Anthony Comegna:

Truth time, everyone. I really don't know what an algorithm is. Sure, the word is batted around a ton these days. We've all heard it. People complaining about Facebook or Twitter shares, shadow banning and the like. But how often, do we of little knowledge, slow down and try to really understand the basics like this? This week and next, the newly minted doctor and professor, Abigail Devereaux, joins us for the tour of the intersections of economics and computer science, ranging from the very simple to the intensely complex.

All right. Well, it's really great to have you on the show, especially given that you only very recently finished your PhD at George Mason. You're one of our very recent graduates here and you and I actually first met back in February at the Artificial Intelligence Discussion Colloquium that we hosted with James Caton, as director or a discussion leader. And we covered that on the show here too. So really, let's start just by catching up a little bit. Tell me about your defense about your dissertation and what your career is looking like now. And I guess, give me whatever your thoughts are about how life has been since all hell has broken loose here on earth.

Abigail Devereaux:

Oh boy, that could be its own podcast. Well, thank you so much for having me on the show, Anthony. I'm really thrilled to be here. That was a wonderful colloquium that we had. I hope we have another one like that in the future, and I would also be happy to host. So something like that. So let's see. Well, the fact that my dissertation during COVID wasn't exactly the way I thought I would go out in a flame of glory, but it was interesting and fun and there was only a couple little technical glitches with it. So the actual technical part of defending one's dissertation during virtually was okay. I had plenty of time to talk. And since I was in the comfort of my own home, I felt a little less stressed, I suppose. I didn't have to provide any snacks to the professors, which I don't know, was a thing that you do, right? So they brought their own snacks.

Anthony Comegna:

Is that a normal practice? I was not aware of that.

Abigail Devereaux:

I don't know if it's normal practice at GMU, but my wife told me that that's what she did when she got one of her doctorates of the two, she had a whole spread laid out. Maybe it's a French thing. I don't know. But I went into that fully expecting to buy snacks and bring them in. But I didn't have to do that. So hey, benefits of COVID right?

Anthony Comegna:

Well apparently, my committee missed out too, because I never need that.

Abigail Devereaux:

I had a great committee. Dick Wagner was my chair. He is brilliant. He writes a book a year. He publishes a good two to three papers on top of that per year, as well as the large amount of service that he does. He still teaches classes, of course. And they're always wonderful and varied and usually based on the books that he's writing at the time. He and I are actually thinking of writing a book about uncertainty, updating uncertainty, since from it's Knightian roots, in the modern era, given our algorithmic way of approaching economics, what do we have to say about uncertainty? So we may be working on that

project together. We'll see. And so now that I've graduated, I didn't have a ceremony which was sad, but I hope someday GMU has something for us, which would be great.

Maybe a combined ceremony. Someday, we will get together and see each other again. But since then, there's the pressure of course, to start publishing. And it's not just about publication, it's about tenure. So I was lucky enough to be hired before the hiring freezes started in COVID. And I feel a little bit anxious for this year's job market. Folks I know, there's many incredible people going on the job market this year, particularly from GMU who I know very well. And it might be a little bit thinner out there in terms of getting positions, but hang in there. I was lucky enough to be hired before the freeze and I have a tenure track position at Wichita State University who has started a new institute called the Institute for the Study of Economic Growth. I am a research fellow there.

So I have a double component, a dual component to my professorship here, my appointment here, which is my activities with the institute and my activities as a professor. So there's two kinds of service, two kinds of publications. The one thing that you do, of course, when you're on tenure track is it's not just about publishing at a high volume. That's something I think I could do without much trouble. I'm a prolific writer. It's more like publishing well. And by publishing well, it's more like having a conversation with mainstream economists or perhaps not even a conversation, but capitulating in some sense to their methodology in order to get published in their journals. And so it's been interesting. I've been dealing with that tension in my own work. I am a theorist and an age where theory is supposedly dead, but an age in which I think theory is as alive and open as it's ever been in social science.

So it's an interesting tension to have to play to the old theoretical ways while trying to blaze a path in a new theoretical frontier. So that's been my experience so far. It's just my first day, I just taught my first class, economies in transition, about economies that attempted socialist or communist experiments and to how that went, the theory behind central planning and socialist calculation, as well as how they're going currently. So I talk about the USSR, I talk about India, I talk about China. And I just had my first class and the students were really plugged in and it was just great. And I love teaching. So I'm happy. I've had a good day. Thanks, Anthony.

Anthony Comegna:

Well, boy, congratulations on getting a tenure track position right out of graduate school like that. That's absolutely fantastic. That's a really major accomplishment. I'm very happy to hear it.

Abigail Devereaux:

Thank you.

Anthony Comegna:

And now, you mentioned some things I need to be sure to follow up on there, because it's just so interesting. And we have grad students in the audience who might be very eager to hear this thing. By the way, it should go mention, even though we're going to talk a lot about algorithms and computer science and theory, you're an economist and that is probably the largest cohort of students we have. So just so everybody's aware, that's the context we're coming from here. Now you mentioned having to do a certain amount of capitulation in order to, as you put it, publish well. And I think we're all at least a little bit familiar with what you might mean, but can you tell us a little bit about what your decision making process is about what you'll capitulate on, in what ways and how far you might be willing to go to get a better publication record versus staying more theoretically sound, as I know that you're very passionate about doing?

Abigail Devereaux:

Right. So this is an interesting tension. So my approach, I suppose, is one of chipping away. So there's this idea that sculptures have that sometimes you look at a piece of raw material, there is a form inside the material and that you're chipping away at that material from various angles. And you may find different kinds of difficulties if you run and not on one angle and it might be two different kinds of materials. It might be partially wood that's fused with a resin and you have to approach it different ways. But to me, the idea is that there is a form that I'm chipping away at, from I'd say the top and the bottom. And that by the top, I mean, I want to create a bridge between, to mix my metaphors completely, between the mainstream conversation, mainstream ideas and the sort of new theoretical path I believe I am blazing.

So I'm chipping away as in, I am a challenge small parts of the infrastructure of mainstream economics in order to have that conversation at the more mainstream theoretical journal level. That is me more chipping away at small parts of the assumption. So I have a growth theory paper that I'm working on with Roger Koppl and Stu Kaufman. And Stu Kaufman's one of the founding members of the Santa Fe Institute, which is a complex system science institute out in Santa Fe, New Mexico. And Roger Koppl, we all know in this audience fairly well, but he's a brilliant guy known for big player theory, and as of late his, expert failure theory. And we're making this growth theory paper where we assume a traditional growth theory model. And for those who don't know in the audience, you have some factors of production, like labor and capital, and then those interact with some technology. And then you have that interact in a certain way, functionally, mathematically to produce output at the end of the day.

And so what we're doing is we're replacing the traditional technology term with one that is a combinatorial process. So this is the idea that new things come into the system through recombination of old things, and through true novelty. So you have an expansion of new things into the system, and you have the pulling in of new elements that can then be combined and recombined with previous elements in the system. And that you do that at each step. You pull in a few new elements, you recombine a whole bunch of old elements to new processes or new things in the system. Think of these as ideas, ideas that have linked different stages of say production together, or maybe you have a phone with different components and you recombine those components, and it turns into a smart speaker. But really you just started with a phone.

You just recombined the components of a phone, its screen, its speaker, its logic board, and you took it apart and you built something else from it. That's a very traditional idea we have in our minds of tinkering of technological progress or tinkering. But we combine this idea that there's new novel things being brought into the system. And it's actually a simple, combinatorial process. So by combinatorial it's just that's just a mathematical function with some interesting, one gamma function divided by another. And those are those exponentials that you multiplied together, like a KOOS or choice function. And then a certain probability that a new thing enters the system or not. And that's a parameter, and that could stand in for institutional permissiveness. For instance, we could have a higher probability that a new combination enters the system and grows the technological capability of the system and therefore the GDP or whatever the output is, however you want to represent that.

So we have ways in which we can parameterize like say institutional environments and that sort of thing. And so what we've done is just made a very simple change. We've just replaced. We've hollowed out and existing template. It's been around since really like the fifties and sixties. We've hollowed that out and we've stuck in a new combinatorial function and the technology part. So that's the way of thinking about what we want to do is represent movement into the adjacent possible. The adjacent possible is that part of possibility space of choice space, that's not yet realized, that can only be imagined. So it does not yet exist, that has, contains things that are truly novel that are unknown

unknowns. We have that element of moving into the adjacent possible, and also this idea of recombination within sort of a soup of existing combinatorial possibilities. And also that you can't realize all of those possibilities at once.

So there's a constraint on knowledge, an almost accosting of knowledge of moving through in this system of combining these elements. It's built into the model, just using this, this new function to replace the technology part of the growth of the growth function. And so that's the way that I talk about building a bridge. Another thing that I'm doing is I'm changing or I'm modifying traditional game theory and creating something that cannot be reduced to a multistage games or repeated games or evolutionary games as we understand them. I have a new game theory called synecological systems theories, synecological game theory, that I can prove that this game theory, it's distinct from a multistage repeated an evolutionary games, as a distinct new kind of game. And so what I'm doing here, and in fact, one of my professors on my dissertation committee asked me, because this was the focus of my dissertation.

He asked me, "Why bother putting this in terms of game theory?" He's like, "This is actually a very general theory that you could make and just say generalize this in an algorithmic way, and then use agent-based computation economics as your main framework." It's because I want to build that bridge to traditional economics. So I build it to sort of the mechanism design folks who are doing that algorithmic game theory right now, and to traditional game theoretical analyses that are more simplified. And I try to build that bridge from what I'm doing to them by explicitly using, utilizing the theoretical concepts and the mathematics that have been employed for traditional game theory and just expanding them a bit so that I can contain the theory that allows for superposition of states. And synecological game theory allows for indirect interaction and entanglement and a sense for which the whole of the system is greater than its parts.

This is something I've been able to do. And synecological game theory is to show the emergence of a spontaneously ordered system. And that is very exciting and not something that has been easy for Austrians to mathematize. Lot of people to cry Austrians as non-mathematic, but no, it's just that they have been trying to keep it real and recognize that the economy is a complex adaptive system and they haven't quite had the language yet to be able. They have to develop. There's really new theory that needs to be developed to talk about complex adaptive systems or economic systems as complex adaptive systems. And they've been doing just fine doing this in a narrative fashion, narrative theory, verbal theory. That's been just fine, language maps to mathematics. And so I don't consider verbal theory using sort of logical argumentation to be inferior to mathematization at all.

But this, way we can bridge between Austrian economics and Ostromian polycentricism theory and the things that we do say at GMU, which are very complex, adaptive, very aware of the asymmetry of time, the absolute movement, the necessity to discuss movement through time. And this sort of thing that we can bridge from those concepts into traditional mainstream theory. That's what I want to do without becoming a servant of mainstream economics. To some extent, if you bargain away your methodology and the complexity in your methodology, you can just become another mainstream economist and you lose the edge that you had as a complex adaptive theorist. And to some extent, this does happen to Austrian economists. But this is not something that I think has to happen and that's why I'm working so hard on these mathematizing and making formal in terms of analytical mathematics or algorithmic mathematics, absolutely perfectly fine, some of these Austrian ideas.

Anthony Comegna:

Now, I'm glad you said all that. Because first of all, I was going to say at some point here, let this interview serve as an example. Plenty of Austrians know their math. That's for sure. Because, oh my

God. At least Austrian economists. I'm a historian and I have quite a bit of trouble with all of this, but you'll help me through it. That's why you're here. And I am wondering though, as you well know, I'm an intellectual historian, so I'm curious to know what are your red lines for compromise? Surely you're not just going to try to publish whatever will get published. So for the kind of work that you do, what are the theoretical red lines that you think are essentially uncrossable without compromising the integrity of the work?

Abigail Devereaux:

Okay. So I think that excoriating any model of its sort of relevant and significant ability to explain the phenomenon in question is that's where I draw the line. To some extent, we all have to make simplifications. All models are wrong, as G.P. Box said. Well, I'm paraphrasing. So all models are wrong to some extent. And this doesn't mean, of course, that we just go dive head first into empirics and never look back because as Karl Popper noted, all empirical data is theory-laden. So we do have to manage these, the very fact that models are wrong and that data is theory-laden. We can then come to the decision that what we have to do is every one of us as a theorist or otherwise, as a modeler, has to sit down and they have to decide what aspects of the phenomenon that they want to study, in my case, it's like a macro-behavior.

I do think that I'm a new kind of macro-economist, or really an old economist that would study macro behavior as a matter of course, but wouldn't call it that in terms of what macro economics has been defined to be, post Keynes. You have to sit down and you have to decide which parts of the system, which kinds of behavior I'm unwilling to give up. So in one sense, I am very against looking at systems that are closed and static, and by which, I mean they have no sense for which they move through time. They only say react and end up at some sort of teleologically derivable result, like whacking a pendulum and knowing that eventually it's going to settle down. So that's one thing that I'm unwilling to give up.

I recognize that models need to have some kind of sense for a true movement through time. This doesn't mean the model has to be indeterministic. I don't even know how you would make an indeterministic model. So it can be deterministic. But the idea is that there is some element of randomness that's not just sort of baked in, stochastic randomness or random walk style randomness. But there's some sense for which the model's outcome isn't preordained. And so that comes along with not assuming the outcome before. And that was one of the things that I find when I'm reading economic theory is often it seems like the outcome is assumed, and then the model is built in order to attain that outcome. To some extent, we do want to explain behavior, but we also don't want to tell just so stories, right?

We don't want to, in essence, just be able to rationalize or create a narrative that fits what we see or what we think are the important effects without then having that model be able to explain more than just that example. So the model itself needs to be robust, and it needs to be able to explain. Not just that example, but one that sort of seems to be far from that sample. It also can't be taught logical. It can't be the reason why the outcome is optimal is because all choices are optimal by definition. You have to explain why you can even make the claim that all choices are optimal or optimized.

That goes into my second thing. So we have the true movement through time. The second one is recognizing a cost to computation and calculation. And as an economist, I can say that's a cost say to understanding or gathering knowledge. It's an epistemological cost. So you cannot know all things in the system. You cannot calculate. You cannot rank all of your preferences over everything because you can't possibly know what everything is, nor can you possibly know what everyone else's preferences are. And you've definitely not had enough time to be able to do this slowly, because everything's changing very

quickly all the time. There's turbulence, there's disequilibrium, there's this true movement through time where unknown unknowns things that you cannot even assign a probability of happening too. So I'm talking outside of Bayesian learning. Unknown unknowns enter the system, things that can only be imagined. And if you haven't had sat back and reflected for 10,000 years on the infinite number of things that can only be imagined, you can't possibly know everything that will happen or everything that will be relevant to your decision-making process.

And you can't either approximate everything that will be relevant to your decision-making process. So there could be very small things that could throw everything off, change the entire course of your life. I mean, this is the stuff of fiction. There's endless books, endless movies about how very small things throw off the entire course of your life. Not talking simply the butterfly effect of initial conditions. I'm talking the fact that there are things that we can't know, and we either can't know, maybe we know that they exist, but we don't know how they will affect us or how they might change our plans. So there's levels of understanding how we choose. There's also our perception, our model, our internal model, that changes all the time.

Dan Klein has a wonderful book. I think it's called Knowledge and Coordination. I'm sorry, Dan, if I've misquoted your title. I can call you Dan now, right? And not just professor. This is something that I'm slowly coming to calling my former professors by their first names, but it makes me very nervous to do this still. I'm very formal about that now. But Knowledge and Coordination, whereby he recognizes that not only are there unknown unknowns in the data, the information that we must absorb and then utilize to make decisions, but that our perception itself, our model of how A causes B or how A is related to, that changes as well. And so you have this planners problem on two levels, and this comes to the public policy point and also the economists as experts point, especially experts that advise public policy, is that you're both dealing with a problem of information that I think was discussed best by Friedrich Hayek.

How knowledge itself is utilized to make decisions and to coordinate our various ends with each other and not just coordinate, but to mediate conflict. Because as Dick Wagner will tell you, and he will make sure that you understand that there is both coordination in a beneficial sense and conflict in life. There is the light and the dark, and we also don't know that avoiding conflict is always a good thing. It could be that going through negative experiences actually make us better in some case, or even society's better in some cases is almost dialectical Marxism here. But it's I think just a simple recognition that the very teleological good things make us good, better things make us better, economic model that we use. And mainstream economics falls far short of our actual experience. So, yes, so I don't like closed aesthetic systems.

I don't like systems that pretend that everybody knows everything all the time. I think they just assume a perfect coordination into the framework of the model itself. And then they conclude, look, things are perfectly coordinated. So that means people and institutions perfectly coordinate. Well no, you assumed that into your model. That's not a conclusion. You certainly cannot deduce that from your model. So there's some strengths on knowledge, on computation. And, boy, those really get you very far. I guess the other thing is that people are different. So I think they have a true subjective economic theory. You can't have an honest, subjective economic theory and have one representative agent that is then aggregated over and represents, say all consumers or all firms. What does subjective value even mean in that case? I don't understand.

You don't have more than one person. There is no subjective value. There's just that one wants, that one set of preferences. That is the same for everybody. And so in that sense, it's like cheating. There's many ways in which I think that the heart of subjective economic theory has been excoriated through little tricks, a little sleights of hand that in fact, objectify subjective valuation and that cardinalize ordinality. Ordinality is say the ability just to simply rank things. Cardinality is assigning those,

is projecting those onto a scale, a scale of real numbers. And that's what economists learned to do in their first micro PhD course. Right? We're told that's fine. It's not fine. And so I'm also a radical subjectivist I would say, and I sort of reject the objectification of heterogeneous value of the way that our values differ from each other.

And so, I do feel very icky about using representative agents for that matter. And why are representative agents used, really? It's not because we think that's the best theory. It's not even because we really believe it's a good approximation. Even mainstream economists will tell you that they don't really think it's a good approximation. What's going on? Why do we use it? Because it's mathematically tractable. That's the reason. That's not a good reason to make a model to me. I know things can be hard and it might be that you have to abandon the analytical framework that you've been working in because it is not suitable or appropriate for the subject matter that you're studying. This is why I strongly advise people who are working in social systems theory to consider using algorithms. Because algorithms are much more diversifiable, easily diversifiable and in a simple manner, that's very intuitive.

And you can teach elementary school students how to program agent based models. It's very intuitive. And you can teach them about social networks. They get it. They get that not everybody is friends with the same people. They get that if Billy is friends with your group and also with the popular kids' group, that he might be able to mediate some favor between you and a popular kid. I mean, we get this.

But yet, these kinds of very intuitive and easily sort of almost too obvious social structures aren't utilized very much in theoretical economics. So I guess some topology, I would say, of social interaction is one of my asterisks that I would expect to have in a model. I'm willing to give that one up. And when I'm doing say growth theory, where they pretend that all agents are particles in the field and that everyone could possibly be related to and interact with everyone else. And I do, to some extent, use representative agents in say a growth theoretical model. But at least I checked the box on computational, that the knowledge is not complete. And also that the system isn't, to some extent, is not closed. So I think I'm willing to trade off some of these points, further points that I still feel very, very icky about working with completely static systems and ones in which knowledge is available, all information is available to everybody all the time.

Anthony Comegna:

Massive thanks to professor Devereaux and my heartiest congratulations again. She'll be back with us next week to continue the conversation and let's hope help us forever stamp out the myth that Austrians are Austrians because math is too confusing. Till then, lovely listeners, keep progress coming.