Interview with Endre Szemerédi

Martin Raussen and Christian Skau

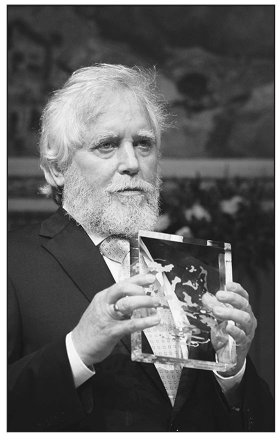
Endre Szemerédi is the recipient of the 2012 Abel Prize of the Norwegian Academy of Science and Letters. This interview was conducted by Martin Raussen and Christian Skau in Oslo in May 2012 in conjunction with the Abel Prize celebration. This article originally appeared in the September 2012 issue of the *Newsletter of the European Mathematical Society* and is reprinted here with permission of the EMS.

Raussen and Skau: *Professor Szemerédi, first of all we would like to congratulate you as the tenth Abel Prize recipient! You will receive the prize tomorrow from His Majesty, the King of Norway.*

You were born in Budapest, Hungary, in 1940 during the Second World War. We have heard that you did not start out studying mathematics; instead, you started in medical school and only later on shifted to mathematics. Were you nevertheless interested in mathematical problems as a child or teenager? Did you like to solve puzzles?

Szemerédi: I have always liked mathematics and it actually helped me to survive in a way: when I was in elementary school, I was very short and weak and the stronger guys would beat me up. So I had to find somebody to protect me. I was kind of lucky, since the strongest guy in the class did not understand anything about mathematics. He could never solve the homework exercises, let alone pass the exam. So I solved the homework exercises for him and I sat next to him at the exam. Of course, we cheated and he passed the exam. But he was an honest person and he always protected me afterwards from the other big guys so I was safe. Hence my early interest in mathematics was driven more by necessity and self-interest than by anything else. In elementary school I worked a lot with mathematics but only on that level, solving elementary school exercises.

In high school I was good at mathematics. However, I did not really work on specific problems and, if I remember correctly, I never took part in any competitions. In Hungary there are different kinds of competitions. There is also a monthly journal *KöMaL*, where you may send in solutions to problems that are posed. At the end of the year the editors will add up points you get for good solutions.



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Endre Szemerédi

I never took part in this, the main reason being that my father wanted me to be a physician. At the time, this was the most recognized profession, prestigiously and also financially. So I studied mainly biology and some physics, but I always liked mathematics. It was not hard for me to solve high school exercises and to pass the exams. I even helped others, sometimes in an illegal way, but I did not do more mathematics than that.

My education was not the usual education you get in Hungary if you want to be a mathematician. In Hungary we have two or three extremely good elite high schools. The best is Fazekas, in Budapest; they produce every year about five to ten mathematicians who, by the time they go to the university, know a lot. I was not among those. This is not a particular Hungarian invention; also in the United States there are special schools concentrating on one subject.

I can name a lot of mathematicians that are now considered to be the best ones in Hungary. Most of them (90 percent) finished the school at Fazekas. In Szeged, which is a town with about 200,000 inhabitants, there are two specialist schools also producing some really good mathematicians. One of those mathematicians was a student of Bourgain at the Institute for Advanced Study in Princeton who just recently defended his thesis with a stunning result. But again, I was not among those highly educated high school students.

R&S: *Is it correct that you started to study mathematics at age 22?*

Szemerédi: Well, it depends on how you define "started". I dropped out of medical school after half a year. I realized that, for several reasons, it was not for me. Instead, I started to work at a machine-making factory, which actually was a very

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good experience. I worked there slightly less than two years.

In high school my good friend Gábor Ellmann was by far the best mathematician. Perhaps it is not proper to say this in this kind of interview but he was tall. I was very short in high school, at least until I was seventeen. I am not tall now, but at the time I was really short and that actually has its disadvantages. I do not want to elaborate. So I admired him very much because of his mathematical ability and also because he was tall.

It was actually quite a coincidence that I met him in the center of the town. He was to date a girlfriend, but he was fifteen minutes late so she had left. He was standing there, and I ran into him and he asked me what I was doing. Gábor encouraged me to go to Eötvös University, and he also told me that our mathematics teacher at high school, Sándor Bende, agreed with his suggestion. As always, I took his advice; this was really the reason why I went to university. Looking back, I have tried to find some other reason but so far I have not been successful.

At that time in Hungary you studied mathematics and physics for two years, and then one could continue to study physics, mathematics, and pedagogy for three years in order to become a math-physics teacher. After the third year they would choose fifteen out of about two hundred students who would specialize in mathematics.

Turán and Erdős

R&S: *We heard that Paul Turán was the first professor in mathematics that made a lasting impression on you.*

Szemerédi: That's true. In my second year he gave a full-year lecture on number theory, which included elementary number theory, a little bit of analytic number theory, and algebraic number theory. His lectures were perfect. Somehow he could speak to all different kinds of students, from the less good ones to the good ones. I was so impressed with these lectures that I decided I would like to be a mathematician. Up to that point I was not sure that I would choose this profession, so I consider Paul Turán to be the one who actually helped me to decide to become a mathematician.

He is still one of my icons. I have never worked with him; I have only listened to his lectures and sometimes I went to his seminars. I was not a number theorist, and he mainly worked in analytic number theory.

R&S: *By the way, Turán visited the Institute for Advanced Study in Princeton in 1948, and he became a very good friend of the Norwegian mathematician Atle Selberg.*

Szemerédi: Yes, that is known in Hungary among the circle of mathematicians.

R&S: *May we ask what other professors at the university in Budapest were important for you. Which of them did you collaborate with later on?*

Szemerédi: Before the Second World War, Hungarian mathematics was very closely connected to German mathematics. The Riesz brothers, as well as Haar and von Neumann and many others, actually went to Germany after they graduated from very good high schools in Hungary. Actually, my wife Anna's father studied there almost at the same time as von Neumann and, I guess, the physicist Wigner. After having finished high school, he and also others, went to Germany. And after having finished university education in Germany, most of them went to the United States. I don't know the exact story, but this is more or less the case. After the Second World War, we were somehow cut off from Germany. We then had more connections with Russian mathematics.

In the late 1950s, Paul Erdős, the leading mathematician in discrete mathematics and combinatorics—actually, even in probability theory he did very good and famous work—started to visit Hungary, where his mother lived. We met quite often. He was a specialist in combinatorics. At the time, combinatorics had the reputation that you didn't have to know too much. You just had to sit down and meditate on a problem. Erdős was outstanding in posing good problems. Well, of course, as happens to most people, he sometimes posed questions which were not so interesting. But many of the problems he posed, after being solved, had repercussions in other parts of mathematics—also in continuous mathematics, in fact. In that sense Paul Erdős was the most influential mathematician for me, at least in my early mathematical career. We had quite a lot of joint papers.

R&S: *Twenty-nine joint papers, according to Wikipedia…*

Szemerédi: Maybe, I'm not sure. In the beginning I almost exclusively worked with Paul Erdős. He definitely had a lasting influence on my mathematical thinking and mathematical work.

R&S: *Was it usually Erdős who posed the problems, or was there an interaction from the very start?*

Szemerédi: It was not only with me, it was with everybody. It was usually he who came up with the problems and others would work on them. Probably for many he is considered to be the greatest mathematician in that sense. He posed the most important problems in discrete mathematics which actually affected many other areas in mathematics. Even if he didn't foresee that solving a particular problem would have some effect on something else, he had a very good taste for problems. Not only the solution but actually the methods used to obtain the solution often survived the problem itself and were applied in many other areas of mathematics.

R&S: Random methods, for instance?

Szemerédi: Yes, he was instrumental in introducing and popularizing random methods. Actually, it is debatable who invented random methods. The Hungarian mathematician Szele used the so-called random method—it was not a method yet—to solve a problem. It was not a deterministic solution. But then Paul Erdős had a great breakthrough result when he gave a bound on the Ramsey number, still the central problem in Ramsey theory. After that work there has been no real progress. A little bit, yes, but nothing really spectacular. Erdős solved the problem using random methods. Specifically, he proved that by two-coloring the edges of a complete graph with n vertices randomly, almost certainly there will not be more than $2 \log n$ vertices so that all the connecting edges are of the same color.

In the United States, where I usually teach undergraduate courses, I present that solution. The audience is quite diverse; many of them do not understand the solution. But the solution is actually simple, and the good students do understand it. We all know it is extremely important—not only the solution but the method. Then Erdős systematically started to use random methods. To that point they just provided a solution for a famous problem but then he started to apply random methods to many problems, even deterministic ones.

And, of course, his collaboration with Rényi on the random graph is a milestone in mathematics; it started almost everything in random graph theory.

R&S: And that happened around 1960?

Szemerédi: Yes. It was in the 1960s, and it is considered to be the most influential paper in random graph theory. Their way of thinking and their methods are presently of great help for many, many mathematicians who work on determining the properties of real-life, large-scale networks and to find random methods that yield a good model for real-life networks.

Moscow: Gelfond and Gelfand

R&S: You did your graduate work in Moscow in the period 1967–1970 with the eminent mathematician Israel Gelfand as your supervisor. He was not a specialist in combinatorics. Rumors would have it that you, in fact, intended to study with another Russian mathematician, Alexander Gelfond, who was a famous number theorist. How did this happen, and whom did you actually end up working with in Moscow?

Szemerédi: This can be taken, depending on how you look at it, as a joke or it can be taken seriously. As I have already told you, I was influenced by Paul Turán, who worked in analytic number theory. He was an analyst; his mathematics was much more concrete than what Gelfand and the group around him studied. At the time, this group consisted of Kazhdan, Margulis, Manin, Arnold, and others, and he had his famous Gelfand seminar every week which lasted for hours. It was very frightening sitting there and not understanding anything. My education was not within this area at all. I usually had worked with Erdős on elementary problems, mainly within graph theory and combinatorics; it was very hard for me!

I wanted to study with Gelfond, but by some unfortunate misspelling of the name I ended up with Gelfand. That is the truth.

R&S: But why couldn't you swap when you realized that you had got it wrong?

Szemerédi: I will try to explain. I was a so-called candidate student. That meant that you were sent to Moscow—or to Warsaw, for that matter—for three years. It had already been decided who would be your supervisor, and the system was quite rigid, though not entirely. I'm pretty sure that if you put a lot of effort into it, you could change your supervisor, but it was not so easy. However, it was much worse if you decided after half a year that it was not the right option for you and to go home. It was quite a shameful thing to just give up. You had passed the exams in Hungary and kind of promised you were going to work hard for the next three years. I realized immediately that this was not for me, and Gelfand also realized it and advised me not to do mathematics anymore, telling me: "Just try to find another profession; there are plenty in the world where you may be successful." I was twenty-seven years old at the time, and he had all these star students aged around twenty, and twenty-seven was considered old! But in a sense, I was lucky: I went to Moscow in the fall of 1967, and in the spring next year, there was a conference on number theory in Hungary—in Debrecen, not Budapest. I was assigned to Gelfond; it was customary that every guest had his own Hungarian guide. I had a special role too, because Gelfond was supposed to buy clothes and shoes, which were hard to get in Russia at the time, for his wife. So I was in the driver's seat because I knew the shops pretty well.

R&S: You spoke Russian then?

Szemerédi: Well, my Russian was not that good. I don't know if I should tell this in this interview, but I failed the Russian exam twice. Somehow I managed to pass the final exam and I was sent to Russia. My Russian was good enough for shopping but not good enough for having more complex conversations. I only had to ask Gelfond for the size of the shoes he wanted for his wife and then I had a conversation in Hungarian with the shopkeepers. I usually don't have good taste, but because I had to rise to the occasion, so to say, I was very careful and thought about it a lot. Later Gelfond told me that his wife was very satisfied. He was very kind and said that he would arrange the switch of supervisors!

This happened in the spring of 1968, but unfortunately he died that summer of a heart attack, so I stayed with Gelfand for a little more than a year after that. I could have returned to Hungary, but I didn't want that; when I first agreed to study there, I felt I had to stay. They (i.e., Gelfand and the people around him) were very understanding when they realized that I would never learn what I was supposed to. Actually, my exam consisted of two exercises about representation theory taken from Kirillov's book, which they usually give to third-year students. I did it, but there was an error in my solution. My supervisor was Bernstein, as you know a great mathematician and a very nice guy, too. He found the error in the solution, but he said that it was the effort that I had put into it that was important rather than the result, and he let me pass the exam.

To become a candidate you had to write a dissertation, and Gelfand let me write one about combinatorics. This is what I did. So, in a way, I finished my study in Moscow rather successfully. I did not learn anything, but I got the paper showing that I had become a candidate.

At this time there was a hierarchy in Hungary: doctorate of the university, then candidate, doctorate of the academy, then corresponding member of the academy, and then member of the academy. I achieved becoming a candidate of mathematics.

R&S: *You had to work entirely on your own in Moscow?*

Szemerédi: Yes, since I worked in combinatorics.

R&S: *Gelfond must have realized that you were a good student. Did he communicate this to Gelfand in any way?*

Szemerédi: That I don't know. I only know that Gelfand very soon realized my lack of mathematical education. But when Gelfond came to Hungary, he talked to Turán and Erdős and also to Hungarian number theorists attending that meeting, and they were telling him: "Here is this guy who has a very limited background in mathematics." This may be the reason why Gelfond agreed to take me as his student. But unfortunately he died early.

Hungarian Mathematics

R&S: *We would like to come back to Hungarian mathematics. Considering the Hungarian population is only about ten million people, the list of famous Hungarian mathematicians is very impressive. To mention just a few, there is János Bolyai in the nineteenth century, one of the fathers of non-Euclidean geometry. In the twentieth century there is a long list, starting with the Riesz brothers, Frigyes and Marcel; Lipót Fejér; Gábor Szegő, Alfréd Haar; Tibor Radó; John von Neumann, perhaps the most ingenious of them all; Paul Turán; Paul Erdős; Alfréd Rényi; Raoul Bott (who left the country early but then became famous in the United States). Among those still alive, you have Peter Lax, who won the Abel Prize in 2005; Béla Bollobás, who is in Great Britain; László Lovász; and now you. It's all very impressive. You have already mentioned some facts that may explain the success of Hungarian mathematics. Could you elaborate, please?*

Szemerédi: We definitely have a good system to produce elite mathematicians, and we have always had that. At the turn of the century—we are talking about the nineteenth century and the beginning of the twentieth century—we had two or three absolutely outstanding schools, not only the so-called Fasori, where von Neumann and Wigner studied, but also others. We were able to produce a string of young mathematicians, some of whom later went abroad and became great mathematicians—or great physicists, for that matter. In that sense I think the educational system was extremely good. I don't know whether the general education was that good, but definitely for mathematics and theoretical physics it was extremely good. We had at least five top schools that concentrated on these two subjects, and that is already good enough to produce some great mathematicians and physicists.

Back to the question of whether the Hungarians are really so good or not. Definitely in discrete mathematics there was a golden period. This was mainly because of the influence of Erdős. He always travelled around the world, but he also spent a lot of time in Hungary. Discrete mathematics was certainly the strongest group.

The situation has changed now. Many Hungarian students go abroad to study at Princeton, Harvard, Oxford, Cambridge, or Paris. Many of them stay abroad, but many of them come home and start to build schools. Now we cover a much broader spectrum of mathematics, such as algebraic geometry, differential geometry, low-dimensional topology, and other subjects. In spite of my being a mathematician working in discrete mathematics who doesn't know practically anything about these subjects, I am very happy to see this development.

R&S: *You mentioned the journal* KöMaL, *which has been influential in promoting mathematics in Hungary. You told us that you were not personally engaged, but this journal was very important for the development of Hungarian mathematics. Isn't that true?*

Szemerédi: You are absolutely right. This journal is meant for a wide audience. Every month the editors present problems, mainly from mathematics but also from physics. At least in my time, in the late 1950s, it was distributed to every high school, and a lot of the students worked on these problems. If you solved the problems regularly, then by the time you finished high school you would know almost as much as the students in the elite high schools. The editors added the points you got from each correct solution at the end of the year, giving a bonus for elegant solutions. Of course, the winners were virtually always from one of these elite high schools.

But it was intended for a much wider audience and it helped a lot of students, not only mathematicians. In particular, it also helped engineers. People may not know this, but we have very good schools for different kinds of engineering, and a lot of engineering students-to-be actually solved these problems. They may not have been among the best, but it helped them to develop a kind of critical thinking. You don't just make a statement, but you try to see connections and put them together to solve the problems. So by the time they went to engineering schools, which by itself required some knowledge of mathematics, they were already quite well educated in mathematics because of *KöMaL*.

KöMaL plays an absolutely important role and, I would like to emphasize, not only in mathematics but more generally in natural sciences. Perhaps even students in the humanities are now working on these problems. I am happy for that and I would advise them to continue to do so (of course, not to the full extent, because they have many other things to study).

Important Methods and Results

R&S: *We would now like to ask you some questions about your main contributions to mathematics.*

You have made some groundbreaking (we don't think that this adjective is an exaggeration) discoveries in combinatorics, graph theory, and combinatorial number theory. But arguably you are most famous for what is now called the Szemerédi theorem, the proof of the Erdős-Turán conjecture from 1936.

Your proof is extremely complicated. The published proof is forty-seven pages long and has been called a masterpiece of combinatorial reasoning. Could you explain first of all what the theorem says, the history behind it, and why and when you got interested in it?

Szemerédi: Yes, I will start in a minute to explain what it is, but I suspect that not too many people have read it. I will explain how I got to the problem, but first I want to tell how the whole story started. It started with the theorem of van der Waerden: you fix two numbers, say five and three. Then you consider the integers up to a very large number, from 1 to n, say. Then you partition this set into five classes, and there will always be a class containing a three-term arithmetic progression. That was a fundamental result of van der Waerden—of course, not only with three and five but with general parameters.

Later, Erdős and Turán meditated over this result. They thought that maybe the reason why there is an arithmetic progression is not the partition itself; if you partition into five classes, then one class contains at least one fifth of all the numbers. They made the conjecture that what really counts is that you have dense enough sets.

That was the Erdős-Turán conjecture: if your set is dense enough in the interval 1 to n—we are of course talking about integers—then it will contain a long arithmetic progression. Later Erdős formulated a very brave and much stronger conjecture: let's consider an infinite sequence of positive integers, $a_1 < a_2 < \cdots$, such that the sum of the inverses $\{1/a_i\}$ is divergent. Then the infinite sequence contains arbitrarily long arithmetic progressions. Of course, this would imply the absolutely fundamental result of Green and Tao about arbitrarily long arithmetic progressions within the primes because for the primes, we know that the sum of the inverses is divergent.

That was a very brave conjecture; it isn't even solved for arithmetic progressions of length $k=3$. But now people have come very close to proving it: Tom Sanders proved that if we have a subset between 1 and n containing at least n over $\log n(\log\log n)^5$ elements, then the subset contains a three-term arithmetic progression. Unfortunately, we need a little bit more, but we are getting close to solving Erdős's problem for $k=3$ in the near future, which will be a great achievement. If I'm not mistaken, Erdős offered US $3,000 for the solution of the general case a long time ago. If you consider inflation, that means quite a lot of money.

R&S: *Erdős offered 1,000 USD for the problem you solved, and that's the highest sum he ever paid, right?*

Szemerédi: Erdős offered US$1,000 as well for a problem in graph theory which was solved by V. Rödl and P. Frankl. These are the two problems I know about.

R&S: *Let's get back to how you got interested in the problem.*

Szemerédi: That was very close to the Gelfand/Gelfond story, at least in a sense. At least the message is the same: I overlooked facts. I tried to prove that if you have an arithmetic progression, then it cannot happen that the squares are dense inside of it; specifically, it cannot be that a positive fraction of the elements of this arithmetic progression are squares. I was about twenty-five years old at the time and at the end of my university studies. At that time I already worked with Erdős. I very proudly showed him my proof, because I thought it was my first real result. Then he pointed out two, well, not errors, but deficiencies in my proof. Firstly, I had assumed that it was known that $r_4(n)=o(n)$,[1] i.e., that if you have a set of positive upper density, then it has to contain an arithmetic progression of length four or, for that matter, of any length. I assumed that this was a true statement. Then I used [the fact] that there are no four

[1] *$r_4(n)$ denotes the proportion of elements between 1 and n that a subset must contain in order for it to contain an arithmetic progression of length k.*

Photo: Eirik Furu Baardsen.

Martin Raussen (left) and Christian Skau (center) interviewing Abel Laureate Endre Szemerédi.

squares that form an arithmetic progression. However, Erdős told me that the first statement was not known; it was an open problem. The other one was already known to Euler, which was two hundred fifty years before my time. So I had assumed something that is not known and, on the other hand, I had proved something that had been proven two hundred fifty years ago!

The only way to try to correct something so embarrassing was to start working on the arithmetic progression problem. That was the time I started to work on $r_4(n)$ and, more generally, on $r_k(n)$. First I took a look at Klaus Roth's proof from 1953 of $r_3(n)$ being less than n divided by log log n . I came up with a very elementary proof for $r_3(n)=o(n)$ so that even high school students could understand it easily. That was the starting point. Later I proved also that $r_4(n)=o(n)$.

Erdős arranged for me to be invited to Nottingham to give a talk on that result. But my English was virtually nonexistent. Right now you can still judge that there is room for improvement in my English, but at the time it was almost nonexistent. I gave a series of lectures; Peter Elliot and Edward Wirsing, both extremely strong mathematicians, wrote a paper based almost entirely on my pictures on the blackboard. Perhaps they understood some easy words in English that I used. Anyway, they helped to write the paper for me. A similar thing happened when I solved $r_k(n)=o(n)$ for general k. Then my good friend András Hajnal helped me to write the paper. That is actually an understatement. The truth is that he listened to my explanations and he then wrote the paper. I am very grateful to Peter Elliot, Edward Wirsing, and to my good friend András for their invaluable help.

R&S: *When did all this happen?*

Szemerédi: It was in 1973. The paper appeared in *Acta Arithmetica* in 1975. There is a controversial issue—well, maybe controversial is too strong a word—about the proof. It is widely said that one of the main tools in the proof is the so-called regularity lemma, which is not true in my opinion. Well, everybody forgets about the proofs they produced thirty years ago. But I reread my paper and I couldn't find the regularity lemma. There occurs a lemma in the proof which is similar to the regularity lemma, so maybe that lemma, which is definitely not the regularity lemma, inspired me later to prove the regularity lemma.

The real story is that I heard Bollobás's lectures from 1974 about strengthening the Erdős–Stone theorem. The Erdős–Stone theorem from the 1940s was also a breakthrough result, but I don't want to explain it here. Then Bollobás and Erdős strengthened it. I listened to Bollobás's lectures and tried to improve their result. Then it struck me that a kind of regularity lemma would come in handy, and this led me to prove the regularity lemma. I am very grateful to Vasek Chvatal, who helped me to write the regularity paper. Slightly later the two of us gave a tight bound for the Erdős-Stone theorem.

R&S: *I've seen that people refer to it in your proof of the Erdős-Turán conjecture as a weakened form of the regularity lemma.*

Szemerédi: Yes, weaker, but similar in ideology, so to speak.

Connections to Ergodic Theory

R&S: *Your proof of the Szemerédi theorem is the beginning of a very exciting story. We have heard from a reliable source that Hillel Furstenberg at the Hebrew University in Jerusalem first learned about your result when somebody gave a colloquium talk there in December 1975 and mentioned your theorem. Following the talk, there was a discussion in which Furstenberg said that his weak mixing of all orders theorem, which he already knew, would prove the ergodic version of the Szemerédi theorem in the weak mixing case. Since the Kronecker (or compact) case is trivial, one should be able to interpolate between them so as to get the full ergodic version. It took a couple of months for him to work out the details, which became his famous multiple recurrence theorem in ergodic theory.*

We find it amazing that the Szemerédi theorem and Furstenberg's multiple recurrence theorem are equivalent, in the sense that one can deduce one theorem from the other. We guess it is not off the mark to say that Furstenberg's proof gave a conceptual framework for your theorem. What are your comments?

Szemerédi: As opposed to me, Furstenberg is an educated mathematician. He is a great mathematician and he already had great results in ergodic theory; he knew a lot. He proved that a measure-preserving system has a multiple recurrence property; this is a far-reaching generalization of a classical result by Poincaré. Using his result, Furstenberg proved my result on the k-term arithmetic progressions. So that is the short story about it. But I have to admit that his method is much stronger because it could be generalized to a multidimensional setting. Together with Katznelson he proved that in 1978. They could actually also prove the density Hales-Jewett theorem, but it took more than ten years. Then Bergelson and Leibman proved a polynomial version of the arithmetic progression result, much stronger than the original one. I doubt that you can get it by elementary methods, but that is only my opinion. I will bet that they will not come up with a proof of the polynomial version within the next ten years by using elementary methods.

But then very interesting things happened. Tim Gowers started the so-called Polymath Project: many people communicated with each other on the Internet and decided that they would try to give a combinatorial proof of the Hales-Jewett density theorem using only elementary methods. After two months they came up with an elementary proof. The density Hales-Jewett theorem was considered to be by far the hardest result proved by Furstenberg and Katznelson, and its proof is very long. The elementary proof of the density Hales–Jewett theorem is about twenty-five pages long.

There is now a big discussion among mathematicians whether one can use this method to solve other problems. Joint papers are very good when a small group of mathematicians cooperate. But the Polymath Project is different; hundreds of people communicate. You may work on something your whole life; then a hundred people appear, and many of them are ingenious. They solve your problem, and you are slightly disappointed. Is this a good thing? There is a big discussion among mathematicians about this method. I am for it. I will soon turn seventy-two years old, so I believe I can evaluate it without any self-interest.

R&S: *Still, all this started with your proof of the Erdős-Turán conjecture. You mentioned Green-Tao. An important ingredient in their proof of the existence of arithmetic progressions of arbitrary length within the primes is a Szemerédi-type argument involving so-called pseudo-primes, whatever that is. So the ramifications of your theorem have been impressive.*

Szemerédi: In their abstract they say that the three main ingredients in their proof are the Goldstone-Yıldırım result, which gives an estimate for the difference of consecutive primes; their transference principle; and my theorem on arithmetic progressions.

R&S: *By the way, according to Green and Tao one could have used the Selberg sieve instead.*

Szemerédi: You are right. However, in my opinion the main revolutionary new idea is their transference principle which enables us to go from a dense set to a sparse set. I would like to point out that later, while generalizing their theorem, they did not have to use my theorem. Terry Tao said that he read all the proofs of the Szemerédi theorem and compared them, and then he and Ben Green meditated on it. They were probably more inspired by Furstenberg's method, the ergodic method. That is at least my take on this thing, but I am not an expert on ergodic theory.

R&S: *But Furstenberg's theorem came after and was inspired by yours. So however you put it, it goes back to you.*

Szemerédi: Yes, that is what they say.

R&S: *We should mention that Tim Gowers also gave a proof of the Szemerédi theorem.*

Szemerédi: He started with Roth's method, which is an estimation of exponential sums. Roth proved in his paper that $r_3(n)$ is less than n divided by log log n. Tim Gowers's fundamental work not only gave an absolutely strong bound for the size of a set A in the interval $[1,n]$ not containing a k-term arithmetic progression, he also invented methods and concepts that later became extremely influential. He introduced a norm (actually, several norms) that is now called the Gowers norm. This norm controls the randomness of a set. If the Gowers norm is big, he proved that it is correlated with a higher-order phase function, which is a higher-order polynomial. Gowers, and independently Rödl, Naegle, Schacht, and Skokan, proved the hypergraph regularity lemma and the hypergraph counting lemma, which are main tools in additive combinatorics and in theoretical computer science.

R&S: *We should mention that Gowers received the Fields Medal in 1998 and that Terence Tao got it in 2006. Also, Roth was a Fields Medal recipient back in 1958.*

Random Graphs and the Regularity Lemma

R&S: *Let's get back to the so-called Szemerédi regularity theorem. You have to explain the notions of random graphs and extremal graphs, because they are involved in this result.*

Szemerédi: How can we imagine a random graph? I will talk only about the simplest example. You have n points and the edges are just the pairs, so each edge connects two points. We say that the graph is complete if you include all the edges, but that is, of course, not an interesting object. In one model of the random graph, you just close your eyes and with probability ½ you choose an edge. Then you will eventually get a graph. That is what we call a random graph, and most of them have very nice properties.

You just name any configuration, such as four-cycles C_4, for instance, or the complete graph K_4; then the number of such configurations is as you would expect. A random graph has many beautiful properties and it satisfies almost everything. Extremal graph theory is about finding a configuration in a graph. If you know that your graph is a random graph, you can prove a lot of things.

The regularity lemma is about the following. If you have any graph—unfortunately we have to assume a dense graph, which means that you have a lot of edges—then you can break the vertex set into a relatively small number of disjoint vertex sets, so that if you take almost any two of these vertex sets, between them the so-called bipartite graph will behave like a random graph. We can break our graph into not too many pieces, so we can work with these pieces and we can prove theorems in extremal graph theory.

We can also use it in property testing, which belongs to theoretical computer science and many other areas. I was surprised that they use it even in biology and neuroscience, but I suspect that they use it in an artificial way, that they could do without the regularity lemma. But I am not an expert on this, so I can't say this for sure.

R&S: *The regularity lemma really has some important applications in theoretical computer science?*

Szemerédi: Yes, it has, mainly in property testing but also in constructing algorithms. Yes, it has many important applications. Not only the original regularity lemma, but, since this is thirty years ago, there have appeared modifications of the regularity lemma which are more adapted for these purposes. The regularity lemma is for me just a philosophy, not an actual theorem. Of course, the philosophy is almost everything. That is why I like to say that in every chaos there is an order. The regularity lemma just says that in every chaos there is a big order.

R&S: *Do you agree that the Szemerédi theorem, i.e., the proof of the Erdős-Turán conjecture, is your greatest achievement?*

Szemerédi: It would be hard to disagree because most of my colleagues would say so. However, perhaps I prefer another result of mine with Ajtai and Komlós. In connection with a question about Sidon sequences we discovered an innocent-looking lemma. Suppose we have a graph of n vertices in which a vertex is connected to at most d other vertices. By a classical theorem of Turán, we can always find at least n/d vertices such that no two of them are connected by an edge. What we proved was that under the assumption that the graph contains no triangle, a little more is true: one can find n/d times $\log d$ vertices with the above property.

I am going to describe the proof of the lemma very briefly. We choose $n/2d$ vertices of our graph randomly. Then we omit all the neighbors of the points in the chosen sets. This is, of course, a deterministic step. Then in the remaining vertex set we again choose randomly $n/2d$ vertices and again deterministically omit the neighbors of the chosen set. It can be proved that this procedure can be repeated $\log d$ times and in the chosen set the average degree is at most 2. So in the chosen sets we can find a set of size at least $n/4d$ such that no two points are connected with an edge.

Because of the mixture of random steps and deterministic steps, we called this new technique the "semirandom method".

Historically, the first serious instance of a result of extremal graph theory was the famous theorem of Ramsey and, in a quantitative form, of Erdős and Szekeres. This result has also played a special role in the development of the "random method". Therefore it has always been a special challenge for combinatorialists to try to determine the asymptotic behavior of the Ramsey functions $R(k,n)$,[2] as n (or both k and n) tend to infinity. It can be easily deduced from our lemma that $R(3,n) < cn^2/\log n$, which solved a long-standing open problem of Erdős. Surprisingly, about ten years later, Kim proved that the order of magnitude of our bound was the best possible. His proof is based on a brilliant extension of the "semirandom method".

The "semirandom method" has found many other applications. For instance, together with Komlós and Pintz, I used the same technique to disprove a famous geometric conjecture of Heilbronn. The conjecture dates back to the 1940s. The setting is as follows: you have n points in the unit square and you consider the triangles defined by these points. Then the conjecture says that you can always choose a triangle of area smaller than a constant over n^2. That was the Heilbronn conjecture. For the bound $1/n$, this is trivial; then Klaus Roth improved this to 1 over $n(\log\log n)^{1/2}$. Later Wolfgang Schmidt improved it further to 1 over $n(\log n)^{1/2}$. Roth, in a very brilliant and surprising way, used analysis to prove that we can find a triangle of area less than 1 over $n^{(1+\alpha)}$, where α is a constant.

We then proved, using the semirandom method, that it is possible to put down n points such that the smallest area of a triangle is at least $\log n$ over n^2, disproving the Heilbronn conjecture. Roth told us that he gave a series of talks about this proof.

Further Research Areas

R&S: *It is clear from just checking the literature and talking with people familiar with graph theory and combinatorics, as well as additive number theory, that you, sometimes with coauthors, have obtained results that have been groundbreaking and have set the stage for some very important developments. Apart from the Szemerédi theorem and the regularity lemma that we have already talked about, here is a short list of important results that you and your coauthors have obtained:*

(i) The Szemerédi-Trotter theorem in the paper "Extremal problems in discrete geometry" from 1983.

[2] *$R(k,n)$ denotes the least positive integer N such that for any (red/blue)-coloring of the complete graph K_N on N vertices, there exists either an entirely red complete subgraph on k vertices or an entirely blue subgraph on v vertices.*

(ii) The Erdős–Szemerédi theorem on product-sum estimates in the paper "On sums and products of integers" from 1983.

(iii) The results obtained by AKS, which is the acronym for Miklós Ajtai, János Komlós, and Endre Szemerédi. The "sorting algorithm" is among the highlights.

Could you fill in some details, please?

Szemerédi: (i) Euclid's system of axioms states some of the basic facts about incidences between points and lines in the plane. In the 1940s, Paul Erdős started asking slightly more complicated questions about incidences that even Euclid would have understood. How many incidences can occur among m points and n lines, where an "incidence" means that a line passes through a point? My theorem with Trotter confirmed Erdős's rather surprising conjecture: the maximal number of incidences is much smaller in the real plane than in the projective one, much smaller than what we could deduce by simple combinatorial considerations.

(ii) Together with Paul Erdős, we discovered an interesting phenomenon and made the first nontrivial step in exploring it. We noticed, roughly speaking, that a set of numbers may have nice additive properties or nice multiplicative properties but not both at the same time. This has meanwhile been generalized to finite fields and other structures by Bourgain, Katz, Tao, and others. Their results had far-reaching consequences in seemingly unrelated fields of mathematics.

(iii) We want to sort n numbers, that is, to put them in increasing order by using comparisons of pairs of elements. Our algorithm is nonadaptive: the next comparison never depends on the outcome of the previous ones. Moreover, the algorithm can efficiently run simultaneously on cn processors such that every number is processed by only one of them at a time. Somewhat surprisingly, our algorithm does not require more comparisons than the best possible adaptive nonparallel algorithm. It is well known that any sorting algorithm needs at least $n \log n$ comparisons.

R&S: *What are, in your opinion, the most interesting and important open problems in combinatorics and graph theory?*

Szemerédi: I admit that I may be somewhat conservative in taste. The problem that I would like to see solved is the very first problem of extremal graph theory: to determine the asymptotic behavior of the Ramsey functions.

Combinatorics Compared to Other Areas of Mathematics

R&S: *It is said, tongue in cheek, that a typical combinatorialist is a bright mathematician with an aversion to learning or embracing abstract mathematics. Does this description fit you?*

Szemerédi: I am not sure. In combinatorics we want to solve a concrete problem, and by solving a problem we try to invent new methods. And it goes on and on. Sometimes we actually borrow from so-called well-established mathematics. People in other areas of mathematics often work in ways that are different from how we work in combinatorics.

Let's exaggerate somewhat: they have big theories and they find sometimes a problem for the theory. In combinatorics, it is usually the other way around. We start with problems that actually are both relevant and necessary; that is, the combinatorics itself requires the solution of the problems, the problems are not randomly chosen. You then have to find methods that you apply to solve the problems and sometimes you might create a theory. But you start out by having a problem; you do not start by having a theory and then finding a problem to which you can apply the theory. Of course, that happens from time to time, but it is not the major trend.

Now, in the computer era, it is unquestionable that combinatorics is extremely important. If you want to run programs efficiently, you have to invent algorithms in advance, and these are basically combinatorial in nature. This is perhaps the reason why combinatorics today is a little bit elevated, so to say, and that mathematicians from other fields start to realize this and pay attention. If you look at the big results, many of them have big theories which I don't understand, but at the very root there is often some combinatorial idea. This discussion is a little bit artificial. It's true that combinatorics was a second-rated branch of mathematics thirty years ago but hopefully not any longer.

R&S: *Do you agree with Bollobás, who in an interview from 2007 said the following: "The trouble with the combinatorial problems is that they do not fit into the existing mathematical theories. We much more prefer to get help from 'mainstream' mathematics rather than to use 'combinatorial' methods only, but this help is rarely forthcoming. However, I am happy to say that the landscape is changing."*

Szemerédi: I might agree with that.

R&S: *Gowers wrote a paper about the two cultures within mathematics. There are problem solvers and there are theory builders. His argument is that we need both. He says that the organizing principles of combinatorics are less explicit than in core mathematics. The important ideas in combinatorial mathematics do not usually appear in the form of precisely stated theorems but more often as general principles of wide applicability.*

Szemerédi: I guess that Tim Gowers is right. But there is interplay between the two disciplines. As Bollobás said, we borrow from the other branches of mathematics if we can when we solve concrete discrete problems, and vice versa. I once sat in class when a beautiful result in analytic number theory was presented. I understood only a part of it. The mathematician who gave the talk came to the bottleneck of the whole argument. I realized that it was a combinatorial statement and if you gave it to a combinatorialist, he would probably have solved it. Of course, one would have needed the whole machinery to prove the result in question, but at the root it actually boiled down to a combinatorial argument. A real interplay!

R&S: *There is one question that we have asked almost all Abel Prize recipients; it concerns the development of important new concepts and ideas. If you recollect, would key ideas turn up when you were working hard at your desk on a problem, or did they show up in more relaxed situations? Is there any pattern?*

Szemerédi: Actually, both! Sometimes you work hard on a problem for half a year and nothing comes out. Then suddenly you see the solution, and you are surprised and slightly ashamed that you hadn't noticed these trivial things which actually finish the whole proof and which you did not discover for a long time. But usually you work hard and step-by-step you get closer to the solution. I guess that this is the case in every science. Sometimes the solution comes out of the blue, but sometimes several people are working together and find the solution.

I have to tell you that my success ratio is actually very bad. If I counted how many problems I have worked on and in how many problems I have been successful, the ratio would be very bad.

R&S: *Well, in all fairness, this calculation should take into consideration how many problems you have tried to solve.*

Szemerédi: Right, that is a nice remark.

R&S: *You have been characterized by your colleagues—and this is meant as a huge compliment—as having an "irregular mind". Specifically, you have been described as having a brain that is wired differently than most mathematicians' [brains]. Many admire your unique way of thinking, your extraordinary vision. Could you try to explain to us how you go about attacking problems? Is there a particular method or pattern?*

Szemerédi: I don't particularly like the characterization of having an "irregular mind". I don't feel that my brain is wired differently and I think that most neurologists would agree with me. However, I believe that having unusual ideas can often be useful in mathematical research. It would be nice to say that I have a good general approach of attacking mathematical problems. But the truth is that after many years of research I still do not have any idea what the right approach is.

Mathematics and Computer Science

R&S: *We have already talked about connections between discrete mathematics and computer science; you are in fact a professor in computer science at Rutgers University in the United States. Looking back, we notice that for some important mathematical theorems, like the solution of the four-color problem, for instance, computer power has been indispensable. Do you think that this is a trend? Will we see more results of this sort?*

Szemerédi: Yes, there is a trend. Not only for this but also for other types of problems as well, where computers are used extensively. This trend will continue, even though I am not a computer expert. I am in the computer science department, but fortunately nobody asked me whether I could answer email, which I cannot! They just hired me because so-called theoretical computer science was highly regarded in the late 1980s. Nowadays, it does not enjoy the same prestige, though the problems are very important, the P versus NP problem, for instance. We would like to understand computation and how fast it is; this is absolutely essential mathematics, and not only for discrete mathematics. These problems lie at the heart of mathematics, at least in my opinion.

R&S: *May we come back to the P versus NP problem, which asks whether every problem whose solution can be verified quickly by a computer can also be solved quickly by a computer. Have you worked on it yourself?*

Szemerédi: I am working on two problems in computer science. The first one is the following: assume we compute an n-variable Boolean function with a circuit. For most of the n-variable Boolean functions the circuit size is not polynomial. But to the best of my knowledge, we do not know a particular function which cannot be computed with a Boolean circuit of linear size and depth $\log n$. I have no real idea how to solve this problem.

The second one is the minimum weight spanning tree problem. Again, so far I am unsuccessful.

I have decided that now I will, while keeping up with combinatorics, learn more about analytic number theory. I have in mind two or three problems, which I am not going to tell you. It is not the Riemann hypothesis, that I can tell.

R&S: *The P versus NP conjecture is on the Clay list of problems, the prize money for a solution being one million USD, so it has a lot of recognition.*

Szemerédi: Many people believe that the P versus NP problem is the most important one in current mathematics, regardless of the Riemann hypothesis and the other big problems. We should understand computation. What is in our power? If we can check easily that something is true, can we easily find a solution? Most probably not! Almost everybody will bet that P is not equal to NP, but not too much has been proved.

Soccer

R&S: *You have described yourself as a sports fanatic.*

Szemerédi: Yes, at least I was. I wanted to be a soccer player, but I had no success.

R&S: *We have to stop you there. In 1953, when you were thirteen years old, Hungary had a* *fantastic soccer team; they were called The Mighty Magyars. They were the first team outside the British Isles that beat England at Wembley and even by the impressive score of 6 to 3. At the return match in Budapest in 1954 they beat England 7 to 1, a total humiliation for the English team. Some of these players on the Hungarian team are well known in the annals of soccer, names like Puskás, Hidegkuti, Czibor, Bozsik, and Kocsis.*

Szemerédi: Yes. These five were world-class players.

R&S: *We have heard that the Hungarian team, before the game in Budapest, lived at the same place as you did. Bozsik watched you play soccer, and he said that you had real talent. Is this a true story?*

Szemerédi: Yes, that is true, except that they did not live at the same place. My mother died early; this is why we three brothers lived at a boarding school. That school was very close to the hotel where the Hungarian team lived. They came sometimes to our soccer field to relax and watch our games, and one time we had a very important game against the team that was our strongest competitor. You know, boarding schools were competing like everyone else.

I was a midfielder like Bozsik. I was small and did not have the speed, but I understood the Hungarian team's strategy. They revolutionized the soccer game, foreshadowing what was later called "Total Football". They did not pass the ball to the nearest guy, but rather they aimed the ball to create space and openings, often behind the other team's defense. That was a completely different strategy than the standard one, and therefore they were extremely effective.

I studied this and I understood their strategy and tried to imitate it. Bozsik saw this and he understood what I was trying to do.

R&S: *You must have been very proud.*

Szemerédi: Yes, indeed I was very proud. He was nice, and his praise is still something which I value very much.

R&S: *Were you very disappointed with the World Cup later that year? As you very well know, the heavily favored Hungarian team first beat West Germany 8 to 3 in the preliminary round, but then they lost 2 to 3 in the final to West Germany.*

Szemerédi: Yes. It was very unfortunate. Puskás was injured, so he was not at his best, but we had some other problems, too. I was very, very sad and for months I practically did not speak to anybody. I was a real soccer fan. Much later, in 1995, a friend of mine was the ambassador for Hungary in Cairo, and I visited him. Hidegkuti came often to the embassy because he was the coach for the Egyptian team. I tried to make him explain to me what happened in 1954, but I got no answer.

By the way, to my big surprise I quite often guess correct results. Several journalists came to me in Hungary for an interview after it was announced that I would receive the Abel Prize. The last question from one of them was about the impending European Cup quarter-final match between Barcelona and Milan. I said that up to now I have answered your questions without hesitation, but now I need three minutes. I reasoned that the defense of Barcelona was not so good (their defender Puyol is a bit old), but their midfield and attack are good, so: 3 to 1 to Barcelona. On the day the game was played, the paper appeared with my—as it turned out—correct prediction. I was very proud of this, and people on these blogs wrote that I could be very rich if I would enter the odds-prediction business!

R&S: *We can at least tell you that you are by far the most sports-interested person we have met so far in these Abel interviews!*

On behalf of the Norwegian, Danish, and European Mathematical Societies, and on behalf of the two of us, thank you very much for this most interesting interview.

Szemerédi: Thank you very much. I am very happy for the opportunity of talking to you.