Enumerative Combinatorics and Applications
ecajournal.haifa.ac.il
ECA **6:1** (2026) Interview #S3I1
https://doi.org/10.54550/ECA2026V6S1I1

Interview with Christian Krattenthaler

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Professor Christian Krattenthaler is a distinguished Austrian mathematician and (now retired) Professor of Discrete Mathematics at the University of Vienna. He is internationally recognized for his groundbreaking work in algebraic and enumerative combinatorics, with influential contributions to determinant calculus, plane partitions, lattice paths, and their surprising applications in physics and special functions. His survey *Advanced Determinant Calculus* has become a central reference in the field, shaping a generation of research. Professor Krattenthaler has received numerous honors, including the Prize of the Austrian Mathematical Society (1990) and Austria's most prestigious scientific award, the Wittgenstein Prize (2007). He is a Corresponding Member of the Austrian Academy of Sciences, a Member of the Academia Europaea, a Fellow of the American Mathematical Society, and holds an honorary doctorate from Université Sorbonne Paris Nord. Beyond mathematics, he is also a trained concert pianist, maintaining a lifelong passion for music alongside his research career.

Mansour: Professor Krattenthaler, first of all, we would like to thank you for accepting this interview. Would you tell us broadly what combinatorics is?

Krattenthaler: Who would be able to do that? Moreover, there would even be split opinions whether some subjects — like graph theory or combinatorial optimisation — are part of combinatorics, or whether they are subjects on their own.

I am certainly unable to tell, even broadly or roughly, what combinatorics is. I claim that nobody can. And it is good that it is not really possible to "define" what combinatorics is, in the same way as it is impossible to "define" what mathematics is.[1]

What I can do better is to say what I think belongs to combinatorics. If there are families of objects, no matter where they come from — algebra, representation theory, topology, computer science, physics, . . . , and it is of interest to count them or learn more about their structural properties, then I consider this as part of combinatorics. In general, my idea of combinatorics has always been as a wide field that integrates a diverse collection of subjects — more or less as the Mathematics Subject Classification does — that certainly include graph theory and extremal combinatorics, probabilistic combinatorics, algebraic combinatorics, designs, association schemes, and combinatorial geometry. Again, this is not meant as a "definition" or in an exclusive sense.

Mansour: What do you think about the development of the relations between combinatorics and the rest of mathematics?

Krattenthaler: Indeed, combinatorics has undergone a remarkable development over the

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[1] In the current version of Wikipedia, we read: *"Mathematics is a field of study that discovers and organizes methods, theories, and theorems that are developed and proved for the needs of empirical sciences and mathematics itself."* Not wrong, but do we now know what mathematics is? Certainly not, in particular given the fact that this "definition" is circular.

past, say 50 years. Still in the 1960s and even 1970s, it had not been recognised as a subject of its own, and the general recognition of combinatorics was low still into the 2000s. I get the impression that this has changed now, even if combinatorics may not be ranked among the "most important" (whatever that may mean) subjects by many people working in these "most important" subjects. On the surface, this change in perception of combinatorics is witnessed by the Fields Medal of June Huh, or the Abel Prizes of Endre Szemerédi and László Lovász. However, it seems to go deeper as it is now more and more acknowledged that also in combinatorial theory there are deep results whose impact goes far beyond combinatorics, even if there is no streamlined theory as, say, in analysis. (On the side: I consider this variety as part of the attraction and fascination of combinatorics.)

Mansour: We would like to ask you about your formative years. What were your early experiences with mathematics? Did that happen under the influence of your family or some other people?

Krattenthaler: It did not happen under the influence of my family.

I was always good in mathematics in school, and, from the beginnings in elementary school, I liked mathematics. However, like probably anybody else, by the age of nine or ten it was "clear" to me that I would become a medical doctor and do a lot of good things for mankind. Somehow, that early fixation went into the background later on.

I liked computations, equations, even the geometry of the early years of high school (that is, elementary geometry; more on "even" later). When I was in grade 9, one of the mathematics teachers at our school (he had in fact a Ph.D. in mathematics) started to offer special lessons and exercises in mathematics which one could take on a voluntary basis. I did that. (I remember that we did intricate integrals, for example.) From grade 11 on, this turned into a preparation course for the Mathematical Olympiad. So I participated in the „Anfängerwettbewerb" ("beginners' competition") and in the last year of high school in the Austrian Mathematical Olympiad. I won a silver medal[2] and thereby qualified for the International Mathematical Olympiad 1977 in Belgrade. (Not very successfully so ...[3])

Mansour: What was the reason you chose the University of Vienna for your Ph.D. and your advisor, Johann Cigler?

Krattenthaler: These are several questions in one. (I know that you could not know, but I will explain.) So let me answer them one by one.

As we discussed earlier, I had some success in Mathematical Olympiads. Furthermore, I had also won some prizes at piano youth competitions. Therefore, it was "obvious" that I would go for studies in mathematics on the one hand and piano on the other hand. My father was very "excited". (Translation into down-to-earth terms: he was not happy at all about the idea of studying two subjects with a low perspective of finding jobs.) But he was pragmatic. He told me that, in the end, I will have to earn my living myself. He said: please go ahead, but be aware that things may not work out as you wish.

Now, living in the Vienna area, it was quite obvious to do your studies in Vienna. If you want to study mathematics, there exist two options: the old,[4] classical University of Vienna, or the Vienna University of Technology.[5] Was I a very practical person? Right. So much for this choice.

The next thing we must know is that in 1977, when I started my studies, the study regulations for studying mathematics were very simple: the goal was to earn a Dr.Phil. There was no Diplom, Bachelor, Master, or whatever, the goal was to earn a doctorate. And to achieve this, one had to write a Ph.D. thesis, which had to be approved, and then

[2]For non-olympians one has to explain that — in difference to Summer and Winter Olympic Games, where the first ranked competitor gets a gold medal, the second ranked gets a silver medal, the third ranked gets a bronze medal, and everybody else gets nothing — at Mathematical Olympiads several gold medals, silver medals, and bronze medals are awarded, according to certain percentages. In the current case, I was indeed ranked second, ex aequo with another participant, who was also awarded a silver medal. To tell the truth, I struggled with the geometry problem — geometry being my weak point at olympiads. Luckily, across a corridor, I could have a glance at the drawing of the very best among us (who was awarded the only gold medal) which gave me the idea of what to do.

[3]Rank 108 out of about 150 participants

[4]The University of Vienna was founded in 1365

[5]The Vienna University of Technology was founded in 1815.

pass a comprehensive exam.[6] That was it.[7] Of course, in practice, you had to do exams in Analysis, Linear Algebra, Number Theory, Complex Analysis, Algebra, Differential Equations, Differential Geometry, Probability Theory, Topology, ..., attend seminars, etc., because otherwise no professor would have accepted you as a Ph.D. student.

At this point, we have to go back to my high school times again. When my mathematics teacher (who held a Ph.D. as well) learned that I wanted to start studies in mathematics, she told me that, at the Mathematics Department of the University of Vienna, there is a mathematics professor, Johann Cigler,[8] who is her brother-in-law. She recommended warmly that I attend courses by Cigler.

Next, we must recall that the Mathematical Olympiads are competitions for high school students. Consequently, the problems that are posed in these competitions must be solvable by methods that are (more or less) taught in high school. In practice, this means that the problems come from the areas of (elementary) geometry, (elementary) number theory, combinatorics, functional equations, and inequalities, in particular, leaving out any calculus (that is, differentiation and integration). Not being a particular friend of geometry, I was most attracted by number theory. Therefore, I thought that I would write a thesis in number theory. And indeed, in my first semester, Edmund Hlawka,[9] already then a legendary number theorist, gave a course on elementary number theory, which was followed in the next two semesters by topics courses in number theory covering large parts of analytic number theory, all of which I followed. What I learned in particular was that (analytic) number theory was a highly sophisticated, intensely studied subject in which it was not so easy to even find a problem that would be suited for a thesis.

On the other hand, I had the advice of my high school teacher still in mind. In my second semester, Johann Cigler taught a course in combinatorics. I have to mention that Cigler was a number theorist by education (thesis advisor: Edmund Hlawka), but had done many other things (functional analysis, harmonic analysis, category theory) since then. In 1977, Cigler was about to change subject drastically. He had seen the papers by Gian-Carlo Rota on umbral calculus. Rota's idea of bringing order into the "chaos" ("bag of isolated tricks") of combinatorics by organising large parts with the help of algebraic theories appealed to him a lot. This is what he tried to convey in this course (poset and lattice theory, incidence algebra, Möbius function and Möbius algebra, umbral calculus, etc.). This also appealed to me. I attended several other courses given by Cigler and soon his seminars. In these seminars, Cigler frequently picked new publications (very present in my memory is Egorychev's book[10]) and presented them in his personal view and with his thoughts on the material. Frequently, he would develop this further and pose problems that the students should look at. I did look at them. Over time, I had solved several of them. At a certain point, I showed Cigler what I had collected. He looked at it and gave me a few more references that I should consult. I did that and added what I found out about them to the existing text. This made my thesis[11] on „Lagrangeformel und inverse Relationen".

Mansour: Your survey *Advanced Determinant Calculus*[12] is widely cited and influential. What inspired you to work so deeply in this direction?

Krattenthaler: This is again a longer story, where I am not entirely sure of the exact origins. What I do remember is that, soon after my thesis, I got very interested in the enumeration of plane partitions. How and why? This is the part that I do not remember exactly. It had certainly to do with the "q-disease" with which Cigler had infected everybody in the

[6] the so-called „Rigorosum"

[7] These regulations dated probably from the 19th century, if not longer back. 1977 however, was the last year when these regulations were in force. Starting from 1978, one had to first make the Diplom before entering doctoral studies. Still no Bachelor's, no Master's degree! These were introduced in the early 2000s only.

[8] Interview with Johann Cigler, Enumer. Combinat. Appl. 5:3 (2025), Interview S3I3.

[9] of the Minkowski–Hlawka theorem fame; see E. Hlawka, *Zur Geometrie der Zahlen*, Math. Z. 49 (1943), 285–312.

[10] G. P. Egorychev, *Integral Representation and the Computation of Combinatorial Sums*, Translations of Mathematical Monographs, Vol. 59, American Mathematical Society, Providence, RI, 1984. (Original Russian edition: G. P. Egorychev, Integral'noe predstavlenie i vychislenie kombinatornykh summ, Nauka, Novosibirsk, 1977.)

[11] See, `https://www.mat.univie.ac.at/~kratt/artikel/diss.html`.

[12] C. Krattenthaler, *Advanced determinant calculus*, Sém. Lothar. Combin. 42 (1999), Article B42q.

combinatorics group at the time. The main result in my thesis is in fact a q-analogue of the Lagrange inversion formula. Since one of the main applications of the Lagrange inversion formula is in the enumeration of lattice paths, applications of q-Lagrange inversion are in the q-enumeration of lattice paths. If you do that, you cannot avoid looking into the work of Percy Alexander MacMahon,[13] and once you are there, the plane partitions are also there.

Anyhow, I was fascinated by what I read in Richard Stanley's two-part survey article[14] "Theory and applications of plane partitions." There are these frequently occurring elegant product formulae for classes of plane partitions (at the time several of them still conjectural), such as MacMahon's formula for the generating function of plane partitions[15] contained in a box, or the more general hook-content formula for semistandard tableaux of a given shape and with bounded entries,[16] etc. However, to derive them was (and is) a highly non-trivial task. If one approaches these problems by the method of non-intersecting lattice paths, then one obtains certain determinants. The product formulae say that one can evaluate these determinants. I tried to reveal the general background of these determinant evaluations. Gradually, I managed to do that. I discovered that the explanation of these determinant evaluations was given by certain multivariable determinant factorisations generalising the Vandermonde determinant.[17]

Later on, I got interested in a conjecture of David Robbins and Doron Zeilberger.[18] It was formulated in terms of a constant term identity but could be converted into a (conjectured) determinant evaluation.[19] Having had some success with determinant evaluations, naively I thought that I could do this one as well. However, it quickly turned out that my multivariable determinant factorisations were of no use here. On the other hand, after further thinking, I realised that the essential idea — namely to play with a variable that is in the game — could still be applied. This led me to, what I call, the "identification of factors method" (what Kuperberg calls the "exhaustion of factors method"). From there, it was still a longer way to actually prove the conjecture of Robbins and Zeilberger, but in the end it worked out.[20]

Clearly, if you have a method at hand that nobody had applied earlier, then you try to see what else you can do with it. Right in time, Jim Propp[21] came up with his "Twenty open problems on enumeration of matchings" that contained many enumeration problems on rhombus tilings that could be attacked by "identification of factors". So, many more determinant evaluations resulted from that source, most of them published jointly with Mihai Ciucu[22,23] and/or Markus Fulmek.[24,25,26]

During this period, due to my interest in determinant evaluations, and because of the experience that I had gathered, I had started to collect interesting determinant evaluations in a list. I had in mind to write an article where I would tell how to go about determi-

[13] P. A. MacMahon, *Combinatory Analysis*, Cambridge University Press, 1916.

[14] R. P. Stanley, *Theory and applications of plane partitions*, Stud. Appl. Math. 50 (1971), no. 2, 167–188, 259–279.

[15] P. A. MacMahon, *Combinatory Analysis*, Vol. 2, Cambridge University Press, 1916 (Chelsea reprint, New York, 1960). Box formula in Sections 429–494.

[16] R. P. Stanley, *Enumerative Combinatorics*, Vol. 2, Cambridge University Press, Cambridge, 1999 (2nd ed., 2011), Theorem 7.21.2.

[17] These are the lemmas in Section 2.2 of Footnote 12.

[18] D. Zeilberger, *A constant term identity featuring the ubiquitous (and mysterious) Andrews–Mills–Robbins–Rumsey numbers*, J. Combin. Theory Ser. A 66 (1994), 17–27.

[19] The conjecture could also be seen as a generalisation of the enumeration of totally symmetric self-complementary plane partitions.

[20] C. Krattenthaler, *Determinant identities and a generalization of the number of totally symmetric self-complemetary plane partitions*, Electron. J. Combin. 4(1) (1997), #R27.

[21] J. Propp, *Twenty open problems in enumeration of matchings*, preprint (1996), math.CO/9801060.

[22] M. Ciucu and C. Krattenthaler, *The number of centered lozenge tilings of a symmetric hexagon*, J. Combin. Theory Ser. A 86 (1999), 103-126.

[23] M. Ciucu and C. Krattenthaler, *Enumeration of lozenge tilings of hexagons with cut-off corners*, J. Combin. Theory Ser. A 100 (2002), 201-231.

[24] M. Fulmek and C. Krattenthaler, *The number of rhombus tilings of a symmetric hexagon which contain a fixed rhombus on the symmetry axis, I*, Ann. Combin. 2 (1998), 19-40.

[25] M. Fulmek and C. Krattenthaler, *The number of rhombus tilings of a symmetric hexagon which contain a fixed rhombus on the symmetry axis, II*, Europ. J. Combin. 21 (2000), 601-640.

[26] C. Krattenthaler, *Schur function identities and the number of perfect matchings of holey Aztec rectangles*, in: "q-Series from a Contemporary Perspective", M. E. H. Ismail, D. Stanton, eds., Contemporary Math., vol. 254, Amer. Math. Soc., Providence, R.I., 2000, pp. 335-350.

nant evaluations, and combine this theoretical part with a list of interesting determinant evaluations. In 1998, Dominique Foata organised a special meeting of the Séminaire Lotharingien de Combinatoire celebrating George Andrews' 60th birthday. At the same time, he invited everybody to contribute an article to the corresponding special volume. With George Andrews having done some of the most intricate determinant evaluations (in the context of plane partitions), it was obvious that this was the point to implement my plan and write down the survey article that I had in mind (and to dedicate it to George Andrews). The result was "Advanced determinant calculus."

After that, I continued to (also) work on determinant evaluations and to collect interesting determinant evaluations that appeared in new publications. In 2004, I gave a plenary talk at the 11th "Conference of the International Linear Algebra Society" in Coimbra, Portugal. Richard Brualdi, one of the important persons in the International Linear Algebra Society, absolutely wanted me to contribute an article to a special issue of Linear Algebra and its Applications dedicated to Determinants and the Legacy of Sir Thomas Muir based on my talk. It was a fortunate coincidence that, in the winter of 2005, I could spend several months at the Mittag–Leffler Institut close to Stockholm. I used much of the time there to write[27] what became "Advanced determinant calculus: A complement", which indeed, in its first part, is the written version of my talk that I had given in Coimbra, and in the second part provides the list of interesting determinant evaluations that I had collected since the first article.

Subsequently, I stopped collecting new determinant evaluations.

Mansour: Among your many results in combinatorics and determinant evaluations, which do you personally consider the most surprising or beautiful?

Krattenthaler: I am afraid that I will mention too many so that you will have to stop me at some point ...

Let me start with "beautiful". I regard the bijective proof of the hook-content formula in "An involution principle-free bijective proof of Stanley's hook-content formula" as very beautiful,[28] using modified jeu-de-taquin moves in a specific way. When combined with the idea of the Novelli–Pak–Stoyanovskii hook bijection, the result is an even "better" bijective proof ("Another involution principle-free bijective proof of Stanley's hook-content formula"[29]). As for a determinant formula, "A remarkable formula for counting nonintersecting lattice paths in a ladder with respect to turns" (article joint with Maria Prohaska[30]) is indeed a very remarkable formula, however due to Aldo Conca and Jürgen Herzog, who made the conjecture. In my opinion, its proof is very beautiful and insightful since it explains the structure of the formula. Another result that I regard as very beautiful is the closed-form product formula for the number of rhombus tilings of a hexagon with a triangular hole in the centre in "Enumeration of lozenge tilings of hexagons with a central triangular hole" (joint with Mihai Ciucu, Theresia Eisenkölbl and Douglas Zare.[31]) Admittedly, the proof is less beautiful (as it is quite involved ...).

For "surprising", I want to mention "Some new formulas for π" (joint with Gert Almkvist and Joakim Petersson[32]) and "A Riccati differential equation and free subgroup numbers for lifts of $PSL_2(\mathbb{Z})$ modulo prime powers" (joint with Thomas Müller.[33]) These two papers concern two completely different problem areas, namely series expansions for π on the one hand and congruences for numbers of subgroups on the other hand. However, the situations and final solutions of the problems were quite similar in character. In the beginning, both problems looked completely hopeless. For the former pa-

[27]C. Krattenthaler, *Advanced determinant calculus: A complement*, Linear Algebra Appl. 411 (2005), 68–166.
[28]C. Krattenthaler, *An involution principle-free bijective proof of Stanley's hook-content formula*, Discrete Math. Theor. Comput. Sci. 3(1) (1998), 11–32.
[29]C. Krattenthaler, *Another involution principle-free bijective proof of Stanley's hook-content formula*, J. Combin. Theory Ser. A 88 (1999), 66-92.
[30]C. Krattenthaler and M. Prohaska, *A remarkable formula for counting nonintersecting lattice paths in a ladder with respect to turns*, Trans. Amer. Math. Soc. 351 (1999), no. 3, 1015–1042.
[31]M. Ciucu, T. Eisenkölbl, C. Krattenthaler, and D. Zare, *Enumeration of lozenge tilings of hexagons with a central triangular hole*, J. Combin. Theory Ser. A 95 (2001), no. 2, 251–334.
[32]G. Almkvist, C. Krattenthaler, and J. Petersson, *Some new formulas for π*, Exp. Math. 12(4) (2003), 441–456.
[33]C. Krattenthaler and T. W. Müller, *A Riccati differential equation and free subgroup numbers for lifts of $PSL_2(\mathbb{Z})$ modulo prime powers*, J. Combin. Theory Ser. A 120 (2013), 2039–2063.

per, Gert Almkvist had explained to me his approach to solve the problem which boiled down to showing that the determinant of a certain sparse but very complicated, big block matrix was non-zero. On the other hand, the subgroup counting problem in the second paper boiled down to computing the Padé approximants of the corresponding generating functions. In both cases, the computer data were extremely interesting. Almkvist had computed the first four determinants and observed an amazing factorisation into (small) prime factors, which indicated that there was an explicit closed-form product formula. On the other hand, the computer calculations for small cases of these Padé approximants showed compelling patterns. However, in both cases, the data were far from giving any hint on a general pattern, but, due to the high complexity of the problems, it was not possible to get more data. It was quite amazing that, by putting more and more parameters into the game, in the end we succeeded in figuring out the general pattern. These (long) processes are described in the corresponding articles. If in these two articles, it may have been the discovery process that was surprising, in "A factorization theorem for lozenge tilings of a hexagon with triangular holes" (joint with Mihai Ciucu[34]) it is the result itself that I find very surprising. To tell the truth, Mihai Ciucu had talked to me about this factorisation for a long time, but I did not really believe it until I figured out a proof . . . Before going overboard, here is a last one: take the quotient of two q-binomial coefficients with the same top argument; if this is a polynomial (in q), then it has non-negative coefficients. Would you believe that? Well, computer calculations and partial results strongly indicate that this is true. It is one of the conjectures in "A positivity conjecture for a quotient of q-binomial coefficients" (joint with Mona Gatzweiler.[35])

Mansour: Plane partitions and lattice paths frequently appear in your research. What makes them so central and rich in combinatorics?

Krattenthaler: For lattice paths, this is obvious: these are just words, unconstrained or with restrictions. Therefore, they are ubiquitous. Plane partitions are "just" two-dimensional (integer) partitions. MacMahon[13] was already obsessed with them in the years around 1900. I believe that the fascination has always been the same (at least for enumerators[36]): they are reasonably simple objects, enumeration problems seem to "almost always" have incredible solutions in terms of beautiful product formulae, but to prove these frequently turns out to be challenging up to almost impossible. Most prominent here is the programme of enumerating plane partitions with symmetries, which was initiated by MacMahon in the early 1900s, but was completed only in 2011 by Christoph Koutschan, Manuel Kauers, and Doron Zeilberger.[37]

Mansour: How do you see the interplay between combinatorics and physics – for example, in statistical mechanics or random matrix theory – evolving in the future?

Krattenthaler: I have nothing to say about the future. As we know, predictions are difficult, in particular if they concern the future . . .

What I want to say is that I am very pleased that the past, say, 30 years have seen an increasing interaction between combinatorics and statistical physics (and parts of probability theory, I must add) that has been — as I believe — extremely fruitful for both (and probability theory). May it continue in the future!

Mansour: For many of your papers, you are admired for their elegance and intricate proofs.

Krattenthaler: Thank you for the compliment. I take it as a two-fold compliment since obviously there is a kind of contradiction between "elegant" and "intricate." Indeed, I consider some of my proofs elegant, but I cannot deny that some of my results — they may even be elegant — have very intricate proofs, and these proofs are not elegant. An example for

[34]M. Ciucu and C. Krattenthaler, *A factorization theorem for lozenge tilings of a hexagon with triangular holes*, Trans. Amer. Math. Soc. 369 (2017), no. 5, 3655–3672.

[35]M. Gatzweiler and C. Krattenthaler, *A positivity conjecture for a quotient of q-binomial coefficients*, Ramanujan J. 69 (2026), Article 13.

[36]As it turned out, plane partitions have also a lot to offer for probabilists; see the arctic ellipsoid and shape theorem of Cohn, Larsen and Propp (*The shape of a typical boxed plane partition*, New York J. Math. 4 (1998), 137–165), etc.

[37]C. Koutschan, M. Kauers, and D. Zeilberger, *Proof of George Andrews's and David Robbins's q-TSPP conjecture*, Proc. Natl. Acad. Sci. USA 108 (2011), 2196–2199.

the latter is "An involution principle-free bijective proof[29] of Stanley's hook-content formula". I believe that the algorithm of the bijection is elegant. But, yes, the proof is long and intricate. By the way, the latter is the reason that it took a long time to publish it because referees would not let it through unless the proof would be similarly elegant as the algorithm ...

Mansour: How do you approach the process of finding the "right" proof?

Krattenthaler: This touches upon a very interesting point. I am indeed a big believer in "right" proofs, even though it is of course impossible to say exactly what a "right" proof is. According to my feeling, a "right" proof should somehow be adapted to the statement of the result and/or explain the form of the result or why this result is true. To mention an example: yes, it is possible to prove the Lagrange inversion formula using complex analysis and contour integrals. However, in my opinion, this is not the "right" proof. The Lagrange inversion formula is a purely formal statement, valid for formal power series/Laurent series. If you set it up right, then all that is required is that the derivative of a Laurent series in z does not contain a term z^{-1} with non-zero coefficient, which is indeed a trivial statement.[38,16]

On the other hand, I have nothing against "non-right" proofs. A proof is a proof. Many of my papers contain proofs that are not "right" (particularly the ones on cyclic sieving ...).

Mansour: And in your view, what is the role of elegance in mathematics?

Krattenthaler: Difficult question. Perhaps the only possible answer is that we mathematicians (at least myself) need it for living. If all proofs were ugly and terribly complicated, then the only attraction of mathematics that would be left would be to offer difficult problems to solve. I guess that would not be enough for me.

Important aside: as with many other things, to decide what "elegance" is is in the eyes of the beholder, respectively may need some work to be able to appreciate. Wiles and Taylor's proof of Fermat's Last Theorem[39,40] is considered/received by most mathematicians as rather impenetrable and difficult. However, here is what Wiles says in a BBC documentary[41] about the moment when he discovered how to overcome the gap that was contained in his first version of the proof: "It was so indescribably beautiful, it was so simple and so elegant ..."

Mansour: You have often devoted years to deep problems. How do you decide which problems are "worth" that investment of time?

Krattenthaler: The one thing that I can say is that I do not follow fashionable trends where some new problem was posed and now everybody is jumping on it, trying to be the first to make progress or even solve it completely. My point is that, if something can be done quickly, then probably it was not so difficult ... I leave this to others. If, after some time, there still remains an open problem, then maybe this is something worth looking at. I know that this would make me a terrible physicist, but fortunately I am a mathematician.

Second, a problem must somehow "speak" to me. I cannot really explain what I mean by that. There must be something attractive, may it be the statement, may it be the challenge, or may it be the conjectured result.

Finally, I must have some idea to attack the problem. If I have no idea what to do with a problem, even after thinking about it for some while, then I should turn to something else.

Mansour: What is determinant calculus, and why is it important in combinatorics?

Krattenthaler: As we all know, determinants arise naturally in linear algebra when we want to provide explicit formulae for the solutions of systems of linear equations. This basic fact explains that determinants are so ubiquitous in mathematics in general.

In enumerative combinatorics, determinants will necessarily appear if we deal with objects that, in some way or another, are in bijection with non-intersecting lattice paths. As Bernt Lindström[42] has shown, the num-

[38] What I have in mind is the proof that is, for example, given in Stanley's "Enumerative Combinatorics II".

[39] A. Wiles, *Modular elliptic curves and Fermat's Last Theorem*, Ann. of Math. (2) 141 (1995), no. 3, 443–551.

[40] R. Taylor and A. Wiles, *Ring-theoretic properties of certain Hecke algebras*, Ann. of Math. (2) 141 (1995), no. 3, 553–572.

[41] BBC Horizon: Fermat's Last Theorem, 1996, available online: `https://www.dailymotion.com/video/x223gx8` (quotation at approximately 42:00).

[42] B. Lindström, *On the vector representations of induced matroids*, Bull. London Math. Soc. 5 (1973), 85–90.

ber (generating function) of families of non-intersecting paths in an acyclic directed graph with fixed starting and end points is given by a determinant. John Stembridge[43] extended this to situations where starting and/or end points are not fixed, showing that the counting is now done by Pfaffians, which in the end are again essentially determinants. Consequently, whenever you deal with plane partitions, standard tableaux, semistandard tableaux, rhombus tilings, domino tilings, vicious walkers and the like, you will have to deal with determinants.

The tilings that I mentioned can also be seen as perfect matchings of hexagonal graphs, respectively square grid graphs. More generally, the number of perfect matchings of planar graphs is again given by Pfaffians or determinants, due to Kasteleyn theory. In physics language, we are talking of the dimer model,[44] and so again its solution involves determinants. We may move further to the six-vertex model, and then again, partition functions (also known as generating functions) tend to be given by determinants or Pfaffians. Thus, they appear also when you deal with alternating sign matrices and related objects.

Determinants appear as well in representation theory and Schubert calculus (Weyl character formulae), and also in the theory of orthogonal polynomials (some of which is explained by Xavier Viennot's[45] combinatorial view of the theory and, again, non-intersecting lattice paths).

So, yes, there are many places in combinatorics where determinants play an important role.

Mansour: In your work, you have extensively used combinatorial reasoning to address important problems. How do enumerative techniques engage in your research?

Krattenthaler: I may not answer the actual question. But let me say that, at the core, I am an enumerative combinatorialist. However, I do look around and try to see where else methods that we apply in enumerative combinatorics can also be applied. Which are the standard methods that we apply in enumerative combinatorics? Bijections, generating functions, recurrences, manipulating combinatorial sums and consequently hypergeometric series, orthogonal polynomials, continued fractions, algorithms for the evaluation of combinatorial sums, singularity analysis and saddle point method when it comes to asymptotic enumeration, to mention a few. This is a large set of tools and methods, and it brought me into contact with many other areas of mathematics and physics, such as commutative algebra, group theory, representation theory, number theory (so, in the end, I did also become a number theorist!), special functions, harmonic analysis, differential geometry, probability theory, statistical physics, and even quantum information theory!

Mansour: What kind of mathematics would you like to see in the next ten to twenty years as the continuation of your work?

Krattenthaler: Honestly, I don't care. I enjoy doing mathematics. I am of course pleased if other people find interesting what I am doing. However, whether my work is continued or not, this is not important for me. On the other hand, I am certainly curious which developments (in whatever directions) we are going to see in the future in general!

Mansour: Would you tell us about your thought process for the proof of one of your favorite results? How did you become interested in that problem? How long did it take you to figure out a proof? Did you have a "eureka moment"?

Krattenthaler: Let me pick a recent result that I obtained in "Bounded Littlewood identities for cylindric Schur functions" jointly with JiSun Huh, Jang Soo Kim, and Soichi Okada.[46] This is indeed an interesting story that illustrates several things at the same time: a very modest initial attempt takes several unexpected turns, and, in the end, a substantial theorem is obtained that was completely unforeseen; generalisation may make things more transparent; if more people come together, more and stronger results will be obtained.

[43] J. R. Stembridge, *Nonintersecting paths, Pfaffians, and plane partitions*, Adv. Math. 83 (1990), 96–131.
[44] P. W. Kasteleyn, *The statistics of dimers on a lattice: I. The number of dimer arrangements on a quadratic lattice*, Physica 27 (1961), 1209–1225.
[45] X. G. Viennot, *Une théorie combinatoire des polynômes orthogonaux. Lecture Notes, LACIM, Université du Québec à Montréal*, 1983.
[46] J. Huh, J. S. Kim, C. Krattenthaler, and S. Okada, *Bounded Littlewood identities for cylindric Schur functions*, Trans. Amer. Math. Soc. 378 (2025), 6765–6829.

The story is in fact told with more detail in Appendix A of that article. It starts with JiSun Huh and Jang Soo Kim trying to find a bijective proof of a theorem of Paul Mortimer and Thomas Prellberg[47] saying that the number of certain walks in a triangle was the same as the number of certain bounded Motzkin paths. Mortimer and Prellberg's proof was not bijective, it used the kernel method to verify that these numbers are the same. The first step that Huh and Kim did was to observe that, in its original form, the identity is not formulated in the most transparent form. They transformed the identity into an identity between the number of certain cylindric standard Young tableaux and certain matchings with restrictions on their crossings and nestings. In this form, a 3-parametric generalisation (the identity of Mortimer and Prellberg has only two parameters) suggested itself.

At this point, Jang Soo Kim contacted me and asked whether I had seen this identity before. I had not. In fact, when I saw it first, it looked very suspicious to me and I did not really believe it. A few computer calculations later, I was convinced. I saw that one could use Gessel and Zeilberger's[48] general reflection principle formula to write down explicit expressions for both sides of the identity. Therefore, the "only" remaining task was to prove the latter identity. I had no idea how to do that since the expressions on both sides were very complicated, and in particular very far from each other.

A "general principle" says that, by making things more general, they may become easier. (We actually applied this principle already once at the beginning.) A special case — so to speak — of that principle is that, whenever one encounters an identity related to standard Young tableaux, then there should exist a more general identity for semistandard tableaux! In practice, this means that there should be an identity for symmetric functions generalising our identity. This principle is based on the simple fact that a multinomial coefficient $\binom{n}{m_1,\dots,m_k}$ can be interpreted as the extraction of the coefficient of $x_1 \cdots x_n$ from the product $e_{m_1} \cdots e_{m_k}$, with e_m denoting the elementary symmetric function of degree m in $x_1, \dots, x_n$. For our identity, this was easy to implement for both sides of the identity. While that could have produced nonsense, the computer said (in small examples) that this symmetric function generalisation of our identity was true.

This is what I reported back to Jang Soo Kim. We also observed that a limiting case reduced to the classical identity equating the sum of all Schur functions with an orthogonal character indexed by a rectangular shape. This was indeed a key observation since there was an equally classical proof of this identity that was summarised in John Stembridge's article[43] on non-intersecting paths and plane partitions. Indeed, aside from a few technical details, by following the steps of this blueprint more or less verbatim, we obtained a proof of the symmetric function identity, and thus of all the more special identities that had been met on the way. Together with Soichi Okada, *the* wizard for this kind of summation identities, we were then able to do much more.

It should be noted, however, that, while we had obtained spectacular (as I believe) new symmetric function identities, we failed to accomplish the original goal: namely, to find a bijective proof of the identity of Mortimer and Prellberg.[49]

Mansour: Is there a specific problem you have been working on for many years? What progress have you made?

Krattenthaler: As I also say later, I would certainly love to see a bijection between alternating sign matrices and totally symmetric self-complementary plane partitions. In 1996, I came up with a parametric generalisation[50] in terms of Zeilberger's[51] Gog and Magog triangles and trapezoids. This generalisation (extending an earlier conjecture of William Mills,

[47]P. R. G. Mortimer and T. Prellberg, *On the number of walks in a triangular domain*, Electron. J. Combin. 22 (2015), no. 1, Paper 1.64.

[48]I. M. Gessel and D. Zeilberger, *Random walk in a Weyl chamber*, Proc. Amer. Math. Soc. 115 (1992), no. 1, 27–31.

[49]Such a bijective proof was found by Julien Courtiel, Andrew Elvey Price and Irène Markovici (*A bijective proof of a theorem of Mortimer and Prellberg on walks in a triangular domain*, Electron. J. Combin. 28(1) (2021), #P1.12). To find a bijective proof of our much more general identities is an open problem.

[50]Conjecture 5 in *Plane partitions in the work of Richard Stanley and his school*, in: The Mathematical Legacy of Richard P. Stanley, P. Hersh, T. Lam, P. Pylyavskyy, V. Reiner (eds.), Amer. Math. Soc., R.I., 2016, pp. 246–277.

[51]D. Zeilberger, *Proof of the alternating sign matrix conjecture*, Electron. J. Combin. 3 (1996), no. 2, Research Paper 13.

David Robbins, and David Rumsey) is very suggestive. If you look at it, you cannot escape getting the idea that some kind of jeu de taquin moves, maybe combined with another combinatorial transformation, should do the trick. On several occasions, I have tried to make this work, but failed miserably. (And I am not the only one. The conjecture is still open; up to now, there is not even a computational proof.) Maybe it is hopeless. Maybe we are just missing the right idea. I don't know.

Mansour: When you are working on a problem, do you feel that something is true even before you have the proof?

Krattenthaler: In my field, this is an ill-posed question. If you work in territory which is characterised by equations and exact formulae, then you "know" that a conjectured formula is true. What I am referring to is, say, formulae for the enumeration of lattice paths or plane partitions that you have calculated by hand and/or computer for, say, $n = 1, 2, 3, \ldots, 10$, or even further. If your formula is true for all these cases (and has a particular form), you "know" that it must be true in general.

Things are different when you work in, say, structural graph theory, or with primes in number theory. There, it does not say much if things work out for the first few instances of your parameters. The counter-example may appear only for very large numbers of vertices or for very large primes. In that sense, I "know" that the conjecture in "A (conjectural) 1/3-phenomenon for the number of rhombus tilings of a hexagon which contain a fixed rhombus"[52] is true, whereas in "On the integrality of the Taylor coefficients of mirror maps, I, II" (joint with Tanguy Rivoal[53,54]) there are conjectures involving prime numbers where we are not entirely sure that they are true.

Mansour: What three results do you consider the most influential in combinatorics during the last thirty years?

Krattenthaler: Obviously, my answer can only be very subjective.

I would begin with Greg Kuperberg's "Symmetry classes of alternating-sign matrices under one roof"[55] where he provides proofs of four of the long-standing conjectures on the enumeration of symmetry classes of alternating sign matrices. He does that by interpreting the relevant classes of alternating sign matrices as instances of the six-vertex model with certain boundary conditions and by finding determinantal or Pfaffian formulae for the corresponding partition functions (also known as generating functions). These ideas have been instrumental in much subsequent work by many authors that in particular led to proofs of the remaining open conjectures on the enumeration of alternating sign matrices with symmetries.

Next Gilles Schaeffer's[56] bijections between planar maps and trees that first appeared in his thesis come to my mind. These have been further developed in the subsequent years and have led to a conceptual understanding of many of the wonderful formulae that exist in the enumerative theory of planar maps. Moreover, these bijections have been the driving engine for the fantastic results on scaling limits of planar maps, with the limiting object being a very fractal random sphere which is called Brownian map.

Another article that I consider extremely influential is "The cyclic sieving phenomenon"[57] by Vic Reiner, Dennis Stanton, and Dennis White. In that article, the authors define that a set S of combinatorial objects exhibits the cyclic sieving phenomenon if there is an action of a cyclic group $C = \langle g \rangle$ on S and a polynomial $P(t)$ such that the number of elements of S that are invariant under g^e is given by $P(\omega^e)$, where ω is a primitive $|C|$-th root of unity. (This did not come out of nowhere; this notion generalises John Stembridge's earlier (-1)-phenomenon.) The original article already contains numerous instances of cyclic sieving phenomena. Since this article, the discovery of cyclic sieving phenomena has almost

[52] C. Krattenthaler, *A (conjectural) 1/3-phenomenon for the number of rhombus tilings of a hexagon which contain a fixed rhombus*, in: A. K. Agarwal et al. (eds.), Number Theory and Discrete Mathematics, Hindustan Book Agency, New Delhi, 2002, pp. 13–30.

[53] C. Krattenthaler and T. Rivoal, *On the integrality of the Taylor coefficients of mirror maps*, Duke Math. J. 151 (2010), no. 2, 175–218.

[54] C. Krattenthaler and T. Rivoal, *On the integrality of the Taylor coefficients of mirror maps, II*, Commun. Number Theory Phys. 3 (2009), no. 3, 555–591.

[55] G. Kuperberg, *Symmetry classes of alternating-sign matrices under one roof*, Ann. of Math. (2) 156 (2002), no. 3, 835–866

[56] G. Schaeffer, *Conjugaison d'arbres et cartes combinatoires aléatoires*, Ph.D. Thesis, Université Bordeaux I, 1998.

[57] V. Reiner, D. Stanton, and D. White, *The cyclic sieving phenomenon*, J. Combin. Theory Ser. A 108 (2004), no. 1, 17–50.

become an industry, with numerous more phenomena having been discovered and proved. Mireille Bousquet-Mélou and I posted a new article on arXiv just a few days ago,[58] in which we prove cyclic sieving phenomena exhibited by trees and tree-rooted maps.

On the probabilistic side, the arctic circle theorem for domino tilings of the Aztec diamond due to Bill Jockusch, Jim Propp and Peter Shor[59] is a milestone, exhibiting the first such scaling limit result in the area. Many more results of this kind were proved subsequent to that article, culminating in "Dimers and amoebae" by Rick Kenyon, Andrei Okounkov and Scott Sheffield.[60] In that article, a general theory for such limiting phenomena for the dimer model on periodic graphs is outlined (even if not everything here may be completely rigorous). Another breakthrough that has to be mentioned here is the theorem by Jinho Baik, Percy Deift and, Kurt Johansson[61] that shows that the length of the longest increasing subsequence in a random permutation, if suitably rescaled, behaves like the celebrated Tracy–Widom distribution. Although, on the outset, this seems to be quite different from the other results that I have mentioned in this paragraph, it implicitly concerns very similar processes.

In another direction, I must mention the work by June Huh[62] and the theory of Lorentzian polynomials due to Petter Brändén and June Huh[63] that paved the way to prove many combinatorial inequalities that had been conjectured for a very long time.

These are more than three? I am sorry.

Mansour: What are the top three open questions in your list?

Krattenthaler: It is known, via separate, deep theorems, that the number of $n \times n$ alternating sign matrices (ASM), the number of totally symmetric self-complementary plane partitions contained in a $(2n)\times(2n)\times(2n)$ box, the number of descending plane partitions (DPP) of order n, and the number of alternating sign triangles of order n are all given by the same formula, $\prod_{i=0}^{n-1} \frac{(3i+1)!}{(n+i)!}$. If you are a combinatorialist there is one obvious question: where are the bijections between these families of objects? There has been some progress on the ASM–DPP bijection due to Ilse Fischer and Matjaž Konvalinka.[64,65] Their bijection being a remarkable result, I believe that it is still fair to say that this is not the bijection that we are dreaming of since it involves involution principle arguments.

So, here are $\binom{4}{2} = 6$ open questions, which is again more than 3 ...

Mansour: As an advisor, you have influenced the careers of many students. What advice do you have for young mathematicians who are just starting their academic journeys?

Krattenthaler: The advice that I give is, first, that it is important to follow one's own interests and that one should enjoy what one is doing. If one jumps on things only for the reason that these are currently fashionable and/or have better job perspectives, then this will likely not work out. Second, I tell young people who aim at an academic career that they should go ahead, and that it is most important that they develop a sensibility for what are interesting problems which at the same time are also solvable. Third, it is important to develop one's own profile and to constantly extend one's own spectrum of interests; doing this, one will learn new methods, and the more methods you dispose of, the more you will be able to do. Moreover, in that way, one will be an interesting candidate for a larger number of job opportunities and a larger number of senior people who decide about job applications. Fourth, one has to be aware that an academic career may not work out, for whatever reason. If this should happen, then one has to be prepared to instead accept a job in the private

[58] M. Bousquet-Mélou and C. Krattenthaler, *Cyclic sieving phenomena for trees and tree-rooted maps*, 2025, arXiv:2512.18656.

[59] W. Jockusch, J. Propp, and P. Shor, *Random domino tilings and the arctic circle theorem*, Discrete Math. Theoret. Comput. Sci. 38 (1998), 173–184.

[60] R. Kenyon, A. Okounkov, and S. Sheffield, *Dimers and amoebae*, Ann. of Math. (2) 163 (2006), no. 3, 1019–1056.

[61] J. Baik, P. Deift, and K. Johansson, *On the distribution of the length of the longest increasing subsequence of random permutations*, J. Amer. Math. Soc. 12 (1999), no. 4, 1119–1178.

[62] J. Huh, *Milnor numbers of projective hypersurfaces and the chromatic polynomial of graphs*, J. Amer. Math. Soc. 25 (2012), no. 3, 907–927.

[63] P. Brändén and J. Huh, *Lorentzian polynomials*, Ann. of Math. (2) 192 (2020), no. 3, 821–891.

[64] I. Fischer and M. Konvalinka, *A bijective proof of the ASM theorem Part I: the operator formula*, Electron. J. Combin. 27 (2020), no. 3, Paper No. 3.35.

[65] I. Fischer and M. Konvalinka, *A bijective proof of the ASM theorem Part II: ASM enumeration and ASM-DPP relation*, Int. Math. Res. Not. IMRN 2022, no. 10, 7203-7230.

sector or serve as a school teacher (for example) although that may not be what one has dreamt of. In any case, there is nothing wrong about it.

Otherwise, I would like to cite Richard Askey. On several occasions, I have heard him say the following:

> "If an authority in the field tells you that you should look at a certain thing, listen! If that authority tells you to *not* look at a certain thing, *don't* listen!"

As Askey explained, there is a key experience that made him say this. It begins with his firm belief that the determinantal formula for orthogonal polynomials in terms of the moments of the orthogonality measure is aesthetically pleasing and theoretically interesting, but otherwise completely useless. This belief was destroyed by his student Jim Wilson, who used exactly that formula to find the — what is now known as — Wilson polynomials, and subsequently led to the discovery of the — what is now known as — Askey–Wilson polynomials,[66] which stand on top of the hierarchy of (q-)hypergeometric orthogonal polynomials.

Mansour: Do you think that there are core or mainstream areas in mathematics? Are some topics more important than others?

Krattenthaler: This is obviously so, although we may heatedly discuss which subjects are more important than others. Clearly, what is mainstream or not is constantly changing and may depend on many things ranging from new developments that have great impact beyond the field in which these developments take/took place, questions coming from other fields, results that — so to speak — close a subject and thereby move it out of mainstream, up to matters of fashion. Unfortunately, politics is made with it. Similar to "pure versus applied mathematics," I would prefer to make a distinction between excellent mathematics and not so excellent mathematics, but one has to live with things as they are. It is very human to try to take advantage of labellings that apply to you but not to others.

I have some sympathy for Doron Zeilberger saying (more or less) that mainstream is not interesting because everybody does the same and thus there will not be any surprises or unexpected developments. In his opinion these only arise if you think differently, that is, in "non-mainstream" ways.

Mansour: What do you think about the distinction between pure and applied mathematics that some people focus on? Is it meaningful at all in your case? How do you see the relationship between so-called "pure" and "applied" mathematics?

Krattenthaler: I have nothing original to say here. The most important distinction is between "good" and "not so good" mathematics, may it be pure or applied. I appreciate both "pure" and "applied" mathematicians if they are good mathematicians. I have no appreciation for those who misuse the labelling for obvious purposes.

Mansour: Artificial intelligence has rapidly transformed many aspects of research and society. How do you see its impact on mathematics, science more broadly, and humanity's future?

Krattenthaler: This is completely unclear at this point, and, consequently, I have nothing sensible to say. It is simply too early. What we are experiencing at the moment is just the peak of (likely) an iceberg. However, what this iceberg is going to bring or imply cannot be said by now. It is for sure that there will be many surprising developments in the near future of which we have not the slightest inkling at this point.

However, what can already be said at this point is that we will have to completely rethink our systems of exercise classes, of exams, of bachelor theses, master theses, etc. How exactly, that may also be too early to say. (Since I am retired, I am no longer concerned ...)

Mansour: You are also a trained concert pianist. Could you tell us about your musical journey, the composers and pianists you admire most, and how you see the relationship between music and mathematics?

Krattenthaler: Again, many questions in one. I will answer them one by one.

In our family, there was always ("classical") music. My father played the piano, and my mother played the violin, although, in my memory, there is neither my father playing the piano nor my mother playing the violin. In any

[66] R. Askey and J. A. Wilson, *Some basic hypergeometric orthogonal polynomials that generalize Jacobi polynomials*, Mem. Amer. Math. Soc. 54 (1985), no. 319.

case, there was always a piano in the apartment and I wanted to learn to play the piano. When I was age 10 my parents managed to find a teacher for me. She was an extremely charming lady. After two years, she told my parents that I had made excellent progress, but now she cannot teach me anything anymore since this is beyond her abilities. She recommended me to a university professor who was also living in the area, and he accepted me as a student. Still during high school time, I won prizes at national youth competitions. As I already indicated, after high school I made the entrance exam to the University of Music and Performing Arts in Vienna[67] and started my piano studies in 1977. Soon I won a local piano competition, and in 1986 I made my concert diploma. In the 1980s, there were phases where I did more mathematics and others where I did more piano playing (maybe, in total, I did more piano playing ...). I played recitals, specialised mostly in playing chamber music, where I performed with members of the Vienna Philharmonic Orchestra and the Vienna Symphony Orchestra, did also a few recordings for the ORF (the Austrian Broadcasting Corporation).[68] Towards the end of the 1980s, it became clear that a career as a professional pianist would not work out since I began to suffer from an irreversible chronic medical condition in both hands. In 1991, I played my last "official" recital. Since then I occasionally play recitals for mathematicians ...

When it comes to pianists whom I appreciate then I must certainly mention Alfred Brendel (who passed away last year), whose well-thought, informed, but at the same time extremely emotional playing impressed me a lot. However, there are many more pianists whom I appreciate. From the same generation, there is Friedrich Gulda, who is not so well-known internationally due to a choice that he made, but he was a truly exceptional pianist, particularly for Mozart and Beethoven. Out of the still living pianists, there is the Grande Dame Martha Argerich who at age 84 (!) is still amazing; I have great memories of András Schiff playing Bach and Beethoven; I heard Pierre-Laurent Aimard in several memorable recitals with very unusual programs (Messiaen, Ligeti, Yves, Art of Fugue) but, for example, also with a fantastic Schumann Fantasy; Leif Ove Andsnes' Grieg is awesome (and also other composers that he plays); of the younger generation Igor Levit is outstanding, who chooses extremely interesting programs, and who for example showcased Frederic Rzewski's[69] magnificent monumental variation set "The People United Will Never Be Defeated!" which I did not know before; Daniil Trifonov is a very interesting, versatile pianist; Víkingur Ólafsson is very eccentric, but the Goldberg Variations by Bach that he performed two years ago were simply ingenious. I guess that I should better stop at this point before more names come to my mind.

Which are my favourite composers? I am afraid that this would again become a very long list. So let me answer a more restricted question: which composers suit me most (as a pianist)? These are Joseph Haydn, Ludwig van Beethoven, Franz Schubert, and Johannes Brahms. It may be a bit surprising that Johann Sebastian Bach and Wolfgang Amadeus Mozart are missing in this list. For Bach, the explanation is that I do not consider the modern piano as the right instrument to play Bach (although I also do that sometimes). Bach's keyboard music is written for the harpsichord, and it sounds so much better when played on that instrument.[70] For Mozart, I have to confess that it is not so easy for me to perform his music. Here is a famous quote from the great pianist Arthur Schnabel: "Mozart — too easy for children, too difficult for adults." What did he want to say? Mozart's compositions for piano are not very demanding technically. However, what is extremely difficult is to let them flow just naturally, as it should be and everyone expects it to be. The earlier-mentioned Friedrich Gulda was simply amazing in doing this. I have more difficulties with that. But I try and, as I believe, sometimes I succeed.

How do I see the relationship between mu-

[67] The official name then was Hochschule für Musik und Darstellende Kunst Wien.
[68] Strangely enough, one of them can be listened to on YouTube:
`https://www.youtube.com/watch?v=WFta4Abgsgo&list=RDWFta4Abgsgo&start_radio=1`
[69] F. Rzewski, *The People United Will Never Be Defeated!*, 36 Variations on a Chilean Song (1975) for solo piano; world premiere performed by Ursula Oppens, 7 Feb. 1976, John F. Kennedy Center for the Performing Arts, Washington, D.C.
[70] I do enjoy playing Bach's organ compositions; on the organ.

sic and mathematics? I have written a longer article exactly on this topic.[71] The short version is that I do not see any substantial connections between music and mathematics, but the fact that there are many mathematicians with a great affinity for music, and even many musicians with a great affinity to mathematics may have its explanation in the fact that both music AND mathematics appeal to both soul AND brain.

Mansour: Would you tell us about your interests besides mathematics and music?

Krattenthaler: There are several. First of all, I used to be a passionate football player.[72] Now I play only rarely since my hip-joint arthrosis has progressed somewhat. Otherwise, I enjoy going to concerts (of course) and opera performances (occasionally), to theatre plays, to see movies in cinemas, and to visit art exhibitions at museums.

Mansour: If you weren't a mathematician, what career path could you imagine yourself in?

Krattenthaler: No idea.

Mansour: Professor Krattenthaler, I would like to thank you for this very interesting interview on behalf of the journal *Enumerative Combinatorics and Applications*.

[71] *Music AND Mathematics? Personal Views on a Difficult Relationship*, English version of a written version of a talk given in the `math.space` in the Museumsquartier in Vienna, May 16, 2013; Newsletter Europ. Math. Soc. **104** (2017) 41–54.

[72] Just to be clear: what I mean is *proper* football, meaning the football that was invented in England, or soccer as one says in America.