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Interview with Igor Pak

Toufik Mansour

Igor Pak did his undergraduate studies at Moscow State University (1989–1993). He obtained a Ph.D. from Harvard University in 1997, under the direction of Persi Diaconis. He became an assistant professor of applied mathematics at Massachusetts Institute of Technology (MIT) in 2000. From 2005 up to 2007, he continued at MIT as an associate professor of applied mathematics. From 2007 to 2009 he was an associate professor of mathematics at University of Minnesota. Since 2009 he is a full professor of mathematics at UCLA. Professor Pak has given talks at many conferences, including an invited talk at the International Congress of Mathematicians in Rio de Janeiro, Brazil in 2018. Professor Pak has served as a member of the editorial board in numerous journals, including Discrete Mathematics, Associate Editor (2009–2017), Transactions of American Mathematical Society and Memoirs of American Mathematical Society, Associate Editor (2014–2018), Pacific Journal of Mathematics, Editor (2016–2018), and Mathematical Intelligencer (2021-).

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

Pak: Thank you, Toufik! It is an honor to be asked. I have thought a lot about this question over the years[1] and concluded that there is no good answer, or at least there is no *one* good answer. There are three types of answers I typically give: serious, contemplative and confrontational, so let me give you a brief version of each.

Serious answer: Combinatorics is a large area of discrete mathematics comprised some interrelated smaller subareas (enumerative combinatorics, graph theory, probabilistic combinatorics, algebraic combinatorics, etc.) Each of these subareas has its own rich culture, goals, and traditions, making a broad generalization neither possible nor desirable.

Contemplative answer: I subscribe to Rota's approach to the question[2] where he compares areas of mathematics to warring countries. The borders are rarely straight lines, as they tend to be produced by centuries of battles, making them not easily describable. Some countries have a nontrivial topology (for example, non-simply connected or even disconnected), some borders are disputed, and even the existence of several countries is subject to debate. On top of that, some countries have various types of federal systems, with only a few laws governing different regions which can have different languages, uniquely distinct history, specialized cuisine, etc.

Now, how is one supposed to define large diverse countries like the USA, Russia or India? The only way is really to give a vague general description, before moving to discussions of history how the country came to be, with its complexity of individual states and regions. Combinatorics as a field has all the complexity

Toufik Mansour is a professor of mathematics at the University of Haifa, Israel. His email address is tmansour@univ.haifa.ac.il
[1]For example, see my old blog post https://wp.me/p211iQ-bQ
[2]G.-C. Rota, *Discrete thoughts*, Chapter 6.

of these large diverse countries. Thus to define combinatorics is to discuss its history of how it started centuries ago with elementary enumerative questions, and in the past decades has rapidly expanded to acquire a vast scientific territory[3]. Then you proceed to define/discuss all subareas[4].

Confrontational answer: Combinatorics is a large advanced area of mathematics, on par with algebra and analysis. It is just as diverse as these areas and thus cannot be easily characterized. It is just as important, interesting, competitive, and highly developed as these areas, and must be treated equally when it comes to hiring, peer review, or research awards. Any attempts to suggest that it is somehow elementary, trivial or recreational are highly disrespectful and come from ignorance[5]. However, any claim that combinatorics is now as accepted and as prominent as other areas, that "we are all combinatorialists now", also comes from willful blindness. The area has been dismissed and mistreated for decades. Right now it is popular and many excellent combinatorialists were hired by the top universities in the last few years. Unfortunately, it is rather premature to declare victory. People's attitudes do not disappear or change overnight — they just stop being openly expressed for the sake of comity and politeness[6]. It will probably take some time until combinatorics is treated as truly equal.

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

Pak: I think it is pretty good actually, better than ever. Despite my previous answer, the new generation of mathematicians tends to be very respectful of the area. These days many students in all fields learned to appreciate and use combinatorial results and ideas in their research. This has been a dramatic transformation over my lifetime, and I expect things to only improve in the future.

Mansour: What have been some of the main goals of your research?

Pak: I worked in many areas of combinatorics, mostly doing problem-solving rather than theory building[7], so I am not sure there is a simple or concise answer to this question. In general, I like to frame questions computationally, from a broad theoretical point of view. For example, when I see a formula for the number of certain combinatorial objects or a closed-form generating function, I am interested if there is a way to sample these objects uniformly at random. And if not, why not? When I see a nice combinatorial identity, I ask if there is a bijective proof of this identity? And if not, why not and what does that even mean? Similarly, I ask what does it mean that some numbers have no close formulas, or a combinatorial interpretation, etc.

My questions tend to be vague and often require some effort to state them in the language of computational complexity, discrete probability or group theory. As a result, sometimes I get to publish in these adjacent fields. However, often enough these questions lead to new unexplored combinatorial questions, which get resolved in rather conventional combinatorics papers.

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?

Pak: I grew up in a working neighborhood in the south of Moscow, without any interest in mathematics or anything else. My parents worked a lot and were happy when I came home with mostly 4s (in the Russian system, the grades are 1–5), and I was happy they never pushed me to do anything after school. I think I played a lot of ice hockey outdoors and never did any homework, that is all I can remember.

Things changed suddenly in the fifth grade

[3]Of the many excellent books on the history of combinatorics, I especially like R. Wilson and J. J. Watkins (editors), *Combinatorics: Ancient & Modern*, Oxford Univ. Press, 2015.

[4]This is the approach I chose when I completely rewrote the Wikipedia *Combinatorics* article `https://en.wikipedia.org/wiki/Combinatorics` (see also a quick discussion on my blog: `https://wp.me/p211iQ-2d`).

[5]For example, see quotes by Henry Whitehead, George Dantzig or Jean Dieudonné on my lengthy "*What is combinatorics?*" collection of quotes `https://tinyurl.com/4v7rwnn2` or a story how such views by Peter May were confronted by László Lovász, in his interview linked here: `https://wp.me/p211iQ-t2`.

[6]Occasionally, their views find a way to reemerge in small ways. For example, see my collections of "*Just combinatorics*" quotes: `https://tinyurl.com/3c6v55ma`.

[7]W. T. Gowers, *The two cultures of mathematics*, in Mathematics: frontiers and perspectives, AMS, Providence, RI, 2000, 65–78.

when I discovered a "new rule" for computing percentages. To compute 60% of 20, I used $(20 \times 60)/100$, which seemed easier than $(20/100) \times 60$, as there was no decimal point issue. I remember getting a 1 on some percentages quiz, which did not bother me. But then the teacher berated me in from of the whole class claiming that I cheated all answers since my "new rule" was not in the teacher's manual and thus could not be true. I complained to my father. I knew I did not cheat, so the teacher must be wrong. My father knew about commutativity of multiplication and sent me to a better middle school further away from my apartment building.

In my new school, the math teacher was very enthusiastic about the subject and sent us all to a local (Chertanovo) math competition. This was one of my unhappiest memory. While I solved only two problems out of five, everyone else claimed to have solved four or five, making me feel inadequate. When the results were announced a few weeks later, turned out that I got the first prize because I did solve two problems and everyone else was just clueless about the meaning of the word "solve".

My father recognized that maybe I should study math more seriously. He asked around and suggested I attend the free "math circle", which happened once a week at a magnet High School 57 in the center of Moscow. Eventually, I became good, got accepted to that high school, some years later to Moscow University, etc. I received a lot of help from many teachers, mentors, and advisors along the way, but it was my father, who figured out what I should be doing with my life.

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

Pak: There were no specific problems, but I did like to read math textbooks. When I was in college, to make ends meet I got involved with used book sales. This gave me access to a lot of textbooks that I read obsessively, learning a great deal of math along the way. One Summer, my large extended family decided to go on vacation together, taking a boat along the Volga river from Moscow to Astrahan and back.

This was a very long, very cheap, and very boring vacation. When others were entertaining themselves by reading mystery novels, I read the first volume of Stanley's *Enumerative Combinatorics*. I was completely fascinated by the book which I read cover to cover even though I could not solve most exercises. I am still working on some of them...

Mansour: What was the reason you chose Harvard University for your Ph.D. and your advisor Persi Diaconis?

Pak: There were two simple reasons. First, I enjoyed learning. I knew that in addition to combinatorics I wanted to learn discrete probability which I did not know at all at the time. I met Diaconis and he was very charming and inquisitive. It was clear to me that if I go work with him I would have to learn a great deal. That is exactly how it worked out.

Another reason was my sorry state of finances. At the time, I was a refugee immigrant to the US, who arrived in New York by himself with about \$250 and a suitcase full of math textbooks. I lived in rather extreme poverty on public assistance, studying English and preparing for my entrance exams (TOEFL and GRE).

My monthly food budget was about \$80, while the application fee to Harvard was a hefty \$75. At the same time, many other grad schools charged only \$50, so after careful consideration I chose to apply to only five of those. A dear friend of mine, Sasha Astashkevich, told me he believes in me. He offered to pay the \$75 fee to Harvard, on condition that I apply and pay him back only if I get in. I did both.

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

Pak: I studied mixing times of various natural random walks on S_n and other related finite groups. I used *strong uniform time* arguments, which are combinatorial rules to stop the random walks so that the resulting distribution is uniform. Few years prior, Aldous and Diaconis[8] introduced this approach to study the mixing times, and I found it to be a nice blend of both combinatorial and probabilistic analysis.

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

Pak: I *always* start with a specific problem. Even if the problem is ill-defined or in-

[8] D. Aldous and P. Diaconis, *Strong uniform times and finite random walks*, Adv. Appl. Math. 8 (1987), 69–97.

approachable, without the problem, I do not feel I know what I am talking about. When you have a good problem, you can try to understand it, dig around, read the literature on the subject, do some explicit calculations, look for bridges to other areas, etc.

Only very occasionally I completely resolve the problem. More frequently, I either resolve some special cases, or reformulate the problem, or discover a related but more accessible problem, etc. Occasionally, more often than that I care to admit, there no progress whatsoever. I do not consider this a failure, as I enjoy the learning process. Sometimes, this problem can crawl back a few years later from a different angle, and I start anew.

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

Pak: Yes. Always. This is partly because I want things to be true, or, more frequently, false[9]. This is often because I have tools for only one direction, and make a convenient bet. I get things wrong sometimes and waste time. Sometimes, you just can not tell which way things will turn out.

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

Pak: I do not know. I think people like my proof of the *hook-length formula*[10] and my paper[11] on *graph liftings*. Based on citations, I think people also enjoy my *partition bijections* survey[12].

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

Pak: Rather than go over a safe list of everyone's favorite million-dollar problems, let me mention a few lesser-known conjectures. First, recall that *Hilbert's Third Problem* remains open for the sphere $\mathbb{S}^3$ (the hyperbolic space $\mathbb{H}^3$ is just as difficult)[13]. Formally, the conjecture claims that two spherical polyhedra are scissors congruent if and only if they have the same volume and *Dehn's invariant*. In particular, is it true that all spherical tetrahedra with rational dihedral angles and the same volume are scissors congruent? We are very far from resolving even this special case.

Second, let me mention a curious *Generating Primes Problem* which asks to give a *deterministic* polynomial-time algorithm for finding a prime on the interval $[n, 2n]$, thus derandomizing *Bertrand's postulate* which has a classical probabilistic algorithm[14]. It is a good bet that one can do this by testing primality of $n + x$, overall $0 \leq x \leq C(\log n)^2$, but this is nowhere close to being proved[15].

Third, there is a fascinating *Skolem's problem* which asks if it is decidable whether a sequence $\{a_n\}$ defined by a linear recurrence with constant integer coefficients and initial values has at least one zero:

$$a_{n+1} = c_0 a_n + \ldots + c_k a_{n-k}, \text{ for all } n \geq k,$$

where $a_0, \ldots, a_k, c_0, \ldots, c_k \in \mathbb{Z}$. This problem is so basic and fundamental, it is rather embarrassing that we do not even have a good intuition which way it will go[16].

Finally, let me mention one problem I definitely *do not want* to see resolved: the *P vs. NP* problem. I can sort of imagine how any solution could destroy a delicate balance in computational complexity, with its powerful theorems, beautiful reductions, and the exhausting multitude of complexity classes[17]. Fortunately, the problem is so difficult it is unlikely to be resolved in my lifetime.

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

Pak: It would be exciting to see more connections and applications of computational complexity to enumerative and algebraic combina-

[9] See my blog post why I truly enjoy disproving conjectures: `https://wp.me/p211iQ-uT`.

[10] J.-C. Novelli, I. Pak and A. V. Stoyanovsky, *A direct bijective proof of the hook-length formula*, Discrete Math. & Theor. Comp. Sci. 1 (1997), 53–67.

[11] F. Chen, L. Lovász and I. Pak, *Lifting Markov Chains to Speed up Mixing*, in Proc. 31-st STOC (1999), ACM, New York, 275–281.

[12] I. Pak, *Partition bijections*, a survey, Ramanujan J. 12 (2006), 5–75.

[13] For example, see Chapter 1 in J. L. Dupont, *Scissors congruences, group homology and characteristic classes*, World Sci., River Edge, NJ, 2001.

[14] For example, see Section 7.1 in A. Wigderson, *Mathematics and computation*, Princeton Univ. Press, Princeton, NJ, 2019.

[15] For example, see W. Banks, K. Ford and T. Tao, *Large prime gaps and probabilistic models*, arXiv:1908.08613.

[16] For example, see J. Ouaknine and J. Worrell, *Decision problems for linear recurrence sequences*, in Reachability problems, Springer, Heidelberg, 2012, 21–28.

[17] For example, see S. Aaronson, $P \overset{?}{=} NP$, in *Open problems in mathematics*, Springer, Cham, 2016, 1–122.

[18] See the expanded version in I. Pak, *Complexity problems in enumerative combinatorics*, arXiv:1803.06636.

torics. Let me refer to my ICM survey[18] for the entry point to the subject.

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

Pak: Objectively, the answer is yes. Some areas are more popular and have more influence than others, it's a fact of life. Should this be the case? Thats complicated. Everything inside of me suggests the areas should be treated equally, without a trace of discrimination. Yet, it is hard to argue with reality.

To resolve this conundrum, think of areas as live organisms. When they are born and taking first steps they are largely weak and unimpressive. When the area matures, acquires refined tools and applications to other fields, it reaches the height of its influence. Sometimes the area gets so popular, parts of it specialize, develop their own identity, tools, and problems, and eventually separate.

As new fields emerge with their own powerful tools and the old applications get exhausted, the old tools can no longer compete. Soon enough, the area goes into decline. Occasionally, new tools or bridges to other fields are discovered and the area reemerges in prominence. That is the circle of life. But as long as you are respectful to other areas at all stages of their development and do not judge them out of ignorance, it is ok to acknowledge the differences.

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?

Pak: I think the distinction is real even if it is a relatively recent phenomenon. Clearly, it is rather important whether you are trying to understand the world as it is, or as you imagine it to be. This does not mean there is some kind of antagonism. On the contrary, pure and applied mathematics have a sort of symbiotic relationship with obvious mutual benefits.

Note that the difference can be delicate. For example, I am thinking of computer experiments aiming towards understanding the nature of some combinatorial objects as pure math, while computer experiments with real-life data sets as applied math, even if the code and statistical analysis are fairly similar. It is the intention that counts, not the type of work.

Personally, I am squarely in the pure camp, in a sense that in my work I do not aim for practical applications. In the rare instances when my work does have such potential, I am happy to leave such development to others.

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

Pak: First, make friends. Develop an extensive network of working relationships both in your area and across mathematical sciences. As you mature and gain expertise, so will your friends. You want to be the one they are going to call when they have questions in your area. When they do, go out of your way to help them. On the other hand, when your research leads you to problems in another field, do not be shy to ask for help. Mathematics is a highly specialized field, so collaboration is basically the only way to overcome that.

Second, learn to write well and give good talks. This will help you stand out and communicate your results both to people in the area and to a wider audience. I am not sure I mastered either skill, but I have thought quite a bit about writing, and encourage you to read both papers[19,20] I wrote on the subject.

Third, be entrepreneurial. Organize study groups, seminars, workshops. Offer to give talks, do not wait to get invited. Attend talks and conferences in other areas. Ask questions and give answers on `MathOverflow`. If you see an interesting problem in the paper you can resolve in some special case, reach out to the authors. Ask your colleagues about their favorite problems. Because – *hey, you never know...*

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

Pak: No, I have nothing to tell. This is not because I want to appear so mysterious, but I do like to keep my private life private.

Mansour: One of your great works is, *Product replacement algorithm and Kazhdan's property* (T), co-authored by Alexander Lubotzky, published in *Journal of the AMS*. What is the product replacement algorithm and why

[19] I. Pak, *How to write a clear math paper*, Jour. Human. Math. 8 (2018), 301–328.
[20] I. Pak, *How to tell a good mathematical story*, Notices of the AMS (2021), to appear, `https://tinyurl.com/2j3cf8re`.

is it important? Could you tell us about the Kazhdan property and how these two concepts are related? Have there been any important follow-up results related to that paper?

Pak: The *product replacement algorithm* (PRA) is a powerful practical method to general random group elements. Roughly, it is a random walk on the graph of generating k-tuples of a finite group G, for a fixed k (it is called the *product replacement graph*). Soon after this algorithm was introduced in 1995[21], a number of mathematicians across different fields attempted to prove the remarkable practical performance of PRA, all with limited success.

I was fascinated by the problem, and spend several years as a postdoc studying it. Eventually, I proved that PRA works in polynomial time using a technical Markov chain argument[22]. This completely resolved the problem from the theoretical computer science point of view, but the mystery remained, as my $O(\log^9 |G|)$ upper bound was nowhere close to the (nearly) linear time experimentally observed performance of the PRA.

With Lubotzky, we realized that the product replacement graphs are the Schreier graphs of the action of the group $\mathrm{Aut}(F_k)$. This immediately implied that these graphs are expanders if $\mathrm{Aut}(F_k)$ has Kazhdan's property (T). The latter was a well-known open problem with a negative answer known for $k = 2$ and $k = 3$, so the implication we discovered was a castle built on sand.

This was back in 2001. Just a few years ago, in a remarkable computer-assisted breakthrough, the property (T) was established for all $\mathrm{Aut}(F_k)$, $k \geq 5$[23,24]. Combined with our work, this proves that the product replacement graphs are expanders indeed, thus giving the most satisfactory ending to the PRA story.

Mansour: Kronecker coefficients, as 'tools' from Representation theory, are used to describe the decomposition of the tensor product of two irreducible representations of a symmetric group into irreducible representations. How do they come into play in combinatorics in general and in your research in particular?

Pak: Multiplication of characters gives a ring structure to the space of characters of any finite or compact group, but the reason the product is a nonnegative sum of characters (as opposed to a virtual character), is fundamentally a consequence of representation theory. This explains why the structure constants of character multiplication play a crucial role in our understanding of the nature of irreducible representations.

For $\mathrm{GL}_n(\mathbb{C})$, these structure constants are called *Littlewood–Richardson* (LR-) *coefficients*. They have a classical combinatorial interpretation as either the numbers of certain Young tableaux or as the numbers of integer points in polytopes (as *Gelfand–Tsetlin patterns* or *Berenstein–Zelevinsky triangles*). The real reason for their compact combinatorial description is a subtle consequence of the highest weight theory, even if this is not how they are usually presented (or how they were invented).

For S_n, these structure constants are called the *Kronecker coefficients*:

$$\chi^\lambda \cdot \chi^\mu = \sum\nolimits_{\nu \vdash n} g(\lambda,\mu,\nu)\chi^\nu .$$

It was shown by Murnaghan back in 1938, that in the stable case they generalize the LR–coefficients, which naturally raised the question if they have a combinatorial interpretation. This continues to be one of the most celebrated problems in the whole algebraic combinatorics, which is also very close to my work.

In part due to the lack of a combinatorial interpretations, Kronecker coefficients are incredibly difficult to compute or even to estimate[25]. Just to give you an idea how little we know about them, for the three *staircase diagrams* $\delta_k = (k-1,\ldots,2,1) \vdash n$, where $n = \binom{k}{2}$, the best known bounds are:

$$1 \leq g\big(\delta_k,\delta_k,\delta_k\big) \leq f^{\delta_k} = \sqrt{n!}\, e^{-O(n)}.$$

The upper bound is probably closer to the truth, so improving the lower bound is a major challenge.

Mansour: In one of your papers *Asymptotics of principal evaluations of Schubert polynomials for layered permutations*, co-authored by

[21] F. Celler, C. R. Leedham-Green, S. H. Murray, A. C. Niemeyer and E. A. O'Brien, *Generating random elements of a finite group*, Comm. Algebra 23 (1995), 4931–4948.

[22] I. Pak, *The product replacement algorithm is polynomial*, in Proc. 41-st FOCS (2000), 476–485.

[23] M. Kaluba, P. W. Nowak and N. Ozawa, *$Aut(F_5)$ has property (T)*, Math. Ann. 375 (2019), 1169–1191.

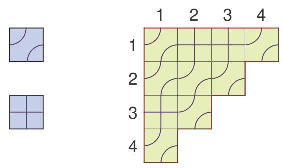
[24] M. Kaluba, D. Kielak and P. W. Nowak, *On property (T) for $Aut(F_n)$ and $SL_n(\mathbb{Z})$*, Annals of Math. 193:2 (2021), to appear.

[25] I. Pak and G. Panova, *Bounds on Kronecker coefficients via contingency tables*, Linear Algebra App. 602 (2020), 157–178.

Alejandro H. Morales and Greta Panova, by studying the asymptotic behavior of the principal evaluation of Schubert polynomials, you partially resolved an open problem by R. Stanley. What were the main ingredients of what you called 'surprisingly precise results'? What about the general case of conjecture? Are there other combinatorial objects, besides permutations, related to that conjecture?

Pak: Schubert polynomials were originally defined by Lascoux and Schützenberger in 1982 motivated by the geometry of flag varieties. They are now some of the central and most heavily studied objects in algebraic combinatorics[26]. While their definition is technical, Stanley's conjecture can be formulated in elementary terms.

Denote by $a(n)$ the number of tilings of the triangular (staircase) region $(n, n-1, \ldots, 1)$ with two type of tiles as in the figure. Here only parallel translations of tiles are allowed, the diagonal boundary must have no crossings, and no two blue curves are allowed to intersect more than once. The first few numbers in the sequence[27] are: 1, 2, 7, 41, 393, etc.



Stanley[28] conjectured that there is a limit $\alpha := \lim_{n\to\infty} \frac{1}{n^2}\log_2 a(n)$. This *Schubert entropy* is surprisingly difficult to establish due to the inherently nonlocal "no double crossings" condition. Assuming the conjecture, Stanley showed that $\frac{1}{4} \leq \alpha \leq \frac{1}{2}$, but the conjecture remains open.

Note that the curves in the tilings define a permutation $\sigma \in S_n$, for example we have $\sigma = (1, 4, 3, 2)$ in the figure. Thus we can write

$$a(n) = \sum_{\sigma\in S_n} a(\sigma).$$

Based on experimental evidence, Merzon and Smirnov[29] made a stronger conjecture, that the largest terms $a(\sigma)$ in the summation appear at layered permutations. In our paper with Alejandro and Greta, we showed that this stronger conjecture implies that $\alpha \approx 0.2932362$ is given by a solution of a certain differential equation.

This is "surprisingly precise" in a sense that normally one would not expect this level of precision from a qualitative assumption, perhaps suggesting that the Merzon–Smirnov conjecture is false for large n. Others might disagree with this interpretation. It would be interesting to figure out what is going on either way.

Mansour: One recent theme in your research activities is "contingency tables". Could you elaborate a little bit more on it?

Pak: *Contingency tables* (CT) are rectangular matrices of non-negative integers with rows and column sums. They play a major role in statistics and appear all over the place – from combinatorial optimization to social choice. In combinatorics, they generalize so-called "magic squares" and can be recognized as bipartite multigraphs with given degrees, or as the RHS of the *RSK correspondence.*

The amount of previous work on contingency tables is so enormous, it cannot be easily summarized. Unfortunately, several major problem on CTs remains out of reach, such as estimating the number of contingency tables in full generality. The hardness of counting the #CTs is another major open problem. In some recent papers with my collaborators, we improve known bounds and establish some surprising new phenomena in some natural special cases. This is an ongoing research project which is very far from completion.

Mansour: You have a nice blog. Some of your articles have turned out to be very interesting and attracted the attention of many readers. How do you select topics to write about? In one of your articles, a few years ago *The power of negative thinking, part I. Pattern avoidance* you explained why people should try to disprove conjectures more often. Is this your usual approach towards conjectures?

Pak: Thank you for the kind words, Toufik. The blog is largely dormant as I post only when I have something interesting to say. I stay away from politics and other day-to-day business, and write only about math and academic

[26] For example, see L. Manivel, *Symmetric functions, Schubert polynomials and degeneracy loci*, SMF/AMS, Providence, RI, 2001.

[27] See `https://oeis.org/A331920`

[28] R. P. Stanley, *Some Schubert shenanigans*, arXiv:1704.00851.

[29] G. Merzon and E. Smirnov, *Determinantal identities for flagged Schur and Schubert polynomials*, Europ. J. Math. 2 (2016), 227–245.

matters. My posts tend to be on a longer side, especially when I tackle a controversial topic and want to be thorough. The reason I choose such topics is usually not because I want to convince anyone of anything, but to represent a minority opinion, to give a piece of mind to people with similar points of view, that they are not alone.

As for conjectures – yes, I often like disproving more than proving. In fact, I expounded on my reasoning in *What if they are all wrong?* recent blog post[30]. Trying to disprove a conjecture is often unglamorous and thus largely neglected. This represents an opportunity for someone not swayed by the crowds, who has what I call "negative thinking".

Mansour: There are many interesting and important formulas in combinatorics. Which of them are the top three for you?

Pak: Herb Wilf[31] argued that good formulas are very efficient algorithms. From this point of view, the *matrix-tree theorem* due to Kirchhoff, the *Kasteleyn–Temperley–Fischer formula* for the number of perfect matchings in planar graphs, and the *Lindström–Gessel–Viennot lemma* for the number of nonintersecting arrangements of lattice paths, are the most general and the most powerful formulas in the area.

Mansour: You have some interesting and nice results related to permutation patterns. Could you list a couple of open problems from permutation patterns that look interesting to you and hope to see a solution?

Pak: Again, thank you. Indeed, the *permutation patterns* is a fascinating and very active area with a number of interesting open problems. Personally, I am mostly interested in the asymptotic and computational questions.

Denote by $\mathrm{Av}_n(\pi)$ and $\mathrm{Av}_n(\Pi)$ the number of permutations in S_n avoiding permutation $\pi \in S_k$ and subset of permutations $\Pi \subset S_k$, respectively. First, is it decidable whether $\mathrm{Av}_n(\Pi) \geq \mathrm{Av}_n(\Pi')$ for all $n \geq 1$, for fixed Π, Π'? If I had to guess, the answer is NO based on our paper with Scott Garrabrant[32].

Second, can one compute $\mathrm{Av}_n(\pi)$ in polynomial time for every fixed $\pi \in S_k$? The answer would be especially interesting for the notorious $\pi = (1, 3, 2, 4)$ pattern. But again, bet on the negative answer for general π[33].

Finally, define the growth constant $c(\Pi) := \lim_{n\to\infty} \frac{1}{n} \log \mathrm{Av}_n(\Pi)$. Can one find the set of permutations $\Pi \subset S_k$ such that $c(\Pi)$ exists and is not algebraic?

Mansour: "The hook-length formula" is a nice combinatorial formula and there has been a search for a "natural" and "satisfactory" bijection which explains the formula until you and your coauthors provided one in the paper "A direct bijective proof of the hook-length formula" published in 1997. Would you tell us about the novelty of this work? Did anyone give a "better" bijection in the last twenty years?

Pak: I did this jointly with Sasha Stoyanonvsky in 1992, when we both were undergraduates at Moscow University. Later, an expanded version also included J.-C. Novelli who found a cleaner proof. The original idea was to use a "two-dimensional bubble sorting" to generate standard Young tableaux of a given shape uniformly at random. The second half of the bijection, i.e. the rule for changing hook numbers, was necessary to justify that.

This was before the internet era when we did not know about *jeu-de-taquin*, or any Young tableaux algorithm other than RSK. Since the original note was published in Russian in a non-combinatorial journal, when I came to America I emailed an English translation to a few experts who were not aware of the paper. They recognized some familiar elements of the proof and popularized it further.

I do not know about a "better bijection", but there are quite a few new interesting proofs of the hook-length formula by many authors. In some way, just about all of them are "better proofs" as our bijective proof when done carefully is actually not all that simple. In fact, I also published a couple of such proofs with other applications[34,35] in mind.

[30] `https://wp.me/p211iQ-uT`.

[31] H. S. Wilf, *What is an Answer*?, Amer. Math. Monthly 89 (1982), 289–292.

[32] We showed that $\mathrm{Av}_n(\Pi) = \mathrm{Av}_n(\Pi') \bmod 2$ for all $n \geq 1$ is undecidable, see S. Garrabrant and I. Pak, *Permutation patterns are hard to count*, in Proc. 27th SODA, ACM, New York, 2016, 923–936.

[33] In the same paper with Garrabrant we showed that computing $\mathrm{Av}_n(\Pi) \bmod 2$ is $\oplus$P-hard.

[34] I. Pak, *Hook length formula and geometric combinatorics*, Sém. Lothar. Combin. 46 (2001), Article B46f

[35] I. Ciocan-Fontanine, M. Konvalinka and I. Pak, *The weighted hook length formula*, J. Combin. Theory, Ser. A 118 (2011), 1703–1717.

Mansour: Your book *Lectures on Discrete and Polyhedral Geometry* has gone through several editions and it is almost completed, at least from the eyes of us as readers. Do you have any plans to write a book that explores various aspects of *Probability and Combinatorics on Groups*?

Pak: No, not in the next few years. I do have a lot of material on the subject based on several courses I taught over the years[36].

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

Pak: In some sense, all combinatorics can be divided into three overlapping parts depending on the type of answer they are seeking: equalities, inequalities, or structures. I think of "enumerative techniques" as proving equalities, such as an explicit product formula, a generating function, a bijection, etc. These are often judged based on their beauty rather than applications, and the best examples are closer to art than to science.

Personally, I am very proud of some of my bijective "artworks", for example the above mentioned hook-length formula bijection, the so-called *Pak–Stanley*[37] *labeling*, the bijective proof of the *MacMahon's Master Theorem*[38], or the combinatorial proof of the *Rogers–Ramanujan identities*[39]. While there is still an occasional value in such bijections, in the 21st century their time is probably over. This is partially due to many successes of the other two camps, whose applications to adjacent fields are making a great impact.

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"?

Pak: There is nothing especially mysterious here. For example, here is the story of the MacMahon Master Theorem (MMT) paper I mentioned earlier. Once, I invited Doron Zeilberger to give a talk at the MIT Combinatorics Seminar which I organized at the time. He spoke about the *quantum MMT* which he proved recently[40]. I was quite excited but also somewhat unsettled.

The MMT is an amazing result, one of the early combinatorial gems, with many classical consequences such as the celebrated *Dixon's identity*. By every indication, the new generalization also looked amazing. Although it was motivated by knot theory applications, it was clearly of algebraic nature, creating more questions than answers. Furthermore, as it often happens to the pioneer papers, the original proof was rather technical, employing some heavy q-calculus and leaving some room for simplification.

I was immediately convinced that once the true nature of the quantum MMT is understood, both a "proof from the book" and some further generalizations will come along. I had a hunch that *quasideterminants* is the right language for the problem. I learned them from Israel Gelfand and Vladimir Retakh[41] some 15 years earlier, back when I was an undergraduate. With Matjaž Konvalinka, my graduate student at the time, we attacked the problem and found both the generalization and the kind of proof we wanted.

As it happened, we learned we have a strong competition. Dominique Foata and Guo-Niu Han[42] were working on their own combinatorial proofs independently from us. They ended up writing several beautiful papers on the subject, extending the classical Cartier–Foata[43] approach to MMT to the quantum setting. In

[36]For example, see my latest lecture notes here: `https://tinyurl.com/mpbmpjb5`.

[37]See Section 5 in R. Stanley, *Hyperplane arrangements, interval orders, and trees*, Proc. Nat. Acad. Sci. USA 93 (1996), 2620–2625.

[38]See Section 2 in M. Konvalinka and I. Pak, *Non-commutative extensions of the MacMahon Master Theorem*, Adv. Math. 216 (2007), 29–61.

[39]C. Boulet and I. Pak, *A combinatorial proof of the Rogers–Ramanujan and Schur identities*, J. Combin. Theory, Ser. A 113 (2006), 1019–1030.

[40]S. Garoufalidis, T. Q. Lê and D. Zeilberger, *The quantum MacMahon master theorem*, Proc. Natl. Acad. Sci. USA 103 (2006), 13928–13931.

[41]I. M. Gelfand and V. S. Retakh, *A theory of non-commutative determinants and characteristic functions of graphs*, Funct. Anal. Appl. 26:4 (1992), 1–20.

[42]D. Foata and G. N. Han, *A new proof of the Garoufalidis–Lê–Zeilberger quantum MacMahon master theorem*, J. Algebra 307 (2007), 424–431, and two followup papers.

[43]P. Cartier and D. Foata, *Problèmes combinatoires de commutation et réarrangements* (in French), Lecture Notes in Math. No. 85, Springer, Berlin, 1969.

[44]P. H. Hai and M. Lorenz, *Koszul algebras and the quantum MacMahon master theorem*, Bull. LMS 39 (2007), 667–676.

a different direction, Phùng Hô Hai and Martin Lorenz[44] were using the *Koszul duality* to give a purely algebraic proof of the quantum MMT.

We needed another idea to stand out, and again I knew something different from my studies of representation theory with my undergraduate advisor Alexandre Kirillov. That was a multiparameter (q_{ij}) generalization by Yuri Manin which he defined in the context of *quantum groups* and solutions the *Yang–Baxter equation*[45]. Miraculously, our approach with Matjaž extended to this setting. Spooked by the competition, we wrapped up within several months, I think.

Mansour: Recently, we have seen some 'unusual and interesting events' occurring in the math community such as resigning from the editorial boards of some well-established journals and founding similar journals. Two examples: a group of former editors of the *Journal of Algebraic Combinatorics* founded *Algebraic Combinatorics*, and recently a group of editors of the *Journal of Combinatorial Theory, Series A* resigned and founded *Combinatorial Theory*[46]. What is your opinion about these recent developments? Do you think this trend should continue with other journals as well?

Pak: Personally, I strongly support these changes. I think it is very important to move away from for-profit publishing. I am very happy that mathematicians in my field are leading the movement. I am also at least a little bit sad. JCTA was by far my favorite combinatorics journal, so I take no joy in its demise. This is just something that had to happen. It is a bittersweet victory.

More broadly, I hope to wake up one day in a world where all mathematics books and journals are electronic and free to download by anyone in the world. I learned of this dream from my former Bell Labs mentor Andrew Odlyzko back in 1995 and became a firm believer. It is been over 25 years since Odlyzko[47] published his predictions, but we are still here, celebrating the move by just two journals. Perhaps, the fact that the expanded version of that paper was published by an Elsevier journal, which continues to do good printing business, was both ominous and at least a little ironic.

Mansour: Suppose the academic world decides to eliminate all journals so that there would be no paper submissions, referee reports, and all other-sometimes unfair and lengthy-publishing processes. Researchers just post their papers to arXiv, and other professional mathematicians read and write comments with their names about the papers. A very transparent evaluation procedure. Do you think that it would have been a better academic world?

Pak: That would be terrible, in my opinion. The peer review has obvious flaws, but it is a very good system that we have learned to rely upon. It is largely a victim of its own success. The universities and government agencies learned to use publication records for hiring, promotion, and research awards. As the academia rapidly expanded, many new journals emerged with uncertain standards in the eye of these institutions, so the competition for publishing in top journals became fierce. This led to various biases, hurt egos, and some reluctance to participate in the process. But it is not a good reason to destroy the system. Rather, this suggests the need to decouple it from institutional use.

Unfortunately, like many other people in the area, I have my own share of unpublished papers, each with its own story. For one reason or another life intervened and they remained in that state for years. Since most of them are downloadable from my website, at least people can see what was done there, but I regret not publishing every one of them.

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

Pak: Yes, sure. I will mention only one, the combinatorial interpretation of the Kronecker coefficients problem that I discussed earlier. I have been thinking about this problem[48] for about ten years, ever since Greta Panova came to UCLA as a postdoc to work with me. Formally, the problem asks whether comput-

[45] Yu. I. Manin, *Some remarks on Koszul algebras and quantum groups*, Ann. Inst. Fourier 37 (1987), 191–205.

[46] `https://osc.universityofcalifornia.edu/2020/12/combinatorial-theory-launches/`.

[47] A. M. Odlyzko, *Tragic loss or good riddance? The impending demise of traditional scholarly journals*, Notices of the AMS 42 (1995), 49–53; see also an expanded version in Int. J. of Human–Computer Studies 42 (1995), 71–122.

[48] I. Pak and G. Panova, *On the complexity of computing Kronecker coefficients*, Computational Complexity 26 (2017), 1–36.

ing $g(\lambda, \mu, \nu)$ is in #P. To no one's surprise, I firmly believe that the answer is negative. Most people in the area strongly disagree, and some are actively working in the positive direction. Greta and I have been making some unsteady progress with no end in sight, but I am confident the problem will be resolved in my lifetime.

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