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Interview with Micha Sharir

Toufik Mansour

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Micha Sharir is a distinguished mathematician and computer scientist known for his groundbreaking contributions to computational geometry and combinatorial algorithms in geometry. He completed his Ph.D. at Tel Aviv University in 1976 under the supervision of Aldo Lazar. He has held various positions at Tel Aviv University since 1980 and was also a visiting research professor at the Courant Institute, where he was the deputy head of the Robotics Lab (1985-89). He is one of the co-founders of the Minerva Center for Geometry at Tel Aviv University. Awards he has won include a Max-Planck research prize (1992, jointly with Emo Welzl), the Feher Prize (1999), the Mif'al Hapais' Landau Prize (2002), and the EMET Prize (2007). In 1996 he was awarded an honorary doctorate degree from the University of Utrecht (1996). He has been a member of the Israeli Academy of Sciences and Humanities since 2018.

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

Sharir: This is a too broad question for me. My research focuses on combinatorics in geometry, or combinatorial geometry as the field is called. Here we have a problem that involves a large number, n, of geometric objects, and we are interested in some structure that is defined on these objects. A simple example would be n points in $\mathbb{R}^d$, and the structure in question is their *convex hull*, which is a convex polytope whose vertices are some of the points. The natural question is what is (an upper and/or a lower bound on) the *combinatorial complexity* of the hull, which in this case means the number of faces, of all dimensions, that the hull has. This specific problem has been studied, and solved, many years ago by Richard Stanley[1], and the result is given in what is known as the *Upper Bound Theorem*, a remarkable name, which does not mention convex polytopes at all, and yet everybody in the area knows what it means.

This is just one instance of an endless list of problems, some of which I hope to mention later in the interview.

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

Sharir: First, combinatorial geometry is, at least in my opinion, a central portion of geometry at large. It has strong branches to many other topics in geometry, such as computational geometry, algebraic geometry, differential geometry, topology, discrete geometry (where one wishes to study discrete geometric structures without bothering about their combinatorial complexity), and many more. Combinatorial geometry has mainly imported tools from these other topics, but has also inspired developments in some of these areas, of which computational geometry is a prime example, as my own research indicates. Combinatorial geometry is also strongly related to probability theory, as many of the tools and techniques that it employs are probabilistic

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[1]R. Stanley, *How the upper bound conjecture was proved*, Ann. Combin. 18(3) (2014), 533–539

in nature. Then, geometry at large branches out to a very large range of application domains, including robotics, mathematical optimization, statistics, geographic information systems, computer graphics and computer vision, and whatnot, and the effect of combinatorial geometry is felt in many of these applications.

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

Sharir: Bluntly, to solve problems in computational and combinatorial geometry. In retrospect, I would say that a major theme in my work has been to establish deeper and broader connections between the mathematical and algorithmic sides of geometry. Many of my colleagues and friends have been doing the same, quite successfully. I would say that, somehow, having a computer science background gave us an advantage over 'pure' mathematicians, and many major results in combinatorial geometry have indeed been obtained by computer scientists.

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?

Sharir: I started to get interested in math in high school. At that time, Joseph Gillis, a prominent mathematician at the Weizmann Institute, was running a popular math quarterly, called *Gilyonot le-Matematika* (mathematical pamphlets) for high school students. I was an avid subscriber and a devoted solver of the math problems that each issue included. Later, the Weizmann Institute ran a science summer camp for high school students, where I learned to program, and later became an apprentice, so to speak, of Amir Pnueli, then a Ph.D. student at Weizmann, where my programming experience and skills have been improving. There were other people who 'showed me the light', like Nira Dyn and Gideon Zwas, each in her or his own way.

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

Sharir: This is not the right question to ask. I have never been a combinatorics person per se, but when I started to get interested in geometry, it was clear that many of the basic problems that one had to deal with were combinatorial in nature, so combinatorial geometry became an integral and major part of my work. As an illustration, one of the first problems I studied in this area was the (first) 'Piano Movers' problem[2] of planning a collision-free motion of a line segment e amid polygonal obstacles. If the total number of vertices of the obstacles is n, a major subproblem is to estimate the combinatorial complexity of (a suitable discrete representation of) the *free configuration space* $\mathcal{F}$ of the moving segment. Since the motion of e has three degrees of freedom, two of translation and one of rotation, $\mathcal{F}$ can be represented as a portion of three-dimensional space, whose boundary is formed by $O(n)$ *contact surfaces*, representing placements at which e makes contact with the boundary of some obstacle. As is often the case in geometric problems, the real goal is to construct $\mathcal{F}$, but before doing so one needs to estimate the complexity of the structure one wants to compute, as this will serve as a lower bound to the worst-case complexity of any algorithm for its construction. It took some time to show that the maximum complexity is close to quadratic in n. Problems of this kind kept piling up over the years. The main subproblem here is to bound the number of *free critical placements* of the moving segment, at which it makes contact with three of the obstacles.

Mansour: What was the reason you chose Tel Aviv University for your Ph.D. and your advisor Aldo Lazar?

Sharir: No sensible reason. Tel Aviv was close to where I lived, and Aldo was the first faculty person whom I approached for an M.Sc. supervision, having sat in some of his classes. The M.Sc. studies then evolved into Ph.D. research. The topic was connected by extreme operators in Banach spaces, a far cry from my current work, but it was instrumental in getting a broader math education and in developing mathematical reasoning tools.

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

Sharir: I would say both. There are several major themes that I have been following, each

[2] J. T. Schwartz and M. Sharir, *On the "Piano Movers'" problem I. The case of a two-dimensional rigid polygonal body moving amidst polygonal barriers*, Comm. Pure Applied Math. 36 (1983), 345–398.

sprinkled with its own set of open problems, but there have also been many 'isolated' interesting problems that popped up and caught my attention.

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

Sharir: Yes, quite often, although my life experience has taught me to be very suspicious of these intuitive jumps, and not to trust them until they are *rigorously* written down. Indeed, quite a few of these euphoric experiences, of a result before a proof, have crashed, but many others have sailed triumphantly to success.

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

Sharir: I would beg to skip the question. As I said, I am not a combinatorics person, and the field is so huge that I am sure I don't see the whole picture. From my corner, though, I would mention the development of probabilistic tools as having had a tremendous impact on geometry, both in the design of efficient randomized algorithms and in the analysis of combinatorial structures.

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

Sharir: I am not sure I know how to count to three. There is a variety of open issues that I would like to study. One is the topic of incidences between points and other geometric objects[3] (lines, curves, surfaces), on which I hope to say a few words later. There has been a lot of progress in this area during the past 40 years, but there are still many open problems. Another topic is the study of substructures in *arrangements* of geometric objects[3]. Roughly, an arrangement is the way in which space is partitioned into connected pieces by the given objects. Without getting into precise definitions, the substructures in question are lower envelopes, single cells, many cells, zones, levels, union of objects, and many related constructs. Yet another research topic is to push further the applications of the polynomial partitioning technique[4], on which I also hope to say a few words later, both combinatorially and algorithmically. But there are many other equally challenging open problems that I would like to put on my list.

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

Sharir: This is not a question I feel comfortable answering. On one hand, there are clearly fundamental areas of this kind (algebra, analysis, and so on), but even there the basic infrastructure that is needed for their wide use in other branches of mathematics has long been developed over the centuries. Areas become more important or less so depending on the fashion, and on new applications that emerge. A classical example is number theory, which has become so important with the emergence of cryptographic techniques in recent decades. Combinatorics is another area that has gained prominence in the past half century, due to a large extent to the influential work of Paul Erdős. The short answer is that I do not really know what the answer is.

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?

Sharir: I am not sure I have an answer for the problem at large. In geometry, the applied side has also made significant progress, witnessed by the emergence of large, well-maintained geometric software systems, of which I would like to highlight CGAL[5], mnemonics for Computational Geometry Algorithms Library, which is not far from its 30th birthday, and which was developed and maintained by a large international group of researchers. Two related basic issues in geometric algorithms are those of *general position* and *exact computation.* Geometric algorithms have real-valued data as input and perform operations on real numbers, but their output, most often, is discrete. In this case, small computational errors may lead to incorrect, and sometimes logically inconsistent output. This is where theory and practice tend to diverge. In the design of theoretical algorithms, one tends to assume both general position and exact computation. For example,

[3] J. Pach and M. Sharir, *Geometric incidences*, in Towards a Theory of Geometric Graphs (J. Pach, ed.), Volume 342 of Contemporary Mathematics, AMS, 185—223.

[4] A. Sheffer, *Polynomial Methods and Incidence Theory*, Cambridge University Press, 2022.

[5] See `https://www.cgal.org/`.

for three input points in the plane, general position would mean that they are not collinear, and exact computation would assure us that, even when the triple is noncollinear but very close to being collinear, we will be able to determine (exactly) whether it makes a right turn or a left turn. On the applied side of the fence, though, general position cannot be assumed, and exact computation must be provided (at a significant cost) or bypassed in a theoretically sound manner. There have been many studies, on both sides of the fence, on how to handle these issues, and I am glad to say that CGAL, like many other systems, handles both issues very successfully.

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

Sharir: Sorry, no helpful advice. I am in computer science, and we see how new emerging and quickly shifting trends affect students' interests. Today everybody wants to study AI, deep learning, or cyber security, and it is becoming more difficult to find students interested in an old (or at least older) fashioned research in geometry. Mathematics sails on calmer waters, as a truth remains a truth, regardless of trends and fashions, which of course do exist there too. In short, one should follow his or her nose and scientific desires, work hard, and hope for the best.

Mansour: Together with Jack Schwartz[6] you are known as pioneers of the study of algorithmic motion planning in robotics. Would you please expand on this?

Sharir: I have already mentioned our first work on this topic. Jack Schwartz was my postdoc mentor at New York University (NYU). I joined his group working on high-level programming languages (in particular SETL, the language that he invented and developed), with very little connection, if any, to my current research interests. One day, in 1981, when I was already back in Tel Aviv, I visited him, and, sitting in his office, all of a sudden, he dropped on me (and on himself) the problem of algorithmically planning a collision-free motion of a line segment amid polygonal obstacles in the plane. This was how his and my interest in robotics was born, and we worked out an algorithm, which has turned into the first paper in the sequence of "On the Piano Movers' problem" papers. However, the most influential paper[7] in the sequence has been number II, which presented a general algorithmic approach, based on computational real algebraic geometry, to solve any (reasonably defined) motion planning problem. This was the starting point of a decade-long activity, in which Jack formed a Robotics Research Lab at NYU, which I helped him run during 1985–1989. The lab was doing both theoretical and applied research, and I have concentrated mostly, albeit not exclusively, on the theoretical aspects. It was during these days that I got drawn into computational geometry. This has started as I was trying to implement our initial motion planning algorithm, and realized how much more technical tools one needs to achieve an efficient implementation.

Mansour: In computational geometry, algorithms often have applications in areas such as computer graphics, robotics, etc. Can you highlight some real-world applications of your research that have had a substantial impact?

Sharir: Not really. My work has been theoretical in nature. Applications have always been a major motivation for studying these problems, but my own work did not have any obvious practical impact. Perhaps the closest I can think of for a 'positive' example was my work on motion planning (and other problems) in robotics. Here too its main impact was to show that the problems that arise in this context are just too hard to solve precisely in practice. Some of these problems are PSPACE-hard (like the coordinated motion of a large number of simple robots), but even when they have a polynomial-time solution, the resulting algorithms are often too expensive to run in practice. The practical effect of these works was twofold. First, it introduced the right terminology and framework in which such problems can be studied. Second, it 'scared' practical researchers into the design of efficient approximate solutions, like sampling-based techniques that have made a lot of progress in the past decade or two.

Mansour: *Davenport-Schinzel sequences* are a

[6] M. Sharir, *Robot motion planning*, Commun. Pure Appl. Math. 48(9) (1994), 1173–1186.

[7] J. T. Schwartz and M. Sharir, *On the "Piano Movers'" problem: II. General techniques for computing topological properties of real algebraic manifolds*, Adv. in Appl. Math. 4 (1983), 298–351.

recurring theme in many of your research articles[8,9]. Could you explain the significance of these sequences in computational geometry and the broader context of your work?

Sharir: Davenport-Schinzel sequences are strongly related, in fact equivalent, to lower envelopes of curves in the plane, or of univariate functions if you will. That is, let $\mathcal{F} = \{f_1, \ldots, f_n\}$ be a collection of n continuous totally defined functions, each pair of which intersect in at most s points, for some constant parameter s. The lower envelope E of $\mathcal{F}$ is the pointwise minimum of these functions, that is, $E(x) = \min_{i=1,\ldots,n} f_i(x)$. In general position, the graph of E is a concatenation of portions of the graphs of the f_i's, and we can associate with E the sequence of the indices of the functions f_i in the left-to-right order in which they appear on E. This is a Davenport-Schinzel sequence of order s on n symbols, and the converse also holds. Concretely, it is a sequence S on n symbols such that (a) no two consecutive elements of S are equal, and (b) for no pair a, b of distinct symbols does there exist a (not necessarily contiguous) alternating subsequence of $s+2$ symbols of the form $a \cdots b \cdots a \cdots b \cdots$.

Davenport and Schinzel[10,11] had discovered these sequences in the 1960s, in the context of minima of a collection of solutions to certain differential equations, and it was Mike Atallah[12] who introduced them to computational (and combinatorial) geometry in the early 1980s. Since then these sequences have found a myriad of applications. Many of these applications are in the study of the complexity of various structures in arrangements of curves and surfaces in two and higher dimensions, such as single cells, zones, vertical decompositions, and more. Many other applications are in geometric optimization, where we want to keep track of the minimum of several quantities, where these quantities vary dynamically. Keeping track of the closest pair among n moving points is a classical simple example.

What makes Davenport-Schinzel sequences so interesting is that their maximum possible length, denoted as $\lambda_s(n)$, behaves rather strangely: it is of the form $n\beta_s(n)$, where $\beta_s(n)$ is a near-constant extremely slowly growing function, related to the inverse Ackermann function $\alpha(n)$[13,14]. So it is almost linear, but not quite linear.

Mansour: You have some experience with mathematics competitions. Do you think that mathematics competitions play a crucial role in inspiring young students for a research career?

Sharir: I think so. Although I don't have enough data to support this feeling, we see quite often in CV's of faculty members the item that they have participated in and won prizes in a variety of national and international math olympiads and other competitions. How crucial is this for advancing their careers I don't know, but it certainly provides a push in the right direction, especially if you are a problem solver by nature.

Mansour: In one of your papers *Counting Triangulations of Planar Point Sets*[15], coauthored with Adam Sheffer, you studied the maximal number of triangulations that a planar set of n points can have, and showed that this number is at most 30^n, improving the known bound of 43^n. Would you say a few lines about this work?

Sharir: This was Sheffer's M.Sc. thesis, and he deserves most of the credit for a careful and meticulous (and fairly long) analysis that has yielded this bound. The problem has a long history, and the fact that the number of triangulations is bounded by c^n, for any constant c, was a major open problem that took some time

[8] P. Agarwal and M. Sharir, *Davenport-Schinzel sequences and their geometric applications, in Handbook of Computational Geometry*, J.R. Sack and J. Urrutia (Eds.), North-Holland, 2000, 1–47.

[9] M. Sharir and P. K. Agarwal, *Davenport-Schinzel Sequences and Their Geometric Applications*, Cambridge University Press, Cambridge-New York-Melbourne, 1995.

[10] H. Davenport, *A combinatorial problem connected with differential equations II*, Acta Arithmetica 17 (1971), 363–372.

[11] H. Davenport and A. Schinzel, *A combinatorial problem connected with differential equations*, Amer. J. Math. 87 (1965), 684–689.

[12] M. J. Atallah, *Some dynamic computational geometry problems*, Comput. Math. Appl. 11 (1985), 1171–1181.

[13] S. Hart and M. Sharir, *Nonlinearity of Davenport-Schinzel sequences and of generalized path compression schemes*, Combinatorica 6 (1986), 151–177.

[14] P. K. Agarwal, M. Sharir, and P. Shor, *Sharp upper and lower bounds on the length of general Davenport-Schinzel sequences*, J. Combin. Theory Ser. A 52 (1989), 228–274.

[15] M. Sharir and A. Sheffer, *Counting triangulations of planar point sets*, Electron. J. Combinat. 18 (2011), P70.

[16] M. Ajtai, V. Chvátal, M. M. Newborn, and E. Szemerédi, *Crossing-free subgraphs*, Ann. Discrete Math. 12 (1982), 9–12.

[17] M. Sharir and E. Welzl, *On the number of crossing-free matchings (cycles, and partitions)*, SIAM J. Comput. 36(3) (2006), 695–720.

to settle. (The first upper bound[16] for c was 10^{13}.) Our work improved an earlier bound[17] of 43^n, and is still, to the best of my knowledge, the best-known upper bound, although it has been noted by Sheffer that, with additional hard work, the constant 30 is likely to be improved. The lower bound (of point sets with many triangulations) was $\Omega(n^{8.65})$, but it has been improved this year, by Rutschmann and Wettstein[18], to $\Omega(n^{9.08})$. So there is still a long way to go. This initial work with Sheffer has later developed into a series of works, mostly joint with Emo Welzl[19], which have produced sharp bounds on the number of various other crossing-free structures on n points in the plane, such as perfect matchings and spanning cycles.

Mansour: Erdős's *distinct distances* and *repeated distances* problems[20,21] for the plane kept many researchers sleepless for years. Finally, researchers came up with elegant solutions for the first problem with the help of algebraic methods. Would you tell us about these problems and their solutions? How about the higher-dimensional versions?

Sharir: Although both problems appeared in the same short article of Erdős in 1946, they have quite different histories. Let P be a given set of n points in the plane. The *distinct distances* problem seeks a lower bound on the number of distinct distances that any such set must have, and the *repeated distances* problem seeks an upper bound on the number of times the same distance can occur in such a set. For the number of repeated distances, the best known upper bound is $O(n^{4/3})$, due to Spencer, Szemerédi, and Trotter[22], 1984, and the lower bound, already noticed by Erdős, is only slighly superlinear. This problem has been stuck for 40 years, with no progress whatsoever, and some people are still trying to crack it. What makes the problem more intriguing is that the upper bound $O(n^{4/3})$ is *tight* in the worst case if one uses other (simple) metrics instead of the Euclidean one, so an improvement of this bound, if one exists, must strongly depend on subtle properties of the Euclidean metric.

The distinct distances problem has a different story. For the vertices of a $\sqrt{n} \times \sqrt{n}$ integer lattice, Erdős showed that the number of distinct distances is $\Theta(n/\sqrt{\log n})$, and conjectured that this is a lower bound for any set of n points. Traditional methods have progressively pushed the known lower bound upwards, up to a bound of $\Omega(n^{0.8641})$. Then a major breakthrough occurred in 2010, when Larry Guth and Nets Hawk Katz[23] have established the nearly tight lower bound $\Omega(n/\log n)$ (the slight gap between the upper and lower bounds is still unresolved to this date).

Moreover, although the Guth–Katz result was truly sensational, the real interest was in the new machinery that they have brought to bear, which was the *polynomial partitioning* technique[4]. This technique is based on the polynomial ham-sandwich theorem of Stone and Tukey[24], mixed with real algebraic geometry. It provides a powerful mechanism for divide and conquer of geometric problems that involve points, algebraic curves, or surfaces in any fixed dimension, which extends and surpasses earlier techniques of this sort. Concretely, for a set P of n points in $\mathbb{R}^d$, and for any specified degree D, one can construct a real d-variate polynomial f of degree at most D, so that each of the $O(D^d)$ connected components of $\mathbb{R}^d \setminus Z(f)$ (where $Z(f)$ is the zero set of f) contains at most $O(n/D^d)$ points of P. Extensions of this result to the case of algebraic varieties of any dimension (instead of points) have also been established.

This technique has mushroomed in the past 15 years into a myriad of works, some involving special cases and variants of the distinct distances problem, and others involving incidences between points and curves or surfaces in three and higher dimensions. Many other

[18] D. Rutschmann and M. Wettstein, *Chains, Koch chains, and point sets with many triangulations*, J. ACM 70 (2023), 18:1–18:26.

[19] M. Sharir and E. Welzl, *Random triangulations of planar point sets*, Proc. 22nd Ann. ACM Symp. on Computational Geometry (2006), 273–281.

[20] P. Erdős, *On sets of distances of n points*, Amer. Math. Monthly 53(5) (1946), 248–250.

[21] J. Garibaldi, A. Iosevich, and S. Senger, *The Erdős Distance Problem*, Student Mathematical Library, volume 56, Providence, RI: American Mathematical Society, 2011.

[22] J. Spencer, E. Szemerédi, and W. T. Trotter, *Unit distances in the euclidean plane*, in: Graph Theory and Combinatorics (B. Bollobás, ed.), Academic Press, London, 1984, 293–303.

[23] L. Guth and N. H. Katz, *On the Erdős distinct distances problem in the plane*, Ann. of Math. 181:1 (2015), 155–190.

[24] A. H. Stone and J. W. Tukey, *Generalized "sandwich" theorems*, Duke Math. J. 9(2) (1942), 356–359.

works have obtained bounds on other kinds of combinatorial structures.

Let me end by noting that the higher-dimensional versions of the distinct distances problem[25] are still wide open, already in three dimensions. The set of vertices of the $n^{1/3} \times n^{1/3} \times n^{1/3}$ grid determines only $O(n^{2/3})$ distinct distances, and it is conjectured that this is indeed the lower bound for any set of n points. But no sufficiently close bound is known.

Mansour: Could you please expand upon some of these applications of the polynomial partitioning technique?

Sharir: Gladly. Let me say a few words about problems involving incidences. In fact, the original Guth–Katz[23] paper was a paper about incidences in three dimensions. An *incidence* between a point p and some geometric object (line, plane, curve, surface) λ is another way of saying that $p \in \lambda$. Obtaining sharp upper (and more rarely lower) bounds on the maximum possible number of incidences between a set of m points and a set of n objects of these kinds has been, and still is, a major research area in combinatorial geometry, and I have been working on it for the past 30+ years[3]. The subject has started with the seminal 1983 paper of Szemerédi and Trotter[26], who studied the simplest case of m points and n lines in the plane, and showed that the maximum number of incidences between them is $\Theta(m^{2/3}n^{2/3}+m+n)$. (This is one of the rare instances where we have an asymptotically tight bound.) What Guth and Katz have shown was that the number of incidences between m points and n lines in $\mathbb{R}^3$ is $\Theta(m^{1/2}n^{3/4} + m + n)$, which is better than the Szemerédi-Trotter bound, provided that no plane contains more than $O(\sqrt{n})$ lines. Without such an assumption, we could have put all the points and lines in a common plane, and get stuck with the Szemerédi–Trotter bound. This improved bound was the main ingredient that Guth and Katz[23] needed to prove a conjecture of Elekes[27], from which an ingenious transformation, also due to Elekes, leads to the lower bound $\Omega(n/\log n)$ on distinct distances.

Scores of additional papers (and I have been a coauthor of quite a few of them) have been written on incidences. It is an interesting curiosity that the first influential paper on incidences, beyond the Szemerédi–Trotter work, was written in 1990 by Ken Clarkson, Herbert Edelsbrunner, Leo Guibas, myself, and Emo Welzl[28], all computer scientists! This is just another manifestation of the strong connection and cross-fertilization between combinatorial and computational geometry. In fact, our interest in incidences was due, to a large extent, to the fact that the tools for obtaining incidence bounds are very similar to, and in fact are taken from, tools for solving algorithmic problems in geometry.

Getting back to the polynomial partitioning technique, it provided a novel algebraic context in which one could study incidences between points and all kinds of curves and surfaces, in the plane but mostly in higher dimensions. As one reviewer put it, this has become a cottage industry, but it has solved an amazing number of hard problems. Many of these problems were considered, before the so-called *algebraic revolution*, to be extremely difficult, unlikely to be solved in our lifetime.

Mansour: Were incidences the main application of the polynomial technique?

Sharir: Initially yes, incidences and variants of the distinct distances problem, but slowly other kinds of problems were also successfully tackled using the new machinery. One major development was the algorithmic aspect of the method. Originally, the construction of partitioning polynomials has been existential in nature, and no efficient algorithms were known. This is still the case for partitioning polynomials of large degree, but for constant degree, we have by now efficient constructions, developed more recently. With these tools available, many new algorithmic results have emerged, dealing mainly with *semi-algebraic range searching*.

As another development, close to my heart, I would like to mention the problem of eliminating cycles in the depth relation of lines in

[25] B. Aronov, J. Pach, M. Sharir, and G. Tardos, *Distinct distances in three and higher dimensions*, Combin. Prob. Comput. 13(3) (2004), 283–293.

[26] E. Szemerédi and W. T. Trotter, *Extremal problems in discrete geometry*, Combinatorica 3 (1983), 381–392.

[27] G. Elekes and M. Sharir, *Incidences in three dimensions and distinct distances in the plane*, Proc. 26th Annu. Sympos. on Computational Geometry, 2010, 413–422.

[28] K. Clarkson, H. Edelsbrunner, L. Guibas, M. Sharir, and E. Welzl, *Combinatorial complexity bounds for arrangements of curves and spheres*, Discrete Comput. Geom. 5 (1990), 99–160.

3-space. In a set L of n lines in $\mathbb{R}^3$ in general position, we say that $\ell_1 \prec \ell_2$, for $\ell_1, \ell_2 \in L$, if the unique vertical line that touches both ℓ_1 and ℓ_2 meets ℓ_1 at a point that lies below its intersection with ℓ_2. The relation $\prec$ can have cycles, and the goal is to cut the lines of L into a small number of pieces (segments, rays, and full lines) such that the depth relation among the pieces is acyclic, a *depth order*. Easy constructions show that in the worst case, the number of cuts has to be $\Omega(n^{3/2})$, and the prevailing conjecture was that this bound is tight: for every set L of n lines in 3-space, its lines can be cut into $O(n^{3/2})$ pieces with an acyclic depth order. This problem was motivated by an application in computer graphics, in the so-called Painter's algorithm, and is more than 30 years old.

Polynomial partitioning turned out to be the pivotal tool that enabled us finally to almost completely solve the problem, a few years ago. Together with Boris Aronov[29], we showed that one can always cut a set of n lines in 3-space into $O(n^{3/2}\text{polylog}(n))$ pieces that have an acyclic depth order.

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 an "eureka moment"?

Sharir: Perhaps my most favorite result[13] is the analysis of the maximum length of Davenport-Schinzel sequences of order 3. To remind us, the problem is to write a sequence S using n symbols so that no two consecutive elements of S are equal, and so that S does not contain any (not necessarily contiguous) subsequence of length 5 of the form $a \cdots b \cdots a \cdots b \cdots a$, for any two distinct symbols a and b. The question is how long can S be. As we know now, the maximum length of S is close to $2n\alpha(n)$, where $\alpha(n)$ is the extremely slowly growing near-constant inverse Ackermann function. Together with my colleague Sergiu Hart[13] (a game theory person who got interested in this 'puzzle'), we worked on the problem for nearly a year, with basically no progress. We were trying to prove a linear upper bound, which was doomed to fail, in view of the aforementioned bound. We managed to transform the problem to a problem involving generalized path compressions on trees, but got stuck there too. Then a miracle happened: I told my colleagues at NYU about the problem, and when Paul Erdős visited NYU, they asked him about the problem, and he told them that Szemerédi had obtained the upper bound $O(n \log^* n)$ for the maximum length (but no matching lower bound). They called me (there was no internet at the time), in the middle of the night, to break the news. Hart and I read Szemerédi's paper, and I told him: If Szemerédi's bound is tight, we have had it—all our work was in vain. On the other hand, if the great Szemerédi did not manage to prove linearity, the true bound is likely nonlinear. So our only hope is that the true bound is 'in between' linear and Szemerédi's bound. The only such bound I could think of was Tarjan's bound[30] in his analysis of the running time of the Union-Find algorithm, which was $\Theta(n\alpha(n))$, for the cost of n operations on n input elements. This "eureka moment", if you will, gave us a push in the right direction, and after a few more months of hard work we managed to show that our generalized path compression scheme also has the same lower bound as Tarjan's bound. Obtaining later the upper bound was very exciting too, but already an anti-climax.

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

Sharir: There are quite a few such problems, some of which have already been mentioned, but one of the most 'annoying' ones, although not one of the main problems on my list, is the *k-set* problem, one of my favorite open problems in combinatorial geometry. In this problem we are given a set S of n points in the plane, say in general position, and a parameter $k < n$. A k-set S' is a subset of S of size k that can be separated from its complement by a line (that is, S' is the intersection of S with a halfplane). The question is what is the maximum number $f_k(n)$ of k-sets that a set of n points can have. In a beau-

[29] B. Aronov and M. Sharir, *Almost tight bounds for eliminating depth cycles in three dimensions*, Discrete Comput. Geom. 59(3) (2018), 725–741.

[30] R. E. Tarjan, *Efficiency of a good but not linear set union algorithm*, J. ACM. 22(2) (1975), 215–225.

[31] T. Dey, *Improved bounds for planar k-sets and related problems*, Discrete Comput. Geom. 19 (1998), 373–382.

tiful breakthrough result, Dey[31] has obtained, in 1998, the upper bound $f_k(n) = O(nk^{1/3})$, but the best known lower bound, obtained by Geza Tóth[32] and later improved by Nivasch[33], is $f_k(n) = n \cdot 2^{\Omega(\sqrt{\log n})}$. This gap is quite tantalizing (for me and others), and I have been trying, on and off, to narrow it, mainly to improve the upper bound. To this day, 25 years later, no progress has been achieved. A lot of progress has been made, by many researchers, including myself, on many variants and extensions of the problem, to higher dimensions, and to dual variants of the problem, but the basic problem is still with us.

Mansour: In a very recent short article[34], published in the Newsletter of the European Mathematical Society, Professor Melvyn B. Nathanson, while elaborating on the ethical aspects of the question "Who Owns the Theorem?", concluded that "Mathematical truths exist and mathematicians only discover them." On the other side, there are opinions that "mathematical truths are invented". As a third way, some people claim that it is both invented and discovered. What do you think about this old discussion? More precisely, do you believe that you *invent* or *discover* your theorems?

Sharir: One cannot help thinking of Erdős and his Book: According to Erdős, God has a Book that contains all the elegant, beautiful, and simple proofs of theorems in mathematics, but he hides the book from us. Only rarely does he allow some lucky person to have a glance at the Book. This was a major reason why Erdős did not like God, and used to refer to him as the Supreme Fascist (SF). Interested persons can find solace in the beautiful earthly version "Proofs from the Book", by Aigner and Ziegler[35]. So Erdős seems to believe that theorems (at least those with a nice proof) are discovered. I tend to agree with him.

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

Sharir: You are most welcome.

[32] G. Tóth, *Point sets with many k-sets*, Discrete Comput. Geom. 26 (2001), 187–194.

[33] G. Nivasch, *An improved, simple construction of many halving edges*, in Surveys on Discrete and Computational Geometry: Twenty Years later (J. E. Goodman et al., editors), Contemporary Mathematics vol. 453, pp. 299–305, AMS, 2008.

[34] M. B. Nathanson, *Who Owns the Theorem?* The best writing on Mathematics 2021, Princeton: Princeton University Press, 2022, 255–257.

[35] M. Aigner and G. Ziegler, *Proofs from THE BOOK* (4th ed.). Berlin, New York: Springer-Verlag, 2009.