Interview with Nobel Laureate Prof. Robert Aumann

By Dragos Ailoae and Emanuel Agu

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This is the continuation of our recent series where we talk with economists and other social scientists about their recent work and their research experience. In this second installment, Dragos Ailoae and Emanuel Agu, PhD students in Economics at The Graduate Center, interview Nobel laureate and distinguished alumnus Robert Aumann who joined the Applied Economics Seminar on September 7 to present his latest paper "A Synthesis of Behavioral and Mainstream Economics."

The three discuss Prof. Aumann's rule-rationality decision-making paradigm, his experience attending City College, his love for New York City and for pure math, as well as his journey from topology and knot theory to game theory and behavioral economics.

Dragos Ailoae - Thank you for taking the time to speak with us. We are curious to learn about the intellectual journey that brought you to your latest paper and, while reflecting on your experiences, we hope you can impart some advice for us hopeful PhD students.

I'd like to proceed roughly chronologically. Your family immigrated to New York City when you were about 7 years old, so you spent some of your most formative years here. What is your relationship with the city, and do you consider yourself a New Yorker?

Robert Aumann – Oh, yes, absolutely! I do consider myself a New Yorker. I came to New York when I was eight, and the first half year I spent at Public School 87 on the Upper West Side, which is still considered one of the best public schools in the United States.

My parents were adamant that my brother and I get good Jewish as well as secular educations, so we moved to the Borough Park neighborhood in Brooklyn, as there were no established Jewish schools in Manhattan. Borough Park at that time was about 10% Italian and 90% Jewish, including some Reform and some Conservative Jews; now it is predominantly Hassidic. We attended an English and Hebrew-speaking elementary school there, and then I went to the Rabbi Jacob Joseph High School on the lower East Side.

But, back to your question: do I consider myself a New Yorker? Absolutely! I have a lot of love for New York. It is a beautiful city, an amazing, throbbing place. I do like it very much and have returned to it often in the intervening years, though I have lived in Israel since 1956.

DA – I must say, you still have a New York accent.

[Laughter] You know, speaking of accents... this is a wonderful story. After completing the doctorate, I did a post-doc at Princeton. One day I was hitchhiking from New Jersey to New York. A car stops, and

the driver says: —“Young man, where are you going?” So I say “I'm going to the city” or something like that.

And he replies: “Southern Germany via Brooklyn!”

He got it exactly right after just a few words! He must have been an expert in accents. “Southern Germany via Brooklyn!” I still remember that.

DA - You are, of course, one of the most distinguished alumni of this school. You graduated from City College in 1950, the second graduate of that decade to receive the Nobel Prize in economics, the other being Kenneth Arrow. Can you talk about your experience attending City College? Did you and Professor Arrow ever cross paths?

RA - I knew Ken Arrow very well, we were good friends. I met him in the 1960s. Every summer from 1970 to 1990, for two months, I joined an economic theory workshop at the Institute for Mathematical Studies in the Social Sciences at Stanford, where Ken was teaching. The workshop was run by Mordecai Kurz; he did a great job. There I got to know Arrow very well; he was a wonderful person. He also visited the Hebrew University, where he was the first director of an annual two-week summer school in economic theory, now directed by his student Eric Maskin.

Why did I go to City College? Initially, because it was free. My parents lost all their money in the move to America, since they were not allowed to take any capital out of Nazi Germany. Here they worked very hard; my brother and I also worked. While at City College, I held down a job in a place that manufactured costume jewelry, running errands and things like that. So, what attracted me to "City" initially was the zero cost. But the math department turned out to be wonderful.

The only research mathematician in that department was Emil Post, a World War I veteran who had lost an arm in the war. A prominent mathematician, he worked in logic, set theory and computability, like Church and Turing. The other professors were beautiful people and good mathematicians, so we got a very good mathematical education there.

I still remember, there was a table in the corner of the very large cafeteria where the better math students ate ice cream sodas, talked math, and played chess. We used to play for nickels. Five cents was money in those days!

DA – In graduate school, at MIT, you studied pure mathematics, but you received the Nobel Prize for your applied work in game theory. For those of us trying to find our path in scientific research, can you talk about how you see the theoretical vs. applied split today?

RA - Applied mathematical research was, in the middle of the last century, looked down upon. Pure math was the name of the game. People did study applied mathematics, mainly stuff like the differential equations of hydrodynamics, that kind of thing. At MIT, at least, nobody in math was doing operations research, and game theory was considered part of operations research. There was very little applied mathematics when I went to school.

I did my thesis in algebraic topology, knot theory. As an undergraduate at City College, I had read a lot of number theory: prime numbers, sums of squares, sums of cubes, etc. I liked number theory, it was the

purest of pure math: it is simple to understand the problems but very difficult to solve them. And, the most important thing: it was absolutely, absolutely useless! That was a hallmark of good math at that time: being useless.

A famous number theorist in the first half of the twentieth century, Godfrey Hardy, once did some work in mathematical genetics. One of his formulas became famous and to this day it's called the "Hardy formula" in mathematical genetics. Late in life, he was asked whether he had any regrets, to which he answered: "Indeed, I regret proving this theorem in mathematical genetics. It is useful, so I'm sorry to have done it."

The uselessness vogue also swept me up at MIT, which is one reason that I studied knot theory. Like number theory, knot theory problems are easy to explain: we are talking about knots, the kind that you tie in ropes, so the problems are very natural, even more so than in number theory. And like in number theory, it is difficult to prove anything at all in knot theory. In fact, in elementary number theory you can prove a few things without too much trouble; but in knot theory you cannot prove anything without very deep methods. Finally, like number theory, knot theory was absolutely useless.

Since knot theory had all the advantages, I decided to do my PhD thesis on the subject. I succeeded, and then went for a post-doc to Princeton, where I joined an operations research consulting outfit attached to the Princeton math department. There, I was assigned a project that I realized had to do with game theory. I forgot to mention that, when at MIT, I had gotten to know John Nash. He had just done a thesis on game theory at Princeton. He explained game theory to me, but at the time it didn't interest me very much. I was much more into knot theory and other branches of pure math.

The project I was assigned originated with Bell Telephone Laboratories. In developing a surface to air missile, they had encountered a problem: what do you do when you're attacked by a squadron of aircraft, some of which are carrying nuclear bombs, but most of which are not? This problem would not arise today, because there are lots of nuclear bombs; but at that time there were very few.

When assigned that project, I immediately realized that it had to do with game theory. And I read up on it and recalled the conversations with Nash and wrote a report. And then game theory began to interest me for its own sake.

Certainly, applied work is more respected today. At my institution, the Hebrew University, it had been respected from the beginning. When I arrived at the university in 1956, I was not looked down upon, though applied mathematics was unfashionable at most mathematics departments in the world.

DA - In 1991, you helped found the Center for the Study of Rationality at the Hebrew University, to explore the rational basis of decision-making in a multi-disciplinary setting. How important was the proximity to experts in psychology, economics, biology in shaping the rule-rationality paradigm for decision making that you introduced in your latest paper?

RA - I do think that the center had a big influence on it. One of the leading members of the center is Maya Bar-Hillel, a prominent psychologist very active in behavioral economics. She is a good friend of Danny Kahneman, was a good friend of the late Amos Tversky, and I think she's a good friend of Dick Thaler and other behavioral economists. I learned a lot about behavioral economics from her, though I'm pretty sure she doesn't agree with me.

What I hope to have gotten across in my presentation is that I'm not critical of behavioral economics. I *am* critical of the emphasis that behavioral economists put on the negative, the exceptional instances where behavior does not jibe with mainstream economics. You know, there used to be a song in the 1940s and 1950s when I was growing up in New York: "Accentuate the positive." The behaviorists accentuate the negative.

But I believe that it is important to understand behavioral economics. My understanding of it comes to a large extent from Maya.

Also, Avi Shmida and the other biologists at the Center greatly influenced the paper. Because of them, evolution – which is fundamental in the paper – has a palpable presence at the Center.

DA - Your paper posits that the rules of the rule-rationality framework are the product of evolutionary processes and that decision makers do not choose the rule. Would you say then that this process happens in what Kahneman/Tversky call "system 1" reasoning?

RA – Absolutely, that's exactly right. System 2 is slow thinking, so in that case we don't need the rules. System 1 is "thinking fast;" the rules, or "heuristics" and "biases" as they call them, are the product of evolution.

Getting back to my earlier point about the behaviorists, they say just what I say. They compare the negative results of behavioral economics to optical illusions. Optical illusions are a big thing in psychology: You put the arrows in one direction and you say that this line interval is shorter than that one when in fact they're exactly the same length; that kind of thing. They compare the mistakes caused by the biases and heuristics of behavioral economics to optical illusions.

That's exactly the point I want to make: we don't bump into things when we travel around, we don't see incorrectly, usually. Of course, we can make mistakes if we purposely contrive an optical illusion. And it's the same thing with behavioral economics. They themselves point this out, but somehow they get carried away by their own research, and only want to prove that sometimes people get things wrong in system 1. They want to prove that, they prove it, and they're right. But in general – and that is my contribution – in general, the system 1 responses are good.

DA - Where would you like to take this research next?

RA - I would like simply to continue studying the biases. If you Google "heuristics and biases", you get a list with some 100 items. I'd like to study each one. In each one that I studied up to now, the rule is good in general, but not in contrived situations. We already have a nice list of 18 or 19, which we covered in the seminar slides. I'd like to continue this research and am asking that people join me in this endeavor.

Maybe we'll find some cases that don't confirm my theory; heuristics that contradict mainstream economics not only in exceptional or contrived scenarios, but also in general. This, after all, is scientific research; it's possible that some heuristics will not adhere to my paradigm. I want to tackle them all.

In the seminar, I mentioned the paper of Yuval Heller and Eyal Winter on rule rationality, which shows that there's room for theorizing, for formalizing the idea of rule rationality. But first I want to do the basic lab work. That's what interests me right now, and I hope that some of you guys will join me.

DA - Any other advice for us PhD students?

RA - Herewith some advice for young people in general: Do what you like. Because when you like it, you're good at it, and when you're good at it, you like it. It's almost a necessary and sufficient condition. Don't do what you think is practical, what you think will further your career. The proximate cause is that you like it and then, ultimately, you'll be good at it.

So, if something attracts you, intellectually or otherwise: go for it!

DA - Thank you for your time professor. We look forward to seeing you again in our seminars.

RA – Thank you very much, my pleasure.