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An interview with Professor Camillo De Lellis

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Biographical sketch

Camillo De Lellis is a mathematician known for his influential work in geometric measure theory, hyperbolic systems of conservation laws, and incompressible fluid dynamics. Born in 1976, he earned his PhD at La Scuola Normale Superiore di Pisa, Italy, advised by Luigi Ambrosio. Among his many contributions, De Lellis is especially well-known for his modernization of Almgren's regularity theory, which relates to singularities of area-minimizing surfaces. He has received many awards for his work, including the 2013 Fermat Prize and 2022 Maryam Mirzakhani Prize in Mathematics.

Leonardo Finzi/Nina McCambridge: *In this interview we want to go over your beginnings in your career, the things that you study, and your outlook on mathematics as a whole. To begin with, what made you like math? Did you always like it?*

Camillo De Lellis: I always liked it. I liked it even when I was a very small kid, even before going to school, so I can't really tell you when I got fascinated with it. It was sort of part of my life since I was very very small. What thrilled me to actually do it professionally? I guess I just liked it along the way and I got better and then at a certain point I felt it was my call...

I always found it amazing that, you know, I could win an argument, even with somebody who was an adult. I mean, I was like 7 years old and we would have a discussion about mathematics, and I would tell them no this is not true, no this is true. At a certain point I showed them I can solve that equation in this way, and that person would have to give up to me... even though maybe they know much more than I do. In pretty much any other subject I couldn't get that satisfaction... And probably it was your experience as well.

LF/NM: *Was there a certain question that you were interested in or was it just like the argumentative style in general that you liked?*

CL: No, just in general. For instance, I had discussions and I maybe solved an exercise in a different way. And the answer sometimes – or the reaction sometimes – was like, "Ok, but, you know, how did you do it?..." And then you explain your logic behind it and the other person [is] like, "It's a completely unexpected thing, it's not what I would have done, but it does work." In which other subjects can you get that? Almost never, right? So that was always, for me, a big highlight of doing mathematics.

LF/NM: *You've spent many years teaching and supervising theses, and you've also dedicated yourself to research. Do you prefer to concentrate on research or teach and interact with students?*

CL: I don't see it as this disjoint, actually. Certainly my interactions with my PhD students were much more about research. But some of the interactions with the undergraduates were as well, when I proposed them to read certain papers which are in certain pieces of research which were more recent. I even had an experience of an undergraduate who could prove an actual important theorem. So yeah, I like teaching, especially teaching to math majors.

For me, mathematics is very interactive. Even now when I'm doing research, even now that I am at the Institute for Advanced Study, much of my time is actually spent working with postdocs who are still young researchers anyhow. And there's a teaching process. Even though I don't have an obligation to teach actually, I still teach courses every other semester.

LF/NM: *Has your attitude towards this changed over the course of your career or have you always felt this way?*

CL: I always felt this way since I started having students, essentially.

LF/NM: *You had previously taught at the University of Zurich for 14 years, and you have gone on to the Institute for Advanced Study. How has the type of work you've done changed?*

CL: Being at a "normal" university, the University of Zurich... I had to teach basic courses at the undergraduate level, then maybe more advanced courses... then for PhD students and so on. I would go through a cycle of this, maybe for 4 years... and then I would arrive at a certain point. And then it would be time to cycle back again because we all share certain type of courses.

Of course, what changed at the [Institute for] Advanced Study, [is that] I'm not teaching. That's a postgraduate university so I'm not teaching at all. But at the University of Princeton, I'm teaching only graduate courses... If I'm teaching, I'm teaching courses that cover material closer to research. That I find more enjoyable. Maybe they're not necessarily related to my research. It could be that I am explaining a piece of mathematics which was done back in the 1930s, maybe? Maybe in an advanced undergraduate class we touch upon some of these aspects, so it's not like I'm teaching only the things which were done in the last like, you know, 10 or 20 years. Right now, I'm teaching a course on material which was invented back in the 60s, so it's 65 years old. But yeah, it got closer, so I've not been teaching calculus, say, in the last 7 or 8 years.

LF/NM: *Given that you've worked in both Europe and the United States, have you noticed a difference between math in the various countries you've worked in? For example, in terms of focus?*

CL: Not when I actually get to talk to PhD students or postdocs. I don't see a big difference there. The PhD studies are organized in a different way in Princeton than they are organized in Zurich, obviously. But, at the level of interacting with students, I would say I see that I'm still talking to them in my office, explaining research topics.

At the undergraduate level there is a big difference, yes... I don't have much experience with the UK, but in Continental Europe, the students who arrive at the university, they are more specialized already. So for instance, in the University of Zurich you would choose your major immediately. So they're already going into math and they're going into math courses whose level is typically higher than the math courses that you would see at the undergraduate level in a typical American university in the first year...

At least for the math curricula, both in Europe and in the United States, the students who actually start college probably don't have knowledge of what it means to prove a theorem. And I think that's common to both. Although in Europe, this may be a later development... When I was in high school, people like me used to learn what a proof is in high school. I mean, not in something as complicated as analysis, but maybe in elementary Euclidean geometry. So that has changed, even in Europe. Many students don't see what a proof is before getting to college.

So I think that has made a math major in college in Europe harder than it was and way harder than here. The system still starts the courses by like giving theorems, proofs, theorems, proofs, but of old, people had seen at least some of this in the high school and now they don't. At least most of them.

LF/NM: *To move on towards more of your research, you are very active in three different fields of math. These are geometric measure theory, transport equations and hyperbolic systems of conservation laws, and incompressible fluid dynamics. How did you get into them? What interests you about these fields?*

CL: I got to some of the topics in incompressible and compressible dynamics through some interests that I developed in geometric measure theory.

Certainly, in all of these areas, what interests me are solutions to these physical problems, maybe the evolution of a fluid or the formation of a soap-film, or things like that. So, one of my biggest interests is whether there's a singularity there, whether the solution is completely smooth or whether there's a singularity event. That could be for compressible fluid dynamics or incompressible fluid dynamics, or whether that is a certain type of singularity from your surface that could be for the minimal surface. And it's a common topic of interesting geometry and mathematical physics. So in that sense there is some common aspect.

But then, of course, at the mathematical level, some of these problems require completely different tools. So how did I get to them? I encountered certain questions, and I [liked] them, and together with the questions, I encountered certain people that got me interested in the questions. So a lot of this is serendipitous, I would say.

LF/NM: *When did your interest in these topics happen? Did they start very early in college?*

CL: At different stages. As a student, the topics I studied were the calculus of variations and so geometric measure theory was part of my earlier studies already. I did my PhD and some of the variational problems I encountered had some point in common with hyperbolic conservation laws, which is how I actually got into hyperbolic conservation laws.

And then in later encounters, I actually found people who asked me questions about incompressible fluid dynamics and found ties between some of these subjects, and the situation likely evolved...

Even now somehow the problems that I'm looking at have evolved compared to what I was looking at 10 years ago. So in part it's just because maybe you don't want to keep doing the same things, and so I migrate to different topics.

LF/NM: *How do you introduce these topics to students and people with less familiarity with them?*

CL: That very much depends on whether I am giving a general public lecture or I'm teaching to students who are already in math or physics.

For a general public, I try to stress the physical aspects, like an experiment with soap film or something else. Many people can do it in a very simple way and can see the formation of soap bubble. Or if they dip a wire into soapy water, which is more complicated, they're maybe going to see triple junctions, if you're lucky. In these situations, you're going to see tetrahedral singularity. I mean, mostly if I'm introducing it to people who are not in math, then I stress on the physical aspect.

If I am introducing these topics to students in mathematics, then something like geometric measure theory is appealing for the very basic theoretical questions that it poses... Like, what is the area of a surface? Well, as long as the surface is nice and smooth, we can pretty much all agree on what the concept is that we would like to use. But what should it be if it starts making some corner or if it has a cusp? Then we can all agree again. But then what if I want to measure the length of the Peano curve? You start asking these very basic questions that you typically ask in math when you have a concept, which at the intuitive level makes sense, but how do you want to formalize it? Like you want to measure, for instance, every set and I want to attach a concept of what is the area of every set. Ok, then you start entering into a question of what is the outer measure, what is the outer dimension? But is it really useful?

Then you start entering into very fundamental questions in mathematics... If you, for instance, take a subject like geometric measure theory, you see that this type of question that I'm asking you right now are the questions that mathematicians, like Felix Hausdorff or Constantin Carathéodory or Abram Besicovitch, asked back in the 20s and 30s... Often, I take a historical path and show people how the subject evolved from some kind of fundamentally natural question. Like, what is the area of the surface or what is the surface, and what does it mean for, say, a soap film to be attached to a wire, for instance? It doesn't leave holes, but what type of holes?

LF/NM: *Many of the topics you have discussed seem applied-adjacent, where you have pure mathematical concepts but you seem to think about physical concepts as well. Are you more drawn to math that has spacial or physical aspects?*

CL: No, not necessarily. I just enjoy basic and beautiful questions. Then, of course, we might agree or disagree on what a beautiful question is. Maybe it's not a coincidence that a lot of these kinds of natural and beautiful questions come from physical models or from something that you can see in nature. But not necessarily all of them.

In general, I work on a problem because I like it... and sometimes I'm saying, "Wow, ok, that really is in nature and I would like to study it." It could be something like with soap-films, but not necessarily. There is a famous conjecture in conformal mappings which is called the Brennan conjecture, and it's about the fact that a certain function has to be in L^4. And I still find it fascinating even though it doesn't seem to be connected to any physical thing (that I know of). I wouldn't be surprised if it's connected in some way, but at least from what I know it doesn't, and I still like to work on it.

LF/NM: *You have many important publications and many accolades, including the Fermat Prize, but what in your opinion has been your most important contribution to mathematics?*

CL: That is a difficult question! I would have to choose between, I think, three possibilities. The construction on incompressible Euler and Navier-Stokes of unreasonable solutions, which go under the umbrella name of convex integration, or the rectifiability theorem for the singular set of area-minimizing currents, which was a long goal for me, or the estimates for the transport equations. One of these three. I wouldn't know which to choose.

LF/NM: *How do you judge importance? Is it the effect it has within the field or within mathematics or the world in general?*

CL: I would say it's a mixture of the effect on the field, how basic I felt that the question was, and how natural and appealing the theorem is and techniques that I have introduced.

So, for instance, two of the things I mentioned I received prizes for, but there is one that I mentioned for which my co-author got a prize but I didn't. (My younger co-author did because it was part of his PhD thesis to collect the estimates for the transport equations.) I found that that is much more elementary than these other two works, but it was, for me, really a turning point in which we had a very natural way of approaching a certain particular thing. And it definitely had an impact on the literature, which is non-trivial! But otherwise I don't know.

I like these three results, they're probably the three I like most from my career, but which one is more important, I really don't know. But, for instance, the one on the compressible Euler and convex integration is way more cited than the other two, by a factor of about five. So definitely more people picked it up and read it and studied it compared to the other two. It's also in a larger subfield. But you could also argue that it's in a larger subfield because it interests more people! So it starts being a question of whether the egg or the chicken came first.

LF/NM: *How do you approach the questions that you research? Do you like collaborating with others on your research or working alone? If you like to collaborate with others, do you like joining other people in answering questions that interest them initially or do you like recruiting people to help you answer your questions?*

CL: I like to bring people into problems that I like. This might be because maybe I'm, in a certain way, lazy if I have to do something all by myself. I might lose motivation at a certain point, so I actually like to start my collaborations very early on some questions and bring people in myself.

It's also an effect of the fact that I have a lot of young collaborators and a lot of students, so you also need to give them something to do. So it's also kind of natural that I act in this way.

LF/NM: *A lot of the work you do is improving proof techniques, so presumably, the ideas you've created, you see them being used a lot in future research. In what particular ways do you see this happening and what questions would you like to see answered?*

CL: When I see a piece of mathematics which is you know very long and complicated, I feel there's a need for some further explanation. Conquering certain problems is a bit like conquering the peak of a mountain.

There's much more about math than just knowing that something is true and something is not true. For example, in the famous problem of the *Bridges of Königsberg*, which asks if you could take a tour of Königsberg without taking the same bridge twice. It's a problem for which you can find possibly many other solutions. I mean, it's a finite problem, so you can probably numerate all the ways you can possibly go around. Then that will answer that this is not possible. But that's not ultimately what interests you. So I often argue with people outside of mathematics that even though mathematics seems to be complicated, at least the aspiration is to make things simpler and more accessible. If you look at getting a math problem, you're getting a concrete answer on how much money [you] have to spend to buy [some] amount of food, which is a simple multiplication. But if I didn't teach you what multiplication means, coming up with the answer is more complicated than once I teach you that.

In a sense, I still see mathematics as striving for a simpler answer than what we have now. So improving the proofs of theorems which already exist is definitely an important part to me.

Which type of questions I would like to see answered... If I could pick the brain of someone who knows *everything*, then I would like to know whether Navier-Stokes forms a singularity in finite time or not. I could be more convinced that there exists a proof that the tangent that comes to minimal services are always unique. I'm reasonably convinced it's true, so my question would be how the hell do you actually prove that, more than just getting the answer. Those are definitely two questions which I would be very curious about.

LF/NM: *Which questions do you think deserve more attention?*

CL: Navier-Stokes has a lot of attention at the moment. Does it deserve that much attention? I think it's difficult for the person who's living the history to really make an informed judgment about that.

I think if you look at the past and the history of mathematics, there have been problems which were thought of very highly, and rightly so, but then there were some other problems which were over-looked and actually generated all sorts of fantastic developments.

I wouldn't really know how to answer that question. I mean, definitely they seem to be fundamental questions together with others that I could come up with. Which one deserves more attention, I don't know.

LF/NM: *Moving on to your outlook on mathematics as a field, you have worked on problems like the Plateau problem and have said that these types of problems give* *deep insight into fundamental definitional questions. What benefit do you find in looking back on those definitions after you've gone down a certain path in research?*

CL: Historically, in that precise context, you see all sorts of tools which were developed to understand, for instance, the Plateau problem which were used in a lot of different situations. If you look at that particular problem you will realize that, for instance, some of the things that people do in geometric analysis, or in Ricci flow and the study of singularities that inevitably led to the resolution of the Poincaré conjecture, a lot of it is inspired by the work on singularities which existed in geometric measure theory to solve the Plateau problem...

For the Hölder regularity of elliptic PDEs with the bounded coefficients, what actually happened is that he was told by a colleague about the problem... and [Ennio de] Giorgi immediately realized that some of the things that he was developing for the Plateau problem, he could actually export.

When I see that particular problem, there's really lots of mathematics which has been developed there and my impression is that because the questions that one is dealing with have such a fundamental, beautiful, and basic nature, they actually end up shedding light in a lot of other problems nearby. I mean, the fact that you're dealing with a natural problem which is appealing and trying to understand basic concepts, very likely it's going to have an effect on nearby fields. Maybe even faraway fields.

But of course, you don't have a crystal ball, so all of this is somewhat speculative. It might be that your mathematics is beautiful, but maybe you'll find that finally it just solves that little piece of mathematics and nothing else. But I think if you look at the history of the subject, you will most likely not find examples of the phenomenon that I'm saying. If you're dealing with a very fundamental question or with something which looks very basic, and if you're able to make a contribution over there which is substantial, it will probably have some consequence somewhere else.

Ultimately, it's not factoring into the way I actually choose the problems that I work on, if I have to tell you the truth. I essentially choose the problems I work on if I'm having fun working on them. It's really that, for me at least, doing research in mathematics should be a pleasure, because otherwise I don't see the point in that.

LF/NM: *Do you believe that people can only make progress in mathematics if they have a genuine interest in it?*

CL: I don't know, I used to think that way. For instance, in a similar situation, if you're an amazing tennis player then is it because you must love tennis? I'm not completely sure. You have to be good at tennis as well but, for instance, in the biography of [Andre] Agassi, it seems to imply that, yes, much of it actually was not really fun but more like the competitive nature.

So I find that in part maybe I am like that, but definitely that is not the main driver of my research. Certainly competition is an important drive for a lot of people, and I'm pretty sure you will find some mathematicians for whom that's probably kind of the main drive... But at least for me, it's more like you're a painter or a sculptor, and you're painting because you like to paint that. I still think of myself as some kind of artist, so I only do things because I enjoy them. And for me, that's the main driver of my research and how I actually pick my topics. Then it's probably no coincidence that [I like] the questions that look more natural. That somehow the aesthetics are driven by something which makes those contributions maybe fundamental at some other level.

LF/NM: *Finally, what are you hoping to achieve with your lecture tomorrow?*

CL: What am I hoping to achieve with my lecture tomorrow? First of all, if a good half of the audience doesn't sleep through the lecture, it's a success!

I just hope to [arouse] the curiosity of people along certain particular aspects, and maybe if there's a message, just to be open-minded about what happens in mathematics. You will see me quoting the reaction of past famous mathematicians to something and see how even though these were the very best mathematicians of the time, and the people that right now we would think of as idols, they still got it wrong at the very basic, fundamental level. But if you see *when* they get it wrong, it's because they seem to get emotional about something and they get it wrong about the unexpected. Ok, the unexpected is always scaring you, in your life and also of course in your professional life.

If there's a message that I want to pass on with the lectures... people could be more open-minded at all levels– certainly in mathematics, but maybe even in their normal life.

In September 2025, De Lellis delivered the 2025 Edmund R. Michalik Distinguished Lecture hosted at the University of Pittsburgh.

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