# Interview with Giovanni S. Alberti

Marie-Therese Wolfram and Marc E. Pfetsch

*Giovanni S. Alberti (born 1987) studied mathematics in Genoa and obtained his PhD from Oxford University in 2014. He was a postdoc at École Normale Supérieure Paris (2014–2015) and at ETH Zürich (2015–2016). Since 2016 he has been a professor at the Department of Mathematics of the University of Genoa, first as an assistant, then an associate (2022), and since 2024 as a full professor. He is a member of the Machine Learning Genoa Center (MaLGa). He received the Eurasian Association on Inverse Problems Young Scientist Award in 2018 and the Calderón Prize in 2025. His research interests lie in the analysis of PDEs, inverse problems, functional analysis, applied harmonic analysis, wavelets, compressed sensing and machine learning. He is currently heading an ERC Starting Grant (2022–2027).*

Giovanni S. Alberti in Oberwolfach, 2023. (Photo credit: Archives of the Mathematisches Forschungsinstitut Oberwolfach)

This interview is part of a series in which ERC grantees are asked about their experiences and research.

Marie-Therese Wolfram: *Let us start with a question that I 'stole' from Martin Hairer: If you had a mathematical wand for fulfilling your wishes, what kind of result would you like to prove?*

Giovanni S. Alberti: I'm an applied mathematician and somehow applied mathematicians try to put together real life and abstract mathematical results. Those two things don't typically go too close together, in the following sense: Theories can be beautiful and perhaps very tough, but they often work in simplified settings. Reality is not always like this. My background is mostly in analysis, I work a lot in function spaces and I try to use those techniques to understand signals in the real world. Function spaces typically work very well for smooth functions and smooth signals. But as soon as you have signals with singularities, the problems become much tougher to analyse. Nowadays, people just use machine learning and neural networks, which can represent real world signals very well. But here the theory is very limited. On the other hand, if you use old style techniques and function spaces, then you know everything about them. But they don't match reality too well.

So if I had this mathematical wand, I think I would try to put these two things together to be able to fully understand models that are very close to reality from the theoretical point of view.

[MTW]: *You say your research examines simplified problems and reality is much more complicated. So in a way you would like to make your models more realistic and the analytical toolbox more advanced.*

GA: Yes. Consider, for example, Shannon's theorem: if you have a bandlimited function, you can sample it at a certain distance, and then you're going to be fine. However, signals in real life are not bandlimited. So what do you do? One popular technique was compressed sensing, where people considered sparse signals. This means that you have only a few nonzero coefficients. But these techniques have become completely obsolete with the use of machine learning. However, we still understand very little about that, and so the question is whether it is possible to have a nice model for functions that is on the one hand fairly close to reality,


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like perhaps neural networks aim to be, and on the other hand is also fully understood from the mathematical point of view.

[MTW]: *Coming back to the use of neural networks. You said that neural networks now outperform traditional approaches. I find this surprising. For example, I come from a PDE background and I have colleagues who work on sophisticated finite element solvers for PDEs, which is the 'classical approach.' Nowadays, people use neural networks to solve PDEs. But to be fair, they don't do very well in many applications. I'm not saying that using neural networks is per se bad. But are they really so much better?*

GA: If you think about PDEs, for example the Poisson equation in 3D with a certain source, then most likely a finite element solver would perform very well. It's linear, simple and fairly quick as well. But let's think about a much more complicated PDE, maybe time dependent, very high dimensional, and perhaps involving particles. In that case, classical solvers may take hours or days to solve the problem, while networks trained with training sets perform much better. The idea is that classical solvers and also classical function spaces are completely agnostic to the actual setting: If you solve a Poisson equation with finite elements, any source in the right-hand side will work. What neural networks do, and that's their power, is that they capture additional structures in the data. For example, with weather forecasts, your initial data will have some structure. This cannot be modelled by using classical function spaces, say Sobolev or Besov, which are well-designed to quantify smoothness. Neural networks, especially convolutional neural networks, have the power to better represent those structures.

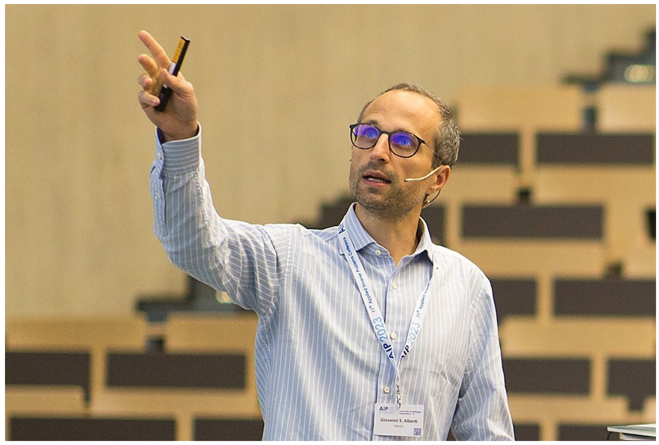
[MTW]: *At the same time, if you then throw something at the neural network that it doesn't expect, it will probably perform very poorly.*

GA: Indeed. That's another discussion, the topic of generalisation: How neural networks are able to go beyond what they have learned.

[MTW]: *What do you think about AI in general? How will it change mathematics?*

GA: First, most of my research is still outside of AI and machine learning. It seems I am one of the few people doing inverse problems and applied harmonic analysis who haven't made AI their main business. Because I still like old-fashioned topics. I believe that AI will probably be useful for mathematics in the sense that it may give us tools for proving things and for developing ideas that are difficult. But this is something that I really don't know anything about.

On the other hand, one can use mathematical techniques to analyse AI. Mathematical analysis can be used to study the properties of the functions that appear in neural networks. This is, of course, oversimplifying things. Another key issue in mathematics is stability. If you change the input to your problem a little bit, what happens to the output? With neural networks stability is a big topic. So I suppose that in that case, mathematics can be useful.



At the Applied Inverse Problems 2023 conference in Göttingen. (Photo by Dr. Markus Osterhoff)

Marc Pfetsch: *Can you tell us a bit about your ERC project?*

GA: The title of my project is "Sample complexity for inverse problems in PDEs." So, instead of solving a PDE, you either know the solutions or something about the solutions, for example, boundary data, and you want to infer the coefficients of those PDEs. Classically these problems have mostly been studied using PDE theory or computational techniques.

Since most of the problems do involve signals, my project aims at putting signal analysis and applied harmonic analysis together with PDE theory. We use techniques of sampling and compressed sensing and also machine learning combined with PDE theory.

Consider the following simplified problem for an elliptic PDE: you want to find its unknown coefficients from its solution. You can either study this problem from a PDE perspective, or you can think about the map from the solution to the coefficient as a map between function spaces. One question could be: What if I know something about my unknown? What if it has some additional structure? Can I recover it with a lower number of measurements? This type of questions can be addressed by PDE techniques, but the assumptions you make on the unknowns are more common for applied harmonic analysis and sampling theory or compressed sensing, where you put assumptions in terms of sparsity.

[MP]: *You mentioned compressed sensing, which used to be very popular in the past, right? I think it somehow slowed down – where do you think this field will go?*

GA: I think that 99% of the people who used to work on compressed sensing type problems, are now working on machine learning problems. Compressed sensing research seems to have reached such a level of maturity that there is not much to say any more. But my feeling is that a lot of those ideas haven't been used in interesting settings. This observation is one of the key aspects of my ERC project.

The main idea of compressed sensing is to reduce the number of measurements by using a priori assumptions on the unknowns. This key idea could very well be applied to inverse problems. How do I leverage some a priori knowledge I have on my unknowns to reduce the number of measurements? Unfortunately, you cannot do this with the current compressed sensing methods. So what my project aims to do is to generalise, extend and possibly develop new compressed sensing results that are general enough to be applicable to inverse problems in PDEs.

Let me just give you one example. One of the main applications of compressed sensing is magnetic resonance imaging, which is based on the Fourier transform. The other very important inverse problem in computerised tomography is based on the Radon transform. To our surprise, there was not even one theoretical paper explaining why you could use compressed sensing for the Radon transform. That's something we finished about a year ago.

[MTW]: *There is a lot of research going on in medical imaging. You said you had some theoretical results? Do they help to improve the performance and results of algorithms in medical imaging?*

GA: Compressed sensing techniques have been applied in practice, for example in tomography. But there was not even a single theoretical result saying: If you assume that your signal is sparse, then you need a number of measurements that is related to this sparsity. There simply was no result of this kind for the Radon transform. Regarding magnetic resonance imaging, the answer is certainly yes: the theory of compressed sensing has indeed improved the performance.

[MTW]: *So how did you get the idea for the ERC project?*

GA: It was a slow process. It wasn't like I woke up one morning and said 'OK, I will write an ERC proposal.' As I said, I had studied inverse problems in PDEs in my PhD. Before, I had focused on wavelets in my master's. During my postdocs, I started exploring the idea of putting these two fields together, developing my own research path that wasn't explored by other people. And we realised that there were a lot of open questions. So I said, well, maybe I should apply for an ERC.

[MP]: *If you look back at the application process. Are there any special things that you noticed? Do you have any advice for colleagues who want to apply?*

GA: I suppose that if you are a top-class mathematician, then maybe you don't have to worry about the things I had to worry about. For me, it took a long time to design the project. It took maybe a couple of months to actually write it down and polish it. But the actual design of the project, I think that was the most important part. I prepared the story of the project with some slides which I would share with friends, colleagues and other people. This really helped me structure the whole thing as well as possible. But I had to apply twice. The first time was during COVID and I passed the first round. There were no interviews and I didn't get it. But then, since I passed the first stage, I could resubmit a revised version the following year. It was based on the feedback I received in the first round, and this gave me quite an advantage, I guess. When I got to the interview for the second time, I already knew a lot of the possible criticism.

I think the key aspect is time. You cannot just wake up one day and apply for an ERC. It's a long process. I would say that it takes at least a year from the moment you start to think about it to the moment when you press the submit button. I think a year is a reasonable time. You shouldn't rush.

[MP]: *I can imagine that some of the feedback is also contradictory. The more people you ask, the more different opinions you get, right? And they are all valuable, but you cannot follow all. I mean, it's a limited space, you have limited time. How did you deal with this?*

GA: Well, absolutely. This is what you also see from the referee reports. I don't know what the panel does, but I suppose that they have to find a midpoint somewhere.

I can give you one example. I didn't have much experience in machine learning. I had maybe two or three papers in machine learning in good journals and conferences. But still it was not my main topic. So one issue was: Do I decide to make machine learning a very minor thing in such a way that people cannot say "you're not an expert"? But then the problem by doing this is that people can say "what are you doing? It's not 2010, we are in 2022." I mean, you cannot say you do only compressed sensing, right? And so I had to find a way in between. What I did was to acknowledge that my background was not 100% from machine learning, and I included a very strong machine learning colleague in Genoa as an additional collaborator. This is what I did, but I don't think that there is an easy solution to this problem.

[MTW]: *Let us change subjects a bit. When and how did you decide to study mathematics?*

GA: During high school, I liked mathematics. But I also liked physics and more practical things such as engineering. In Italy, the system allows you to sign up for a university degree until the very last moment. I decided five days before the start of the classes. During the summer after my final high-school examination, I bought some books on physics, mathematics and other topics. I read those books and realised that I found mathematics easier than the others. This opinion has become stronger and stronger over the years – a lot of people believe that mathematics is hard, but I really think the opposite: Mathematics is simple. It is based on very few rules and as soon as I have to think about physics, I find it much more complicated. And if I think about non-STEM disciplines like economics, politics or medicine, I find those even more difficult. That's why I studied maths – because it's easy.

[MTW]: *And why not pure maths?*

GA: I started as a pure mathematician. I did my whole bachelor and master mostly as a pure mathematician. But I took some courses in applied maths, and I was always fascinated with things that are somehow in between. That's where I would put myself now. If you are a pure mathematician you would think I am a very applied mathematician. If you are an applied mathematician, you would think I am pure. So I would think I'm somewhere in between.

[MTW]: *Where do you think applied maths should go?*

GA: I don't know. I mean, as I said a couple of times, I am not too keen in following what everybody else does. I cannot say anything about what is going to happen in ten years. It is possible that nobody is going to be using neural networks any more. I don't know. So I don't know where applied mathematics is going.

[MP]: *What do you think about the surrounding fields of mathematics? I think applied mathematics is quite successful, and there is also a downside because the surrounding fields pick it up in their own fields. They also do mathematics, so it is hard to distinguish a paper written by people from signal processing for people in maths. What is your view on this? Is it important to keep mathematics together? Is it good that it somehow distributes?*

GA: I like that mathematics distributes and it's used by others. But I also believe that a deep knowledge of the mathematical abstract theory is important. What I mean by this is that of course you can do numerics of PDEs, for example, without knowing basically anything about functional analysis or weak formulations. You just discretise the PDE and solve the linear system. But still, I do believe that you need a deep knowledge to understand the behaviour of those methods.

I think this applies everywhere. For example, if you want to invert the Radon transform, you can just apply the classical filtered back projection. But it's important to understand the involved function spaces, what it means that the Radon transform is ill posed and that its inverse is discontinuous. Only then you can fully understand these problems.

Giovanni S. Alberti during his acceptance speech for the Calderón Prize at the Applied Inverse Problems 2025 conference in Rio de Janeiro. (Reproduction: FGV EMAp, Photo by Ana Santiago)

I am happy that people from engineering use mathematical tools. But I think that for us, it is very important to defend the importance of the theory in understanding those phenomena.

[MTW]: *This is a very beautiful answer.*

[MP]: *How would you value the cultural background? I have the feeling that mathematicians can be identified easily after saying a few sentences. Maybe an electrical engineer would use very different language. Do you think that this difference is important?*

GA: I wouldn't say that we are better than others, of course. But what I like about maths is that the abstraction allows mathematics to be much more general than other fields. So I think the key cultural advantage of mathematics is that with basic knowledge and tools we are able to understand and analyse phenomena of very different kinds. Maybe an engineer struggles to achieve this.

As a side note: I'm not an expert in mathematics education. So I don't know what's most useful for students from primary and secondary schools. But nowadays, at least in Italy, there's a trend to say we should avoid mathematical abstraction: Let's just make everything concrete and give many examples. Of course, I understand and I value examples and practical things in mathematics, but I do also value its abstraction.

[MTW]: *In a way I do agree. At the same time, I am often quite struck how engineers come up with ideas that work. They often have great intuition. They might not be able to tell you why on earth this works, but how do they come up with that? I am not defending or judging. I just think both approaches have their own merits.*

[MP]: *I would like to test your opinion on something related. I fully agree with the abstraction point, but that also has a disadvantage. Namely, that then mathematics is often hidden behind the application. Often only the applications are visible, at least to the outside. The groundwork that we do is not visible any more. This fact is also reflected, I think, in funding. If you look outside the ERC on the European level, it's all tied to applications and not to fundamental research. So what's your view on this?*

GA: That's a tough question. You should have sent this to me before. [Laughs.]

It makes me think that most parts of my ERC grant (I applied to the mathematics panel) are about theorems and understanding phenomena that I will certainly not use in practice. But whenever I explain my ERC project to people who are not experts, I would just start with medical imaging and other things, because that's the way people can understand it.

It is important for people to understand that the elementary mathematics we study at school, for example arithmetic and geometry, play an important role everywhere in our lives: those basic building blocks are fundamental and important. Now, the world, and the science with which we describe it, have evolved a lot. Then, in the same way, I argue that the new building blocks for this understanding are given by pure mathematics, as both a tool and a language. But having said that, I have no definite solution to that.

[MTW]: *What career advice would you give if you look back?*

GA: One simple and possibly obvious advice is to enjoy yourself as much as you can. Try to find the fun in what you do and don't look at it only as a job.

Maybe one thing you don't hear so often is the importance of independence. Even as a PhD student and especially as a postdoc, you should gain independence from your supervisor soon. I think it is important to distinguish yourself from your supervisors and to find your own directions.

I've always found this important for two reasons. First, because in this way what you do is different and so people will not identify you as the student of X or someone who has done similar things that Y has done. Moreover, this is the only way you can actually do really new things. You can try to explore new directions that others haven't tried.

Another advice is about communication of science: I think that young, but also senior researchers, tend to focus more on what we do, and not much on how we write things and how we explain things to others. I believe that it is very important for young PhD students and postdocs to understand the value of writing good papers. That the readers say: "Ah, I understand this." It is also very important to give good talks, in which the audience understands what's going on. People don't pay too much attention to this, especially in pure maths. I've listened to many talks and typically the focus is only on the results. So, if the theorem is beautiful 'that's it.' Perhaps 'that's it,' but it should be well communicated.

[MTW]: *What has helped you in the past to develop this independence?*

GA: In my case, what happened was that one day at the end of my second year of PhD, my supervisor comes to me and tells me: "Next year I will be in Paris [I was in the UK]. So we will meet three or four times and that's going to be it." So that's what helped me in my independence. Of course, it was an extreme approach. [Laughs.]

[MTW]: *Did you then collaborate more with other people?*

GA: Yes, for instance at conferences, I would meet people and start side projects, but maybe more as a postdoc.

[MTW]: *For the academic writing, I think, it's nice advice. But learning this is not very easy.*

GA: Well, you know, the modern machine learning approach would be to use a classification approach. So you see many examples, many bad papers and many good papers. You see many talks, many bad talks and many good talks. And from those examples you learn. But I agree it's not easy.

[MP]: *But if you don't have the motivation to do it well, then it will not happen.*

GA: Right, absolutely. Perhaps the risk is that the motivation stops whenever you finish the proof of the theorem. Then you just write it down and see if it's correct.

After I obtained my ERC project, I've read a few proposals of colleagues. In some cases, these were written by mathematicians who are much stronger than me and the projects themselves were, mathematically speaking, excellent. But if you write them in such a way that a reader falls asleep after the first page, then you probably don't get an ERC.

[MTW, MP]: *Thank you for the insightful interview.*

Marie-Therese Wolfram is currently a full professor at the Warwick Mathematics Institute. Before that she held research positions at the University of Vienna, the Radon Institute of Computational and Applied Mathematics in Linz and the University of Cambridge. In 2023, she received the London Mathematical Society Whitehead prize 'for her groundbreaking contributions to applied partial differential equations,

mathematical modelling in socio-economic applications and the life sciences, and numerical analysis of partial differential equations.'
Since 2022, she has been member of the Committee for Applications and Interdisciplinary Relations (CAIR) of the EMS.

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Marc Pfetsch obtained his PhD in 2002 at TU Berlin, where he was a teaching assistant from 1998 to 2002. He then was a postdoc at Zuse Institute Berlin from 2002 to 2008. In 2008, he received his habilitation in mathematics at TU Berlin. From 2008 to 2012 he was a full professor for mathematical optimization at TU Braunschweig. He has been a professor for discrete optimization at TU Darmstadt since 2012. His research fields include compressed sensing, mixed-integer nonlinear optimization, symmetry handling in optimization and energy networks. Since 2003, he has been involved in the development of the now open-source solver framework SCIP. Since 2018, he has been member of the Committee for Applications and Interdisciplinary Relations (CAIR) of the EMS.

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