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JHET INTERVIEWS: KAREN VAUGHN BY BRUCE CALDWELL

Karen Vaughn received her BA in economics from Queens College of the City University of New York in 1966 and her PhD from Duke University in 1971. From 1978 to 2004 she taught at George Mason University. She attended some of the earliest meetings of the History of Economics Society (HES) and was the editor of the *HES Bulletin*, which was the precursor of the *Journal of the History of Economic Thought.* Professor Vaughn has served as the president of the HES and the Southern Economic Association (SEA), and was the founding president of the Society for the Advancement of Austrian Economics. She has written books on John Locke and on the Austrian tradition in economics, and numerous articles on a variety of topics in professional journals (the list of her publications is available as an online appendix to this interview).

Professor Vaughn was interviewed on September 24, 2020, about six months into the COVID madness, and the interview was video recorded. The transcript was edited for clarity.

Caldwell: So, Karen, let's get started. To begin at the beginning, please tell us a little about your personal life, where you were born, where you grew up. You know, just those early days and anything you want to share before we get into more professional things.

Vaughn: I was born in New York City, in Manhattan, in 1944, towards the end of the war. My father was a Norwegian. I would say he was an immigrant, except he was technically here illegally. He was visiting his aunt in Port Washington, Long Island, when the Germans invaded Norway, so he was stuck in the US for the duration. He met my mother, who was from Port Washington, and got married in 1943. They moved to New York City before I was born. I was raised on 65th St. and 1st Ave. in Manhattan. Manhattan then was still largely a collection of small neighborhoods. We moved to Astoria, Queens, when I was ten years old. I went to Bryant High School in Queens. Since I was an only child, I got a lot of attention from my mother. I loved school; I started taking piano lessons when I was ten years old and thought I was in heaven. I was never at the top of my class, but I was really pretty high up and school was always fun for me. I went to college at Queens College in New York City. That was before it became the City University of New York. The city colleges were all separate entities, and they were competitive and tuition-free. Because of that, I could afford to go to college. I came from a working-class family—when my father was actually working. My mother got a secretarial job when I was thirteen, and for most of my adolescence, she was the main breadwinner of the family.

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When I entered Queens College, I planned to major in music with a concentration in piano. I really thought I wanted to be a professional musician. My aim was to be a concert pianist, but in Queens College, when I heard the other students play, I realized I would never be able to compete with them. Then, at the end of the first semester of my freshman year, a very significant thing happened: I was diagnosed with tuberculosis and had to drop out of school for the whole spring semester. It was during that time that I started re-evaluating my life plan and decided I was not going to be a music major. When I returned to school I drifted around for a while. I tried education classes and hated them. The classes showed me that I would do anything other than teaching at a city school. I took a lot of philosophy classes, but I saw no income-earning future in that for me. I took one economics course and didn't much like it because it was the Principles in summer school. I was an English major for two and a half years because I couldn't think of anything else to study and I liked to read. I didn't find economics until late in my junior year.

Caldwell: Please tell us a little more about the Queens College experience. Did you meet any interesting people there? Is this when you started to meet some of the people who would be important later?

Vaughn: I got an excellent education there and I probably learned more during those years than any other time in my life. While I was home, recuperating from tuberculosis, I was reading anything I could get my hands on and a friend of mine gave me a copy of *The Fountainhead* to read. I had never heard of Ayn Rand, and I thought, oh, this is great stuff. When I was finally able to go out of the house, I bought a copy of *Atlas Shrugged.* I loved novels. I loved reading. I knew nothing about politics at that time, and Ayn Rand started my thinking about it.

When I went back to school, I couldn't really decide how to incorporate these new ideas into my life. But I was fascinated by the notion of individuality. It was all a model in my head. Then I saw a posting on the bulletin board at Queens College for a meeting of the Students for Free Society (SFS) about a lecture on Ayn Rand's theory of sex. I had never heard of SFS, but the topic was compelling so I went. I thought the lecturer got Ayn Rand all wrong so I turned around and walked out. And this young man came running after me. It turned out it was Larry Moss, and he said, "Don't go, don't go." I said, "Well, I'm not going to listen to this." He said, "Oh no, we just did this to get people in here." He was trying to start an organization that would counter Students for Democratic Society (SDS). In Queens College, New York City, in the early '60s, SDS was a big deal. In SFS, the most we ever had was eight or nine members. But Larry was convinced we were going to be a bulwark against the left.

I started going to those meetings and I realized, if I think this whole view of society is good, I need to know more about it, and I basically needed to know more about economics because so much of the philosophy was based on economic reasoning. I signed up for micro theory in the fall. I was the only woman in the class, I was the only non-economics major, and I was one of the few people who got an A in the class. I thought maybe this is where I should go with my life, so I switched over to become an economics major.

Larry introduced me during this time to Murray Rothbard. I had heard about Austrian economics from Larry, but I didn't have a clue what it was all about. I just

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thought it was like regular economics with an emphasis on free markets and supported libertarian politics. Rothbard was certainly very libertarian. He talked about economics, but he seemed more interested in spearheading the libertarian revolution. He was entertaining and he could sing songs, he was welcoming; you could drop in any evening and his salon was in full swing. I met Walter Block there, I think I met Mario Rizzo and Jerry O'Driscoll there. I kept meeting these people who were trying to sell me on libertarian anarchism. Rothbard was big on anarchism. I thought this was a non-starter myself, but I liked him. He had an apartment up on the West Side, not too far from Columbia University. A lovely old rent-controlled apartment. It was crammed full of books. There was only one room in the house that didn't have books. His wife put her foot down and there were no books in the bedroom.

But then I went to graduate school and I was more focused on my graduate work than on the whole libertarian Austrian stuff.

Caldwell: You went to Duke University. How did you decide to go there for graduate school?

Vaughn: That would be an interesting story for historians of economic thought because it was my undergraduate history of economic thought classes that got me interested in the field. Queens College offered a two-semester sequence in the history of economic thought. It was taught by a professor named Joseph Soudek. He was a real classical scholar, and he didn't believe in superficial research. I think it took him fifteen years to write his article on Aristotle, because he felt he needed to relearn classical Greek and brush up his Latin. He believed that Aristotle was fundamental not only to the beginning of economics but to the development towards a marginalist theory of demand.

His course emphasized development of the theory of value and monetary theory. I loved history anyway, and this was a good way for me to really grasp what economics was all about. I've often reflected that maybe we should start off by teaching students the history of the discipline rather than throwing the techniques at them at the Principles level. I know that that approach really helped me incorporate what I was learning at that time in my other classes.

In my last semester, my senior year, Soudek ran a special seminar on Aristotelian influences on economics, and you had to be invited to this seminar, he wasn't taking just anybody. He personally interviewed everyone who applied to be sure they were suitable. I had done well in his class, but the fact that I had about twelve credits in philosophy made him think maybe I was worthy of taking his seminar. Soudek believed that Aristotle was fundamental to understanding the development of value theory leading up to the Marginal Revolution. He assigned each of us in the class a figure in the history of economics to work on from this perspective. I was assigned John Locke. I had, of course, read some John Locke in my philosophy classes, but the thought that he had economic ideas was something new. That's when I did my first research on John Locke. I did well in the course and developed a mentoring relationship with Dr. Soudek. One day, he called me into his office and he asked me where I was going to graduate school. I was not sure where I wanted to go, but I had been thinking about it and I answered I was going to apply to the University of Chicago. He immediately said: "No! You must not go there! You must go to Duke!" I

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had heard of Duke but didn't know much about it. He continued: "And you must study with Spengler. I will write to him personally." I trusted him so I looked up Duke and I sent in an application, and not only did I get in but I also got an NDEA title IV fellowship. I went there specifically to study with Joe Spengler.

Caldwell: I take it you did study with Joe Spengler?
Vaughn: Yes. I was one of the few people in the entering class who signed up for his history of thought class. Most of the other students thought the class was one of these tedious things you have to get through and so put it off to the second year, but I couldn't wait to take it. I signed up partly so I could get a feel for who Spengler was. He was amazingly impressive. He did not give organized lectures. He just came in with a topic and talked, and he'd put all these piles of books on his desk and never refer to one of them, never referred to a note, and we would all be madly writing down every golden word that he said.

I did well in the two courses I took with him, so when it came time to settle on a dissertation topic, I knew I wanted to work with him. I wasn't sure I wanted to resurrect John Locke. But the more I thought of it, I had the head start there. I proposed that I'd write on the economic theories of John Locke, and Spengler was very supportive. And that's about all the guidance I got. He was not a hands-on dissertation advisor at this time. I knew you were supposed to consult with your dissertation advisor. But I wouldn't hear from him, so I'd call him up and say, "Gee, I think I should come and talk to you," and he'd say, "Oh, that's fine," and he'd take me to lunch at the Oak Room. He did this with all of his dissertation students, and he'd make sure we ordered lots and lots of food. I think he thought we were all starving. The food would come, and he would eat his in about three minutes and then, when your mouth was full, he would ask what you were doing, where you were in your work. I would tell him where I was, and then he'd say, "Well, you should look at this," and then he'd give me about fifteen different places to look. And then I'd be off on my own.

But I have to say he was easy to work with. He was getting towards the end of his career and I was one of his last PhD students. This would have been in 1968 when I was working full time on my dissertation and I didn't finish it while I was at Duke. I actually didn't finish it until 1971. When I finally sent it to him, it took him four months to read my draft. After I prodded him a little, it came back with all kinds of good comments written on it. I defended in the spring of 1971.[1] I felt very privileged to be able to write with him.

Caldwell: You mentioned that you were the only woman in your undergraduate economics class. What was it like to be a woman studying economics at the undergraduate and graduate levels during those days?
Vaughn: You know, I was thinking about that. Somebody else asked me about that recently. To tell you the truth, I didn't pay any attention to that. Especially that first

[1] Editor's note: The title of her dissertation is "An Examination of the Economic Theories of John Locke," and the committee members included, in addition to her advisor, Joseph Spengler, Craufurd Goodwin and David Davies from the Economics Department, and Glenn Negley from Philosophy. The fifth examiner is not identifiable from his signature.

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micro class. The fact that I was the only woman didn't intimidate me. The fact that I was the only non-major in the class did intimidate me. I heard all the men in the class moaning and groaning about how micro was the hardest class. After I caught on—it took me a couple of weeks to get used to the reasoning—I thought it wasn't all that hard. I wound up tutoring a few of them so it never occurred to me that being a woman was anything different, or any reason for me to feel different.

When I got to graduate school, there were four women in the incoming class out of sixteen or something like that. We used to laugh that four was Duke's quota, but it was a joke. I always felt completely competitive, and I never felt like anybody was singling me out. Well, there was one young professor who didn't stay very long, and he was hitting on all of the women. He would have been fired right away now. But in those days, he got away with it for a while until they finally bounced him. That was the only case. I just felt completely confident. I didn't think the fact that I was a woman in economics mattered all that much.

Caldwell: Did you take a job at George Mason University (GMU) immediately after that, or where did you go? You said you didn't finish till '71. Were you working in academia, or what was your path?

Vaughn: I first taught at the University of Tennessee in Knoxville. My husband, Garry, and I got married in '68 in our third year of graduate school. He was also in the economics program, so we were looking for jobs together. We were also kind of babes in the woods with trying to get a job. I loved Duke, but I have to say I didn't get much help in understanding the job search process. Here it was the spring of '69, we had no jobs, so we were trying to figure out what to do the next year because after three years of support, Duke would take our money and our offices all away. We had to get out of there. Luckily for us, in spring, mid-March, I think, the University of Tennessee received money for four slots that they could fill. The chairman—Hans Jensen—called around the local schools, he called Duke, and our chairman said they had two people, a married couple, looking for positions. We flew out to Knoxville and interviewed, got hired on the spot. After that experience, Garry and I had a very unrealistic view of what it was like to get jobs together. We thought you just go interview and they offer it to you, because that's what happened.

We went to Knoxville in the fall of '69. When I first got there, my dissertation got put on the back burner. I was young, I was moving to a new place, I had classes to teach, and I really didn't get my head around what it was to be a grown-up working academic for a while. But when I did, I kind of buzzed my way through, and I got my degree in the spring of '71.

We stayed at Tennessee until 1975, when neither of us got tenure. That was kind of a shock to me because it was the first time I ever failed in anything academic in my life. At first, I started doing the usual resentful "Oh, it's because…." I never said it was because I was a woman. I always thought, well, they don't like what I do, and then I sort of took stock and I realized I just hadn't worked hard enough, and I really hadn't. I just wasn't happy in academics. I didn't think I was going to stay in it. I felt kind of isolated at Tennessee, I had really no one sharing some of my basic interests whom I could talk to. And I was thinking maybe this isn't the life for me. I briefly flirted with becoming a real estate agent.

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When we didn't get tenure, and we were thinking about what to do next, Garry got offered a job on the Senate Subcommittee on Antitrust and Monopoly. We would be moving to Washington. Lots of economists live in Washington with lots of jobs and I thought I'd just get out of academics and try something else.

Then I found out I was pregnant and I had to rethink my life plan. I realized I wanted to at least have the option to go back to academics. After Jessica was born, I really knuckled down and I worked hard to have a decent job interview paper. That paid off, since I got the job at George Mason in '78.

Caldwell: I will just tell you something reassuring, given that you are a Duke graduate and I'm a graduate of the University of North Carolina (UNC). They also didn't give us any information on how to get into the job market and perform successfully, and I got my job at UNC Greensboro really late in the year, like in April or May. A very similar sort of story there. It may have more to do with history of economic thought being the field than anything because, I think, even then it was not the highest-placed field in the profession. But it is very familiar territory that you are going over as I think back on my past as well. You took your job at GMU in '78 but in 1974 you attended a meeting in South Royalton that in subsequent histories of the American Austrian Economics revival, so to speak, plays a very—maybe you think an oversized—role in the accounts, but it seemed like it was a very important meeting, and you were there. I'd like you to reflect on how you came to go to that meeting, how you found out about it, what it was like, what happened there and maybe afterwards also. Why do you think it was so important, or at least is said to be so important in the later accounts?

Vaughn: Actually, I have to back up a little. In 1973 I was invited to a conference on twentieth-century economic history at Cornell University where Murray Rothbard was one of the two speakers. The other one was Forrest McDonald from Brown University. It was the first academic conference I had ever been to, and I found it academically exhilarating. So when, the next year, I got an invitation to attend a conference on Austrian economics in South Royalton, Vermont, I was eager to go. As I said before, I had been introduced to Austrian economics as an undergraduate, but I really didn't know much about it. This seemed like a good way to learn more about Austrian economics, and besides, it was an all-expenses paid week in Vermont in the summertime and I knew there would be people there who would be interesting to talk to. I decided that I couldn't pass that up.

Let me give you the setting first. It was sponsored by the Institute for Humane Studies (IHS). South Royalton was a little town that had seen better days, but it was quite nice, with the town square in the middle, the iconic church on one side and the inn on the other side, and the little shops. The inn from the outside looked lovely. What we didn't know is that it had been recently purchased by the new owners to renovate it and they really had not done much renovation, so it was a little crumbly on the inside. The worst thing was there was only one bathroom on each floor, so that led to certain traffic jams in the morning. But the food was great and there was a lot of camaraderie. There were almost fifty people there.

The speakers were Murray Rothbard, Israel Kirzner, and Ludwig Lachmann. The schedule was organized into three lectures a day, one for each of the speakers. We all sat around a big horseshoe table. There was a joke because there was a crack in the

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ceiling, and we'd all make fun of the fact that every day the crack seemed to get just a little bit bigger.

The intent of the conference was to bring together not only people who already were very familiar with the Austrian tradition—there weren't all that many of them in those days—but also other people who had expressed some interest in either IHS or had some congruence with the ideas of either Ludwig von Mises or Friedrich Hayek. It was an eclectic group. It was an outreach. I actually thought that was a great idea because if you are interested in advancing interest in Austrian economics, you can't do it by just preaching to the choir.

Well, Murray Rothbard didn't like that at all. He wanted it to be an in-group. He was always thinking strategy, and his strategy was based more on libertarianism than Austrian economics. He saw Austrian economics as something he could use to bolster his strategy of espousing libertarian ideas. Whereas the other two speakers were very different. I was so impressed with Israel Kirzner. I had never met him. I thought he was the most careful scholar and that he gave the most careful lectures. I mean, they could be kind of dry, but you never felt that he skipped a hard argument. That's what I liked about him.

And then there was Lachmann. I had no idea what he was talking about. Of course, if you have heard Lachmann speak, you know that it was hard not to focus on his accent instead of the substance of his talk. So I kind of dismissed him because he just didn't make any sense to me, but Kirzner I took very, very seriously.

Rothbard I started off really liking, but by the end I just was not happy with that man, and I will give you a personal anecdote. He gave a lecture in the course of which he criticized mathematical economics and he put an equation up on the board and said that the equation did not capture the market process. I thought his talk was almost flippant. When it was the question-and-answer period, I raised my hand and I said, "Murray, I want to believe you, but if I were to tell my colleagues that mathematical economics is a mistake and it shouldn't be used to talk about economic phenomena, what would I say to them? How do I defend this position?" He said: "You heard me. You heard the lecture," and he went on. He would not answer my question, like he was insulted that I should ask that. Steve Pejovich was there, and he got really upset and he raised his hand and he said, "You didn't answer the lady's question!" and Murray looked at him and just glared at him and said, "Next question." I thought, what's the matter with this guy that he can't back up what he said? The last thing I wanted to do was to defend mathematical economics; I just didn't really understand his criticism. Well, it turns out that the reason he didn't want these "outsiders" was because he didn't want to debate. He wanted indoctrination. After that I kind of wrote him off as somebody who didn't have much to offer to me. On the other hand, I got really, really interested in Kirzner's work and then really started to take the Austrians seriously.

There's one other anecdote that you might be interested in: Milton Friedman used to summer in New Hampshire, but South Royalton was very close to the New Hampshire border. Friedman was going to drop in and talk to people—he knew a lot of the people there. When word got out that Friedman was going to come, everybody got so excited. The afternoon started off with a cluster of people surrounding Murray Rothbard; then Friedman arrived and it was like a swarm of locusts just swooshed over to him. Murray, you looked at his face, and it was like thunder. He was really ticked off that he was no

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longer the center of attention. I have to say I did not have warm feelings towards Friedman that day. I tried to talk to him; I can't even remember what I said. All I remember is his response was, "Well, that just shows you don't know what you're talking about." I said, "Okay, Murray won't answer my question, Friedman says I don't know what I'm talking about," so I just talked to the other participants. But it was clear Murray felt this huge rivalry with Friedman. After all, Friedman had published *Capitalism and Freedom* and he was a defender of libertarianism, and Murray wanted to be the primary defender of libertarianism.

Caldwell: The meeting ends up being important in the history of the Austrian group because so many of the people who later became kind of the principals of the next generation were at that meeting.

Vaughn: That's definitely true. I think the reason that so many people in that meeting wound up being really important players in Austrian economics was that it helped to create a community. Up until then, as far as I can tell, there were pockets of Austrians here and there and they were not necessarily in distinguished universities: Wabash College and Hillsdale College and Grove City College, not that they were bad, but they were not prominent places. You get educated and then where would you go? At first Murray Rothbard was important in that he was a contact person. The IHS had started before that and they were also kind of a little catalyst. But there was no way for people who were interested in the Austrian perspective to work together in a community, and research isn't done in isolation—most good research, that is.

Well, I have to say South Royalton had sort of a camp-town meeting feeling and I think it was largely because there was such an excitement that this was a setting in which people could share ideas and get feedback and feel like they weren't isolated, and frankly feel like they weren't oddballs in the world.

But it wasn't just South Royalton that kept it together, because IHS actually planned to do three meetings in three years in a row to build on the enthusiasm from South Royalton. The next year there was one in Hartford, Connecticut, and Hayek came to that one. Leland Yeager was a speaker at that one, and there were a lot of repeats from South Royalton. And again, the conference enhanced the notion that there are things we could work on together. This was before the Internet. You sent papers back and forth to each other.

And then the third conference was a smaller group who were invited to Windsor Castle, England, to give invited papers that would go beyond Mises and Hayek. The papers were eventually published in *New Directions in Austrian Economics.*[2] I didn't go to Windsor Castle because my daughter was born in 1976 and I couldn't leave her. I felt bad about that. But that was one time I took offense at being treated like a woman. I didn't get an invitation to Windsor Castle, and so I called George Pearson, who was with IHS, who had sent out the invitations. I called him up and I said, "George, why didn't I get an invitation to Windsor Castle?" He said, "Well, I thought as a new mother you probably couldn't come." I said, "Well, I would have

[2] Editor's note: Spadaro, Louis M., ed. 1978. *New Directions in Austrian Economics.* Menlo Park, CA: Institute for Humane Studies.

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liked an invitation." He sent me an invitation and I wrote back and said, "Thank you very much, but I can't come. I have a newborn."

South Royalton was a catalyst, but I think there is another thing that is linked up to South Royalton, although it doesn't get as much attention. At the Southern Economic Association (SEA) meetings in 1974, Larry Moss organized a session called "Ludwig von Mises, a Critical Reappraisal." (Mises had died in October 1973.) And he got some support I think from IHS to offer lunch to people who came. They really thought they'd be begging people to show up. Well, Murray was one of the speakers, as were Kirzner, Larry Moss, and William Baumgarth. Fritz Machlup was the chair of the session, and I was the only discussant. The room was jam-packed and it was a fair-sized room. There were well over 100 people in that room who came to hear about the Austrians, about Ludwig von Mises. Hayek had just won the Nobel Prize earlier that year. Still, it was amazing how many people showed up. I think that was another indication that there was real interest in the Austrian perspective at that time, and there was a willingness of people who didn't identify with it to learn about it.

Caldwell: We're talking about Karen Vaughn in the '70s and going into the early '80s. Do you want to say anything about some of the research that you were doing during that time or publications that you had? I know your paper on the socialist calculation debate came out in 1980. That was an important paper, but could you say anything about that or any of the other research that you were doing then?

Vaughn: I published my Locke book in 1980. I had finished that essentially by the end of '75, but then we moved to DC and I had Jessica to deal with. I didn't really know how to shop it around. Actually, it was either Jim Buchanan or Dick Wagner who said I should send it to the University of Chicago Press, and I thought, geez, Chicago, I can't believe you, but I still sent it. It turns out George Stigler was the reader, I found out much later. They accepted it, but you know how long a process publication is. It was 1977 when I sent it to them, but it didn't come out until 1980. Still I was grateful because having a book accepted by the University of Chicago Press helped me get my job. I really didn't do much with Locke after that because by then, I was interested in the Austrians. As a consequence of being the discussant on that session on Mises at the SEA, I had to read Murray Rothbard's paper on economic calculation. I knew nothing about that. I mean, actually, I knew nothing about most of the papers. I spent the entire summer after that researching and reading anything I could find on Austrian topics. I got really intrigued by the economic calculation debate. And the more I read, the more I thought, Murray is giving Mises all the credit, he's paying no attention to Hayek. Murray didn't like Hayek; he thought Hayek was a turncoat because he wrote *The Constitution of Liberty* and admitted you could have welfare of some kind. I thought that was just wrong because I thought Hayek was the one who really made the arguments that tended to stick and that really engendered debate. I made that point in my discussion.

In 1977, a year after Jessica was born, Larry Moss asked me to write an introduction to a book that eventually was published by Liberty Press. It was a reprint of Trygve J. B. Hoff's book *Economic Calculation in the Socialist Society.*[3]

[3] Editor's note: The book was originally published in 1938 in Norway, and an English edition appeared in 1949. See Hanisch, Tore Jørgen, and Arild Sæther. 2003. "The Forgotten Dissertation: Trygve J. B. Hoff, *Economic Calculation in the Socialist Society.*" *History of Economic Ideas* 11 (3): 43–73.

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I accepted because I thought that was something I could do from home because I still had a lot of my notes from the Mises session at the SEA meetings. It turns out I still spent a lot of time at the Library of Congress doing research. I wrote this really incredibly long introduction. Larry told me not to skimp, just to make it long, go over every argument. Then when I wanted to go back into the job market that year, I hoped I would turn it into a publishable paper. I cut the introduction down, I rethought some arguments, and used it as a job paper. Once I got the job, I finished it up and sent it off. *Economic Inquiry* published it. I was really proud of that paper. I felt like it was the first serious journal article I ever wrote, and it was the one that I thought really set me on the course of my future research. Without having written that paper, I don't know what I would have been doing. It opened up every issue you can think about in how you use economics in the real world, every theoretical issue. I became this huge Hayek fan after that. In the course of that initial research, I also found Jim Buchanan. I didn't know him before. Rothbard had referenced the *L.S.E. Essays on Cost* in his talk. I read the LSE cost book and saw there was a reference to *Cost and Choice.* I read *Cost and Choice* and became a huge Buchanan fan. The next article I wrote that I thought was really significant was "Does It Matter that Costs Are Subjective?" *Cost and Choice* made me wonder if subjectivist cost was important. The professional consensus was that subjectivist cost was fully incorporated into conventional micro theory, but I concluded that subjectivist cost did matter in significant ways. So those are two things that I think kind of set me off on my course. After that I did most of my thinking, believe it or not, while I was home with the baby.

Caldwell: I would like to switch gears again and ask you about the early days of the HES and your various roles in it. What was the organization like during those days? How did you get involved? You served as the editor of the *HES Bulletin*, which was the precursor to the *Journal of the History of Economic Thought (JHET).* Tell about those early days of the HES and your participation in it.

Vaughn: I'll start back in 1973, because that was the year that I got a letter inviting me to a conference in Chicago on the history of thought; there was yet no society. The conference was organized, I think, by Warren Samuels, Bill Grampp, Neil de Marchi. It was exploratory to see if there was enough interest to form a society. I was at University of Tennessee in Knoxville then, and Hans Jensen was the department chairman and a historian of economic thought, so we both went to that meeting. I think there were seventy people there. It was a fair number, more than the organizers had expected. It was just a one-day conference in a nice hotel in Chicago.

That was during the time when I was wondering if I really wanted an academic career, whether I really wanted to do history of thought. The conference gave me confidence that maybe this was a field that I could get immersed in. For me, that history of thought conference was similar to what I said about South Royalton, it was part of that feeling of a community that you need if you are doing research. I came away from that conference more encouraged about working in the history of thought. I did write a paper after that on John Locke's labor theory of value. It didn't get published until 1980, seven years later, but the idea was born after I returned from the Chicago conference.

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The next year was the official first meeting of HES, which was held at the University of North Carolina. The meeting followed a format I really liked. In the early days, because there weren't enough people to have multiple sessions, everybody was together for every session. That was a real benefit because you didn't wind up spending half your life choosing what session to go to or floating from one session to another. That really helped the sense of community. I especially liked that I was a discussant at that first meeting.

I have an anecdote about that. At that time, I had never discussed a paper, had never even participated in a meeting. I had to discuss a paper on "Locke, Vanderlint and Hume." I felt confident about Locke and Hume, but had never heard of Vanderlint. I wasn't too worried about that, since I expected to get the paper well before the meeting. I kept waiting for the paper and waiting for the paper and I never got it. At the opening cocktail party I found the author, a young guy from Hawaii whose name I've forgotten, and complained that I had never gotten his paper. He said he hadn't had time to send it to me because he had just finished it right before he left Hawaii. I told him I needed it so I could prepare the discussion. That night I read the paper and it was clear I didn't know anything about Vanderlint and couldn't fake it, so I spent half of the next day in the UNC library. Fortunately, UNC has a great library. But I was not at all pleased with the author. Even worse, in the paper he said some foolish things about John Locke, a big mistake. I don't think I've ever been so critical of a paper for the rest of my career. I really lit into him. All I can remember now is that he had written something about the analogy of the wheels of progress and the steam engine, I think. It so annoyed me that I practically yelled, "The wheels were waterwheels!" After all, he should have known that because his subjects lived in the seventeenth and early eighteenth centuries! At the break a professor from the University of Athens, his name was Lazarus Houmanidis, invited me to come to a conference the next spring in Athens. I was really excited by the prospect of a conference in Greece. I went there the next spring, but that's a whole other story.

In the early days of HES, you knew everybody. What was really nice is that it wasn't little clusters of the newbies here and the big shots there, no cliques, everybody interacted. For someone just starting out, it was supportive in a way that maybe changed as the conferences got bigger. I went to the meetings in '74, '75; I had to skip '76 because Jessica was only four months old at the time, but then I went to every other one up until the '90s. The meeting in 1975 was in Boston. That's where I first met David Levy. He was discussing my paper on Locke and the recoinage controversy. I sent him my paper a month or so before the meetings and I got a letter back from David saying he had read and loved my paper and that we were on the same wavelength. He included one of his papers that was intended to show me what he meant. I read his paper and I couldn't make head nor tail of it. But that started a friendship that still persists.

Caldwell: Were there any other people you would like to mention whom you met during those early days? You knew Larry Moss, as you said, well before, because he was the one who introduced you to Rothbard. But were there other people who were significant at the time?

Vaughn: A word about Larry. It wasn't just the Rothbard connection. He was the one who told me I should take the history of economics courses at Queens College.

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He told me there were only a couple of worthwhile economics courses: Micro, Macro, Money and Banking, and History of Economic Thought. All the rest of the economics courses were just applications. He was very certain back when he was twenty.

When I think back, Warren Samuels was just really a great person to know. He didn't share anarchist libertarian thought, but he was so open-minded, and you could talk to him. Abe Hirsch was somebody else I thought was really, really nice and pleasant to be with. Of course, I met Vince Tarascio and Sam Hollander. I met Bob Coats at the conference in Greece. There were many others, but after forty-five years, I can't remember all their names.

Caldwell: Were there many women then in the history of economics?
Vaughn: A few. Bette Polkinghorn was one. Ingrid Rima and I bonded in the bathroom of the girls' dorm when we had the meeting in Boston. In those early days you stayed in these crummy dorms and had communal bathrooms. Ingrid was somebody I met and liked. There were just a few other women, whose names, again, I can't remember.

Caldwell: You were the editor of the *Bulletin* and then you were president of the HES in 1992–93. It was also around that time that the *Bulletin* became *JHET*. Would you talk about that period?
Vaughn: I can tell you about the *Bulletin.* It was 1978, I was unemployed at the time, but I had just gotten the job at George Mason University starting that fall semester. I understand that at the Executive Committee meeting that spring, they were talking about starting a bulletin.

For the previous meeting, some scribe would take notes at all of the sessions so there would be a record of all of the sessions. The Executive Committee thought it would be a nice idea to do more than simply report on the meetings, maybe publish some announcements and research queries. As I understand it, because I was obviously not at that meeting, there was a controversy about whether the society should even do that because some of the people, Bill Grampp was one of them, were concerned about competing with the *History of Political Economy* (*HOPE*). *HOPE* was fairly new at that time. All new journals have to be careful about their survival, so it was a reasonable worry. That's why instead of trying to start an HES journal, at first there had been some talk of whether the Society could become affiliated with *HOPE* so *HOPE* could be the official journal of the Society. I don't know the ins and outs of that. I know that that didn't happen, so instead, the committee wanted to start a smaller, more limited publication—a bulletin. But from what I understand, the committee wondered who would be willing to be the editor. They decided they needed somebody young, and who would probably be at a less prestigious university where the university would feel like this was a benefit, and who was willing to put in the time. Larry Moss was actually at that meeting and he piped up, "Karen Vaughn. She just got a job at George Mason," and he said everybody turned and said, "Where, what? What's George Mason?" So definitely I was at a non-prestigious university, so they offered the position to me.

I immediately called Bill Snavely, who was going to be my department chairman, and I asked if I could get any financial support for publishing the *Bulletin.* As long as

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"published at George Mason University" was on the cover, he was okay with it. That's how I came to be the editor. It was a challenge because I had to build it from scratch; there was no template. I can remember spending the first two years continually writing to members to please send me something I could publish. It turns out a lot of people said it was really useful.

One person who was so supportive was Royall Brandis. He's somebody I met whom I thought of as a really good friend, and he always tried to send me copy for every issue. Someone who was not supportive was Bill Grampp. I think he didn't like me, he was always kind of sniping at my heels and putting down the *Bulletin.* I had to go to all of the board meetings, but he insisted I couldn't speak at them because I wasn't officially a board member. Other than that, editing the *Bulletin* was a little bit tedious, but I felt like it was worthwhile. I did it until I became chair of the Economics Department. I did one issue after that and saw it was impossible.

As for the *Journal*, I had nothing to do with that. Don Walker, who had taken over the *Bulletin*, enlarged it. He called me and said they were thinking there was enough room for a second journal in the history of economic thought. He asked if I had any objections to changing the name and I said, no, I thought this was a wonderful idea. I thought the society needed a journal. What led up to this is that there was just a whole lot more research to publish. In 1978 we were lucky that *HOPE* could survive. By 1982 or something like that, there was lots of material coming out, so I thought it was a great idea.

Caldwell: Any comments about you being president of the Society?

Vaughn: Other than that, I was thrilled to be asked. It came at a perfect time in my life because I resigned as department chair in 1989, and it was kind of an acrimonious departure for me. I was feeling a little down and was wondering what I was going to do next. I got that call, I was at home, and I perked up and said, "Oh yes, I'd love to do it. Thank you." I don't know if they still do it, but in those days when you were president elect, you planned everything about the meeting. There was no help whatsoever. We had it at George Mason, and I arranged for the housing since we had decent dorms by then. I had to arrange for the rooms, I had to arrange for the meals, choose the flowers for the table, and organize the sessions. I had index cards for each paper and for the name of every discussant. I laid out all the sessions on my dining room table, and I'd be moving the index cards and realize I had two sessions at the same time or someone slotted to be in two sessions at once. I mean, it took me a long time to get that worked out. It was like a full-time job. But it was a successful meeting, and I was really happy I did it.

I had organized a meeting at George Mason for Bob Coats when he was president because we couldn't go to Nottingham. He called me up and said, "So you're at this crummy university, you'll probably be delighted to have this kind of publicity." But he was right. At least I didn't have to organize the program, just all the logistics. And then I got a call two years later from somebody else who said, "We really liked the George Mason venue and I'd like to have my meeting there," and I said, "No, enough is enough. I'm not doing that again." So I had some experience when I was president elect. Then being president, I loved writing my address, I loved giving it, I got a standing ovation at the conclusion—I was just on top of the world.

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Caldwell: Let's go back to your experience at George Mason. You mentioned that you were chair from '82 to '89, but it was even before you became chair that you were instrumental in bringing various groups to George Mason that would end up being quite significant for the future of the university and in a way turn it, certainly in terms of its Economics Department, from a place that people might not know too much about to being a place that everyone, certainly in the history of economics, but I think more broadly in the economics profession, knows about. There was the Center for the Study of Market Processes at Rutgers and then the Center for Study of Public Choice that was then at Virginia Polytechnic Institute in Blacksburg, Virginia. Please tell us some stories about how all that came about and your role in it and what it was like.

Vaughn: First I have to start with the Market Processes Center because that set the ball rolling. When I first got there, George Mason was a very poor university. The Economics Department offices were in a little old ranch house off campus. My office was in the basement with a little door to the outside. Every Monday morning I'd come in and sweep all the bugs out of my office because they had come in under the door. But I was grateful to have a job, so it was fine.

We were so young. Most people there were brand-new assistant professors and maybe not the first choice of a lot of other universities. Publishing was not a big deal in the university when I first went there. But our chairman, Bill Snavely, really wanted to build the department. I was working away in my office one afternoon, I guess it was early 1980, when I got a phone call from George Pearson, from IHS. George was telling me about a new Austrian program at Rutgers, Newark, that was having problems. They wanted to expand the Austrian program, but they were having trouble getting the rest of the faculty to go along with it. Abe and Eva Hirsch were there at the time. The couple of faculty who wanted to start the Austrian program wanted to hire another faculty member who would specialize in Austrian economics as an ongoing research program, but the rest of the faculty just wanted to hire a historian of economic thought who had written something about the Austrians. There was clearly a conflict of visions. Pearson told me they were looking to move to another university and I said, "Well, send them here. We've got nothing going on here and they would be a big asset to our department." Sure enough, just days later, I got a call inviting me to give a talk at Rutgers, Newark, and to meet Richard Fink, the director of the program, and Joe Salerno, one of the faculty members. Don Lavoie was a graduate student at New York University at that time, but he came to my lecture that day. That is where I met Don for the first time. I gave my talk on the calculation debate, and Don and I got into a big discussion afterwards. He was still working on his dissertation on the same topic. We took the tube back to Manhattan together and we discussed the calculation debate the whole time.

When I got back to GMU, I talked to my chairman, he invited Richard Fink down for an interview, offered him the job, and in the fall of 1980 Richard came along with three undergraduate students. That year, we got a course in Austrian economics approved by the faculty. Then Rich said he could raise money for a center, but we needed four people to make it work and we only had two. But GMU was expanding then, we were always hiring, so we had slots to fill that year.

First, we went to the administration to get permission to start the center, and then I basically read the resumes of all of the young Austrians who were interested in the

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positions. I wanted to hire Don Lavoie desperately because I thought he was just a jewel. And then Jack High had come from University of California, Los Angeles, and had an interesting dissertation. The problem was our chairman wanted to give us only one slot and we wanted two. But he was a very, very gentlemanly and democratic person, he always went with what faculty voted on. So I lobbied every single person on the faculty, and sure enough, when it came to vote on offers, they all voted for both Lavoie and High, and poor Bill Snavely, his face fell, he was not happy. But it turned out to be the right thing to do because then we got the funding for a center, and beginning in the fall of '81, we started having invited lectures, a weekly discussion group or seminar. I mean, it started to build life in the department, even if you didn't agree with the Austrian perspective. We were bringing people like Hayek in, we brought Buchanan in, we were creating an intellectual environment that was more or less lacking up until then.

That, indirectly, is how the Public Choice Center move came about, because it was at the American Economic Association (AEA) meetings in 1981 that I ran into Jim Buchanan. I should back up and say I had been at several Liberty Fund conferences Buchanan ran at Blacksburg and had gotten to know him pretty well. And at the Public Choice cocktail party at the AEA meetings, he was telling me he didn't really like what was happening at the Virginia Polytechnic Institute (VPI). He said that instead of having a distinctive program, the chairman wanted to be a second- or third-rate Harvard. He felt his center was being slighted, and he just said, "I don't know, if it goes on like this, Karen, we're just going to have to come up and move in with you." I didn't react much at the time, but I went back and I started thinking about what he said and wondering if he was serious. I started talking to our department chairman and to my colleague Jim Bennett, and I said I didn't know whether we should take this seriously or not, but Buchanan said he might want to move to GMU, and if he meant it, it could be a good thing for us. Snavely put me on a plane down to Blacksburg the next week so I could talk to Buchanan and talk to everybody in the Public Choice Center. It turns out, later on I learned Jim was just so thrilled that he was in such demand that I would come the next week after talking to the chairman. I don't know if he initially was all that serious about moving to GMU, but that kind of tipped the balance in our favor. Buchanan started negotiating with the administration, and I kind of brokered those negotiations. I was a spokesperson for both sides, and so a Memorandum of Understanding concerning the move was signed that spring. Buchanan sent one person, Mark Crain, to us in the fall of '82 to help prepare for the rest of the Center's faculty—all six of them—to move to GMU in '83.

As a punishment from my involvement, they made me become department chair. The administration and the dean felt I was the perfect person to work on the integration of the Public Choice Center into the Economics Department. And our then current chairman was about to be let go anyway, so I was appointed acting chair; I thought I'd be acting chair for a year, and when the Public Choice Center moved, we could then search for a new chair. But Jim really liked the idea of me being chair. He felt comfortable, he thought he could trust me, and he prevailed upon me to go up for election for actual chair. I did that job for seven years and, let me tell you, it was not fun.

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Caldwell: Do you have any choice war stories?

Vaughn: I think I was in a different position from a lot of chairs. Most chairs join a department where the faculty all know each other. In my case, I had to help integrate seven new members into a department of fewer than twenty people. There was bound to be some friction. Also, here I was in a department that many of the rest of the university faculty were wary of because we didn't fit their mold. On top of that, even though we weren't paid very much, the rest of the university faculty thought the economics faculty were paid too much. And then, what happens is the president takes a whopping chunk of the budget to hire seven people for one department. When you think about it and you're in the English Department and you're begging for another slot and seven go to Economics, you're not happy.

Caldwell: That was for all of the additional Public Choice professors.

Vaughn: Yes. Seven people came and, even though they didn't get any salary increments for moving to George Mason, they just got the salary that they had been paid at Virginia Tech, it was a lot more than people at George Mason in other departments were earning, so there was potential animosity there. But we had a president who was not particularly democratic, and he thought this was the way to build the university, and he thought if you start with one department, then that sets up momentum for other departments. It turns out it was the right strategy as it worked really well.

I had to go to all of the chairs' meetings with the dean: I thought my role was to make friends and not try to justify our new hires so much but to be a team player in university matters. I liked all the other chairs and we got along just great. In the department, my role as chair was to smooth the integration of the Public Choice Center into the rest of the department. This was made somewhat difficult since we had seven new faculty members who, probably justifiably, thought they were more accomplished than everybody else in the Economics Department.

I also had the Market Processes Center to worry about and then the original faculty members who didn't belong to either center but who supported the move completely. I didn't want to see them get shut off and pushed out of department affairs. I wanted them to have a role to play, too. So I had three factions to try to keep happy and that's where my headaches came. I never had a headache with the administration. I always got along, I got anything within reason that I needed, but keeping those factions from conflict was a balancing act. Jim was helpful with that, he didn't want any fights like he had had at VPI. What he wanted was a peaceful life to do his work.

If I could insert another footnote, I know he's recently been criticized for being part of some kind of conspiracy to get rid of democracy; he was the most non-political person you could imagine. One of the reasons he was doubtful about the move was he didn't want his faculty running down to Washington and giving testimony. He wanted them in their seats doing their academic work. He really didn't think everyday politics was worth getting involved in. He thought being in Fairfax rather than in Washington would provide something of a buffer.

Anyway, he helped because some of the Public Choice Center people could have been a little disdainful of the Austrians, as you might guess. But he always supported the Austrians. One of the reasons he thought of the move initially was that he liked

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the Market Processes Center. He said he always found the Austrians fun, interesting to talk to. After he got there, he remarked on several occasions that the Austrian graduate students were the most interesting ones he taught. So he was helpful with that, but there still were rumblings from some of the disaffected, and mostly in the Public Choice Center.

Caldwell: What's amazing is that that was just the beginning. I know it was after the time that you were chair, but then Vernon Smith was brought in with the experimental group, another large number of people, and much later, Pete Boettke started the Hayek Center. George Mason seems to have just gone from strength to strength in that regard. Do you have any reflections on that? Do you think it was just that once the momentum was there, it just kept going?

Vaughn: First of all, our president loved that strategy. It worked so well the first time. The next group he brought up was from University of Virginia to set up the School of Information Technology. He was really on board with that kind of thing. But as far as the trajectory of the centers in the department, I do think it was largely a question of momentum. You didn't mention it, but Henry Manne became dean of the Law School. I had nothing to do with that, but Henry knew Jim and knew the Public Choice Center was there. I mean, I knew Henry, but it wasn't like I had any role in that. The Law School was not accredited when I first got to GMU. And when they wanted accreditation, they had to pump some more resources into it, and they wanted a new dean, and Henry came up and he did a lot to build that Law School.

By the time Vernon Smith brought his experimental group to join the department, I was out of the institution-building business. Tyler Cowen was the one who engineered that move. And again, it was part of a culture now at George Mason, to get a group of people in and bring them together because then they bring their culture with them, their research dynamic, and that feeds back to the rest of the department. Unfortunately, we couldn't keep Vernon Smith because our former dean went to Chapman University and he liked that strategy, too. So he just paid them a lot more money to move there. But you can't blame him. I will take a little bit of credit for Pete Boettke. He was a former graduate student of mine, one of the best. I recruited him for our faculty, but the Hayek Center is all his doing.

Caldwell: Let's go to another phase of institutional parts of your life. You were president of the Southern Economic Association (SEA) and you were also the first president of the Society for the Development of Austrian Economics, which has its annual meeting at the Southern Economic Association. It strikes me that when I first started out, the SEA was simply a regional economics association where there were the Western and the Eastern, and it was just another one of those. But in the last twenty-five years or so, it seems to have become a place that is a magnet for many of the fields that are important at George Mason University: Austrian economics has a lot of sessions at these meetings, the experimental group, Public Choice, and the rest. Please tell us a little bit about your experience with the SEA and also the Society for the Development of Austrian Economics.

Vaughn: They're actually very much connected because I was president elect in, I think it was '94. As president elect you have a certain number of sessions you have to fill. I had public choice sessions, I had history of thought sessions, and I had Austrian

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sessions. The meeting was in Orlando, Florida, right next to Disney World. During the conference, I went around and counted the number of people in some of the sessions. Most sessions, if they had four people in the audience, that was a lot. But the Austrian sessions were packed. The rooms were full, and the reason was that many people who wouldn't have gone to the SEA otherwise found out that there were, I think, four sessions on Austrian economics. So they came for the Austrian sessions.

One evening I was in the bar, and I started talking to some of our former students. We wandered in to the restaurants to have dinner to continue talking. They all had the same complaint, which was they liked their jobs but had no one to talk to on their faculties. They were scattered all over the US and felt like they were by themselves. It was great for them to come and be able to talk to people interested in the same subjects they were. So I started thinking about how they could participate more in the SEA.

I was president the next year, and at the following board meeting in the spring, Joe Jadlow reported that attendance was down at the meetings and the Society was diminishing. It was suggested that we try to get affiliate groups to align with SEA like the AEA had. I just popped up and said that my Austrian organization would be happy to do maybe five or six sessions at every meeting. There was no Austrian organization at that time, but I thought we could have one. The board approved and I got six Austrian sessions to organize for the coming meeting. So I sent out a letter to everybody I could think of who was interested in Austrian economics, telling them about the possibility of forming an association, and inviting them to an organizational meeting concurrent with the next SEA meeting. I believed that an Austrian Society shouldn't be an in-group, that we should be reaching out to people who were interested in the aspects of Austrian economics that overlapped with their work, that we should try to grow the research agenda.

Then I organized a dinner at the conference hotel. I was so afraid I couldn't get twenty-five people to come to that dinner because then I'd have to cancel it. I wrote to all the people planning to attend, inviting them to the dinner, and I asked them to please let me know right away if they were coming because if I didn't get twenty-five, I would have to cancel. We ended up with almost forty people at the dinner and a lot more who didn't want to pay for the dinner but showed up for the organizational meeting. There, we decided we could have a society. I got a small committee together and we met at George Mason the following spring. I wrote up a constitution that I modeled on the HES constitution because that's the society where I was in on the beginning as well. I had seen how HES turned out, and I thought this was a really good way to approach an Austrian Society.

After we formally organized, I was the first president. I thought we shouldn't have more than six or eight sessions at the SEA meetings. I wanted them to be really good quality because we were basically trying to attract people to come from the outside. One of the reasons I wanted to do this in conjunction with the SEA is that a lot of our former students had to get their way paid to be able to go to a meeting. They weren't going to get their way paid to go to some unknown Austrian Society meeting, but they would get their way paid to go to the SEA meeting. The SEA would benefit because they would have to become members and that would boost the SEA membership.

At the first official meeting, we had six carefully curated sessions. We had a good turnout once again, and then more people showed up and then more and more. After

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the first couple of years, I was involved but I stopped taking a leadership role. I was on the board two years after being president, but after that I just was happy to go to the meetings. And after a while, the Society for the Development of Austrian Economics had so many people wanting to present papers that they had to have concurrent sessions. I retired in 2004 and dropped out completely for a while, and then when I went back, I was astonished. I couldn't believe how many sessions and how many people were involved.

I think it's been a good thing for the SEA, and as far as our topics dominating, it's partly because of the Austrian sessions, but it's partly because some of the topics were timely and important. I'm very proud of that. That was my major entrepreneurial move in my lifetime.

Caldwell: I'd like to ask you about your last book, which I have a copy of here, *Austrian Economics in America: The Migration of a Tradition.* How did you come to write it and what are some of the major arguments you make in there? I found it to be a very important book and I would like to have you talk a little bit about it.

Vaughn: Again, it goes back to the history of thought. I was invited to give a paper at a conference on Carl Menger at Duke University in 1989. I had resigned as chair in '89 and had to lick my wounds for a little bit and think about what I was going to do next.

I had written the Palgrave entry on Menger, the 1987 Palgrave, so I felt I could turn out a good paper. But I didn't know what aspect of Menger I wanted to write about. Having been in the Market Processes Center and all that time thinking about Austrian issues, I realized that the important parts of the Austrian revival came from Menger. I titled my paper "The Mengerian Roots of the Austrian Revival." I gave the paper at the conference, and I was happy it was getting published.

But I started to think that it was really a pretty good outline for a book that just needed to be fleshed out with maybe a few additional themes added. I decided to write the book and it didn't take me as long as you might think. It took me just a little over a year to write it because I had so much background material.

The theme of the book was an exploration of a major current controversy stemming from the Austrian Revival. When I first started going to Austrian conferences in the late '70s and early '80s, the primary question was whether Austrian economics was really any different from mainstream economics. Maybe it's just a neoclassical economics in words. There were arguments for that because a lot of the formulations sounded like supply, demand, market clearing, partial equilibrium, and so maybe that's all it was. But then there were these inconvenient bits that didn't fit really well, such as Hayek's knowledge essays: his three essays in the '40s, "Economics and Knowledge," "The Use of Knowledge in Society," and the third one, "Meaning of Competition." All those three didn't fit into the neoclassical economics in words. I had been pondering that for a long time. But the major controversy that was the backbone was one that really got to the heart of what Austrian economics was all about.

During the late '70s and early '80s, Ludwig Lachmann and Israel Kirzner carried on a debate about the usefulness of equilibrium notions for explaining economic order. As we know, when Kirzner wrote *Competition and Entrepreneurship,* he was trying to show the profession that you couldn't have partial equilibrium theory

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without an entrepreneur. The entrepreneur was the coordinator that allowed equilibrium to be attained. His view was you needed the notion of equilibrium in order to explain order in the economic world. If you didn't have that, you had no explanation for why things were as orderly as they were. So equilibrium still had to play a role in economic theory. Lachmann, on the other hand, was kind of all over the place. He was not a systematic thinker, but he kept introducing ideas that undercut arguments for equilibrium theorizing. Both of them agreed that general equilibrium was kind of useless and misleading, but Lachmann was even very skeptical about partial equilibrium theory. I didn't know which side I was on for the longest time because Lachmann would talk about radical uncertainty. Kirzner would say, if it's all radical uncertainty, nobody could do anything, yet people still were acting. Lachmann would counter that time is important, and there's the subjectivity of preferences, the subjectivity of knowledge and of expectations. He had lots of bits about expectations, about time, but he also would talk about institutions. And to me it was kind of buzzing confusion. I agreed with things he said, but I didn't know how to put it together. My book was a way to think through these issues. I should say that most of my good papers were me trying to think through something because I didn't know what I believed until I got going on it.

I started with Menger and how he was different, sort of William Jaffé's deconstruction of the Marginal Revolution. I thought Menger's focus on process was important and it was important to later Austrians. And so I did the chapter on Menger and then I did one on Mises. But Mises, I always thought, was more certain of things. He never seemed to be groping for an answer. In a way it came out as almost too didactic, and I thought he never gave enough room to the uncertainties and to the knowledge problems that Hayek emphasized.

But the important chapter was my chapter on the Kirzner-Lachmann debate, and so I set it all out. Of course, Lavoie was on the Lachmann side. I had great respect for Don, who was my real colleague at George Mason because he and I saw the world in very much the same way, and we had common interpretations. I didn't quite go all the way with him on hermeneutics, but I could see the point.

Anyway, as I was writing the book, I laid out my view of the evolution of the new Austrians and how Austrian economics changed when the earlier generation came to America. I talked about Rothbard and all that, but it was all preparatory to this debate. I laid out the debate, and then when I got to the final chapter supposedly resolving all the issues, I just didn't know what to say. I couldn't figure it out yet, sitting there thinking, I don't know how to resolve this, so I said some wimpy thing like, "Well, I'm not sure how this is going to play out, but it seems to me like it's going to be more Lachmann than Kirzner." I always felt bad, but I had to finish the darn thing. I couldn't spend six more years trying to figure it out for myself. So I wrapped it up and all of my reviews were great. Everybody loved it. Well, I had one really bad review, but that was a personal issue. At any rate, all of the other reviews said they loved the way I laid out the Austrian perspective, but one person (and I give him credit), said, "It was quite disappointing to get to the end and not find out where it is going." And I was disappointed, too, so after my book was published, my plan was to try to figure out what I thought about the whole equilibrium matter and where I thought Austrian economics should go. That's what I spent the rest of my research years on.

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Caldwell: Did you ever figure it out?
Vaughn: I think I did.

Caldwell: So what's the answer?
Vaughn: I wrote two more papers. I muddled around for a long time. I was going to write a book called "Reinterpreting Hayek." But by this time family problems were impinging on my research. I was first introduced to Hayek because of my research on the calculation debate. Later on I got interested in Hayek's social theory and political theory. I wrote some papers based on the *Constitution of Liberty* and on *Law, Legislation and Liberty,* and taught a graduate course on Hayek's political economy. Hayek's social theory comes in for a lot of criticism because of his evolutionary view of cultural change. Rules of social order, political as well as norms and habits of everyday life, emerge, but because of limited knowledge, it is impossible to predict the long-run consequences. The ones that benefit the society will likely persist, but the ones that have detrimental consequences will disadvantage the society. In an evolutionary competition, those societies will lose out to the ones that have hit upon better rules and cultural norms. He believed that English-speaking countries had inadvertently hit upon rules that would allow the market to flourish and this allowed both economic growth and personal liberty. His theory of cultural evolution was criticized by both his supporters and his detractors, and often for good reasons. He was criticized, of course, for instead of basing selection on the individual, he had group selection, and, of course, group selection was anathema to evolutionary theory at the time.

I defended Hayek's argument about group selection. Human beings have rules, and rules are for groups, they aren't for individuals. So what he is really talking about is the selection of rules, and not just formal rules but informal rules. But with no clear selection mechanism, I still thought his social evolution was rather weak. Then it dawned on me it may be weak for social evolution, but it worked for economics because markets do have a selection mechanism. The model for a progressing economy is an evolutionary model. In biological evolution, species evolved through genetic variation, but in a theory of market evolution, what was the analog to species? That is where Lachmann came in. Lachmann used to talk about institutions as "points of orientation," and it occurred to me that market institutions were the analog to biological species. Austrians generally paid lip service to institutions, but except for monetary institutions they did not talk much about them. It finally occurred to me that if you wanted to explain order without equilibrium, the order had to be a product of the institutional environment. Like biological evolution, competition among market institutions leads to change, and because markets have a clear selection criterion, profitability, in general the change was beneficial to consumers.

So I wrote a paper called "Hayek's Implicit Economics: Rules and the Problem of Order." What I argued was that economic activity tends to be largely routine for people; large chunks of it are routines. Routines are sort of informal rules that one adopts. But there are times when you deviate from your routine. Say you notice a new product or a new kind of business enterprise that you decide to try out, which causes you to deviate from your routine. If the new product or enterprise seems more satisfying for you than your previous one, you will likely

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change your routine. As Hayek wrote, competition is a discovery procedure, and the result of this discovery procedure is the alteration of behavior that leads to changes in existing institutions as well as the creation of new ones. The implication is that you can have an orderly society and you don't have to have market clearing in every market. It's price flexibility that counts. We agree that supply and demand works, but what explains the overall order? You don't need an overall theory of equilibrium. What you need is a theory of how institutions change, and they change due to entrepreneurial activity, but change comes about from solutions to new problems. These solutions then become part of your repertoire of routines you take for granted.

I thought that this was perfectly consistent with what Hayek was trying to say. In his discussion of market order in volume II of *Law, Legislation and Liberty*, he falls back on more neoclassical formulations of economics, but an evolutionary theory of markets really fits better with his conception of a catallaxy, since it takes account of learning, discovery, and orderly change. I thought this was the core of what you needed to build an Austrian economics that was not just neoclassical economics in words. It was a recognition that rational decision making was only part of economics.

After I published that article, I became aware of complex adaptive systems and some of the computer work that was done simulating these complex adaptive systems. I thought, aha! The economy is a complex adaptive system; that's exactly what Hayek was groping towards. I don't claim to be an expert on this, but I did know that Hayek had been deeply involved in all of the early cybernetics work and in systems theory, so I thought he might have had that in his mind when he was talking about this evolutionary process. So I wrote a paper called "Hayek's Theory of the Market Order as an Instance of the Theory of Complex, Adaptive Systems." And those two papers together I thought could set off the Austrians. Here you had a technique for doing, if not empirical research, at least simulation studies of complex market processes. You had a framework where you looked at institutions not as barriers to people satisfying their preferences but as means of solutions to problems that allow them to think about something else. Those were the last two important papers I wrote.

And actually, that's when I thought I should write this book, but I had some personal issues that I had to deal with, and I just couldn't get my head wrapped around it. And to this day, I'm kind of sorry I didn't stick with it. I published a few papers about Hayek and liberalism later on, but that was long after I retired.

Caldwell: This has been wonderful. One of the things that I'd like to close with is that you started out by saying that you were very interested in music, and it was only after I knew you for many, many years that I came to find out that you are involved in opera singing. Why don't you tell us about that?

Vaughn: I'm a wannabe musician. I played the piano up into my forties. I actually took lessons again in my forties. I gave a recital. I loved the piano. But then I developed tendonitis in my left arm, and I couldn't play and I couldn't get rid of it. I was very frustrated and I needed some musical outlet. I always used to sing. I sang with a couple of choruses when I was younger. As department chair, I had had a secretary who had the job only because she was paying off her student loans to music school. When the loans were paid up, she resigned and set up a voice studio, so I

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called her up and said, "You know, Carolyn, I'd like to take a couple of voice lessons. Would you be willing to teach me?" I was forty-six at the time and had no ambition except to sing a little better than I already could. I started studying with her and I thought, oh wow, this is great. Oh, I love it. I couldn't wait for my lesson every week. I'd get up an hour early every morning to practice before I went to work. Unfortunately, Carolyn had a rare disease and died very young. After Carolyn died, I found a teacher at the university because that way I didn't have to drive someplace else to take a lesson. Then I found out that George Mason had an opera workshop.

When I think back, I was kind of gutsy because I was in my fifties by then and all these students were graduate students mostly. They were in their twenties, although some were returning students who were a little bit older, but I was clearly the oldest in the group. I just figured if I'm ever going to learn anything, I have to ignore the age gap and get on with it. They gave me little parts to sing the first year; the second year I had a few more parts, and I was getting better. Then I really got the acting bug because opera is about acting, too. I then heard of a two-week opera workshop held in the Washington area, so I auditioned and got accepted. The workshop was like an opera boot camp. It was all day long and you had acting lessons, you had stage movement, you had diction lessons, and then you had three scenes that you had to have memorized before you arrived at the workshop. It was a challenge that first year, but I loved it.

The first time I auditioned, I was so nervous. I sang the whole aria flat and then just left. Surprisingly, they were going to let me in anyway. But I had thought they weren't. I had made other plans, but the next year I got in. I was assigned three substantial opera scenes, between fifteen and twenty minutes long. Again, I was the oldest in the workshop, but I did pretty well and kept going back for six seasons. The workshop became the highlight of my year and, frankly, one of the reasons I could retire when I did is because I had something else to go to.

Then I got accepted into Friday Morning Music Club, which is a performance club for professional musicians, music teachers, and very serious amateurs. We gave lunchtime concerts in various places around the Washington DC area, three performers, twenty minutes each. I did a couple of those every year. I also had made a friend at the workshop and we would do recitals together. Then two summers in a row I signed with the Amalfi Coast Music Festival. The first summer, I had to sing Zita in Puccini's *Gianni's Schicchi.* I'm a soprano and Zita is an alto role, but because Zita is an old lady, as the oldest person there, I had to sing it. The second year, I sang Second Lady in Mozart's *The Magic Flute,* a much more appropriate role for my voice. And then I sang with some local groups and I sang some smaller operas. I sang Dido in Purcell's *Dido and Aeneas*, and I sang Lia in Debussy's *The Prodigal Son.* The last opera I performed was the title role in *Alcina*, a Handel opera that's not performed very often. It is really difficult, but I sang it and nearly killed myself. I was exhausted by the end of the last act. At that time, I was sixty-five, and I said, "Okay, maybe no more whole operas after this." But I still kept singing. When we planned to move to Georgia, six years ago, I thought I'd probably hang it up. As my farewell to Fairfax, I gave a big solo recital where I sang all my favorite pieces, arias, art song cycles, some duets. But after we moved here, I met a teacher I liked so I'm still singing. The audience for the kind of music I like isn't quite as big here in Roswell, Georgia, as

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it was in the Washington area, but I still sing. I've given a couple of lunchtime concerts of musical theater and one joint recital of classical music with a tenor I met a few years ago. I also joined an eighty-voice audition chorus, which has helped keep me busy.

Caldwell: Well, Karen, this has been both insightful and delightful. I'm so glad that you were able to sit for this interview. Do you have any closing thoughts or remarks?
Vaughn: This sounds like something you'd say at your retirement dinner, but early on I fell in love with economics. I mean, when I took my first economics course, I thought, my God, I've got the keys to the kingdom. I can now understand the world. Economics is a way of thinking. In every other part of your life, you can't help applying that way of thinking. And so no matter what happened in my life, that was important. I had my doubts for a while. I got to George Mason thinking, God, what am I doing in this awful place? And then it just got more and more and more interesting. Even when I had all my problems as chair, I still loved economics and I love the puzzles and I love trying to figure things out.

I never really thought I was a star at it, but I always felt I wrote something halfway decent. I actually have to say this, even though I retired in 2004, I'm still getting citations to things I wrote in the '80s and '90s. That suggests I've had some modest success.

I guess my closing word is, through all the ups and downs and all the times I was discouraged, I'm really glad I didn't become a real estate agent, I'm really glad I stuck with academics because it's been a good life. The academic life is perfect for someone like me. I liked teaching, I liked the research, I didn't like writing, but mostly I didn't like getting started writing. I loved the constant intellectual stimulation. And I am a person who has a very, very high demand for autonomy, and academics gave it to me, so I couldn't imagine doing anything else. Thank you for letting me talk about myself for all this time.

SUPPLEMENTARY MATERIAL

To view a bibliography of Karen Vaughn's publications, please visit https://doi.org/10.1017/S1053837221000213

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