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Interview with Volker Strehl

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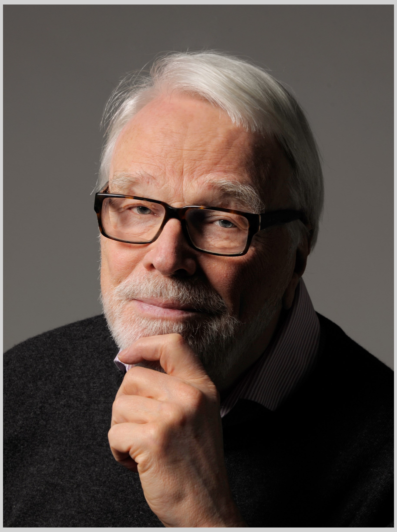

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Volker Strehl studied mathematics and physics at Friedrich-Alexander Universität (FAU) in Erlangen-Nürnberg, Germany, and at ETH Zürich in Switzerland. In 1971 he obtained a Diploma in Mathematics (M.Sc. degree) at FAU under the supervision of Konrad Jacobs, and in 1973 he obtained a Ph.D. in Mathematics under the joint supervision of Konrad Jacobs and Dominique Foata. He then joined the Computer Science Department (CSD) of FAU as (an equivalent of) an assistant professor in Theoretical Computer Science. In 1990, he obtained the degree of a Dr.-Ing. habil., required for becoming a 'Privatdozent' (1991) and a Professor of Theoretical Computer Science (1996). In that same year, he joined the Chair of Artificial Intelligence at CSD, which in 2010 became the new Chair of Theoretical Computer Science. His research interests include Enumerative Combinatorics, Mathematical Methods in Computer Science (Complexity Analysis of Algorithms, Automata, Grammars, and Formal Languages, Information and Coding Theory), and Computer Algebra. Together with Dominique Foata and Adalbert Kerber, Professor Strehl was co-founder of the Seminaire Lotharingien de Combinatoire in 1980 and is still active in this ongoing enterprise. He acted as an organizer and as a member of the permanent program committee of the series of conferences Formal Power Series and Algebraic Combinatorics (FPSAC) from 1990 to 2005, and also as a member of the editorial board of the Electronic Journal of Combinatorics for many years.

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

Strehl: In my opinion, the word 'combinatorics' does not name a coherent body of mathematical objects and knowledge about them, unlike 'number theory', say. There are subfields: enumerative combinatorics, graph theory, combinatorial designs, algebraic combinatorics, extremal combinatorics, combinatorial optimization, each of which has its own concepts and methods. There are interrelations between these subfields, sure, but to me, these interrelations appear generally weaker than the relations that each of these subfields has with disciplines outside combinatorics, from where motivation comes and where it is applied. Thus I prefer to speak of 'combinatorial methods', by which I roughly mean the investigation of discrete structures, their analysis and synthesis, their enumeration by appropriate methods, and their applicability in other parts of mathematics, physics, economy, etc. In that sense 'combinatorics' is for me more a mode of thinking and working rather than a well-deliminated part of mathematics.

A characteristic of combinatorics, in contrast to many other fields of mathematics, is that you can usually do combinatorics 'from

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scratch', i.e., start working on problems (of any degree of difficulty!) without first absorbing a conceptual, quasi 'bourbakian', overhead and knowledge body. (No wonder that there is no combinatorics volume in Bourbaki.)

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

Strehl: In my personal experience, this has been a delicate question for a long time. When I finished my Ph.D. with a thesis in combinatorics, I had become aware of the fact that many mathematicians did not take combinatorics seriously. A common attitude was: combinatorics is a 'bag of tricks', here and there sophisticated proofs, but hardly any 'deep' theory (and a lot of trivial stuff or feed for 'recreational mathematics'). When discussing this question, you have to keep in mind the German (some say: feudal) system of 'Chairs', with the 'Ordinarius' on top. When it comes to re-occupying a chair or creating a new chair, then other chair-holders would determine its orientation and filling. For the 1970's I cannot remember a single chair devoted to combinatorics in any German math department (with the exception of one in *Discrete Mathematics* in Berlin held by Martin Aigner). Of course, there were mathematicians from algebra (like Adalbert Kerber and Heinz Lüneburg), from geometry (like Peter Dembowski and Hanfried Lenz), from optimization (like Martin Grötschel), or probability (like Konrad Jacobs), who were interested in combinatorial methods and encouraged young people to engage in combinatorics. But generally, combinatorics was decidedly less well established compared to traditional fields like, say, number theory, algebraic geometry, numerical mathematics, or partial differential equations. This 'marginal' situation of combinatorics had consequences for the job situation. So in the early 1970s, young mathematicians with a background in fields like discrete mathematics, logic, numerical mathematics turned to the rapidly growing field of computer science. Due to a nationwide initiative, computer science departments and courses of study were created, with lots of positions to be filled. That was my situation at Erlangen too, but with an important difference to most other German universities: at Erlangen, computer science became part of the (new) Faculty of Engineering, while mathematics remained part of the (traditional) Faculty of Science. Thus already by the organization, the departments of mathematics and of computer science had few points of contact, and many mathematicians looked at computer science with reservation (and envy) anyway. On the other hand, the traditional 'true engineers' did not consider computer scientists on an equal footing: graduates from computer science did not obtain the prestigious title of a Diplom-Ingenieur. To round up this scenario of 'difficult' relations between the disciplines, I mention that more practically oriented colleagues from computer sciences viewed the mathematically oriented ones with suspicion, because their 'excessive' mathematical ambitions might discourage young people from selecting computer science as their subject.

Mansour:$^{(a)}$ 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?

Strehl: As a scholar, I was interested in mathematics and physics and I took profit from good teachers. A kind of predisposition may have played a role: my elder brother was a theoretical chemist, my father was an engineer, and my grandfather *Karl Wilhelm Andreas Strehl* was a physicist who had obtained a Ph.D. from the FAU in 1900. By the way: the famous invariant theorist Paul Gordan has been his mathematical mentor. My grandfather is still known among amateur astronomers for establishing a criterion for the imaging quality of telescopes, also known as 'Strehl ratio', based on diffraction theory. I have not known him personally, but I think that he would have been satisfied to learn that his grandson loves Fourier theory and has taught mathematical methods for image processing for many years. I am grateful to my elder brother who has introduced me to computer programming and operating at a time (around 1964) when personal computers were decades away and when only a few privileged people had access to one of these rare 'electronic brains'.

Mansour:$^{(b)}$ What have been some of the main goals of your research?

Strehl: I will concentrate on three aspects:

(1) Combinatorics and Special Functions,
(2) Combinatorics and Computer Algebra,
(3) Combinatorics and Algorithm Analysis,
where in all three cases 'combinatorics' should be read as 'enumerative combinatorics' in the first place. This indicates that the goal of 'counting' or 'enumerating' objects (for me) lies mainly in its interactions with other fields of mathematics of computer science. Numbers and cardinalities per se are of secondary interest, but properties of their generating functions (rational, algebraic, D-finite, transcendent) and their asymptotic behavior are. The point of counting and enumeration lies in a better structural understanding of the objects of interest. To cite a folklore example: once you find out (experimentally, say) that a class of objects is counted by the Catalan numbers, then you know (or imagine) a way to systematically generate the objects in a grammar-like way – and you have immediate access (via Neil Sloane's *Online Encyclopedia of Integer Sequences*[1], say) to a wealth of information about families of objects that behave in the same way.

(1) I cannot resist citing a phrase from an article by Adriano Garsia and Jeff Remmel[2]:

"It has become increasingly apparent . . . that the special functions and identities of classical mathematics are gravid with combinatorial information. This information can be expressed in the form of correspondences or more precisely *encodings* of objects into words of certain languages and natural bijections between different classes of languages. The classical identities appear as relations between enumerators of words by suitable statistics.

At present, a systematic study is taking place to mine this information out of the classical literature. The increasingly rich inventory of correspondences has led to new identities as well as more revealing proofs of the old ones."

This same quotation has been used by Dominique Foata at the occasion of his survey talk given at the *International Congress of Mathematicians* in Warsaw 1983, where he mentions the activities of different groups and illustrates these ideas by showing the combinatorial contents of a bilinear generating function for the Meixner polynomials[3]. The combinatorics is of the same type that we had used in our joint work[4] on multilinear generating functions for the Laguerre polynomials, and which was extended to the Jacobi polynomials at about the same time by Foata and Pierre Leroux[5], and studied by Leroux and myself[6]. My ultimate goal was to divulge the combinatorial meaning of Bailey's bilinear generating function for the Jacobi polynomials in terms of what we had been named *Jacobi configurations*, see $^{(d)}$. The constructive proof is contained in my habilitation thesis.

Later on, I turned to other subjects and problems, but my interest in this combinatorics of special functions has been ignited again in 2013 when Karol Penson together with three Italian collaborators submitted for publication in the *Séminaire Lotharingien de Combinatoire* (SLC) a manuscript on *Lacunary generating functions for the Laguerre polynomials*. This work[7] was published in volume 76 of the SLC and I was happy to complement their work by an article *Lacunary Laguerre Series from a Combinatorial Perspective*[8] (in the same volume). Indeed, I was able to provide additional insight in their (purely analytical) work which leads to natural generalizations.

(2) In 1984 I had the occasion to participate in a conference held in New York that brought combinatorics and computer algebra together. I saw many 'big names' in action, George Andrews, Richard Askey, Bill Gosper, the Chudnovsky brothers, ... and I was convinced: that is the way to go! Back home

[1] `https://oeis.org/`.

[2] A. Garsia and J. Remmel, *A combinatorial interpretation of q-derangement and q-Laguerre numbers*, Europ. J. Combin. 1:1 (1980), 47–58.

[3] D. Foata, *Combinatoire des identités sur les polynômes orthogonaux* [*Combinatorics of identities involving orthogonal polynomials*], Proceedings of the International Congress of Mathematicians, Volume 1, 2 (Warsaw, 1983), 1541–1553, PWN, Warsaw, 1984.

[4] D. Foata and V. Strehl, *Combinatorics of Laguerre polynomials*, Enumeration and Design (Waterloo, Ont., 1982), 123–140, Academic Press, Toronto, ON, 1984.

[5] D. Foata and P. Leroux, *Polynômes de Jacobi, the generating function, interprétation combinatoire et fonction génératrice*, Proc. Amer. Math. Soc. 87:1 (1983), 47–53.

[6] P. Leroux and V. Strehl, *Jacobi polynomials: combinatorics of the basic identities*, Discrete Math. 57 (1985), no. 1-2, 167–187.

[7] D. Babusci, G. Dattoli, K. Górska, and K. A. Penson, *Lacunary generating functions for the Laguerre polynomials*, Sém. Lothar. Combin. 76 (2016–2019), Article B76b.

[8] V. Strehl, *Lacunary Laguerre series from a combinatorial perspective*, Sém. Lothar. Combin. 76 (2016-2019), Article B76c.

I talked to my colleagues from mathematics, who were not convinced, and to those from computer science, who were not enthusiasts either. Engineers preferred Matlab anyway. I had nothing to demonstrate but the system *SMP* by Wolfram, which was deficient in several ways. I tried to convince colleagues who were responsible for teaching mathematics to engineers, but they were afraid that they had to completely rework their curricula if computer algebra software was admitted. 'Pure' mathematicians were not interested in a computer algebra system that did not fit their expectations 'out of the box' – computer programming as a standard part of formation in mathematics was still far away. In short: it was a long struggle until computer algebra software (Maple, Mathematica) became widely accepted. At Erlangen, I was a kind of lonesome fighter for computer algebra. In those, at times frustrating days, my enthusiasm was upheld by my close cooperation on various levels (research, organization of conferences, exchange of students) and friendship with Peter Paule from the *Research Institute for Symbolic Computation* (RISC) at Linz (Austria). He had a background in q-analysis, including the theory of partitions, which complemented my interest in the enumeration. In particular, we worked together on symbolic summation, binomial and hypergeometric identities (as propagandists for the Zeilberger-Wilf method), applications to enumeration, in various ways[9,10,11,12].

(3) As a teacher in the CSD (with a considerable teaching load) I had to prepare a broad spectrum of courses that fitted with my mathematical interests: logic and computability, discrete mathematics (though not combinatorics), automata and formal languages, information and coding theory, complexity of algorithms, cryptography, computer algebra). Donald Knuth and Philippe Flajolet were my seminal figures. I am not sure whether a substantial part of my students was impressed to see how automata and formal languages can be used for counting, or if they appreciated complexity analysis through complex analysis (i.e., convergence radii of formal series). But some of them were, and they were then best prepared for my lectures in computer algebra.

Mansour: Were there specific problems that made you first interested in combinatorics?

Strehl: When preparing for the Diplom exam (Masters degree) my major fields of interest and study were Analysis (with Heinz Bauer), Probability (with Konrad Jacobs), and Numerical Analysis (with Wilhelm Specht). I asked Jacobs to be my advisor for a thesis subject in probability. You must know that Jacobs was a mathematician with very broad interests. He said: "I am a generalist, not a specialist", and when he retired in 1993 he stated: "Time for generalists is over, today I would not be able to make a career the way I did". Besides probability (and ergodic theory in particular) he was interested in dynamical systems, physics, economics, logic, geometry, and more, and he was always looking for interesting new developments in other fields that were accessible for non-specialists. (He even published a book series for that purpose). He would then ask a student to familiarize himself with the new stuff and then report to him about it so that he could integrate it into his universe. That is how he understood the task of being an advisor. At the time when I approached him, a new specialty in probability, named 'fluctuation theory', very much combinatorial in character, had emerged. So it became my task to tell Jacobs about the arcsine-law, Sparre Andersen's equivalence principle, Spitzer's exponential formula, and all that. And I liked the stuff.

Mansour: What was the reason you chose the Friedrich-Alexander-Universität Erlangen for your Ph.D. and your advisors Konrad Jacobs and Dominique Foata?

Strehl: It was a continuation of my relationship with Jacobs: he offered me support, a kind of grant, for preparing a Ph.D. thesis under his

[9]P. Lisoněk, P. Paule, and V. Strehl, *Improvement of the degree setting in Gosper's algorithm*, J. Symbolic Comput. 16 (1993), no. 3, 243–258.

[10]*Symbolic Computation for Combinatorics* Δ_1, Proceedings of an MSI-Workshop at Cornell University 1993, P. Paule and V. Strehl (eds.), Special volume of the Journal of Symbolic Computation, vol. 20 (5/6), 1995.

[11]G. E. Andrews, P. Paule, A. Riese, and V. Strehl, *MacMahons Partition Analysis V: Bijections, Recursions, and Magic Squares*, in: Proceedings of the Euroconference Algebraic Combinatorics and Applications (ALCOMA), Gößweinstein 1999, A: Betten, A. Kohnert, R. Laue, A. Wassermann (eds.), Springer Verlag 2001.

[12]P. Paule and V. Strehl,*Definite Summation and Hypergeometric Identities*, Section 10.2.1 in the Computer Algebra Handbook, J. Grabmeier, E. Kaltofen, V. Weispfenning (eds.), Springer Verlag, 2003.

supervision, which meant that I had to find the subject for myself – that was his principle for accepting a candidate: he should not need guidance, and he should convince the supervisor with his ideas and results.

Mansour: What was the problem you worked on in your thesis?

Strehl: I must confess that my first ideas for a thesis subject failed. I realized that my proposal was too ambitious. Jacobs asked a former colleague about my ideas, who gave him the hint that a young french mathematician, Dominique Foata, had recently published work in the same direction. Jacobs immediately invited Foata to come to Erlangen and to work with me. This was the beginning of a long and fruitful collaboration and friendship that lasts till today.

Mathematically the thesis was about the so-called *André polynomials*[13,14], which refers to the work of Désiré André in the late 19th century on the interpretation of the tangent and secant coefficients as enumerators for alternating permutations. Schützenberger and Foata had revitalized these ideas in the early 1970's. As the title *Geometrische und arithmetische Eigenschaften der André-Polynome*[15] of my dissertation suggests, I added and studied further properties of these polynomials. Incidentally, one of my recent articles, *The Poupard-Entringer matrix sequence*[16] is joint work with Foata and Han, and still deals with the same (surprisingly rich!) subject.

Mansour: What would guide you in your research? A general theoretical question or a specific problem?

Strehl: I love to digest elegant theories with interesting models, in the sense stated by one of my high school teachers: "There is nothing more practical than a good theory". But I also have the spirit of a nutcracker. Indeed, I have very often been asked for help by colleagues who had run into difficulties in solving a particular problem of combinatorial character. It drives me if I know that others have tackled it in vain. I could give examples from determining the spectrum of communication networks to problems in algebraic geometry. One particular project of that kind will be described in $^{(f)}$ below.

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

Strehl: Definitely yes! That is where computer support plays a dominant role. I create examples, try to extract patterns, and generate 'empirical truth' before embarking on formal proof.

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

Strehl: Generally, I do not like 'rankings' or 'charts', especially in areas (unlike sports) where linear orderings do not make sense. Instead of answering your question, it appears more entertaining if I describe my personal encounters with three truly remarkable and influential papers – in historical order, not qualifying importance. Here is the list:

(1) *Multiplication of multi-digit numbers by automata*[17].

(2) *On sets of integers containing no k elements in arithmetic progression*[18].

(3) *Proof of the alternating sign matrix conjecture*[19].

And here are my comments: (1) Admittedly, this is not a combinatorics paper and surpasses the 30 years limit by far. In addition, the paper is very short (3 pages, Doklady style) and hence not very readable. But it contains a fact that every mathematician should be interested in: the usual way of doing multiplication of integers in positional number systems, the 'school method', is not the most efficient way to perform multiplication! In my classes on *complexity analysis of algorithms* it has always been a paradigmatic starter, explaining the notions 'complexity of an algorithm' and 'complexity of a problem', and shattering the students confidence in that what you learn in

[13] D. Foata and V. Strehl, *Rearrangements of the symmetric group and enumerative properties of the tangent and secant numbers*, Math. Z. 137 (1974), 257–264.

[14] D. Foata and V. Strehl, *Euler numbers and and variations of permutations*, Colloquio Internazionale sulle Teorie Combinatorie (1973), Atti dei Convegni Lincei 17 (1976), 119–131.

[15] https://www.mathgenealogy.org/id.php?id=21362.

[16] D. Foata, G.-N. Han, and V. Strehl, *The Entringer-Poupard matrix sequence*, Linear Algebra Appl. 512 (2017), 71–96.

[17] A. A. Karatsuba and Y. P. Ofman, *Multiplication of multi-digit numbers by automata*, Soviet Physics Doklady 7 (1963), 595.

[18] E. Szemerédi, *On sets of integers containing no k elements in arithmetic progression*, Acta Arithmetica 27:1 (1975), 199–245.

[19] D. Zeilberger, *Proof of the alternating sign matrix conjecture*, Electron. J. Combin. 3(2), The *Foata Festschrift* (1996), #R13.

school for mundane purposes like multiplication is always the optimal method. The school method takes $O(n^2)$ operations on digits for the multiplication of two n-digit integers. The method described in the mentioned article reduces this to $O(n^{\log_2 3}) = O(n^{1.59\dots})$ digit operations, which really makes a big difference, that is, for arithmetic with numbers as used in cryptographic protocols. And the (recursive) method is easy to explain, it is surprising that it had not been discovered earlier.

When I started teaching I used to name this method the 'Karatsuba-Ofman method', after the heading of the article and following practice in some books on the subject. So this naming entered into the printed handouts for the students that I made available online. Until one day (in the late 1990s, if I remember correctly), when I received an angry email from .... Karatsuba's daughter! In furious words she told me that it was absolutely incorrect to name the multiplication method after her father and Ofman. The latter 'had nothing to do whatsoever' with the algorithm and she urgently requested that I should immediately correct my lecture notes and never use the faulty naming again.

I found out that I had not been the only person to receive such complaints from her. I made the requested corrections, but I also tried to find out how the wrong impression about authorship had emerged. It seems that the article had not been written by the named authors, but by – another surprise! – the famous academician Andrei N. Kolmogorov! He seems to have posed the problem to prove that $O(n^2)$ is the true problem complexity of integer multiplication to his students in a seminar, and Karatsuba rapidly came up with 'his' method as a counterexample. Ofman had worked on other stuff that is marginally mentioned. Kolmogorov was so excited by the result(s) of his student(s) that he himself wrote the Doklady article and published it under his student's names, not clearly giving credits.

I mentioned my passion for Fourier methods in $^{(a)}$, and hence I have to add that Karatsuba's method was superseded in 1971 by a famous algorithm due to Arnold Schönhage and Volker Strassen[20] that uses FFT-based techniques and brings complexity down to $O(n \log(n) \log\log(n))$ – which is still not the (asymptotically) best known method, and the precise lower bound is still unknown. By the way: Strassen and I have the same Ph.D. advisor: Konrad Jacobs.

(2) In the spring of 1974 news spread that Endre Szemerédi had solved a long-standing conjecture by Pál Erdös and Pál Turán on arithmetic progressions in sets of integers of positive density. I was able to obtain a preprint (I cannot remember in which way) and for the upcoming summer semester, I organized a seminar where this long and complex paper was to be studied and exposed by the participants in full detail. This turned out to be really hard work, understanding all the details and getting all estimates right. Among the (few) participants there was my thesis advisor Jacobs, he was 'passive', i.e., he rarely intervened, but he took notes meticulously. After that summer semester, Jacobs followed an invitation to Israel, where he had several friends from his specialty, ergodic theory. He had been invited to give a talk and he spoke about *New Results in Combinatorics*, where he presented what he had learned from our seminar: Szemerédis Theorem (which still had not been published). As he told me later, during the talk one of his dear friends, Hillel Furstenberg, sitting in the front row, apparently started snoozing after a while. He seemingly did no longer pay attention to the speaker. After the talk, Jacobs, somewhat irritated, asked his friend whether his talk had bored him. Furstenberg vividly responded: "No! No! Not at all! During your talk and inspired by it I suddenly had an idea how to tackle such a problem with the tools of ergodic theory." This was the starting event of what became well known as Furstenberg's approach[21] A new aspect, a combination of Fourier methods and combinatorics, was brought to the field by Gowers[22] in 2001, and by the celebrated Green-Tao Theorem[23] about arithmetic progressions of arbitrary length in the set of prime numbers (which

[20] A. Schönhage and V. Strassen, *Schnelle Multiplikation großer Zahlen*, Computing 7 (1971), 281–292.

[21] H. Furstenberg, *Ergodic behavior of diagonal measures and a theorem of Szemerédi on arithmetic progressions*, J. Anal. Math. 31 (1977), 204–256.

[22] W. T. Gowers, *A new proof of Szemeréi's theorem*, Geom. Funct. Anal. 11:3 (2001), 465–588.

[23] B. Green and T. Tao, *The primes contain arbitrarily long arithmetic progressions*, Ann. of Math. 167:2 (2008), 481–547.

has density zero). Many variants and generalizations of Szemerédi's theorem and the various proof methods have been worked out since their origin in the 1970s. Fully in awe, I cite Tao's words when in his talk at the International Congress of Mathematicians at Zurich in 2006 he called the various proofs of Szemerédi's theorem the 'Rosetta stone' for connecting disparate fields of mathematics.

(3) In 1994, Foata celebrated his 60th birthday and Doron Zeilberger brought up the idea of dedicating to him a special volume of the *Electronic Journal of Combinatorics*, and he asked me if I would be willing to act as an editor. I agreed and I could convince Jacques Désarménien and Adalbert Kerber to join me in this job. We solicited contributions and finally received a total of 27 papers that appeared in 1996 as volume 3(2) under the title *Foata Festschrift*[24].

One of the submitted papers was very special, something I had never seen before or after: it was Zeilberger's, I am inclined to say *legendary* article[25] on the proof of the alternating sign matrix conjecture. This is a text of 84 pages, packed with formulas, identities, inductions, derivations, forward and backward references. Doron was aware that no single referee (or a few referees) would be able or willing to undertake a checking and refereeing 'as usual'. So Doron organized the whole text in small pieces, each just a few lines long, and assigned to each piece a *checker*, who just had to verify the local deduction step – assuming that everything leading was correct and verified. Doron himself provided a list of 93 *proof checkers*, and if you go through the list then you really have a *Who's Who* of Enumerative Combinatorics of the day. In addition: each proof checker is characterized by Doron in nice words. The graph of proof pieces and their immediate dependence relation was very complex, and it contained cycles (due to inductive arguments). So what was needed was a *super-checker*, who could verify the assembly as a whole without checking each detail. Fortunately (for me as an editor) David Bressoud was willing to take this responsibility, I was and still am immensely grateful to him. His book *Proofs and Confirmations. The Story of the Alternating Sign Matrix Conjecture*[26] tells the story from his perspective.

Coming back to the original question: my choices for (1) and (3) are admittedly very subjective. If you forced me to name instead of a single result, or better: a stream of research, then I would opt for Szemerédi-Furstenberg-Gowers-Green-Tao and all the others who were involved.

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

Strehl: Again, I do not like rankings and charts. But one open problem (not for me to tackle, although I know quite a bit about it and I have treated it in my lectures in theoretical computer science over many years) continues to occupy me both from an intellectual and from a combinatorial viewpoint: the *P-vs-NP problem.* It is on the list of seven *Millenium Problems in Mathematics*[27], set up by the Clay Institute in 2000, together with, e.g., the Riemann Hypothesis. Someone called it *A gift from Computer Science to Mathematics*, and I am satisfied because it shows that the mathematical community now appreciates problems from computer science on a level playing field. The research on the *P-vs-NP problem* abounds with decision and optimization problems from discrete mathematics, not just any kind of problems, but real problems of high practical interest. Every combinatorialist should be aware of this (and most are by now, I guess). Computer scientists and mathematicians have learned to carefully discriminate between 'what can be done (or not) in principle' ((un-)decidability questions), and 'what can be done (or not) in practice' (complexity theory and complexity analysis). A fundamental lesson in this business is to find out the differences between 'deciding' and 'verifying', e.g., as a popular example: whether a graph <u>has</u> a hamiltonian circuit, or whether a collection of edges of a graph <u>is</u> a hamiltonian circuit. That is a question that touches epistemology. But also enumerative combinatorics is involved, I just mention the concept of #P-completeness

[24] `https://www.combinatorics.org/ojs/index.php/eljc/issue/view/Volume3-2`.

[25] D. Zeilberger, Proof of the refined alternating sign matrix conjecture, New York J. Math. 2 (1996), 59–68.

[26] D. M. Bressoud, *Proofs and confirmations. The story of the alternating sign matrix conjecture*, MAA Spectrum, Mathematical Association of America, Washington, DC; Cambridge University Press, Cambridge, 1999.

[27] `https://www.claymath.org/millennium-problems`.

and one of my favorite examples: it is easy to decide whether a bipartite graph has a *perfect matching*, but it is in general extremely difficult to count them. Just to sum up: this is a wide field with many exciting results, already 'classic' and recent, and deep open problems.

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

Strehl: It should become clear in the course of this interview that, despite some recurring themes, my work is not so coherent that something has emerged that should be continued as 'my work' as an entity. I have picked up problems and methods from my fields of interest and I have tried to contribute to the knowledge about them. If others find it interesting and try their hands, brains, and computers on it, I am satisfied.

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

Strehl: That is too big a question. I have never been a 'guru' or 'judge' and I have no aptitude to become one.

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?

Strehl: That is a notorious and multi-faceted question with a long history. My favorite mathematician of antiquity is Archimedes. If you had asked him that question, he would not have understood. For me, he represents the ideal blend of theory and practice in mathematics. So I am really not inclined to see or to draw for me a demarcation line between 'pure' and 'applied' in mathematics. Think of giants like Euler and Gauss, it just made no sense to them. But I am afraid that my opinion is naively unrealistic these days. Let me give you two examples to illustrate the 'cultural gap' that you find.

(1) Princeton University Press has brought out two massive encyclopedic volumes: "The Princeton Companion to Mathematics"[28] (2008) and "The Princeton Companion to Applied Mathematics"[29] (2015), each of which (separately) are fantastic compendia, I find. But when looking for intersections or cross-relations between them, I am disappointed and worried. It seems that there have emerged two worlds that decreasingly communicate. Or is it the sheer amount of knowledge produced by either party that separates them?

(2) If have witnessed (from a safe distance) bitter conflicts in math departments when it came to the distribution of resources and opening of positions. Extreme expressions of opinion were: "You 'pure people' don't receive research grants, so you don't merit new positions; instead, transfer to us those which become vacant", and "You 'applied people' fill your positions with so-called mathematicians, only running computers, who never have proved anything". Money, positions, influence etc. are the obvious battlefields where deeper differences and prejudices materialize.

There is another concern that I have. Mathematics will be (or already is) affected by the new computerized methods that go by names like simulation, data science, machine learning, etc. For Applied Mathematics this trend is unavoidable, I think, but it would deepen the cultural gap between them and mathematics in the traditional definition–theorem–proof style.

Mansour: What advice would you give to young people thinking about pursuing a research career in mathematics?

Strehl: When working in mathematics, phases of frustration are to be expected. Be prepared for that, and do not despair. Instead, refresh your mind by reading, say, a few chapters from "Proofs of THE BOOK[30]" and take comfort in saying: There is no guarantee that things come out that beautifully and elegantly

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

Strehl: 'Nature' plays an important role for me in many ways, from alpine hiking to botanical excursions, from photography to support for conservationist projects, from gardening to climate change, and more; another extended

[28] *The Princeton companion to mathematics*, Edited by Timothy Gowers, June Barrow-Green and Imre Leader, Princeton University Press, Princeton, NJ, 2008.

[29] *The Princeton companion to applied mathematics*, Edited by Nicolas J. Higham, Mark R. Dennis, Paul Glendinning, Paul A. Martin, Fadil Santosa and Jared Tanner, Princeton University Press, Princeton, NJ, 2015.

[30] M. Aigner and G. M. Ziegler, *Proofs from THE BOOK*, Springer, 6th. edition 2018.

area of interest is classical music (mainly piano and chamber music); I am concerned with politics and social matters, both for current affairs and from a historical perspective.

Mansour:$^{(c)}$ You also have a Ph.D. in Engineering and were involved in several projects. Would you tell us about one of your engineering projects for which combinatorics played a crucial role?

Strehl: To avoid a potential misunderstanding: My Ph.D. in Engineering, formally a *Dr.-Ing. habil.* in Theoretical Computer Science, only superficially turned me into an engineer, because – as mentioned above – at the FAU computer science belongs to the Faculty of Engineering. This degree, the 'Habilitation', grants full academic rights to the bearer and is based on the evaluation of merits in research (publications) and teaching (lectures, supervision of theses over many years). In addition, the candidate has to present a substantial and (at least in parts) original thesis – not just a compilation or synthesis of completed work – within a limited amount of time (1 year). My habilitation thesis *Zykel-Enumeration bei lokal-strukturierten Funktionen*[31] (Cycle enumeration for locally structured functions) is a 300-pages memoir, written in the spirit of the Montréal school of combinatorial species (Joyal, Leroux, Labelle, Bergeron, my friends from spending one year as an invited professor at Montréal in 1983/4), extending former work (partly in cooperation with Foata and Leroux) on the combinatorics related to Hermite, Laguerre, Jacobi polynomials. Various versions of multivariate Lagrange inversion in combinatorial terms play a central role. This was definitely not an engineer's work, but my colleagues from the CSD and the referees succeeded in convincing the 'true' engineers that this work was relevant for computer science! The thesis is really a work in enumerative combinatorics, unfortunately written in German (as the rules required in those days). I have sent copies to several colleagues, but I guess that only my Montréal friends have worked through the text from the beginning to the end – this effort has left traces in the standard treatise *Combinatorial Species and Tree-like Structures*[32] by Bergeron, Labelle, and Leroux. Substantial parts of my thesis, but not all, have been published piecewise elsewhere, written in English.

To make a relation with computer science from an engineering viewpoint, I would like to mention that about the same time (around 1990) when preparing my habilitation thesis I had a joint project with IBM that dealt with the testing, evaluation, and further development of the computer algebra software *Scratchpad 2*, which later became *Axiom*[33]. As a part of this project, we came up with an implementation of the cycle index series from species theory, as well as a package for computing with finite field extensions and their applications in algebraic coding theory. Really nice stuff from a conceptual point of view, but unfortunately suffering from innate problems of inefficiency resulting from the categorial typing system of *Axiom*. Later on, IBM terminated its activities in computer algebra and sold Axiom to a company that specialized in numerical software. Then it disappeared, it may still exist in zombie versions. I would have liked to see what can be achieved with such an ambitious 'categorial' setup with today's processors and software tools.

Mansour: Every discipline has its own methodology and thinking style. When we read articles from physics, mathematics, philosophy, or engineering, we observe the distinctions. What do you think about the differences between researching engineering and mathematics? How hard was it to switch from one to another?

Strehl: My by far most ambitious, time, and energy-consuming engineering project was the creation and administration of a special interdisciplinary course of studies called *Computational Engineering* at the Faculty of Engineering of the FAU. Its start in 1997 was encouraged and supported by the federal government of Germany as a part of an initiative to increase the percentage of students from foreign

[31] V. Strehl, *Zykel-Enumeration bei lokal-strukturierten Funktionen*, Habilitationsschrift, Institut für Mathematische Maschinen und Datenverarbeitung der Universität Erlangen-Nürnberg, 1989.

[32] F. Bergeron, G. Labelle, and P. Leroux, *Combinatorial species and tree-like structures*, Translated from the 1994 French original by Margaret Readdy, With a foreword by Gian-Carlo Rota, Encyclopedia of Mathematics and its Applications 67, Cambridge University Press, Cambridge, 1998.

[33] `http://axiom-developer.org/`.

countries at German universities. In our case, engineering students with a B.Sc. were invited to come to Erlangen with the goal to obtain a M.Sc. in the new discipline of computational engineering. The otherwise very strict rules for admission were liberated, mastering the German language was not a prerequisite. In a sense, we had to invent the rules and the mode of operation ourselves — and none of the very few main actors had any prior experience in such an undertaking.

I will not describe all the difficulties that we had to master, but one point is interesting: the local initiative came from a colleague from computer science, I was hired as a study advisor, but actually occupied the role of a manager. An interesting (and delicate point) was that the 'true' engineers from electric engineering, mechanical engineering, chemical engineering, science of materials, mechatronics, etc., who had to supply most of the courses, viewed this enterprise with reluctance or even disaffirmation. How could a degree in 'computational engineering', administered by computer scientists (i.e., 'non-engineers') be awarded by a Faculty of Engineering that stands in the respectable tradition of the German Diplom-Ingenieur? Needless to say that computer scientists did not have that class consciousness and had no problem switching their course language to English, as required.

It was a hard time (until 2007, and not much spare time for doing combinatorics) which taught me a lot about the different ways of thinking of engineers and computer scientists.

Mansour: Together with Dominique Foata and Adalbert Kerber, you founded a well-known, long-standing journal *Séminaire Lotharingien de Combinatoire* (SLC). Would you say a few words about the idea to establish it?

Strehl: Oh yes! At first: the SLC did not start as a journal, instead. it was created as a conference series in 1980. The basic motivation was this: young people from Europe had difficulties getting access to the legendary *Oberwolfach meetings*, where the 'big shots' from all over the world gathered in the idyllic Black Forest in Germany, not far from Strasbourg in France. Participation was 'by invitation only', i.e., the organizer of a one-week meeting had to present to the Oberwolfach director a list of colleagues that should be invited – and nobody else had a chance to get admission. We repeatedly had that problem with combinatorics meetings. It was Foata's idea to organize on a private basis combinatorics meetings shortly before or after an Oberwolfach meeting in combinatorics, so that young people could get into contact with celebrities from elsewhere. He asked Kerber, then at Aachen in the Rhineland, and myself to support this idea by alternately acting as local organizers for our meetings. We agreed, and since Aachen and Strasbourg were historically related by belonging to the middle part, *Lotharingia* (Lothringen, after Lothar, one of the grandsons of Charles the Great), after the dissection of the Holy Roman Empire into three parts (Treaty of Verdun in 843), Foata proposed to name these meetings *Séminaire Lotharingien.* [By the way: Erlangen is situated in *Franconia*, but the city did not even exist at that time. It would have belonged to the Eastern part (*Austria*), and the same applies to Bayreuth, where Adalbert Kerber moved shortly after the creation of the SLC.]

As for the SLC, we started by meeting three times a year and later changed to two meetings annually. As a rule, a meeting is held in a quiet place outside the big cities, runs over 2 and 1/2 days, usually, there are two invited speakers who lecture for 3 hours each. The remainder of the time is reserved for contributed talks (no prior selection, that is why it is called a seminar) and informal gatherings. In September 2021 we will have the 87nd meeting in Bad Boll in Southern Germany. In the early years, we had printed proceedings which were collected by the organizers and then composed and printed at the math department of the Strasbourg University. Since volume 32 (1994) and with the wider availability of TEXwe have switched to the format of a refereed electronic journal, following the example of Herb Wilf's Electronic Journal in Combinatorics. New volumes appear in time with the semi-annual meetings, but submissions from other sides are always welcome. At present Christian Krattenthaler (Vienna, Austria) acts as Editor-in-Chief, assisted by Jean-Yves Thibon (Marne-la-Vallée, France) and myself.

Mansour: One of your former students, Uli Sattler, in a recent interview to *Künstliche In-*

telligenz, answering the question on *Who are the people who inspire or inspired you most*? mentioned that you had a superb interaction with students. How do you feel when you read compliments about your supervision?

Strehl: I feel happy, of course! And I am grateful to Uli Sattler for stating publicly that my efforts over the years (with many lectures, seminars, and supervised B.Sc. and M.Sc. theses) have born fruit. In a sense, it was understood to me to offer my students ambitious lectures and furthermore any support for successfully completing their studies and, if appropriate, a good start into a scientific career. Uli Sattler is exemplary for that. There are others, notably students who took my computer algebra classes, who established themselves, I would like to mention: Jürgen Gerhard (who is co-author of the book *Modern Computer Algebra*, with J. v.z. Gathen, and has an important position at Maplesoft), Carsten Schneider (RISC) and Christoph Koutschan (RICAM), both at Linz where I had sent them for earning their Ph.D. with Peter Paule. Roberto Pirastu, by the way, is an unusual case: he was a truly excellent student and he also obtained his Ph.D. at Linz with Peter Paule, but immediately after that event entered a religious order.

Mansour: In one of your influential works, co-authored with Roberto Pirastu *Rational summation and Gosper-Petkovsek representation*[34], among others, proposed a new summation method for rational functions. Would you tell us about the main ideas behind this method?

Strehl: Well, the details are rather technical, but I will try to sketch the overall idea. The problem to be solved algorithmically is that of *indefinite summation* for rational functions (over some field k) in one variable. Given a rational function α, we want to determine rational functions β and γ such that $\alpha = \Delta\beta + \gamma$, where Δ is the difference operator with respect to the variable. β and γ are generally not unique, so one wants γ to be as small as possible (under a suitable degree criterion for the denominators of β and γ). Call two monic, irreducible polynomials f, g *shift-equivalent* if $g = E^i f$ for some integer i, where E is the shift-operator in the variable. The problem to be solved is first localized to shift-equivalence classes of polynomials appearing as irreducible factors in the denominator of α. This splitting is possible without factoring this denominator. Treating the summation problem over any single shift-equivalence class uses a concept that originates from the famous algorithm for indefinite hypergeometric summation by Bill Gosper (1978). For any proper rational function α, i.e., the degree of the denominator is less than the degree of the numerator, there are polynomials p, q, r such that

$$\alpha = \frac{Ep}{p} \cdot \frac{q}{Er} \quad \text{with} \quad \gcd(q, E^i r) = 1$$

for all $i \geq 1$. Marko Petkovšek (1992) showed that uniqueness of this representation can (essentially) be obtained by adding the requirement $\gcd(p, r) = 1 = \gcd(p, q)$. The polynomials p, q, r can be by computed by gcd and resultant computations, i.e., without factorization. This is what we called the *Gosper-Petkovšek representation* of a proper rational function. Localized to any shift-equivalence class (no problem, because it is a purely multiplicative statement) this representation provides all the information to optimally solve the summation problem over this class and then to piece together all the locally optimal solutions to a globally optimal solution.

Mansour: In one of your works *Lacunary Laguerre series from a combinatorial perspective*[8], you provided a combinatorial interpretation of Laguerre polynomials as the enumerators of a discrete structure (injective partial functions). Would you elaborate on this work and possible future research directions?

Strehl: As already mentioned above, *Laguerre polynomials* and *Laguerre configurations* appear very early. I spent the winter semester 1980/81 in Strasbourg and started working with Dominique Foata on the combinatorics of Laguerre polynomials, with special attention to so-called multilinear generating functions. We started by setting up a combinatorial model and then tried to re-derive with it the so-called *Hille-Hardy* formula[35], i.e., the bi-

[34] R. Pirastu and V. Strehl, *Rational summation and Gosper-Petkovšek representation*, J. Symbolic Comput. 20 (1995), no. 5-6, 617–635.

[35] H. M. Srivastava and H. L. Manocha, *A Treatise on Generating Functions*, J. Wiley, New York 1984. For earlier derivations by Miller and Lebedeff, see e.g. A. Erdélyi et al., Bateman Manuscript project, Volume 2.

linear generating function. Our goal was to obtain multivariate generalizations that were motivated by the combinatorial model. We succeeded, and the (new) analytical results were published in the Comptes Rendus[36]. The combinatorial part was first published in the proceedings of the Waterloo Silver Jubilee Conference[4]. I have used the ideas, concepts, and results in many subsequent papers, see below for the Jacobi configurations, in my habilitation thesis[31], in the recent article on lacunary Laguerre series[8] that you mention. We (i.e., Dominique Foata and myself) are very satisfied to have learned recently that the *Foata-Strehl* model is still alive in the work of others[37].

Mansour:[(d)] You have a series of papers are on the *Combinatorics of Jacobi configurations*. Would you tell us about Jacobi configurations and elaborate more on the related research?

Strehl: After dealing with the combinatorics and a bilinear generating for the Hermite polynomials, the *Mehler formula* (Foata, 1978), the Laguerre polynomials and the Hille-Hardy formula (mentioned just before), it was natural to take the next step up in the Askey-Wilson schema of orthogonal polynomials to the Jacobi polynomials. As already mentioned, a combinatorial model had been presented by Foata and Leroux[5].

To describe what this is, I first talk about *Laguerre configurations*[4]. These are ordered pairs of disjoint finite sets (A, B) together with an injective mapping $f : A \to A \uplus B$. The connected components under f are either cycles fully contained in A, and $cyc(f)$ denotes their number, or f-chains of points in A and ending in B. Points in B which are not f-images are singleton components. Summing over all Laguerre configurations $((A, B), f)$ with $A \uplus B = [n]$ with the $1+\alpha$ acting as f-cycle enumerator (α a parameter) one obtains the polynomial

$$\mathcal{L}_n^{\alpha}(x) = \sum_{A \uplus B=[n]} \sum_{f: A \to A \uplus B} (1 + \alpha)^{cyc(f)} (-x)^{\sharp B},$$

which is essentially (up to a normalizing factor $n!$) the classical generalized Laguerre polynomial $L_n^{(\alpha)}(x)$. The exponential generating function is easily obtained from this. Considering a bilinear generating function, i.e., a generating function for products $L_n^{(\alpha)}(x) \cdot L_n^{(\beta)}(y)$, means from a combinatorial perspective that pairs of superposed Laguerre configurations over the same set have to be considered.

Then the combinatorics for Jacobi: consider ordered pairs of disjoint finite sets (A, B) as before, but now injective functions $f : A \to A \uplus B$. and $g : B \to A \uplus B$, i.e., pairs of complementary Laguerre configurations. Now the question about the possible connected joint components of the pair (f, g) is less simple. An equivalent way to describe the situation is: consider (A, B) as a bi-coloring of the set $C = A \uplus B$, and mappings $h : C \to C$ with the property that each point from C has at most one h-preimage of each color. I let you the pleasure to find out exactly what the connected h-components look like, and, if you are ambitious, the task of determining the exponential generating function for these objects, with cycle counting parameters α and β for uni-colored cycles of either color.

The Jacobi polynomials have many interesting and important properties, specialisations and applications. I have written several articles[6,38,39,40] studying the interrelations between their analytical properties and the combinatorial model. My habilitation thesis, see [(c)] and [(f)], presents all this, and as a high point a combinatorial proof of Bailey's bilinear generating function for the Jacobi polynomials. I have also extended the model for the Jacobi polynomials to a much more general setup in my habilitation thesis: *locally structured endofunctions*, as the title of this thesis promises. This relates the combinatorial models à la Jacobi to the concept of multivariate Lagrange inversion, dear to my Montréal friends, in particular Gilbert Labelle.

Mansour: One of your interesting results is *Counting Domino Tilings of Rectangles via Re-*

[36] D. Foata and V. Strehl, *Une extension multilinéaire de la formule d'Erdélyi pour les produits de fonctions hypergeometriques confluentes*, Comptes Rendus Acad. Sci. Paris 293 (1981).

[37] For example, see A. D. Sokal, *Multiple Laguerre polynomials: Combinatorial model and Stieltjes moment representation*, arXiv:2104.08516, [math.CA], 2021.

[38] V. Strehl, *Jacobi configurations I: complete oriented matchings*, in: Combinatoire Énumérative, Montréal 1985, G. Labelle et al. (editors), Springer Lecture Notes in Mathematics vol. 1234, Berlin (1986), 294–307.

[39] V. Strehl, *Jacobi configurations II: a rational approximation via matching polynomials*, Actes du Séminaire Lotharingien de Combinatoire, 13e session, G. Nicoletti (editor), Publ. IRMA Strasbourg 316/S-13 (1986), 112–123.

[40] V. Strehl, *Jacobi configurations III: the Srivastava-Singhal generating relation revisited*, Discrete Math. 73 (1988), 212–232.

sultants[41]. You have reproved the classical formula for enumerating domino tilings of a rectangle due to Kasteleyn, Temperley, and Fisher. While rediscovering the results, one usually gets new insights into the original problem. What about your proof? Would you tell us about the main ideas behind it?

Strehl: I have thought for a long time that the KTF-formula deserves a truly combinatorial proof. It was clear to me (and perhaps others) that the result could be expressed as a resultant of Chebyshev polynomials, which are known to enumerate one-dimensional domino tilings. So the problem was to find out how one-dimensional and two-dimensional tilings are related combinatorially, and how this relation is expressed with the help of resultants. The key idea was to turn domino tilings of rectangles into systems of non-intersecting lattice paths and then to apply the celebrated Lindström-Gessel-Viennot[42,43] result which says that (under certain conditions) non-intersecting lattice path families are counted by the determinant of a matrix that encodes arbitrary lattice path families with given endpoints by a sign-reversing involution argument. For this to work in the situation of domino tilings the underlying graph (a rectangle) had to be augmented appropriately. So the proof is combinatorial, but it is not a bijective one, because of the sign-reversing involution.

The benefit of this approach is that it generalizes seamlessly if one replaces the Chebyshev polynomials in one variable by a multivariate version of these polynomials, which then encode the positions and orientations of the dominos. This leads to a particular kind of complementary tableaux (in the sense of tableaux appearing in relation to symmetric functions), the enumeration of which in the very simplest case turns out to give one of the classical Cauchy identities for the Schur functions.

Mansour: You have worked on several problems from combinatorics and theoretical computer science throughout your career. Which one is your favorite?

Strehl: I will give you one, not a big one, which is perhaps somewhat typical for my approach. In algorithmics, I was very much interested in sorting methods. My favorite sorting algorithm is named 'Shell sort'[44], after its inventor Donald Shell (in 1959). It is easy to implement (much easier than 'quicksort') and quite efficient (but not quite as efficient as quicksort), depending on the setup of parameters. The basic idea for sorting a linear array of keys is: first sort along disjoint arithmetic progressions of the same gap with which cover the array, and then iterate the procedure with successively smaller gaps until the whole array is sorted.

I find Shell sort attractive because of its complexity analysis poses interesting questions (not all resolved yet). The basic information needed for the analysis is: what is the total number of inversions of 2-sorted permutations of length $2n$? (*2-sorted* means that entries in even-numbered positions and entries in odd-numbered positions are already sorted separately.) The simple answer is $n \cdot 4^{n-1}$. This result appears in the literature in several places (Knuth, Sedgewick, Hofri, Flajolet-Vitter), using generating functions or involved summation techniques, all authors complaining about the difficulty of their proof. Sedgewick writes: "It is somewhat surprising that such a simple result requires such a long and complicated derivation". I took these complaints as a challenge and gave a direct combinatorial proof in *Inversions in 2-ordered permutations – a bijective counting*[45]. In the Appendix of this (very short) article, I present a one-page diagrammatic proof, i.e., a 'proof without words' and without any formula, just showing some Dyck-like lattice paths, cut into pieces, reflected, reordered, and pasted. That's all!

Another of my papers (together with my student Robert Stoyan), *Enumeration of Hamiltonian circuits in rectangular grids*[46] deals with the enumeration of hamiltonian cy-

[41]V. Strehl, *Counting domino tilings of rectangles via resultants*, Special issue in honor of Dominique Foata's 65th birthday (Philadelphia, PA, 2000), Adv. in Appl. Math. 27 (2001), no. 2-3, 597–626.

[42]I. Gessel and X. Viennot, *Binomial determinants, paths, and hook length formulae*, Adv. Math. 58 (1985), 300–321.

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

[44]`https://rosettacode.org/wiki/Sorting_algorithms/Shell_sort`.

[45]V. Strehl, *Inversions in 2-ordered permutations–a bijctive counting*, Bayreuth. Math. Schr. 28 (1989), 127–138.

[46]R. Stoyan and V. Strehl, *Enumeration of Hamiltonian circuits in rectangular grids*, Journal of Combinatorial Mathematics and Combinatorial Computing 21 (1996).

cles on grid graphs. The grid graph $G_{m,n}$ is the vertex set $[m] \times [n]$ (with $[n] = \{1, 2, \ldots, n\}$, as usual) with the nearest neighbor edges. A problem discussed in the the early 1990s was whether, when considering for fixed m the Hamiltonian cycles of $G_{m,n}$ as a language over the alphabet of possible columns, this language was rational or not. On the one side, from conditions on what was admitted for two neighboring columns, it appears to be a so-called *local language*, and hence rational. On the other side: *hamiltonicity* of a subset of edges is a *global* and not a *local* condition. In that situation, we devised a procedure which for each specific m constructs a finite automaton that accepts precisely the language of hamiltonian cycles on $G_{m,n}$. In this way, we are able to provide the rational generating functions for small values of m. The size of the automata grows very fast with m, so we did what we could do with our computers at that time.

Mansour: One of your interests is computer generation and enumeration of combinatorial objects. Would you describe some of the enumerative results obtained by computers? What do you think about computer-assisted proofs?

Strehl: I guess that your question is not about my own work, for which I would point to the examples given in this interview, but more generally about the role of computers in combinatorial research.

I remember very well the truly hot discussion in 1976 about Appel and Haken's computer-assisted proof of the *Four Color Theorem*. If there had been a vote to decide about the acceptability or not of such methods, I think that a majority of mathematicians would have voted against it. I have no problem with computer-assisted proofs. For me, the computer has become an indispensable research tool in several ways, such as telescopes for astronomers and particle accelerators for physicists. I can do explorations, verifications, constructions in a way that would be impossible otherwise. But, and that is a matter of taste or moral, if it comes to proving, I prefer to rely on traditional methods, because (see [(e)]) I want to make clear what for me is "interesting" or "difficult" and what my line of reasoning is. I maintain the rule that "A good article needs a raison d'être", and that's what still needs a human co-author.

Speaking about computers in combinatorics, in particular, I would like to express my appreciation for the work of two of my former students: Carsten Schneider and Christoph Koutschan.

Carsten had as his Ph.D. project the realization of a summation algorithm by Michael Karr (1981/1985), the difference analog of the celebrated Risch algorithm for integration. To our knowledge, Karr's ideas had never been properly implemented before. Carsten did a fantastic job in penetrating and reworking the theoretical basis of the algorithm, and then in implementing it. When he was finished with his project I urged him to look for demanding applications. He succeeded in an astounding way: he got into contact with the physicist Johannes Blümlein from the DESY research institute, working in quantum field theory, and together they put Carsten's program into action with incredible success. This is a symbiosis of 'theory' and 'application' in perfection.

Christoph took another direction, in short: the Zeilberger way. He pushed computation of difference operators for hypergeometric sums to unbelievable extremes and excelled e.g. with applications in lattice path and plane partition enumeration. His article *Proof of George Andrews's and David Robbins's q-TSPP conjecture*[47] was awarded 2016 AMS David P. Robbins Prize. I am immensely satisfied with that, even if the object he juggles with is so big that one never would try to print them. I can understand and love the setup of the combinatorial problem and the plan of attack, but the results in detail for me (and presumably for any other human mathematician) are beyond comprehension.

Chapeau! for Carsten and Christoph.

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

Strehl: I will be brief here because the examples from my work that I refer to in this interview speak for themselves. Combinatorial reasoning for me is: analyzing and synthe-

[47] C. Koutschan, M. Kauers, and D. Zeilberger, *Proof of George Andrews's and David Robbins's q-TSPP conjecture*, Proc. Natl. Acad. Sci. USA 108:6 (2011), 2196–2199.

sizing discrete structures with the goal to obtain information about their counting and enumeration properties, mainly in terms of summations, recurrence formulas, generating functions. ‘Enumeration’ is almost always there, either explicitly or behind the curtain. So I am usually *not* interested in identities that are just identities, maybe true or not, maybe difficult or not. I could set up machinery (and, indeed, if have done so around 1990, at the beginning of the Zeilberger-Wilf era, just for fun), that constructs and proves automatically binomial identities of arbitrary size and complexity, but no ‘semantics’ whatsoever. ‘Complexity’ and ‘Truth’ alone do not imply ‘Interest’, or ‘Importance’, let alone ‘Beauty’.

Mansour:$^{(f)}$ 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”?

Strehl: Here I would like to tell you the story about a very particular result, that I like a lot (the result and the story). In spring 1992 I received an email from combinatorial colleagues at the university of Bielefeld (Germany), asking whether the following binomial identity:

$$\sum_{k=0}^{n} \binom{n}{k}^2 \binom{n+k}{k}^2 = \sum_{k=0}^{n} \binom{n}{k} \binom{n+k}{k} \sum_{j=0}^{k} \binom{k}{j}^3, \quad (1)$$

which relates the Apéry numbers $a_n = \sum_{k=0}^{n} \binom{n}{k}^2 \binom{n+k}{k}^2$ and the Franel numbers $f_n = \sum_{j=0}^{k} \binom{k}{j}^3$, was true for all n. The question originated from work in diophantine approximation by the number theorist Assmus Schmidt from Kopenhagen (Denmark), who at that time was a guest at Bielefeld and who had asked the local combinatorialists for help, because that was the kind of problems combinatoralists seemed to be good for. They had made extensive numerical verifications, but without obtaining a clue for a proof. Needless to say that the identity is not contained in H. W. Gould’s collection of combinatorial identities. The conjectured identity (1) gripped me immediately for several reasons: it was motivated by a number of theoretical problems (not just an identity); it dealt with objects that I had some experience with (Apéry and Franel numbers, Legendre polynomials, so it was a *hypergeometric* question); it was a challenge to prove it, in particular, it would be an ideal test case for the just emerging algorithmic Zeilberger-Wilf technique; if true, what would it mean combinatorially? The whole panorama of aspects and possibilities excited me, I started exploring it and within a very short time I could give a positive answer. More than that! I could immediately provide a two-parameter generalization:

$$\sum_{k=0}^{n} \binom{n}{k}^2 \frac{(1+\alpha+\beta+n)_k}{(1+\alpha)_k(1+\beta)_k} = \sum_{k=0}^{n} \binom{n}{k} \frac{(1+\alpha+\beta+n)_k}{(1+\beta)_k} \sum_{j=0}^{k} \binom{k}{j} \frac{(1+\beta+k-j)_j}{(1+\alpha)_j}, \quad (2)$$

where $(a)_n = a(a+1)(a+2)\cdots(a+n-1)$ is the familiar rising factorial, alias *Pochhammer symbol*. How was that possible? When analyzing the purported identity (1) and concentrating on the appearance of the coefficients $\binom{n}{k}\binom{n+k}{k}$ of the Legendre polynomials on both sides, I realized (the “eureka moment”) that I had seen something similar before. I recalled my efforts (three or four years earlier, in my habilitation thesis[31]) to give a combinatorial proof of Bailey’s bilinear generating function for the Jacobi polynomials in terms of Appell’s F_4 series (from 1938), already mentioned in $^{(b)}$. And indeed, a quick check showed that identity (2) is a consequence of (indeed, it is equivalent to) this classic result, where α and β are the usual parameters for the Jacobi polynomials $P_n^{(\alpha,\beta)}(x)$. So implicitly I had given a combinatorial proof of the conjectured identity even before having seen it. I was so amazed by this lucky surprise that I wrote an extended tutorial article[48] covering all the different aspects of binomial identities: hypergeometric, combinatorial, algorithmic, including six (!) different proofs of identity (1). In all modesty, I think that this article is still interesting reading.

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

Strehl: There are several ‘loose ends’ that wait for completion and to which I return from time to time. At this place, I would like to mention one particular problem that has kept me busy for many years, from 2009 on, to be precise. At the 63rd meeting of the SLC, Christian Krattenthaler known for his superb

[48] V. Strehl, *Binomial identities – combinatorial and algorithmic aspects*, Discrete Math. 136 (1994), 309–346.

expertise in determinants introduced me to a young physicist, Arvind Ayyer, who had told him about a problem for a very particular class of matrices. He had a precise conjecture about their characteristic polynomials, but had been unable to prove it. He had asked several experts for help, but nobody had been able to do it. I promised to try my hands on it when being back home at my computer. So I did, with nothing more than the definition of the matrices and a vague idea that they were related to a problem in combinatorial statistical physics, namely a Markov process that goes by the name *Asymmetric exclusion process with annihilation*.

Ayyer and Mallick[49] from Saclay (France) had just completed a long and detailed paper on that process – with just one item missing: the evaluation of the characteristic polynomial of the generator matrices of that process. Examples showed that the polynomial factors nicely into linear factors. I tried this and that, with no result, and then followed the advice that I had often given to my students (in image processing and quantum algorithms): "If you have no idea what to do, try orthogonal transforms and watch carefully what happens". I took the Hadamard transform (for good reasons) and obtained matrices that looked quite similar to the input matrices – but it appeared to me that after a suitable rearrangement of rows and columns a triangular matrix would show up, which would immediately prove the conjecture. The nutshell had a crack, but it took a while to figure out what precisely that 'suitable rearrangement' would be in general. Anyway, the idea worked and the conjecture was solved. The joint article with Ayyer[50] reports about the problem and the successful approach.

But I went further: it was clear to me that the same 'Hadamard-trick' would work for a generalized model with many more parameters. But then I asked myself: 'What is the partition function for this generalized model?´ Small cases gave hints about the answer, but the proof (via the *Transfer-Matrix Method*) turned out to be a hard piece of work which I published only recently in volume 81 of the SLC[51]. This is not the end of the story. During my investigations, I examined a certain linear system of equations, parametrized by strict partitions, that has vectors of rational function as solutions. The denominators behave quite nicely, but the numerators are unwieldy – but it appeared to me that they might somehow be related to symmetric polynomials, notably Schur polynomials over two or more alphabets. I was able to make this precise and to prove it in some interesting cases, but an answer, in general, seems elusive. The story so far is contained in another article that appeared recently in a book[52] devoted to my longstanding friend Peter Paule, with whom I share the passion for the triad of enumerative combinatorics, special functions, and computer algebra.

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

[49] A. Ayyer and K. Mallick, *Exact results for an asymmetric annihilation process with open boundaries*, J. Phys. A: Math. Gen. 43 (2010), 04503.

[50] A. Ayyer and V. Strehl, *The spectrum of an asymmetric annihilation process*, DMTCS proc. AN (2010), 461–472.

[51] V. Strehl, *The Fully Parametrized Asymmetric Exclusion Process With Annihilation*, Sém. Lothar. Combin. 81 (2020), Article B81a.

[52] V. Strehl, *Trying to Solve a Linear System for Strict Partitions in 'Closed Form'*, Algorithmic Combinatorics: Enumerative Combinatorics, Special Functions and Computer Algebra, In Honor of Peter Paule on his 60th Birthday, Veronik Pillwein and Carsten Schneider (eds.), Springer Nature 2020.