Interview with Prof. Andrzej Schinzel

The Proof is in the Pudding

In 2012, Prof. Andrzej Schinzel received an honorary degree from the Adam Mickiewicz Institute in Poznań. In previous years, he received similar diplomas from the University of Caen and the Cardinal Stefan Wyszyński University in Warsaw

Academia: You study number theory. What do you find so fascinating about it; what made you want to study it?

I became interested in number theory as soon as I started university in 1953. Professor Wacław Sierpiński held seminars on the subject, which were only available to first-year students. It was very unusual: seminars were usually only open to more advanced students. So I went along, and the subjects the Professor discussed were fascinating. Number theory is unusual in that there are a lot of problems that are simple to formulate and understand without much experience, but difficult to solve. In other fields, in order to understand a problem you generally need a lot of background knowledge.

Has a lot of progress been made in number theory, or is it a field with many problems remaining unsolved?

A bit of both. Several famous problems, originally posed decades or even centuries ago, have been solved during my lifetime. However, we still have a long way to go. To start with, there's one of the oldest and best-known problems – the Goldbach *conjecture, dating back to 1742 – stating that each integer greater than 5 can be written as the sum of three primes. Two problems dating back to antiquity also remain unresolved, although neither is very useful. I use the word "useful" in the sense used by G.H. Hardy, the outstanding English mathematician in his seminal work "A Mathematician's Apology". A mathematical problem is considered to be useful if it's linked to several other problems. If it is completely isolated, then it may be difficult, but it is not necessarily useful. Problems of perfect numbers, dating back to Antiquity, are rather isolated in number theory. The Goldbach conjecture is different; since it is linked with many others, it is considered to be a very useful problem. Last year I delivered a lecture in Kraków during the conference of the Polish Academy of Arts and Sciences covering certain unsolved problems in number theory. I also discussed seven other open problems.*

How is it that so many problems that have been unsolved for decades or even centuries have been solved in recent years?

Without a doubt, solving one problem helps us resolve others. The most famous problem of number theory was Fermat's Last Theorem, which remained unsolved for over 300 years, until a proof was finally published in 1995. Pierre de Fermat noted the theorem in the margin of a copy of a Latin translation of Diophantus' Arithmetica, famously with a note stating: "It is impossible to separate a cube into two cubes, or a fourth power into two fourth powers, or in general, any power higher than the second, into two like powers. I have discovered a truly marvelous proof of this, which this margin is too narrow to contain." The solution of the theorem by Andrew Wiles was immediately followed by the solution of another useful problem concerning arithmetic progression formed by exponentiation. But that's not the only reason why so many problems have been resolved in recent decades. Theory has undoubtedly moved forward, and problems are quite distinct from theories: theories develop much more slowly. This doesn't always depend on individual problems, although the most likely reason is that in recent years, the number of people actively working in number theory and mathematics in general has increased significantly. It's hard for me to make a numerical guess on the spot, but the number of published mathematics papers has been growing exponentially. All papers are reviewed by the Mathematical Reviews of the American Mathematical Society. Professor William LeVeque used it to extract all reviews of number theory, and published them in six vast volumes covering the period between 1940 and 1974. The original tomes covered a period of over 30 years; they were followed with another six volumes, covering 1974-1984, with a similar number of papers published in just a third of the time. Of course, today everything is online, although may people my age prefer using books.

A mathematical problem is „useful" if it's linked to numerous other problems. One that is isolated might be difficult, but not useful.

Has the development of the Internet had an effect on progress in number theory?

I have no evidence for this, although there is no doubt that the rapid development of computers has made a contribution. They have helped us refute hypotheses which had been in place for over a century, and in one case for over 250 years.

Do you ever have a feeling that mathematics is, as described by some, touching the absolute? Something outside our everyday lives; something tangible and "provable"?

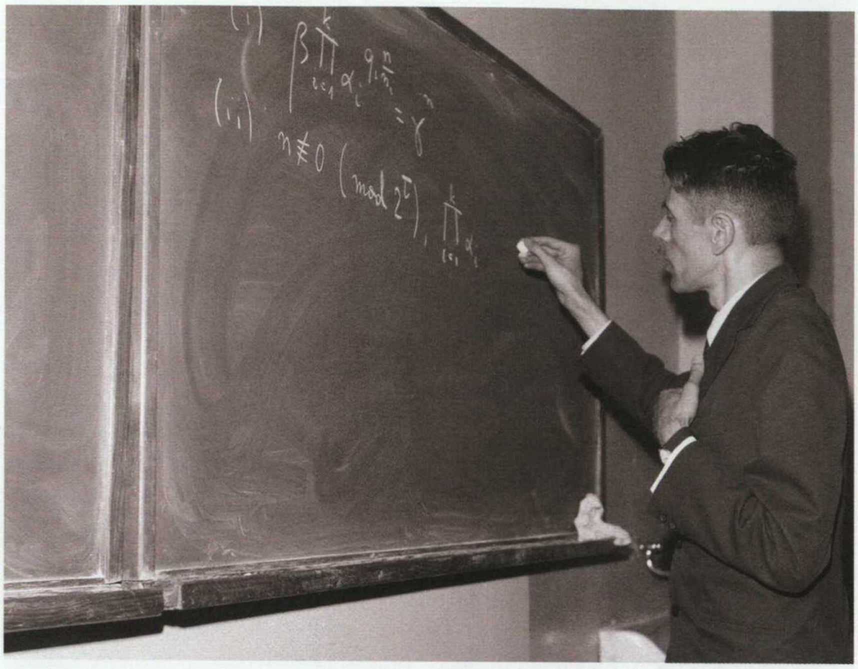
What I like about number theory is that it involves open questions that generally concern issues that can be verified. Other mathematical theories can be very abstract, while number theory tends to concern natural numbers. We can check whether a hypothesis is true up to, say, a million, or given the existing computational power even up to a trillion. The problem is deciding whether the given theory is true in general or not. Mathematicians look at specific problems in a general way, which distinguishes them from IT specialists who tend to examine the problem at hand as deeply as possible. Those are two completely different approaches. There was a Chinese boy, very widely written about, who started studying mathematics at the University of California in Los Angeles when he was 12 years old. A few years later, I asked a colleague from LA how the boy was getting on. He said that the student did attend his lectures at first, but he was soon seduced by the "magic of metal knobs," and instead of simply verifying whether a theory is true or not, he wanted to know how to test it for trillions of instances. That's the basic difference between mathematical and IT approaches to open questions.

Do you have a preferred field of mathematics?

I've worked in many different areas, but my best work has been in polynomials.

You published your first paper when you were 17 years old.

Yes, in 1954. It was submitted by Prof. Sierpiński to the bulletin of the Polish Academy of Sciences. My second paper was co-written with Prof. Sierpiński; we published a total of 7 papers together. He was a great influence on me until I graduated from university. Afterwards, when I was a postgraduate student – it was the equivalent of today's doctoral studies – I met Prof. Pal Erdős, the outstanding Hungarian mathematician. Later on, in 1960, after I completed my PhD at the PAS Institute of Mathematics, I had a year-long Rockefeller scholarship



Rutowska Grażyna/ National Digital Archives

"I spent many years - almost 30 - looking after the mathematical Olympiad," says the Professor.

to Cambridge and Uppsala. It's thanks to Prof. Sierpiński that I received the scholarship. You could pick no more than two locations; Cambridge was a renowned centre for number theory, while Professor Trygve Nagel was still lecturing in Uppsala, and I was fascinated by his work. But I ended up learning more in Cambridge, especially with Professor Harold Davenport. We also co-authored several research papers. Sadly, both Sierpiński and Davenport passed away in 1969. Sierpiński was an elderly man, but Davenport really left us before his time.

You were a fast mover: your first publication when you were 17, your PhD at 23, and you became a professor when you were just 37.

It only took me two years between getting my PhD and my habilitation, which I completed here at this institute in the autumn of 1962, a year after returning from my scholarship. Really, I have spent most of my career here. I've only promoted 7 doctoral students so far; that's not many considering I've been qualified to do so for 50 years. One of them, Professor Henryk Iwaniec, a member of PAS, is now a renowned mathematician. Unfortunately he moved to the US in 1983 and has lived there ever since. Iskander Aliev, originally from Russia, is also an outstanding scientist, but he also emigrated, and he's now a professor in Cardiff in Wales. Another former student, from Sweden, now specializes in applied mathematics rather than number theory. Marcin Zakarczemny was also an excellent student of mine; he just defended his thesis recently, getting a distinction. I really can't complain of a shortage of talented students, although here, at the Polish Academy of Sciences, we have far less contact with young people than at universities, and that's a shame. On the other hand, there is more time for research. I've only ever worked at a university once. In 1964, after my habilitation, I spent 9 months in Ohio, where I worked as a visiting assistant professor. There were surprisingly many of us working in number theory. I've also been commissioned to deliver lectures at the University of Warsaw and a few universities abroad.

But you've had other students, too - rather younger.

I spent many years - almost 30 - looking after the mathematical Olympiad. I was chairman of the main committee a few years, and for the following 7 years I was responsible for mathematics competitions between Austria and Poland. Education ministers of the two countries met up one day, and without consulting any mathematicians, came up with the idea for the competition. It went on to run for 29 years. It was always the second lineup of students that participated: the top six winners of the Polish mathematical Olympiad were sent to the international competition, the next six to the Austria-Poland competition. The event has since been replaced by the Middle European Mathematical Olympiad. During the first Austria-Poland competition, we introduced team contests alongside individual competitions. The whole team could work together on solving the set problem. We set the two teams three tasks, which were of a similar difficulty to those in the individual competition. This proved to be a terrible mistake; both teams solved all problems very well. We had no idea who should get the prize, and there was only one: a crystal cup with a plaque. We couldn't exactly cut it in half. A few months later, we decided that the tasks set for the team competition must be much more difficult. The new system worked very well, and from the next competition we never had a problem in deciding the winner, because one team always stood out. There was even one year when both teams solved the geometry task incorrectly.

What about your own heroes? What are your recollections of Prof. Sierpiński - a great name in mathematics?

Prof. Sierpiński was always extremely supportive of me and all his students. His lectures could be very difficult; however, he specialized in unsolved problems, and here he could really close the gap between renowned professors and the students. In 1953 he was already over 70, but the rules *were different then. The department board could extend his contract for a year at first, and then for another three years, so Sierpiński remained a fully-active professor for 50 years. He was first commissioned at the University of Lwów by Emperor Franz Joseph, and finally closed his professional career when a statute changing the previous rules came into force in 1960.*

It can be said that you are continuing your family's traditions. Your father was a well-known doctor, your mother an artist.

I spent my early years in Sandomierz. I have close emotional ties to the town and still maintain close contacts there, although my family has all left. My remaining relatives have all moved to different cities. The school I attended still exists; it's one of the oldest schools in Poland, founded in 1602. Every 5 years it holds a reunion, and I'll be attending the event in early October this year. Sandomierz is very beautiful, although it isn't a major town. Today there are just 25,000 inhabitants, and while I was growing up there, there were just 10,000 people living there. As far back I can remember, my father always worked as an ophthalmologist. He originally trained as a general practitioner, but he changed qualifications before I was born. He ran a private practice before the war, although he was always very socially-minded. After the war he worked in the national health service. My mother didn't work; she was a painter, mainly of landscapes and portraits. She held exhibitions before the war, and also afterwards, within my memory. In 1966, my parents moved to Warsaw, and my father retired. We lived together until my father passed away in 1974. Mother survived him by many years. Yes; it's probably true to say that she passed her artistic talent onto me. Mathematics is a very intellectual pursuit, although it also has a strong aesthetic component. For example, we frequently describe various proofs as being beautiful.

Was mathematics affected by the political situation in a similar way to natural sciences?

There was political pressure when I started university. This was between 1951-1955, when the political situation was especially unpleasant. I was witness to this, but I didn't experience it in the same way as Sierpiński. But my pet hate has always been and probably will be that we are always required to plan what we're going to do next year. That's the way things were under Communism, and that's the way they are now. In contrast to other technical fields, in mathematics what we are going to do largely depends on whether we have an idea, and what kind of an idea it is. And of course it's impossible to tell whether next year we'll have the right sort of idea or not. We can only plan what we have already thought of. Once you have an idea, you can plan how you're going to develop it - that's it. I've often pointed this out, and I'm sure it must be obvious to the decision-makers, but nothing ever changes. We should move away from this requirement for precise planning in basic science, but I doubt it will happen. We are always required to plan precisely what we're going to do, and then write a detailed report outlining how we achieved it. Of course now we have the additional problem of funding. How can we know whether what we're doing in basic science will turn out to be profitable or not? Does it automatically mean it's not worth doing, because we don't know? Nowadays everything is assessed by whether it will have a measurable outcome. Meanwhile, in mathematics, the time between formulating a theory and finding a practical application can be extremely long. For example, research conducted in Ancient Greece on second-degree curves was first used by Johannes Kepler in the 16th century. This means that almost two millennia passed between when the theory was first formulated and its first practical application. The process can be much shorter in other cases, but it is not unusual for it to be longer than a human lifetime. This seems difficult to grasp for people who have no real contact with science - for example politicians.

Interview by
Patrycja Dołowy
and **Agnieszka Pollo**,
Warsaw, July 2012

Prof. Andrzej Shinzel, ordinary member of the Polish Academy of Sciences, has been working at the PAS Institute of Mathematics since 1960. Between 1986-1989, he was deputy director of the Institute. Member of editorial committees of journals including *Mathematica Slovaca, Acta Arithmetica, Lithuanian Mathematical Journal*, and *Bulletin of the Polish Academy of Sciences - Mathematics*. Author of approximately 300 scientific publications and two books. Active member of the Polish Academy of Arts and Sciences (PAU), honorary member of the Hungarian Academy of Sciences, Polish Mathematical Society and the Sandomierz Scientific Society, ordinary member of the Warsaw Scientific Society, member of the German Academy of Sciences, and corresponding member of the Austrian Academy of Sciences. Winner of numerous prizes and awarded many medals, including the Polonia Restituta Order, the Pro Ecclesia et Pontifice medal, and the medal of the Commission of National Education. In 1951, as a 14-year-old high-school student, he received the 1st prize in the mathematical Olympiad. Between 1969-1999, member of the Main Committee of the Mathematical Olympiad.