Michael Christ

by Diogo Oliveira e Silva*

To start, how did your interest in mathematics first take shape?

As a child, I had a sense that somewhere in the world there were deep-thinking people who nobly worked to understand the world, and that all of humanity admired this as a matter of course. I'm not sure how this idea originated; mine was not an intellectual or science-oriented family.

Arithmetic problems were fun, but math was another school subject like all the rest until a turning point at the start of high school. I was 14, at a large public school in the suburbs of Milwaukee, Wisconsin, with students from a wide spectrum of backgrounds. The school selected a small group for an algebra class with an extraordinary teacher. I don't recall volunteering, or being asked whether I wanted to be in that class. We learned logic before algebra, and about cardinality of infinite sets before learning to complete the square for a quadratic polynomial. A few months after first learning arithmetic with negative numbers, I was asked to prove that adjoining the square root of two to the rationals produced a field. I guess it was the conjunction of predisposition with the right environment at the right age.

How did your undergraduate years shape you, and what changed when you moved into graduate school?

I attended Harvey Mudd College in southern California, a very small, focused place—the only major subjects offered at that time were engineering, chemistry, physics, and math. There were 88 students in my graduating class. The faculty

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excelled as undergraduate teachers, rather than researchers. For someone from Wisconsin lacking sophisticated preparation, it was an excellent environment for mastering the basics of undergraduate mathematics. I took advantage through plenty of formal coursework. For a while no one much noticed me. Then I tried the Putnam exam in my third year, and suddenly I seemed to be someone. Becoming a member of a college faculty, perhaps doing a bit of research, emerged as a possible career path, one of which I had been only dimly aware.

Harvey Mudd didn't offer much exposure to research, but there were glimpses—especially Robert James (of Claremont Graduate School), who worked in Banach space theory. Sandy Grabiner taught an exciting course at neighboring Pomona College. John Greever ran a formative course on point set topology in the Moore style, in which students engaged with definitions and learned by doing. There's a template: get started, become engaged, and allow the learning to follow.

For graduate school I chose the University of Chicago, not through shrewd advice, but because the catalogue listed names already encountered: Herstein, Mac Lane, Kaplansky, Zygmund, Swan. When I visited, people were welcoming, especially Bill Beckner. Berkeley was also on my radar—Henry Helson kindly showed me around—but the place felt enormous and impenetrable. Chicago felt more personal.

What was the analysis scene like at Chicago when you arrived?

One felt a department in transition, with some of those great names nearing the later stages of their careers. In my very first term I chanced upon a wonderful course offered by Robert Fefferman, based on Stein's book on singular integrals. On Bob's recommendation, I subsequently attended Calderón's lectures. Professor Calderón spoke with remarkable deliberation, so much so that initially it was a challenge to pay attention. Years later, he explained that he consciously avoided preparing his lectures because he wanted the audience to think with him, word by word, and he had learned from experience that preparation led him to go too fast. He usually lectured on his own work, recreating it step by step at the board. Once or twice a semester he would become stuck—really stuck—and the audience, postdocs and graduate students, would try to sort it out together. Professor Calderón was a proud man, whose willingness to subject himself to this occasional embarrassment testified to his commitment to meeting his audience on a basis of equality. I tried to absorb not only the mathematical content, but also his way of thinking at a measured pace and analyzing step by step.

A remarkable aspect of the program was the procession of talented young people passing through, including Bob Fefferman, Beckner, Zimmer, Jones, Wolff, Kenig, Phong, Jerison, Uchiyama, Janson. Sometimes a course would be offered by one of them for as few as two students—Once or twice those two were my friend David Barrett and myself. A gift.

The leap from being a good learner to doing research was a struggle. There's a real gap. Calderón first proposed to me a monumental problem, boundedness for the Cauchy integral operator on Lipschitz curves, which had been at the heart of his own research since the early 1960s. This was both generous, and wildly overambitious. I was far too naïve, and made no progress. When I thought I had something, Peter Jones gently, kindly, and devastatingly disabused me of that notion. The actual start came later, almost casually: immediately after an expository seminar talk, Calderón suggested I try for a generalization. A few days later, I had it. Someone else obtained the result simultaneously or slightly earlier, so there was no publication, but it got me moving. Once I began working on something of my own, questions and problems began to flow.

After Chicago you spent time at Princeton. How did that change your trajectory, particularly in relation to Eli Stein and Joe Kohn?

Much of my time in Chicago was invested in observing the work of extraordinary researchers. At Princeton, it was time to produce. Stein, Charles Fefferman, and Kohn were my models there, while my fellow postdocs were also important influences and friends.

Stein's influence was profound, both through his writing,

Michael Christ at UW-Madison with former PhD students (from left to right) Malabika Pramanik, Kevin O'Neill, Michael Goldberg, Diogo Oliveira e Silva, Betsy Stovall, Taryn Flock, A. Martina Neuman, Dominique Maldague, Loukas Grafakos.

and in person. The most helpful thing he did, though simple, was decisive: On our very first meeting, within days of my arrival on campus, he proposed a specific problem related to the Radon transform—hand-picked for me, modest in its initial scope, but also a bit open-ended. It wasn't a monumental problem that had been open for decades; it wasn't a blank wall; it was a pool I could dive into. The problem connected to Kakeya-type questions via the k-plane transform, offering rich terrain with room for both success, and productive failure. The crucial point was that he got me started.

Joe Kohn's influence was also pivotal. As a graduate student I'd studied his work in several complex variables with Narasimhan, who admired it deeply. At Princeton, Joe buttonholed me with an idea about a problem that turned out to be connected to many topics I'd been studying: pseudodifferential operators, PDE, several complex variables, singular integrals, nilpotent groups, and parametrix constructions. Suddenly those strands came together. Over a period of years, that interaction spawned several projects, including L^p estimates for solutions of the $\bar\partial$ and $\bar\partial_b$ equations, analyticity, global and local regularity of the Bergman projection, and weighted inequalities for $\bar\partial$. I might never have pursued those directions without his encouragement, and without his pointing me towards a specific goal. He was incredibly generous in sharing a specific idea along with more general thoughts about microlocal analysis and $\bar\partial_b$. To see so senior a figure so passionate about his research, in his undemonstrative way, was itself a lesson.

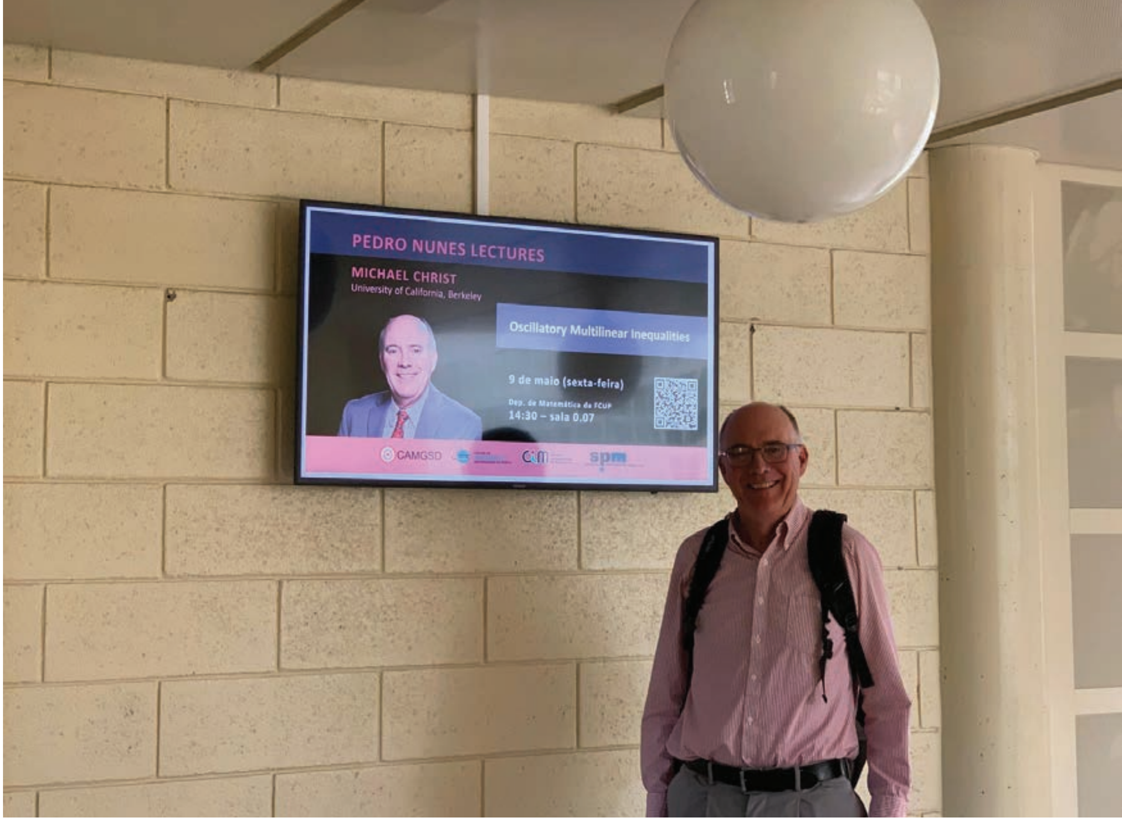
Why harmonic analysis? Was it a plan, or happenstance?

Largely luck. I felt that the two most interesting things I'd studied as an undergraduate were algebraic topology, and Cohen's work on the continuum hypothesis—both quite far from where I ended up. Early on at Chicago I sensed unknown depths in Bob Fefferman's wonderful course on singular integrals. I learned that my understanding was superficial; I knew proofs without grasping underlying principles. The challenge of understanding more deeply, together with the example of mathematicians whose passion was so palpable, drew me in.

You've done influential work on sharp inequalities. Where did that interest come from?

Partly from early exposure to Beckner's work on the Hausdorff-Young and Young convolution inequalities—beautiful theorems about familiar objects, viewed from a less familiar perspective. A lecture by Burkholder also impressed. Constants matter in applied settings, but also in pure contexts, where they can encode structure. Those exposures lingered in the back of my mind for years. Eventually points of contact emerged with topics I knew—for instance, with Radon-type transforms—and I followed those connections. It wasn't a grand strategy; more a matter of recognizing a seam of exposed ore, and digging into it.

Recent developments have pushed parts of harmonic analysis into the spotlight. Do you have predictions about what comes next?



No. In my own work, I try to develop threads that emerge organically. I focus on problems on which I feel I might make headway.

When you're stuck on a problem—which, for most mathematicians, is most of the time—what do you actually do?

Yes, I guess I'm always stuck. The joy is in doing. I like to make progress on specific questions. It's difficult to discern when persistence becomes obstinacy. It's easy to become attached to an idea that had produced a smidgen of progress, and to keep pushing long after it's ceased to bear fruit. One needs to step back, to look around, to change approach, sometimes radically, without prematurely abandoning a promising line. I try to keep a few problems in play so as to shift perspective. But most essential is to get started and to keep going.

How have collaborations figured in your work?

I've had many collaborations of different flavors, though I also work alone quite a bit. Some collaborations involve intense back-and-forth. In others, two people bring complementary skills or perspectives, exchange what's needed, and the project is a merger of distinct contributions from both. I've had particularly fruitful interactions with people whose backgrounds were very different from mine; that contrast can be productive and fun. Michael Weinstein and Sasha Kiselev are good examples.

My interaction with Joe Kohn, though not a formal co-authorship, had an enormous impact. I've enjoyed successful collaborations with Tony Carbery, Daryl Geller, Detlef Müller, Terry Tao, Daniel Tataru, Christoph Thiele, Steve Wainger, Jim Wright, and others. There were too brief but memorable collaborations with people whose time was cut short—Jose Luis Rubio de Francia and Jean-Lin Journé.

I've collaborated formally with several of my PhD students; in the latter half of my career I consciously tried to do a joint project with each.

Collaboration brings energy and ideas one wouldn't reach on one's own. Solo work carries its own satisfactions. Both are great.

Is there a result you're especially proud of—perhaps for the idea behind it or the way it came together?

I'm particularly happy with my work on global irregularity of the Bergman projection, which took twists and turns. The question was whether the Bergman projection maps functions C^∞ up to the boundary (of a smoothly bounded pseudoconvex domain in higher-dimensional complex space) to functions likewise C^∞ up to the boundary, or whether it can manufacture singularities out of nothing. I showed the latter.

Important works of Kohn, of Fefferman, and of Bell and Ligocka had elevated this question about complex analysis in several variables to a position of intense interest. David Barrett, my fellow student for five years in Chicago and fellow postdoc in Princeton for four more, had made a striking yet inconclusive advance in the negative direction. He had shown that the projection does not map Sobolev spaces H^s to H^s for large s for particular examples, the so-called worm domains, a lovely family of domains invented earlier for a quite different purpose by Diederich and Fornaess.

I set out to establish an affirmative result, for those same worm domains, by proving a weaker Sobolev inequality, with H^s mapped to H^{s-1}. A certain density lemma would also have been needed to conclude the analysis, but as density lemmas are typically the easier step, and as the desired affirmative conclusion would imply the validity of such a lemma, the density step was initially relegated to the back burner.

This strategy may not have been wise, but it worked too well. In the end, I realized that I had an *a priori* inequality for every *s* with no loss of derivatives at all, directly contradicting Barrett—provided that the density lemma held. Therefore the density lemma had to be false; therefore the affirmative conclusion could not hold.

I had made an embarrassing error in an earlier attack on this problem, so getting it right was especially satisfying. I also have affection for some smaller, more obscure works. One loves all of one's children.

What problems are on your desk now?

I'd like to understand what I call implicitly oscillatory multilinear integrals. This year's Pedro Nunes lectures are in part an introduction to this topic. It's a modest-looking, easily formulated question, which I believe addresses a natural foundational issue in multilinear analysis, even while the significance of its applications remains uncertain. I also have a backlog of unpublished but largely completed projects which I'd like to shepherd through publication while I still can.

You've supervised many students. Do the benefits outweigh the costs?

It's not a cost-benefit calculation. It's a part of the job, an opportunity, a privilege, to help people begin to do research for themselves. I've tried to do for others what helped me, especially by finding something on which success is within reach. Early success builds momentum and one begins to feel that a subject is one's own.

Motivation is a black box. Human psychology is a mystery. I don't know how to inculcate a drive to solve problems. What I can do is model engagement, exemplify dedication to teaching at both the undergraduate and graduate levels, and try to pass along ideas.

Who are some mathematicians whom you especially admire?

Many are already mentioned above. There are too many to name, but any response must include Lennart Carleson, Jean Bourgain, Guy David, and three Berkeley colleagues alongside whom it has been a privilege to work for decades: Craig Evans, Daniel Tataru, and Maciej Zworski.

Would you like to share a personal anecdote?

My name is a result of circular reasoning, an inauspicious beginning for a mathematician. My grandfather, a child of immigrants, changed his legal name in mid-life from that of his parents, to Christ. The process required a witness, who attested to his identity. His witness was identified on the legal documents as Sarah Christ, his wife, who was born Sarah Gorman but had taken her husband's surname, Christ, upon marrying him—twenty years earlier. The name thereby presupposed itself.

We've talked about research, teaching, mentoring—and life beyond mathematics. What's the trick to keeping it all going?

I try to keep going. That's all.

Is there anything else that you'd like to add? Is there a question we should have asked?

I was delighted to have been able to refer to the 16th century mathematical work of Pedro Nunes for an example in the 2025 Pedro Nunes lectures!

I can't think of any questions that you missed. In truth, I might have preferred no questions at all.

We may decide later whether to print that. Thank you so much, Mike.

Thank you. It's been a special privilege to have known, taught, and collaborated with you, Diogo.