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Interview Kenneth Ribet, winner Brouwer Medal 2017

"No one had ever accused me of proving a theorem before"

On April 12th, 2017, Gunther Cornelissen and Jan van Neerven interviewed Ken Ribet at the occasion of him winning the Brouwer Medal 2017.

Professor Ribet, congratulations on winning the Brouwer Medal! We would like to ask you some questions about your scientific career and endavours.

High school: the 'maximum learning programme'

How was your high school education? You went to the same high school as Feynman (and Madoff, for that matter): the no-longer-existing Far Rockaway High School.

"I grew up in a part of New York City that is pretty far away from the centre. Students in New York City who were very oriented towards mathematics or science tended to apply to specialised high schools, like the Bronx High School of Science, Stuyvesant High School, or Brooklyn Tech. I didn't want to spend hours every day on the bus or the subway, so there was one local high school, and that was my high school.

At that time, the educational system had not yet developed this idea that lots of resources should be spent on bringing everyone up to the same level, but instead, they would identify specially talented students and put them in exceptionally strong classes. I was in a programme that, believe it or not, was called the 'maximum learning programme'. The students in this programme went from class to class together (this was called 'block programming'), we got to know each other very well and we had among the best teachers in the high school, so I had a very good preparation in mathematics, science, English and social studies. Foreign languages were not block programmed because some people took French and some people took Spanish, but all of the core courses were block programmed."


Photo: Gil Cavalcanti

Ken Ribet at the NMC 2017, holding the Brouwer Medal

Then was it already clear before you entered high school that you had a 'science-talent'?

"Well, the New York school system identified me as a 'smart person', but some of the students with me in the programme were talented in English, social studies or history. It was not focussed specifically on science."

How was the mathematics education?

"It was great. First of all, I learnt a lot from the teachers, but we also had a math team and I learnt a lot from the other students. There is one fellow who went on to do research mathematics, another became a specialist in mathematical problems and he published many books about that. We were always pushing each other to learn clever techniques to solve geometry and algebra problems. I learnt a little bit of calculus from these other students even before I had my first calculus course.”

Did you also take part in competitions, such as Math Olympiads?
“Math competitions weren’t so well organized in those days, but we did a few, and I was OK. I was not a big star but I would sometimes get an honourable mention.”

It is quite surprising that these kind of special schools already existed in those days. Nowadays it is rather common to offer special programmes to talented students.
“My high school had something like 3000–4000 students, and this programme was just for a very small number, something like 50 students per year.”

What were the typical topics and what was the level?
“Just the standard high school topics, but during the summer of 1964, before my last year in high school I was in a summer programme for science students at Brown University, which is actually the university I ended up attending and I was attracted to the university because I enjoyed that summer so much. We had three courses. One in computer programming where I learnt how to write programmes in original Fortran (typed into punch cards); another in calculus that was taught by an engineering professor, who had absolutely no interest in limit arguments and mathematical subtleties, but he taught us how to compute and it was great! That’s how you should learn calculus at first, through calculations, and I really liked that. Then there was a third course in material science — which coincidentally is the specialty of my older daughter — but in that course, I learnt absolutely nothing.

There was another thing that was somewhat special. We had a kind of ‘math talent class’, an additional math class, when I was in my last year of high school, where we did a lot of problems from a book from around 1891 called ‘Higher Algebra’ by Hall and Knight.[1] It was just full of tricky problems from the competitions in Cambridge University and I learnt a lot of strange mathematics; algebraic manipulations, discrete mathematics, all sorts of topics.”

In hindsight, has this benefited you?
“Oh, I am sure. I got very excited about math, and by the summer after I left high school, I really understood about epsilons and deltas for the first time, I found it really great. So I was full of enthusiasm and ready for university.

Godement used to say that in France, the students in the Lycée were just stuffed full of information and by the time they got to university, they knew quite a lot but they were very tired. In the USA, students learn much less, but when they get to college, they are really ready to learn. I think that was true for me. I was very excited about going to university, I was 17 years old and I was in great anticipation of what I would learn there from these professors, whom I considered to be humungous figures.”

But you enrolled in chemistry.
“That is not exactly correct. What happened is that I had a very strong high school chemistry teacher. When I was graduating from high school I thought: ‘You know, this chemistry is really great; maybe I will major in chemistry.’ And over the summer, when I learnt about epsilons and deltas, I completely changed my mind and decided that mathematics was going to be my subject. In fact, by the time I got to university, I decided to abandon chemistry completely.”

Your story on epsilons and deltas is quite interesting. What is your opinion on the big calculus books that nowadays precede a decent class in analysis?
“I think the American system is fairly reasonable, that students learn calculus and then they take a separate course in real analysis and they learn what really goes on in calculus. The reason the books are so thick is simply that the publishers want to be able to sell to the largest number of universities and each university has some necessary conditions that have to be satisfied by a calculus book, so it tries to satisfy all of them. So you get these big books, it is a big juggernaut, with supplementary material, online homework, guide for the instructor, guide for the student, solution manual. All of this has to be prepared, so the whole thing is a very big project.”

Undergraduate years at Brown University: interacting with Ireland and Rosen

Then you went to Brown University. How was that?
“Oh, I loved it. Brown — where my younger daughter is a student now — is the same now as it was back then: there is lots of research, but there is a big focus on the undergraduates; on giving them lots of attention and lots of time and opportunities to learn different things. The professors in the Brown Math Department liked me and were willing to spend lots of time with me. The first course I took was a year-long course in linear algebra and multivariable calculus, but it was an honours course, so the linear algebra was abstract linear algebra, which I learnt for the first time and which I thought was absolutely beautiful.

I also liked multivariable calculus. We had a book called *Modern Multidimensional Calculus* by Monroe[2] and I never exactly knew what was going on, but I liked the fact that there was actually a formal definition of what a differential was, as a linear form on a tangent space. The book was full

Short biography of Kenneth A. Ribet

Kenneth Alan Ribet was born on 28 June 1948 and grew up in New York, where he attended a selective programme at Far Rockaway High School, before entering Brown University to study mathematics. He received his PhD from Harvard, directed by John Tate, in 1973. After some postdoc years at Princeton, he moved to Berkeley, where he has been a professor ever since.

He published about 60 papers and had around 25 graduate students, and was an editor of many journals and book series, including the acclaimed *Springer Graduate Texts* series.

He is a member of the U.S. National Academy of Science, and has received the Fermat Prize in 1989, and, this year, the Brouwer Medal of the Royal Dutch Mathematical Society. Since 2017, he serves as President of the American Mathematical Society.

Photo: Gil Cavalcanti

Ken Ribet delivering the Brouwer lecture at NMC 2017

of Greek letters and the idea was that by thinking abstractly, you would gain more information. But I am not sure that really worked.”

Does the interest of Brown faculty on undergraduates mean more individual tutoring, or group tutorials?
“It just means more availability and less rushing to the next seminar or conference.”

What was your first encounter with number theory?
“In my second year I took my first abstract algebra course, which I also loved. By chance, just by walking in the math department, in the mail room I met a professor who started interrogating me. ‘What are you studying, what are you doing, what did you just learn?’ I said I was in this abstract algebra course, so he asked: ‘What is an example of a cyclic Galois group?’ So I said, you take an extension of finite fields, but he said: ‘No, no, I want number fields, characteristic zero.’ I think I might have come up with an example involving cyclotomic fields. This was my first encounter with Kenneth Ireland. He turned to me and said ‘Read this’ and handed me André Weil’s paper on number of solutions of equations over finite fields,[3] which was basically where he made the Weil conjectures. Except he was unwilling to say it in print, because he was worried that he might be wrong. He wrote this article and went around lecturing about it, and in public, orally, he would explain what he really had in mind, but not in the article. The article is completely explicit, calculating numbers, using Gauss sums and Jacobi sums. The article expresses each idea in several different ways. I could follow it all line by line. It seemed pretty interesting and I liked the fact that this Brown professor was interested in me. Then he gave me another paper, the sequel, called ‘Jacobi sums as Grössencharaktere’ by Weil.[4] I read it and sort of understood it. But then there was all this number theory, and I took a course in number theory and that got me interested in arithmetic.”

So you are not like Andrew Wiles, who in the famous BBC-documentary[5] states that even as little kid he already came across the Fermat Conjecture and he wanted to solve that.
“I was not motivated by and had absolutely no ambition to solve a great problem or to be a great mathematician, but my ambition, locally, was to be like the professors that I had in math classes. I really liked them, I respected them, I thought they were doing something worthy. This was the time of the Vietnam war, where a lot of people that I knew in business, like my parents’ friends, were doing things that were tangentially related to the war effort and they were seeking profit. And these people at university were more like monks, very serene and motivated by the love of knowledge. They seemed to me to be really people to emulate.”

Was this in particular true for the mathematics professors, or was it more general?
“Well, I liked the academic atmosphere in general, but the math professors were the

ones that I saw and they were very much unlike the adults I saw when I grew up, who were in business. My first professor, Frank Moore Stewart, who taught me linear algebra and multivariable calculus, looked like some farmer, but he was in pursuit of knowledge and he had spent a great deal of time with one of the historically black colleges in the South. Brown University had a partnership with Tougaloo College in Mississippi, which I had never heard of before I went to Brown. There was an exchange program, where a few Brown students who go to Mississippi and a few Mississippi students would go to Brown, and some faculty members would go to the other university. This guy had spent some time teaching at Mississippi and he was deeply committed to equality and pacifism. I was very impressed and I had never met anyone like that before coming to university."

Graduate years at Harvard: interacting with Tate and Serre; the Antwerp conference

Then you went to Harvard.

"The two professors that mentored me, Michael Rosen and Kenneth Ireland,[6] just told me what to do. 'This is your subject. You are going to study arithmetic geometry. Apply to these universities: Harvard, Princeton, Brandeis and Chicago.' So I applied to these four universities, and I guess they planned to write me very good letters of recommendation, and I got into all four. I reported back that I got these four envelopes back and they are offering me admission and they said to me: 'Go to Harvard and work with Tate', and that was it. So I said: 'OK.'

I showed up at Harvard in September 1969 and I knocked on John Tate's door and I said: 'I am Ken Ribet, your new student.' He seemed rather taken aback by this, but he never said anything negative, he said: 'OK.' "

Had you already done your qualifying exams?

"Oh, that was a real embarrassment. Namely, at the end of my first year at Harvard, there were written exams everyone had to take and this was at a time when the whole university was barely hanging together. There were people on strike, the USA was bombing Cambodia, people demonstrating in the streets and classes being closed down. There was a big anti-war effort and anti-authority sentiment. The exams were two sessions of three hours each, and I came into the exams, dashed off answers on paper and left after half an hour; two times. It is clear that my answers were lacking in detail. Apparently, the Harvard faculty wanted to fail me, because grading by the strict criteria, I didn't have a passing score, but Tate intervened and said that this guy is really good and you can see from his answers that he knows what is going on, so we'll pass him.

I spent the next two years taking all kinds of courses, from Tate and John Coates. It seems there was an endless supply of visitors: Swinnerton-Dyer gave a course on modular forms, and Birch would come sometimes, and Serre must have been there, although I don't remember that. I learnt a lot. Finally, I came into Tate's office in my third year, and I asked when to start research; I had no idea how to do it. Tate said 'What are you doing?' and I said I was in this course, and this course, et cetera, and he said: 'Maybe you should go to fewer courses and work on this problem.' He gave me some problem that involved the Birch–Swinnerton-Dyer conjecture and elliptic curves with complex multiplication. I couldn't figure out what he had in mind, it just didn't make any sense to me. I came back to him and said that I didn't like this problem. You imagine most advisors are barely willing to give a student one problem, but Tate just gave me another problem. He and I were reading a paper of Serre on the Galois action on division points of elliptic curves. We had a seminar where we were working through the manuscript line by line and taking turns giving lectures. I understood the paper pretty well, so Tate said: 'Why don't you do certain abelian varieties?' (later called 'of GL_2-type'), as an encore. I said that sounds like a good thing and I started working and learnt something by mimicking some arguments in Serre's book on Abelian ℓ-adic representations and elliptic curves.[7] I thought I was doing a computation, and I came to Tate and told him what I had computed and he said: 'Well, that's a very nice theorem.' No one had ever accused me of proving a theorem before. It was a transformative moment when I actually had been told that I had done some research, because I had no idea what research was or how you would go about doing it, so it was very empowering.

He told me to continue on, and I went to the Antwerp conference in 1972 where I met Serre.[8] I told Serre what I was working on and what I had done so far. But as you can see from the Serre–Tate correspondence, Serre already knew about me; Tate had written to him. 'This kid Ribet is coming to Antwerp; be nice to him, he is very shy.' When I met Serre he was standing on his head, demonstrating J. Frank Adams's method of drinking beer while standing on your head. He had a big crowd of admirers around him and I didn't know when I could possibly ask him about my thesis, so I decided to just interrupt him with: 'Excuse me, professor Serre, I want to ask you some questions about abelian varieties.' Everybody who was there started laughing. But Serre asked me who I was, and when he remembered who I was, he started talking to me. He told me how to continue my thesis, what to do, and how it would work out. I actually found this very depressing, I was working on something, working, working, working, and Serre, in thirty seconds could see the entire sweep of what I was doing and tell me what to do next — literally while standing on his head. It was in some sense very discouraging. What am I ever going to do in mathematics and will anybody care about me when there are these other people who can do the thing ten times better and thirty times faster."

Postdoc at Princeton, interacting with Katz and work with Deligne

"Nonetheless, I continued writing my thesis, handed it in, and graduated.[9] During the year when I wrote my thesis, which was my fourth year at Harvard, I really had the impression that no one would ever care about what I was writing and that I would get a job in some really obscure university, not in a major centre. When I applied for jobs, I wrote to about sixty universities, hoping that one of them would pay some attention to me. It was really quite a shock that in January of the year I was writing my thesis, very early in the hiring cycle, I got a phone call from Princeton offering me a job. It was the craziest thing that ever happened to me. Like everybody who came to Princeton as a postdoc — at the time my title was 'Lecturer', the phrase 'Postdoc' was not used yet in mathematics — I was terrified that the senior faculty would find out that they hired someone who was completely stupid. Everybody had that fear. I showed up and my mentor was Nick Katz, who was wonderful and very supportive. He asked me some question that I was able to answer during my first year, and this is how I got into this collaboration with Deligne about totally real fields."

What was it like to work with Deligne?
"I did not really work with Deligne. Katz told me you had to prove the irreducibility of some moduli space in characteristic p and Katz had already found in his Antwerp article that this amounted to the surjectivity of some Galois representation, which in the case of elliptic curves was proved by looking at the image of some inertia group around supersingular points; a method that did not work for higher-dimensional abelian varieties. Katz just asked me a very narrow question about some Galois representation, where he translated all the difficulties into one place where I was supposed to be knowledgeable, and asked: 'How do you prove it has a big image?' I said, the way we usually do this is using Frobenius elements, and I made some argument using Honda-Tate theory showing that the Frobenius elements fill the thing up, so I solved the problem and did it literally overnight. Katz said 'Holy cow, this is the missing piece!' and asked me to write to Deligne, who replied that now we could write a joint paper. He already had a manuscript, a letter to Serre, outlining what to do, but with this missing piece. Working with Deligne meant that I had to understand this manuscript. So I went to Paris and spent the first year there (1975–1976) at the IHES and mostly during that year I tried to understand the manuscript. I saw Deligne every day but I didn't talk to him all that often, since I didn't have all that much to say. My main contribution to that paper was the irreducibility in characteristic p, and then there was another point, which had to do with powers of 2. The idea was that the p-adic L-function was integral (a p-adic power series), except that when $p = 2$ and you look at the Eisenstein series, the constant term is an L-value divided by 2^n where n is the degree. It would be very natural to think that L-values for the 2-adic L-function were divisible by 2^n, but after appropriate symmetrisation, Deligne could prove in complete generality only that they were divisible by 2^{n-1} and had some general situations where he knew they were divisible by 2^n and a whole class where he couldn't decide. He did one calculation for one totally real quadratic field, where he saw it wasn't divisible by 2^n (by 4 in that case) and he wondered what happened in general. I did a few more examples and gave a lecture about it at Orsay. Hendrik Lenstra immediately saw what the answer was supposed to be. Getting that hint from Lenstra, I worked and worked and worked and compared Eisenstein series and theta series and actually proved that the surmise of Lenstra was correct. So my second significant contribution to that joint paper had to do with powers of 2 in the 2-adic L-function. So I did two different things for that article."

Antwerp again, and Grothendieck

The Antwerp conference was organised by Willem Kuyk, the mathematical great-grandfather of one of us, a Dutch mathematician that had gone to Belgium to found the mathematics department at Antwerp. On the web page of William Stein, we found an interesting conference picture[10]*, in which you identified many participants and to which you contributed a little note about the alcohol consumption at that conference.*
"There was wine served at lunch and dinner. I often sat with Bombieri who used the following ploy: as we sat down, we would pour out the first bottle; so we had an empty bottle and we got a second bottle, and then that would get you started. There was also a lot of drinking in the evening after dinner, with beer and wine and ping-pong. The food and wine at that conference were just excellent."

This gregariousness seems very much in line with the reputation of the algebra group at Antwerp.
"At the time I was still a graduate student. What I also remember from the conference was that after every talk, students would gather around Nick Katz in front of the building. He would explain to them what the lecture was really about. The most elementary lectures I would understand but the more advanced things required some help. Listening to Katz was just great."

Was this the conference where Grothendieck refused to attend because of the conference being funded by NATO?
"Somehow I managed to miss the first day of the conference by mis-timing some trains. I came to Europe a week or two before the conference and travelled with some friends, and the morning the conference began I was in Switzerland.

At the first day proceedings, Grothendieck was making a big stink, protesting and interrupting. The second day, when I came, Grothendieck was sitting in front of the building where the lectures were held, with a huge bouquet of helium filled balloons. I knew who Grothendieck was because he came to Harvard when I was

Conference photo from the Antwerp conference, 1972

Photo: http://wstein.org/antwerp_photo

a graduate student, but I don't think he recognised me. So the only direct conversation I ever had with Grothendieck was that I went up to him and asked him: 'Excuse me, why are you sitting here with balloons?' He looked at me and said: 'Because they are nice.' That was the only sentence he ever spoke directly to me.

No, I take it back. He spoke other sentences to me, because when I was at Harvard, he was already on his 'Survival' kick. When visiting universities, he would lecture in mathematics only if he was allowed to give a general lecture about the evils of doing mathematics. I went to both his algebraic geometry lecture, which was on the definition of étale cohomology, and in the evening to his public lecture, as did all math graduate students, and he was going on that we should go till the soil and not waste our time doing mathematics. I asked: 'Excuse me, I have spent three years now learning higher mathematics, including arithmetical algebraic geometry. I think I am pretty good at it and that I would be able to make a contribution, whereas if I go and till the soil as a gardener, I won't be any better than any other person, probably a lot worse. What do you say to that?' He gave an answer, but speaking to the public. I don't think his answer was all that satisfactory."

Reaching out to young people

The NAW would like to reach more young people. As President of the AMS, which suggestions do you have?

"The new editor of the *Notices of the AMS*, Frank Morgan, has devoted a tremendous amount of energy to the journal. He has worked to make the articles shorter and more visually appealing. In particular, articles tend to have short bibliographies — or none at all. He has worked to move routine material out of the *Notices* on the grounds that it can be found easily online. Frank has edited many of the submissions quite heavily; the practice of editing submissions is common in the literary world but rare in the mathematical community.

When I was a student, graduate students would be members for free; graduate schools would nominate them. Then it was very natural to continue once they had a real job. The real benefit was that I would get my own copies of the *Notices* and the *Bulletin*. I could take them home and read them. Nowadays the University is subscribed to these publications as institutional members. That means that when you are at the university, you can read all these publications on your laptop or tablet. A benefit of electronic copies is that you can do searches. Young people are less and less married to physical copies. You can decide not to be a member and still have the electronic versions available through the university. So basically the problem which the AMS is attempting to counter is the attitude that young people might have of 'what's in it for me?' When I was a young faculty member I joined the society because everyone did, it was the thing to do. When I came to Berkeley I also joined the Faculty Club. Once you pay dues to become a member of the club, you get a discount on you purchases. But many will say that it is not worth it. Mathematicians are very good at calculating the benefits!"

What are the main challenges that the AMS is facing?

"Many challenges have been identified in the strategic plan, but an obvious one is to make sure that its electronic products are still relevant in an age when Google does everything for free. For instance MathSciNet, the online review service for mathematical research papers, is constantly adding more capabilities. Certainly it is very important to the Society to make sure that MathSciNet stays ahead. People often don't realise everything the Society is offering, such as the remote access button in MathSciNet which is unknown to most users. We do regular surveys among our members who then request all sorts of features that we already have."

You have said elsewhere that the importance of printed journals is declining.

"Fields Medallist Timothy Gowers will tell you that the future of publishing in journals is overlay journals: you publish on arXiv and all the journal does is to anoint certain of these articles. That's it, very little work needs to be done. An argument can be made that commercial publishers, including one in this country, are exploiting the mathematical community by extracting a huge profit from a product that is basically powered by free labour from the mathematical community. Overlay journals are one extreme, commercial publishers the other, and many people think that the Societies are a good compromise: they are non-profit organisations and they are really serving the mathematical community. The AMS has a Gold Open Access version of its Transactions and Proceedings journals and charges $2000 per article. That is what it actually costs to take an article and process it. There are all sorts of arguments at to whether or not these are real costs. The AMS makes money on some of its publishing activities, but this money is turned back to the mathematical community through the many services that the AMS provides to the profession. At the end of every year the balance should be even."

Three theorems of Ken Ribet

A central role in Ribet's work is played by so-called 'Galois representations', a way to look at extensions of number fields through the eyes of representation theorists, connecting them to algebraic geometry through automorphic forms and Shimura varieties.

Ribet rose to fame when he published what is now known as the 'Herbrand–Ribet' theorem [11], a result on class groups of cyclotomic fields that was spectacular not just as a statement, but also from the point of view of the *methods* that he used; a whole new generation of number theorists copied successfully his algebro-geometric ideas to solve purely number theoretical problems, and the paper made a lasting impact on the field of Iwasawa theory.

Ribet proved the ε-conjecture [12], effectively implying that, through a method of Frey, proving the Taniyama–Shimura conjecture would solve Fermat's Last Theorem (which Andrew Wiles subsequently did).

In a famous work with Deligne [13], he showed congruences between special values of L-series of real quadratic fields (known to be rational by work of Siegel), implying the existence of p-adic L-functions, which seemed very inaccessible at that time. Again, the method of Deligne and Ribet changed the landscape on how to approach such questions: suddenly, analytic statements were approached through modern algebraic geometry and a very detailed study of moduli spaces.

New developments in number theory

Let's talk about recent developments in the field. Is there something that gets you really excited? If you would have two years to study only something mathematical, what would it be?
"Well, it would clearly be perfectoid spaces." [14]

I actually wrote that down as next subject! Two of our Utrecht students attended the Arizona Winter School about that, I guess you were not there?
"No, but their inventor Peter Scholze was in Berkeley for a semester fairly recently and he gave a course. I have tried to follow the beginning of the course and I was just completely blown away by (1) the beauty of the subject and (2) the amazing command of an entire landscape that he has. He was lecturing to many people who were experts in different corners, like (ϕ,Γ) -modules and different ways of expressing Galois representations in somewhat concrete terms and Scholze understands all of this in his new framework in some illuminating way that completely surprised the original perpetrators of the subject, who were sitting in the audience shaking their heads. Obviously, as you see from the joint work of Scholze and Jared Weinstein,[15] when you take modular curves and you add lots of p-power level, in the end you are dealing with a perfectoid space, and these recent developments where you associate Galois representations to mod-p cohomology classes are just mind-blowing. Certainly, if I were starting out and could sit for two years and think about nothing but learning a new subject, that would have to be it."

Then being president of the AMS is not the best strategy, is it? Do you still have time for research?
"You know, I have done lots of different things in my life and the AMS is very interesting, learning about the society and anticipating in its activities and this is some way in which I grow, even if it is not learning perfectoid spaces. One of the things with being president is that I teach only half as much, and even when I am teaching and I have to go away for an AMS activity, a graduate student is there ready to replace me; this is part of the agreement between my university and the AMS."

Chances are that you will meet the President of the United States?
"Every year there is a ceremony for the National Medals of Science. Presumably I get invited if a mathematician wins. If I would find myself in a room with Donald Trump, I would try to have an affable conversation without much substance. I would tell him that I admire his Twitter style and would ask him if he is having a good time. I probably wouldn't have more than three minutes to talk to him!"

Professor Ribet, thank you very much for the interview!

During the interview, we asked Ken Ribet to describe how the AMS reaches out to young people (a continuing point of interest, also for KWG and NAW), and this is the answer that the executive director of the AMS, Dr. Catherine Roberts, later provided by email:

"There are many ways that the American Mathematical Society supports young people interested in Mathematics. Frank Morgan has devoted a tremendous amount of energy in transforming the *Notices of the AMS*. I think his attention to short, interesting articles with lots of illustrations and his addition of 'Lecture Samplers' and the 'Graduate Student Section' make this publication, freely available online, more interesting and accessible to students. Our members have established the Epsilon Fund, which supports many math programs and summer math camps for high school students. We provide travel funds to students interested in attending our conferences. Last year, at the Joint Mathematical Meeting, we ran Mathemati-Con jointly with the Mathematical Association of America. This event was free and open to families and celebrated mathematics with engaging presentations as well as the national finals of our popular quiz program, *Who Wants to be a Mathematician?* These are just a few of the programs I can think of that might be of interest to young people, and I encourage those interested in mathematics to explore our website at www.ams.org."

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