to build s. This is possible if r is "large," and then $Z(s+t) \subset Z(s)$, and has lower dimension. That is Serre's method. This was a totally different kind of argument than what I had seen before.

Let $\mathcal{P}_r(R)$ be the set of isomorphism classes of rank r projective R-modules, and define $\mathcal{P}_r(R) \longrightarrow \mathcal{P}_{r+1}(R)$ by sending P to $P \oplus R$. With $d = \dim(R)$, Serre's theorem says that these maps are surjective for $r \geq d$. Inspired by the topological analogue, I was able to show that they are injective for $r > d$. This brought increased attention to "stable range" phenomena in algebra.

Then came Grothendieck's Riemann–Roch Theorem, fortunately published in a seminal paper of Borel and Serre. Grothendieck wanted to wait until the ideas were further developed, but the ideas were too fertile to delay publication.

One of these ideas is the K-functor, the "Grothendieck group" $K(X)$ of vector bundles on an algebraic variety or scheme X, treated as something like a cohomology theory. Atiyah and Hirzebruch ran with this and launched topological K-theory for topological spaces X, making $K^n(X)$ equal to "K of the nth suspension of X."

To me it then seemed natural to try to construct some algebraic analogue of these developments. For a ring R, one starts with $K_0(R)$, the Grothendieck group of finitely generated (left) R-modules. When R is commutative, Noetherian, and regular, Grothendieck proved a kind of "homotopy invariance": $K_0(R) \longrightarrow K_0(R[t])$ is an isomorphism. Thus, a finitely generated projective $R[t]$-module is "stably isomorphic" to $P[t]$, where P is an R-module. Combined with the stability theorems, this gave partial affirmation of the "Serre Conjecture" that, when R is a field, projective $R[t_1, t_2, \dots, t_d]$-modules P are free; this now followed from homotopy invariance and stability theorems if $\mathrm{rank}(P) \geq d+2$. The Serre Conjecture was fully proved about a decade later by Quillen and Suslin.

It was natural to seek a construction of higher algebraic K-groups, $K_n(R)$, $n \geq 0$, but there was no satisfactory algebraic analogue of the suspension. I looked directly at the case $n = 1$. Topologically, $K_1(X) = K(SX)$, the Grothendieck group of vector bundles on the suspension SX of X. SX can be viewed as the union of two cones over X, glued along their common base X. Let E be a rank n vector bundle on SX. Since the cones are contractible, E can be identified with the trivial rank n bundle on each cone. Then E is defined by a "gluing" automorphism of the trivial bundle on the "equator" X of SX, in other words, by an element $\alpha \in GL_n(C(X))$, where $C(X)$ is the ring of continuous real functions on X. Up to isomorphism, E is determined by the homotopy class of α, i.e., the image of α modulo the identity component $GL_n^0(C(X))$

Figure 4. Left-to-right: Lisa Carbone, Peter Kropholler, Elizabeth Schneider, Hyman Bass, Sal Liriano, and Ilya Kapovich, Albany Group Theory Conference, 1993.

Figure 5. Left-to-right: Michel Raynaud, Hyman Bass, and Armand Borel, Corsica, 1970s.

of $GL_n(C(X))$, so $K_1(X) = \lim_n GL_n(C(X))/GL_n^0(C(X)) = GL(C(X))/GL^0(C(X))$.

To define an algebraic analogue of this, one needs an algebraic proxy for $GL_n^0(C(X))$, with $C(X)$ replaced by any ring R. For various reasons it was found reasonable to use the group $E_n(R)$ generated by the elementary matrices $I + te_{ij}$, with $t \in R, i \neq j$, and e_{ij}, $(1 \leq i, j \leq n)$, is the standard basis of the space of $n \times n$ matrices. Further, define $K_1(R) = GL(R)/E(R)$. I proved some stability theorems for K_1 analogous to those for K_0. With Alex Heller and Dick Swan, we proved the analogue of homotopy invariance for K_1. In my book, I constructed some exact sequences relating K_0 and K_1 [Bas68].

Though the rings $C(X)$ mediated the connection of the algebra to topological K-theory, it turned out that an entirely different part of topology had, much earlier, developed an interest in a slight variant of $K_1(R)$, when R is the possibly noncommutative integral group ring $Z\pi$ of a group π. This was "simple homotopy theory," asking whether a homotopy equivalence $X \longrightarrow Y$ is "simple." J. H. C. Whitehead defined an obstruction that lives in what we now call the Whitehead group $Wh(\pi)$, where π is the common fundamental group of X and Y. We can write $Wh(\pi) = K_1(Z\pi)/[\pm\pi]$, where $[\pm\pi]$ denotes the classes of diagonal matrices with entries in $\pm\pi$. Whitehead established the basic algebra of elementary matrices, showing for example that $E(R)$ is the commutator subgroup of $GL(R)$, and his student Graham Higman did the first substantial calculations of $Wh(\pi)$, for finite abelian π. This algebra provided the basic tools for K_1 calculations.

For R commutative, the determinant decomposes $K_1(R) = R^\times \oplus SK_1(R)$, $R^\times$ being the unit group of R and the latter coming from the special linear group. Moreover there are relative groups $SK_1(R, J)$ defined for ideals J of R. And already for $R = \mathbb{Z}$, calculation of the groups $SK_1(Z, J)$ encountered the century-old "congruence subgroup problem" for $SL_n(\mathbb{Z})$ with $n \geq 3$. This enlisted the efforts of several mathematicians, notably Milnor and Serre, that culminated in our paper on the Congruence Subgroup Theorem [BMS67]. This resolved the SL_n congruence subgroup problem ($n \geq 3$) for arithmetic rings, and related it to reciprocity laws in number theory.

These diverse connections (to algebra, algebraic geometry, topological K-theory, simple homotopy theory, functional analysis, number theory) showed that these still modest developments of algebraic K-theory had more mathematical promise than I had anticipated. It intensified the interest in constructing a "good" higher algebraic K-theory. So it seemed like algebraic K-theory had lots of intriguing tentacles, some extending beyond my expertise, but no mathematical center.

This prompted me to organize a conference assembling all of the diverse clients of this emerging subject. In the proposal to the National Science Foundation for this conference I said something like, "There are lots of people with these related interests, but they're in very different areas. Just bring them together and let the human chemistry work." It was a spectacular success, thanks to Quillen, who brought with him a fully developed construction of higher algebraic K-theory (two versions), together with fundamental computational tools. This won him the Fields Medal. The conference proceedings were published in three thick volumes of the Springer Lecture Notes in Mathematics [Bas73a, Bas73b, Bas73c].

4. 1970s–1980s: Writing with Bourbaki

Interviewer: You were a member of a group who published mathematical textbooks under the pseudonym Nicholas

Bourbaki. How did you come to work with Bourbaki? And what was your role in the group?

Bass: When I was invited, it was quite a shock to me. It was Jean-Louis Verdier who communicated the invitation. He was a close friend and colleague. I ended up being a member from 1970–1982.

Bourbaki is not focused on new mathematics. Instead, it's about a comprehensive and rigorous way of writing a clear and concise exposition of contemporary mathematics. This mathematical writing must be coherent across a huge landscape, and this leads to some things that are cumbersome from an expository point of view.

In typical writing, you localize your subject area, and you can adopt conventions that are not needed to be consistent with conventions in another domain of mathematics. But Bourbaki attempts to coherently talk about the whole landscape of mathematics. There are then many cross-referential things that have to be consistent with each other in a way that domain-specific writing would not entail.

Whatever likes or dislikes people have about Bourbaki's style, the writing is done by mathematicians who in their own professional lives are gifted expositors. For instance, Serre is a beautiful writer, and very lovely to read, but he also writes things in Bourbaki that fit the Bourbaki style. Writing in Bourbaki is a different kind of project. It doesn't have pedagogy so much as logical order and coherence and rigor in mind as the governing feature. It has a conciseness that comes from a severe, minimalist sort of elegance. In some cases, like Lie theory, I think Bourbaki's exposition is both original and beautiful.

To the extent that I have a kind of strong allegiance to mathematical proof, I guess the style of my writing, even though sometimes careless, sort of fit that style.

One interesting thing about Bourbaki is its working culture. At the time, it was a small group, with varying size that involved up to a dozen people or so. They have this big architecture of the whole enterprise of mathematics in view. The early books are about foundational structures, for example set theory, algebra, and topology. Then the later chapters treat higher order structures like functional analysis, manifolds, and Lie groups where different foundational structures intertwine and are synthesized. There are various chapters that have to be either written or maybe revised and updated, and they all fit into this big design.

So how does this writing get accomplished? The nature of the work is largely social. There's a chapter to be written. It's either new or it might be a revision. The group collectively has some conception of what the chapter is supposed to be about, what the overall content is, what it will emphasize. And then some*one*—an individual group member—is assigned to write a draft of that chapter.

Their funds are modest, they come from publication of these volumes, and they all go into supporting the work. But at least the conditions of the work, the ambience, and things like that, they're not luxurious, but they're very comfortable and pleasant.

At that time, they met together in a "congrès" roughly three times a year. They went to a remote place—very nice, but quiet, and decidedly not very public. Other people present had no idea who this weird group (of men) was, or what they were doing. The remote locations included Provence, Corsica, and Lake Como, and we stayed in some comfortable auberge and ate very well. For Corsica we took this fast train south from Paris, and had a luxurious meal on board.

The group was very animated but relaxed. Everybody was very casually dressed. They might have been wearing shorts or whatever. We sat around and someone read, line by line, the draft of the current version of a chapter. The reading traded off at various stages when a reader needed relief. The draft was chewed apart. People raised all kinds of critiques and suggestions—mostly things that they didn't like. The author, of course, was present and participated in this sometimes brutal feedback. The only compensation—detailed notes were recorded for whatever had to be changed, how things should be done differently—is that the draft author didn't have to rewrite it. The next draft was assigned to someone else. A chapter may have gone through many, many cycles only to eventually not die but go *au frigidaire* [in the freezer]. In other words, it may have been put into long-term storage, possibly forever. Also, rejected parts of earlier drafts often ended up in the exercises.

It's a very systematic and very exacting kind of process. And, of course, it's anonymous. Other scientists wonder how all this really high-level intellectual effort, and time, without personal recognition or compensation, can work.[5] An often publicly discussed question was, "Why isn't category theory in Bourbaki?" Well, it was difficult to figure out a way to do that without completely revising Bourbaki's foundations in set theory. Deligne wrote a very nice draft on category theory, and these ideas are *au frigidaire*. Interestingly, while Bourbaki does not define functors, he permits himself to use the expression "proprieties fonctorielles" in some section headings (though not in the text).

My participation in Bourbaki was personally a unique kind of experience, and, by design, largely invisible to the rest of the mathematical world.

Interviewer: This does sound like quite a bit of work. So in Bourbaki, the individuals perhaps become famous, but do not receive credit for what they contribute to Bourbaki.

[5]*See* The Bourbaki Gambit *by Carl Djerassi, 1996, Penguin Publishing Group.*

Bass: What drew me to mathematics was the beauty of mathematical thinking. I'm a very slow thinker. I've always been surrounded by people that were much smarter, much quicker, had a bigger kind of vision of the field than I did. What I valued was to be able to be in the presence of people like that. I took a lot from Bourbaki. The exposure to that milieu and to those people was something precious.

For example, I would say that probably the best piece of work I did was the paper on the congruence subgroup problem. It has three authors [BMS67]. I actually drafted the paper myself, and I can imagine that the other two were not entirely happy with the way it was put together; it could very naturally have been either two or three different papers. For example, the contribution that Serre made came late in the development. I made decisions about some of the terminology which, in retrospect, probably would have been better attributed to Milnor.

But what I valued about the work was not so much what I did and what I wanted credit for, but the fact that the whole package together was such an impressive and satisfying assemblage of ideas. That to me was most important. I was awestruck by the fact that this natural question about matrix groups was not only deeply connected to explicit general reciprocity laws in number theory, but that solution of the problem would have required discovery of those laws had they not already been known. For example, I don't think Serre and Milnor, even though they each appreciate the other's work, had ever written anything together. So in this sense the ideas that they each contributed are more complementary than intersecting. The whole coherent package was to me something that I appreciated more; it was not so much about individual credit. For me it was a question about the aesthetic of the ideas being collected together. It might have been actually more helpful to have separated out parts of it in a way.

To answer, "Why do you surrender this time and effort to do something like Bourbaki?"—That's just part of the fabric of that whole disposition. I never had any self-image of being a kind of big mathematical creator of ideas and things like that, but I really treasured the idea of being in that environment of really beautiful, high-level mathematical thinking. That was most important to me.

5. 1990s–Present: Mathematics Education

Inquiry into mathematical work of teaching. *Interviewer: In the 1990s, you started to become more involved in K–12 mathematics education. Your longest collaboration in mathematics education is with Deborah Loewenberg Ball. How did the two of you meet?*

Bass: There was a workshop organized by Ed Dubinsky at the Educational Development Center, on undergraduate education. Deborah and I were among those there. Later, I

Figure 6. Hyman receiving the National Medal of Science from President George W. Bush, 2007.

had more formal contact with her through the Mathematical Sciences Education Board.[6] We invited her to give talks on some of the projects we were discussing intermittently over time. In this way, we had contact that was institutional in nature, but gradually she was telling me more about the research she was doing on teaching.

As an elementary school teacher, she was always wanting to improve her teaching, but she was troubled by her teaching of mathematics. She felt that there were some understandings of mathematics that she didn't have, and she needed to find out more. So she took some math courses, which were helpful. But she was uncomfortable importing a disciplinary perspective of mathematics into the classroom without knowing how this perspective was needed for teaching practice. This led to her inquiry on the question, *what is the mathematical work of teaching?* But she wasn't convinced that she would see all the relevant mathematical happenings, even if she were able to view a recording of her own teaching.

It's not often that you see this kind of intellectual humility. People can be very strong at the things they specialize in. But it takes humility to recognize that when you get into some new territory, your ways of knowing your ideas were only for certain kinds of purposes that may *not* apply to a different context. Your knowledge contributes but does not govern.

She then did something that I also like to do when I want to understand something better. I like to find the people that I think know and understand the thing most deeply, and I go to them and try to get better understanding. She decided to recruit some mathematicians (I was one of them), not to tell her what mathematics is

[6] *Hyman served on the Mathematical Sciences Education Board, created by the National Research Council, as a member (1991–1993) and chair (1993–2000).*

Figure 7. Left-to-right: Deborah Loewenberg Ball, Rep. Vernon Ehlers, Hyman Bass, and Roger Howe at the AMS lunch briefing for members of Congress and their staff on Capitol Hill, July 26, 2001.

appropriate for teaching, but to look at the actual practice itself. She asked them to watch videos of teaching, guided by the question, *what do you see that is mathematically significant and relevant?* At that point, we had substantial intellectual contact, not only about policy, but also about the mathematical work of teaching.

In these videos, she was teaching third graders. The data we analyzed were amazing. The videos were accompanied by detailed transcripts. The student notebooks and work were all photocopied. Student interviews and the teacher's journal were also provided. So it's an amazing corpus of data to use to really try to understand how mathematics is involved in teaching practice. Nowadays, anyone can do video on their phone. But back then, in 1989–1990, it was a formidable enterprise to collect this kind of longitudinal documentation at this scale.[7] The analysis of this corpus of data laid empirical groundwork for the notion of *mathematical knowledge for teaching*, an idea now ubiquitous in policy and practice of the mathematical education of teachers.

Connection-oriented mathematical thinking. *Interviewer: When did you decide to turn your intellectual focus to math education?*

Bass: I've never been a quick thinker, even in K-theory, where I was heavily involved from the beginning. Eventually, once the subject became highly active, there were people that were running with it far faster than I could, and it took on a life that was much bigger, with lots of ramifications that were well beyond my expertise.

In education, there are a lot of things I'm interested in, and even have some insights. But I know that there are lots of people in the field who've thought about it for longer, and probably with more insight. I don't feel that I'm in a position to say something that goes beyond what they are already able to do well.

Figure 8. Hyman at a meeting on Professional Development of Mathematics Teachers at Oberwolfach, 2007.

So I tried to think, where is something that I feel has been under-treated, and where I could have new insight? Lately it's beginning to coalesce around the idea of mathematical connections, about the unity of mathematics. It is about the coherence, in a large sense, of mathematical ideas that should be part of mathematical education.

A question here is, *if you think about mathematics as a body of knowledge, skills, and practices that people should learn, how do we structure an educational system to equip people with those resources?* These practices include analysis and specialization, which oppose synthesis; and decomposition, which opposes composition. You can break mathematics down into parts, and decompose it—that's the analysis. Doing this, we can identify coherent chunks of the discipline that you would teach and give names to them like number theory, algebra, geometry, probability, calculus, whatever. We have this core structure which represents a decomposition of the discipline, the "geography" of school mathematics.

But what about synthesis?

I've looked at a lot of curricula and teaching. Even in undergraduate teaching, students experience these courses as islands that are disconnected from each other. It's a siloed structure. And this has effects. When students get a problem, the first thing they do is typecast the problem. This is a number theory problem. This is a geometry problem. This is a calculus problem.

Then, if the problem solution requires that they deploy resources from more than one domain, and if it's not in the type that they categorize the problem, they unconsciously forbid themselves to cross that boundary and to another domain. They haven't been told not to do that. They just don't do that.

It's like when people first learn geometry, no one says, *you're not allowed to draw auxiliary lines in that figure*. But it is a cultural impediment. So there are lots of signals that suggest that even when students have had a very good education, and they can show highly developed skills in

[7] *For an account of a particularly remarkable episode of this year, see* [Bas15, pp. 634–636].

Figure 9. Left-to-right: Hyman Bass, Charles E. Wilkes II, Deborah Loewenberg Ball, and Yvonne Lai at the University of Nebraska-Lincoln, May 2024.

each area, they still don't come away with a sense of the kind of coherence of mathematical enterprise as a whole.

Of course, in the outside world, problems are mostly multidisciplinary; they cross boundaries. Even within mathematics, this is true.

One message that I take from the learning of sciences is that how much knowledge you have, or what knowledge you have, is not sufficient. What's more important is how well developed are the connections between its parts. There are various pieces of evidence for this, and one from cognitive psychology is related to a concept called *transfer*. If you gain knowledge of A, can you apply that to B, when B is more or less the same as A structurally?[8] Transfer is about whether you can export knowledge to structurally similar problems in different contexts. Experiments show that transfer very rarely occurs.

So I've been thinking about, *How can you teach people to make mathematical connections?*

Of course, there are many kinds of connections. Some of them are fairly obvious. If you give a drill sheet of exercises, those problems are all related, and students see that they're related.

I'm interested in connections that have some depth and subtlety. One reason that I'm drawn to this is that some of the big breakthroughs in math, and also in science, are when people realize that two things that seem unrelated, that perhaps are in different parts of the subject, are actually connected in some deep way.

For example, Poincaré's realization that the transformations of Fuchsian functions he was looking at were identical with the transformations of non-Euclidean geometry came to him as he was boarding a bus. Or take the Langlands Program. It relates number theory and Galois representations to harmonic analysis. This is not a theory; it's a program that people will spend another several decades on.

Mathematics is about finding patterns. Mathematicians see a pattern when, in their own practice it seems like, in diverse contexts, they're doing essentially the same work over and over again. When that happens, they ask, *what is the "thing" of which these are all special cases?* That's a kind of conceptual question, because it's about a new concept that doesn't yet exist.

Abstraction is not such an exotic thing. Numbers are abstractions. The number five. Where's the number five? Can you point to it? No. But you can hold up five fingers, and that's an example of five things. And I can say, if another thing is an example of five things, I can concretely implement that I can show that there's a one-to-one correspondence between the elements. So saying how two things are the same is concrete. But that sameness is an abstraction, whose existence is conferred only by giving it a name (like "five"). In this sense, "christening is conceptual creation."

Now, we can't enable students to experience Poincaré's kind of deep revelation or to bring forth an elementary version of the Langlands Program. For this one needs broad profound knowledge and insight plus creative genius. But I have been seeking an accessible and developmentally appropriate simulation of the kind of thinking involved. I call it, "connection-oriented mathematical thinking" (COMT). And I can give lots of accessible examples of that.[9]

But COMT does not happen spontaneously. Discerning and using mathematical connections is not something we can expect from students without dedicated instruction. So the question becomes, how can you teach COMT? Those are the ideas I've been exploring for the past few years. Though COMT, like proving, is a high-level mathematical practice, it has developmentally meaningful expressions already in the early grades.

My focus is not on transfer. Instead, I foreground *the making of the connections*. The point is that even if you may be able to solve A and B independently, you still may not see the connection between them. And, sometimes, even when problems seem like they're mathematically equivalent, they aren't, but for subtle reasons.

Even with fairly elementary problems, it can be challenging to classify or type them according to how structurally connected they are, in particular whether they are

[8] *For instance, if you learn negative numbers on a number line at school, can you use this to make sense of money exchanges?*

[9] *Some examples may be found in* [Bas17], *for instance when Hyman demonstrates a general measure theoretic structure that underlies a set of problems on mixing liquids, discrete counting, and area.*

actually equivalent or isomorphic. There are various materials that I have been developing to design instructional activities for this kind of thinking.

Mathematics and education. *Interviewer: Looking back, what do you think has improved for mathematics since you were early in your career? And what do you think has become more challenging?*

Bass: Generally mathematics continues to flourish, addressing new problems not only internal to mathematics, but also coming from science and technology, and I don't think that will ever cease as long as we don't destroy civilization, which is unfortunately something we can no longer rule out. Here's something related regarding the demographics of academic mathematics. What's the economic infrastructure of academic mathematics and of the sciences in general? In the sciences, it's laboratories. If you are a department chair in the sciences, and you appoint a new faculty member, to set them up, you declare a salary, and in addition there has to be what used to be half a million dollars, and maybe much more now, just to set up their laboratory.

On the other hand, mathematicians have a different economic foundation. I think that for mathematicians, the counterpart of the lab is the classroom—especially advanced graduate courses that act as seedbeds for ideas in the subject. The culture of mathematics has a different structure than it does in science. In a research-active mathematics department, the list of seminars has no counterpart in other fields. Huge time and intellectual effort go into these. Faculty don't typically receive formal credit for running and attending seminars.

Mathematics departments typically have one of the larger faculties in the STEM fields. What allows mathematics faculty to have the size that it does? The economic foundation is educational: the instructional mission. Mathematics is an enabling discipline for sciences and technology. When foundational subjects such as calculus, linear algebra, and differential equations are taught, mathematics has jurisdiction over the instructional terrain. And as universities depend more and more on tuition for income, instruction becomes an increasingly crucial resource. It is what sustains reasonable faculty size, and it's very precious.

Mathematicians do not sufficiently appreciate how much of a stake they have in the quality of what they do with undergraduate instruction. It's basic economics. Other people, especially the engineers, often covet that territory, and rightly so. They feel that they are far better equipped to teach calculus to engineers, because they know how it will be used. This argument is not without merit.

Eilenberg was chair of the department at Columbia when such a challenge occurred. Bypassing normal process, he did not go to the dean to argue the case. Instead, he went straight to the university president. He said that Columbia is a first-rate research university, and a research university without a first-class mathematics department is not tenable. A first-class mathematics department needs a reasonable faculty size that can cover enough areas with deep mathematical strength.

Now, Columbia does not have a huge math department, but for its size, it has relatively a large instructional role compared to the size of the university and the college. And if they lose that instructional real estate, it means a critical reduction of the department size, and therefore its quality. It is a question of critical mass. This argument was persuasive.

But this argument is not purely about the merit of the mathematics. It also carries a message that the quality of the instruction has to justify the size. Unfortunately, this is not always sufficiently communicated to the young mathematicians who come into the department and don't realize how much they have at stake in it and in the quality of the instruction.

This has changed slowly and in good ways over time.[10] You can no longer say that if someone is doing really outstanding mathematics, it doesn't matter how inept they may be as a teacher. The argument can further be made that, in fact, improving the quality of instruction will improve the quality and productivity of the mathematics. This is a commitment that must be a part of department culture, and must be nurtured and supported by department leadership.

Interviewer: In the Math Wars of the 1990s to 2000s, you served in various governance positions, including in the AMS, International Mathematical Union (IMU), and the National Academies. What do you think was the largest impact of these Math Wars?

Bass: I think the Math Wars were somewhat artificial. Still, they set things back for a long time.

It's a complicated story. One of the things that mathematics and mathematicians suffer from is a kind of elitism, or even arrogance. In the Math Wars, some mathematicians have tried to assert their authority because they proved some admirable theorems, and they have a high position in a good university. Therefore they're not subject to question. It's like society decides that if you have some really highly developed skill and some technical area, that therefore you're wise and have insights and things to offer to almost any area of human concern. Even the assumption that when engineers are out of employment, because

[10] *For instance, see the account of Chevalley* [LM99, pp. 6–7].

some industry has shifted, that they are qualified to be teachers, is part of this. This implicitly trivializes the wide-ranging professional skills demanded by quality teaching.

Mathematicians are very particular and fastidious about treating the subject matter with rigor and integrity. But, interestingly enough, even good mathematicians don't always agree about the best way of rendering these ideas—much less knowing about the entailments of teaching.

So the fact is that there is often a general kind of contempt for teachers. People may not express this directly. It's just that teachers have no standing. There is a stance among some mathematicians that teachers are not to be listened to, but rather that teachers should listen to us. That's a real barrier to progress.

How are you going to elevate the level and standing of the teaching profession? This is complicated. There are lots of problems even within education to be able to make that happen. For example, developing high-level practice involves high-level critique. If you're an artist or an architect, there's a practice of "crit" by peers. In studying medicine, there are rounds. In an art studio, other artists gather around your draft painting, or something you've done, and they ask really good, insightful questions. Why did you do this? What do you think about positioning something differently? What about the color, the composition, ...? Artists and architects and surgeons are accustomed to that. They're serious practitioners who learn from peers. There are many fields where that's done very effectively.

But teaching is very private. Teachers often feel unsafe when other people come in and try to evaluate what they're doing—and for good reason. The way evaluation has been conducted has been so punitive and unconstructive, and has sometimes been done by people who lack adequate understanding of the work. So there's much that has to change. Mathematicians have not helped make it safe for people to develop and improve culture. If you want to improve the practice of a huge profession, you're not going to advance the cause by deriding the members of that profession.

Of course, debate is appropriate. Education has to evolve, and there are good ideas in the field such as the concept of continuous improvement and looking at things at the system level.

The work that Deborah does is very much on the ground at the level of practice. Nothing can be fully consummated without seeing what practitioners do with it at the ground level. But there also has to be thinking more at the systemic level. But, ironically, the actual work of teaching is the most understudied part of education.

Interviewer: What stands out to you about your presidency of the IMU's International Commission on Mathematics Instruction (ICMI)?

Bass: Until 2006, the presidents of ICMI were accomplished mathematicians with an expressed interest in education.

That is how I became president of ICMI, 1999–2006. This was not a fulfillment of ambition. Quite the opposite; I received a call from IMU president David Mumford, inviting me to be considered for election. While I had become involved in US mathematics education and had chaired the Mathematical Sciences Education Board at the National Academy of Sciences, I had no experience with ICMI, and had only a superficial understanding of mathematics education research. So, I was surprised by the invitation, and considered myself unqualified to lead an international community of scholarship in which mathematical expertise was important, but far from the whole story. After a few days' inquiry and reflection, I decided to accept the invitation, with the intentions to "learn on the job," and to critically examine the process by which I became president, which seemed questionable.

The learning was steep, and it continues. I worked closely with secretary general Bernard Hodgson, and took much counsel from Michele Artigue and other ICMI Executive Committee members. Mathematics education research was a field in rapid development, still struggling with self-definition. While well short of maturity, it included some powerful and visionary intellectual leaders. It seemed unnatural to me that the field's leadership be chosen by an assembly of research mathematicians with only partial expertise, and for whom education was a significant, but mostly peripheral concern. This was essentially a culturally colonialist model. And so, with the support of the ICMI EC, though it was skeptical of success, I proposed that the election of the ICMI EC be not by the general assembly of IMU, but by the general assembly of ICMI, attached to the International Congresses on Mathematics Education (ICMEs), not the ICMs. Together with Bernard Hodgson, we drafted the appropriate revision of the bylaws, and I successfully argued for their adoption at the 2006 IMU general assembly. While this may seem a technical issue, it represents a radical cultural power shift for ICMI. It is the most consequential legacy of my presidency.

Interviewer: What is your wish for the future of interactions between mathematicians and educators?

Bass: Well, overall, I would say harmony. I think that a lot of progress has actually been made on this.

For mathematicians to take education more seriously—not just respect for K–12, but even at the university level.

Not everybody is expected to do that with equal intensity. But much more respect and standing has to be given to the people in their departments who do that work. Mathematicians need to recognize this work as equally important to, and even synergistic with, the research that they do, and, in fact, as something that will enrich their research.

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References

[Bas63] Hyman Bass, *On the ubiquity of Gorenstein rings*, Math. Z. **82** (1963), 8–28, DOI 10.1007/BF01112819. MR153708

[Bas68] Hyman Bass, *Algebraic K-theory*, W. A. Benjamin, Inc., New York-Amsterdam, 1968. MR249491

[Bas73a] Hyman Bass, *Algebraic K-theory, i. "Classical" algebraic K-theory, and connections with arithmetic (vol. 1)*, Springer, 1973.

[Bas73b] Hyman Bass, *Algebraic K-theory, ii. "Classical" algebraic K-theory, and connections with arithmetic (vol. 2)*, Springer, 1973.

[Bas73c] Hyman Bass, *Algebraic K-theory, iii. "Classical" algebraic K-theory, and connections with arithmetic (vol. 3)*, Springer, 1973.

[Bas76] Hyman Bass, *Euler characteristics and characters of discrete groups*, Invent. Math. **35** (1976), 155–196, DOI 10.1007/BF01390137. MR432781

[Bas15] Hyman Bass, *Mathematics and teaching*, Notices Amer. Math. Soc. **62** (2015), no. 6, 630–636.

[Bas17] Hyman Bass, *Designing opportunities to learn mathematics theory-building practices*, Educational Studies in Mathematics **95** (2017), no. 3, 229–244.

[BCR01] Hyman Bass, Lisa Carbone, and Gabriel Rosenberg, *The existence theorem for tree lattices (appendix)*, Tree lattices, 2001.

[BK90] Hyman Bass and Ravi Kulkarni, *Uniform tree lattices*, J. Amer. Math. Soc. **3** (1990), no. 4, 843–902, DOI 10.2307/1990905. MR1065928

[BL01] Hyman Bass and Alexander Lubotzky, *Tree lattices*, Progress in Mathematics, vol. 176, Birkhäuser Boston, Inc., Boston, MA, 2001. With appendices by Bass, L. Carbone, Lubotzky, G. Rosenberg and J. Tits, DOI 10.1007/978-1-4612-2098-5. MR1794898

[BMS67] H. Bass, J. Milnor, and J.-P. Serre, *Solution of the congruence subgroup problem for* $\mathrm{SL}_n (n \geq 3)$ *and* $\mathrm{Sp}_{2n} (n \geq 2)$, Inst. Hautes Études Sci. Publ. Math. **33** (1967), 59–137. MR244257

[Car01] Lisa Carbone, *Non-uniform lattices on uniform trees*, Mem. Amer. Math. Soc. **152** (2001), no. 724, xii+127, DOI 10.1090/memo/0724. MR1841021

[CR03] Lisa Carbone and Gabriel Rosenberg, *Lattices on nonuniform trees*, Geom. Dedicata **98** (2003), 161–188, https://doi.org/10.1023/A:1024053813643. MR1988428

[LM99] T. Y. Lam and A. R. Magid (eds.), *Algebra, K-theory, groups, and education*, Contemporary Mathematics, vol. 243, American Mathematical Society, Providence, RI, 1999. On the occasion of Hyman Bass's 65th birthday; Papers from the conference held at Columbia University, New York, November 6–7, 1997, DOI 10.1090/conm/243. MR1733562

[Ree88] Mina Rees, *The mathematical sciences and World War II*, A century of mathematics in America, Part I, Hist. Math., vol. 1, Amer. Math. Soc., Providence, RI, 1988, pp. 275–289. MR1003177

Hyman Bass

Lisa Carbone

Yvonne Lai

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