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MD INTERVIEW

AN INTERVIEW WITH JAMES J. HECKMAN

Interviewed by

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James Heckman is one of the most important and influential scholars to have graced the economics profession. His work is deeply rooted at the intersection of economic theory and empirical microeconomics, and he has made significant contributions to the study of labor economics, microeconometrics, and the use of micro data in macroeconomic analysis. Heckman's work is motivated by the scientific method, in which theory is held up to the scrutiny of the data and empirical analysis is informed by economic theory. During the course of his work, he has made lasting contributions to the study of sample selection bias, duration analysis, heterogeneity, and treatment effects in microeconometrics. In labor economics, he has applied these econometric methods to the study of labor supply and life-cycle dynamic models of unemployment, wage growth, and skill formation. In addition, he is the leading scholar on the evaluation of active labor market programs. As an applied microeconomist, one cannot do research on labor supply, sample selection, duration models, or life-cycle dynamics without encountering Jim Heckman's work.

In addition to these seminal contributions to labor economics and microeconometrics, Heckman has applied his tools and methods to the study of the effect of civil rights programs on the economic progress of African Americans and the general equilibrium effects of investments in higher education. Heckman's research continues to flourish. Most recently, his research has embraced the interdisciplinary study of investments in children's cognitive and noncognitive skills. He advocates early interventions to improve the socioeconomic prospects of disadvantaged children.

To date, Jim Heckman is one of the most productive and widely cited economists in the profession. He has published or edited eight books and has published 276 articles, with several currently under review. According to the *Web of Science,* between 1975 and 2010 his articles have been cited 11,361 times,. He is ranked as the fourth most cited economist on RePEc. In short, James

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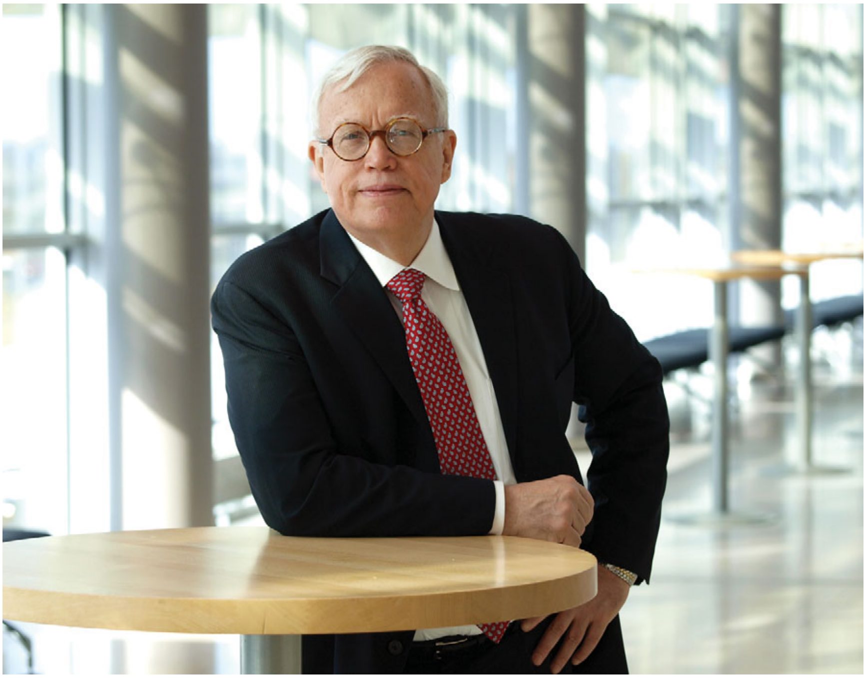

FIGURE 1. James J. Heckman, photo by Rolf H. Seybolt, 2008.

Heckman's impact on the economics profession has been enormous. Given his immense research productivity, it is hard to imagine that Jim Heckman has time for anything besides writing papers. The opposite is true—he is actively engaged with graduate students, having served as principal adviser on 55 dissertations and on scores of other dissertation committees.

James Heckman was born in Chicago and raised all over the country, was educated at Colorado College, and received his doctorate in economics from Princeton University in 1971. He is currently the Henry Schultz Distinguished Service Professor of Economics at the University of Chicago, where he has been a member of the faculty since 1973. He is Director of the Economics Research Center at the Department of Economics of the University of Chicago, Director of the Center for Social Program Evaluation at the Harris School of Public Policy of the University of Chicago, Senior Research Fellow of the American Bar Foundation, and Professor of Science and Society at the Geary Institute of University College Dublin. He directs the Pritzker Consortium on Early Childhood at the Harris School of Public Policy.

James Heckman's contributions have been widely recognized by the economics profession and the academy in general. He has been a Fellow of the Econometric Society since 1980; winner of the John Bates Clark Medal in 1983; a Member of the American Academy of Arts and Sciences since 1985; a Member of the National Academy of Sciences since 1992; a Fellow of the American Statistical Society since 2001; a Fellow of the Society of Labor Economics since 2005; a

Fellow of the *Journal of Econometrics* since 2005; a Fellow of the International Statistical Institute since 2008; a Fellow of the American Association for the Advancement of Science since 2008; winner of the Jacob Mincer Award from the Society of Labor Economists in 2005; winner of the Dennis J. Aigner Award in 2005 and 2007; a resident member of the American Philosophical Society since 2008; and a member of the National Academy of Education since 2010. In 2000 he was awarded the Nobel Prize in Economics for his seminal contributions to the study of sample-selection bias and self-selection models, duration analysis, and the evaluation of active labor market programs.

I interviewed James Heckman at the 2006 ASSA meetings in Boston and updated the interview in 2010. The initial interview began in the University of Chicago interview suite and was interrupted by a previously scheduled meeting. We concluded the initial interview later that day in a hospitality room at the meetings. We updated the initial interview by e-mail. Over the course of the initial three-hour interview, I was struck by Heckman's passion for the science of economics. His early experience studying physics has informed his lifelong approach to combining economic theory with rigorous econometric methods. As a scholar, he strives to make economics align as much as possible with the scientific method. I was also impressed by Heckman's intellectual drive and curiosity. As a Nobel Laureate, he has had the opportunity to don the mantle of a public intellectual—something he actively avoids. Instead, he is driven to pursue research on topics that interest him. The transcript was edited by both Heckman and myself. I have inserted footnotes for related papers that were discussed in the interview.

PERSONAL BACKGROUND

Ginther: Thank you for doing this interview. I would like to start with a little bit of personal background. I read your "Lives of the Laureates" essay [Heckman (2004)]. I was fascinated by the role of religion in your early life and I was wondering if you could talk a little bit about that and whether or not you have any involvement with religion today.

Heckman: Like many people, I have had strong religious influences in my background. There is a fundamentalist tradition in my family going back many generations and my parents were devoutly religious. Religion gives you a sense of certainty. It makes you feel that you have the right answers to really big questions and that you've grasped the truth. Losing that certainty, as I did by age 14, can be very disorienting. I have often wondered if some of my own passion for the truth in data has not just been what Freud would call "transference" (laughs). I have moved from religion to economics. I believe that there is a provisional truth. Not absolute unchanging truth, but a sense that there is a reality out there waiting to be discovered. There is something out there that can be understood with data and models.

I'm not opposed to religion. I have a lot of relatives who are devoutly religious. I wouldn't denigrate anyone who has any particular religious feeling. At the same time, I personally cannot accept religion as an aspect of my daily life as I once did when I was young. In my case, I found an alternative to religion in empirical research. When you're taught that something is the truth, and then begin to question that truth, it leads to a natural desire to have an alternative truth. So I think this helps to explain my passion for empirical truth, which is, however, an imperfect substitute for the sense of absolute truth given by religion.

Ginther: Throughout your career, you have been interested in issues related to race. I would like you to talk about how your interest got started on this topic as a young person and how it continues today.

Heckman: Race is a big issue for me. My interest arose from early experiences, when I moved to Lexington, Kentucky. I was born in Chicago and spent the first twelve years of my life in Chicago, with a brief sojourn in St. Louis. Then my parents moved to Lexington, Kentucky. This was in the last years of the segregation era—when "Jim Crow" laws were still in force.

Lexington was different from Chicago. It was an interesting old Southern city. Once, my sister and I were riding the bus going into town. I remember going on the bus and seeing a lot of black people at the back and white people at the front of the bus. It struck me as odd. My sister and I liked to ride in the back of the bus because the bus had a big bay window and we could see everything from it. We were told not to do that, that it wasn't appropriate. Then as we got downtown, we realized that there were separate fountains, separate benches. This amazed us. We didn't know about this.

The South was undergoing transformation when I encountered it, and Lexington had a Southern way of life—a Southern way of life that treated blacks and whites separately. I went to a segregated seventh grade, and my sister went to a segregated high school. The sharp separation stunned us. We couldn't believe it. The next year we moved to Oklahoma. We lived in Oklahoma City for a year, and the same separation of races was going on there. The Greyhound bus station had "White" and "Colored" fountains. If you went to the movie theaters, blacks were supposed to come in from a different entrance and sit in separate sections. This made an impression on me. It was my first awareness of the great American problem of race.

Anybody growing up in that era cannot forget that 1954 was the time of the Supreme Court decision that challenged segregation: Brown v. Topeka Board of Education. The whole South was caught up in the turmoil. I think it was 1955 when the buses were integrated, or at least attempts to do so by Rosa Parks and Martin Luther King were made in Alabama. Within ten years of that time, the racial system changed greatly.

The era of the 1960s was one of great optimism. Many thought that Lyndon Johnson's "War on Poverty" was going to eliminate all poverty, not just between blacks and whites, but throughout the entire society. One of my great surprises was the failure of many of its programs. In fact, many things really didn't change that much. Some things did change. Overt discrimination definitely ended. It was amazing how quickly that happened.

Much later I wrote about this change in a paper about South Carolina in Butler et al. (1989) and Heckman and Payner (1989). But I have to admit my ulterior motive. When I started that work on South Carolina, my motive was to follow the Chicago school line, which was that social change was not that important a factor in black economic progress. I thought blacks were getting more educated, and that there were secular forces that would have ended segregation without any need for government intervention. George Stigler had very strong political views that government could do little good. By the time I was studying the South Carolina data, I was on the Chicago faculty.

Any careful look at the data actually shows a big break in the time series, showing changes in relative black economic status around the time of the passage of the 1964 Civil Rights Act. There is no way around that fact. I spent years trying to show otherwise, and finally I said, "No, I can't do it."

Everything I do now is still motivated by race even if it's not directly mentioned. It's related to my work on cognitive and noncognitive skills. I wrote a paper on cognitive and noncognitive skills with two strong graduate students, trying to explain the black–white gap [Heckman et al. (2006a)] We broke the paper up and Urzua (2008) wrote up the black–white portion of our work. I think the race gap is still an open question. It's a fascinating issue and an important issue because blacks still have not been fully assimilated into the mainstream of the American economy. The gaps between blacks and whites on many measures are still enormous. I don't know how much of your life you've lived in an inner-city area?

Ginther: I have lived around the country

Heckman: Where I live now is close to some very poor neighborhoods. I'm reminded of the race gap daily. What bothers me is that there has not been as much progress at the bottom. If one looks at the median statistics based on data that throw away all the nonworkers, one can claim "median real black income is rising." Median real black income isn't rising if you count the dropouts. It's been flatter than a pancake for the last thirty to forty years. Median real income for black *workers* is rising, but the black labor force participation rate has been declining for prime-aged males and it's been stagnating for females. I think that raises a pretty interesting set of questions.

Ginther: So you moved a lot as a kid. What was it like to show up at a new school and always be the smartest one in the class?

Heckman: I am not sure I was the smartest one in the class, but moving around is a very educational experience. A lot of my moves occurred when I was in intermediate school. I saw worlds that I didn't know about. I spent most of my first 12 years in Chicago, then I moved to Lexington. I got to love Lexington—the Blue Grass area. My family moved to Oklahoma City and then to Denver. Denver is wonderful.

One thing you learn when you move so often is to be self-reliant. You break up with old friends, and you start with new ones. Being the new person in a school can be very hard for some people. I learned that I could adapt, but I could also change. On one move, I changed my first name. I used to go by my middle name, which is Joseph. People would call me Joe. My grandfather was named Jim. When he was alive, my parents didn't want two Jims in the same family. So when the time was ripe, I changed from Joe to Jim. Moving allows one to shed an identity and try new identities, which I did. I think it is a very healthy thing to move.

I especially liked Denver. In my high school years, Judy Collins was young and one could go see her at the coffee houses. There was an atmosphere that was very nice in Denver at the time.

Ginther: There is one more personal question. Some economists have sort of a partnership with their spouses in terms of research, I'm thinking mostly of Milton and Rose Friedman. Has your family had any sort of influence on your research? Or do you talk with them about what you do?

Heckman: Yes but let me qualify. Don't forget that Rose Friedman was a trained economist. She, Dorothy Brady, and Margaret Reid are among those who I would argue are the real discoverers of the permanent income hypothesis. I don't know if you knew that. If you read the permanent income theory [Friedman (1957)], you realize there was a group of scholars working with Brady, Friedman, and Reid who had collected large amounts of data on consumption and income.

Margaret Reid was an interesting person. At the time I got to know her in the early 1970s, she must have been in her seventies or maybe even early eighties. She was a tough customer. She probably put up with a lot of guff over her life. She had great empirical skills and deep honesty. She really believed in what she was doing. She did many budget studies, and found that the average relationship between consumption and income was highly variable, depending on how much variance there was in family income. The story that she told me herself, and Friedman confirmed, is that she went to Friedman and said, "we [her group] have these data and we have found this very strong empirical relationship. Is there a better way to organize the data?" Friedman had just read a paper by Durbin on errors-in-variables [Durbin (1954)], and he said, "You know we can explain it by an errors-in-variables model."

My wife is not an economist. She is trained in sociology. She worked on occupational attainment and social networks. Books by Jencks and work by Featherman and Hauser were around the house. She had many books on psychology and anthropology, too. Talking to her and reading her books and journals, I got a broad education in social science.

My wife is a tough critic. She's very smart, but until recently she has not been a co-author. She's not technically oriented. She probably is more inclusive in her thinking, so she really challenges me. We are now working together on evaluating an iconic childhood program in Reggio Emilia, Italy.

A long time ago, my wife and I were going to some event. We got into an argument and we were overheard. I was making some point and her rebuttal was, "Just because Milton Friedman said it is correct, doesn't make it correct." Her remark became known at the time among the students at Chicago. So she has always been a very tough critic, and in that sense, very helpful.

Other influences have entered my life: my son and daughter. My son is a postdoctoral research fellow in the physics of string theory at the Institute for Advanced Study in Princeton, NJ. Every once in a while, I ask him a technical question. My daughter worked for me on a project on the GED [Heckman et al. (2010a)]. She writes well and she is quite intelligent and has made some very thoughtful observations on the GED work. So I get stimulation from my family, but it's not like seminars at home. My wife actually tries to protect me from tendencies that probably would lead to seminars in the home and constant discussion of economics.

Ginther: It is always nice to have that. So what drew you to economics? I know that you were involved with Frank Oppenheimer and physics. Was it your interest in social issues?

Heckman: This is a good question. The Oppenheimer connection was accidental. [Robert] Oppenheimer led the group of Manhattan Project scientists that built the atom bomb. He was a celebrity in the 1950s. People respected scientists a lot more then than they do now. I think at that time that there was a societywide respect for scholarship, and academics were genuinely respected in the 1950s. The relevance of physics was evident in the breakthroughs in understanding and harnessing the atom.

His brother, Frank Oppenheimer, was my link to this world, and it was an accident that I got to know him through two high school physics classes. He showed me another world. My father didn't graduate from college. He went to one semester at the University of Oklahoma and that was it. My mother stopped at high school. I had no particular academic orientation. Then I met this guy who had a Ph.D. He not only had living links with people that I had heard about, but also brought them into our classroom. There was a famous book about physics called "One Two Three ... Infinity" by Gamow (1961). He and Robert Oppenheimer were two of the creators of the theory that nuclear fusion was the source of stellar energy. Gamow lectured to our high school class. Frank Oppenheimer knew many luminaries because he traveled in the same circles as his brother. He had been at Los Alamos. He had also been a rancher in Colorado for a few years because he couldn't get an academic job in the 1950s because he had been a communist. This was the era of McCarthyism and the Red scare. To my good fortune, the local superintendant of schools in Jefferson County, Colorado hired him to teach high school students at a time when he could not get a job at any college. There was a countywide competition to take his class. He taught twelve students from the whole county.

Oppenheimer took us to his home, put on Beethoven records, and would talk about the philosophy of science. He later went on to found the Exploratorium in San Francisco. Every time I go to it, I think I am back in my high school physics classroom.

It was an amazing world that he showed us. I had never felt so excited about ideas before this encounter. I thought I was going to do big time science. However, as I went on with my life, there was always something else besides physics that motivated me. I was very much interested in issues of race, and Oppenheimer himself was getting negative about physics. He said that the future breakthroughs in science would be in biology. I think he was correct. I thought about doing biology, but until recently it has never appealed to me.

Physics was very interesting. But I had the sense that some of the bloom was off the rose at the time. At the same time, I liked math. I did a lot of math in high school and college. I also found myself reading a lot of social science. I had the viewpoint at the time, especially during the early years in college, that I would do social science as a hobby. The serious stuff was the physics and the math, and I would be a physicist. At that time, there was a widely held perception of the physicist as a philosopher, as a statesman, and as a technician. By accident, I discovered economics. I was very lucky to have gone to a liberal arts college—Colorado College—that let me sample many different interests: philosophy, math, anthropology, literature, economics, and physics.

At Colorado College, it was common for students to be able to take classes as one-on-one tutorials, so I took a lot of tutorials. I enjoyed that format, so I took many small credit classes. For a lot of them I had to write a paper. I had one-on-one interactions with a very smart professor of economics, Ray Werner. I remember reading and rereading Arthur Lewis's book. It is "The Theory of Economic Growth" [Lewis (1955)]. I thought that this was social science at the highest level. It had political science, sociology, and a lot of economics—a unified theory. The problem of understanding economic development fascinated me.

In the 60s there was a belief that economics was a science and that economists could change the world. I don't think most people, even economists, believe that anymore. I think that most people have given up on that vision, which is sad. There was a sense then, based on the experience with the Kennedy tax cut, that economists could devise effective policies to control the business cycle. It looked like economists knew a lot about the economy. I was drawn to the power of economics. In the same course where I read Lewis, I also read Ricardo and Smith in the original and had many discussions with Ray Werner. We talked a lot. I got very interested in economics in his course and took some more economics. I ended up with a math major at college because it was easy and I had already taken many courses that qualified for the major. With math, you just take a few classes, write a few problem sets. But I got passionately interested in economics and social science. And I haven't turned back.

GRADUATE SCHOOL AND THE DISSERTATION

Ginther: Let's skip ahead to grad school. You started at Chicago but then went to Princeton. Did you go to Princeton because of Lewis?

Heckman: I went to Chicago for a year, and I was not ready for it. At the time, Chicago was a very big graduate program. I had gone to a small liberal arts school. The number of graduate students in the economics program at Chicago was probably two-thirds the size of my college class. It was a very impersonal experience for me. I also wasn't happy with how I was being trained. I had wanted to go to Princeton because Lewis was there, and I was very motivated by that. The professor who introduced me to economics said, "No, go to Chicago, it's much better; Milton Friedman is very good."

I really liked Milton Freidman. I took his classes, and he was kind to me. I did well in his classes. He was a very busy, very famous guy. Nonetheless, he would come to graduate student parties and would interact with us in groups after his lectures.

At that time, Friedman was doing battle with Keynesians and the prevailing viewpoint in economics, so it was interesting being around him. In my first (and only) year, I heard a lecture by Friedman on what we now know as the natural rate hypothesis. It was one of the most interesting lectures I ever heard. I had a long discussion with him about it after the class and, like a lot of students, I was very enthusiastic about this idea.

There were some negative aspects of Chicago. It was impersonal. A lot of the program was built around a cult of Friedman worship. I sensed overtones of religious devotion that repelled me. The development sequence at Chicago at the time was not so strong. I didn't take development in my first year, but I was looking to do so in the second year.

Living in Chicago at that time was very tough. I had lived in Colorado Springs, and when I had lived in Chicago as a child I lived out in the suburbs. Living in the inner city was a real shock at that time. I had never really lived in the inner city and that was a time when the inner city was going bad. There were National Guard troops two blocks away from my apartment, and there were riots going on. It was really a very stressful time.

I did not suffer much personally. I had some things stolen, but that was common at the time. I remember the sickening feeling during a prelim exam of looking out the window and watching a thief steal my bicycle.

I became attached to Harry Johnson, from whom I took a very stimulating course and who became a kind of father figure to me. We talked about the field of economic development. He was a big fan of Arthur Lewis and he said, "Chicago isn't going anywhere in development," which [in retrospect] I think was wrong. Ted Schultz had just finished his book, "Transforming Traditional Agriculture" [Schultz (1964)], but what did I know?

I didn't like Chicago, and I felt very happy when I arrived in Princeton. It was a smaller town, a safer town. I wonder if I should have stayed in Chicago, but I felt at the time that the training that was being given there was not so strong mathematically. I was a mathematician and everything at Chicago was very intuitive. Actually, there was nobody doing mathematical economics at Chicago except Uzawa, but he was leaving. There had been a tradition at Chicago of informal price theory, which in part is still there. It's part of the Chicago tradition, but at the time I didn't fully respect it.

There were many prominent physicists and mathematicians at Princeton. There were some really serious people in applied mathematics and economics. Kuhn, Tucker, Baumol, Feller, Quandt, and Morgenstern were there and I knew their work. The formalism there appealed to me.

Ginther: So I know that there were a number of people that you worked with at Princeton but I haven't identified who your dissertation adviser was.

Heckman: Well, that's a good question. For a variety of reasons, I have always felt myself an outsider. I don't know how to classify myself in economics. I am a loner. I do not like groupthink, which, if anything, has become more important in economics. In addition, a lot of the values I hold are not the mainstream values in the profession.

Let me be more precise. When I was at Princeton I had a lot of interaction with Richard Quandt. Quandt is very smart, and I learned a lot from Quandt. He gave me papers on transportation demand to read—which we now know as discrete choice. I remember being fascinated by his work.

I took Lewis's class at Princeton, and I got to know him very well. But as I got into development with him I began to realize that he was getting up in years. He was from a different generation, so the work that was so appealing to me two or three years before, his broad synthesis, I realized was not part of mainstream economics anymore. There wasn't a very strong empirical basis for what he was doing; all he had were interesting observations. He was a very wise man, but he was not very empirical or formal, and I had both empirical and formal interests.

There was a technical side to development at that time and there were people who were doing linear and nonlinear programming in development. Baumol was a big influence at Princeton at that time. Baumol and Gomory were doing integer programming. The development economists would write down integer programming problems for development that were technically very interesting. I took a lot of mathematical economics from some very good people. However, I began to realize that the mathematical economics of the time had little insight into the forces producing economic development.

I took a growth theory class, and I got very fired up on the theory of economic growth, which was distinct from the theory of economic development. As I absorbed this material, I realized that this work had little insight into why some countries grew and others did not. What surprised me later was to see the revival, in the 1980s and 1990s, of the models I loved in the 1960s and I had the sense of déjà vu that the models gave little insight into the true causes of economic growth. I took my general exams in development and growth theory and Lewis chaired my examination committee. Though I had been attracted to Lewis, I was slowly weaning myself off of him.

I started thinking of a thesis topic. I was committed to the study of economic development. I realized I didn't want to solve programming models. I did a lot of demography, too, because I thought demography was an important aspect of economic development. There was a brilliant professor of demography at Princeton named Ansley Cole. He was a legend at the time. He had done work on stable population theory models. He led me to the Lotka equation, an integral equation. It has multiple roots and the multiple roots govern the convergence of human populations to the steady state rate of growth. The first root is the steady rate of population growth but the higher roots, which are always in complex conjugate pairs, govern the rate of convergence to the steady state.

I spent about six months investigating the Lotka equation. One day I looked at my work and thought, "I am going back to mathematics in doing this work; this has absolutely nothing to do with a deeper understanding of the economics of population growth."

Al Rees was teaching a class in labor and I signed up for it. It turned out I was the only one who signed up for it and Rees was very hurt, since nobody was taking the class but me. He wanted to cancel the class. I pleaded with him that I wanted to learn labor. So we agreed to have a tutorial. This put me right back in my old undergraduate instruction mode. He gave me the worst grade I got at Princeton. He was tough, very tough, but he taught me a lot. For example, I pored over the human capital model by Gary Becker and Jacob Mincer (then at Columbia) that Harry Johnson had first exposed me to. Mincer came to Princeton and by chance I went to his workshop. His paper was on labor force participation over the business cycle—about added workers and discouraged workers. I was very taken with it. Somewhere along the line, I got deeply interested in labor economics.

I found labor economists were doing some really exciting things. There were real research opportunities in a new field to do formal work as well as serious empirical work. There was a group working on labor markets at Princeton. Black, Fair, and Kelejian were working on a model of the U.S. labor market. Bowen and Finegan were writing a book on labor force participation patterns across demographic groups. I talked to Bowen a lot, even though he was provost at the time. George Johnson, who was at Michigan, and Orley Ashenfelter were there working on problems of unionism and the economy. They introduced me to Gregg Lewis's work on the effects of unions on wages. There was an ongoing labor workshop.

The first negative income tax (NIT) experiments were being conducted by Rees and others at that time. I assisted in them. I interviewed people, and enrolled some of the first participants in the NIT. I became interested in the magnitude of income and substitution effects that govern participation in the NIT.

The War on Poverty was creating an edifice of evaluation at the time I was in graduate school. It fostered the creation of microdata to evaluate its programs. As a result, large amounts of microdata became available. The SEO survey, which was the first year of what we now know as the PSID, started around that time I was in graduate school. The NLS surveys also became available then. I did a lot of studies of the labor supply of women ages 30–44 to estimate income and substitution effects. From the first NLS survey, I learned a lot of interesting facts. I took a lot of classes with John Tukey, who was a legend, very empirical, very smart. I felt that I was doing science and it was also social science. Suddenly I felt labor joined with statistics was the way to go.

My committee consisted of very different economists, many of whom did not interact professionally with each other. Quandt was on my committee for a while, but then he quit because he read my thesis draft and said that nothing like it would ever pass his judgment. He thought my mathematics was too clumsy. Rees said he couldn't understand it. I was using Samuelsonian comparative statics models with bordered Hessians. I remember Rees sent me a letter saying he really supported me. However, he wrote that to his knowledge a Hessian was a mercenary hired by the British during the Revolutionary war. I was a little bit lost, and Ashenfelter was too young to be on my committee. He was still a student. He was an influence, although a bit of a rival too. The person who had the biggest impact on my thesis is a guy who's probably less well known: Stanley Black at the University of North Carolina. He just retired. Black was a Tobin student trained in macro. He was very smart and knew a lot of dynamic economics, and I was interested in life-cycle labor supply. He started talking to me because Rees said he couldn't read my work and because Quandt refused to read it. I migrated over to people like Stanley Black and Harry Kelejian. I think in the end Rees was my third reader.

So I really didn't work with any single person or follow the teachings of any mentor. But that is part of my style. The same factors that led me to break with religion—I don't like authority. I am a quintessential loner. I don't join clubs. I'm not accepted by them. I would probably refuse membership if offered it in most clubs.

My thesis was eclectic. I learned a lot of optimal control from Black. I was able to be much more precise about understanding life-cycle dynamics. He was a very tough, very good critic. He would read every draft I wrote and would rake me over the coals. I would work hard to make good drafts. If I had to thank any persons for help on my thesis it would be Black and Kelejian, but I had huge influence from Quandt. Ashenfelter and I started writing papers together when we were both graduate students. We wrote a couple of papers on labor supply that grew out of our interactions on my thesis. Kelejian taught me a lot of econometrics. Indirectly, through him, I became a student of Art Goldberger.

SELECTION MODELS AND LABOR SUPPLY

Ginther: So that takes me to your thesis and questions of selection. So how did you develop the idea for the selection problem? I know that the idea is a statistical component but there is also a theoretical component of the Roy model and I was wondering which came first, the statistics or the Roy model?

Heckman: Tracing intellectual origins is a difficult matter. I think the origin of my interest in the selection problem lies in labor economics and in particular, in my study of labor supply. One influence was H. Gregg Lewis, now a mystery man, a person who is totally forgotten in modern economics. Have you ever heard of H. Gregg Lewis? He was at Chicago for many years and retired at Duke. If you look at his papers (and you can, they are on deposit at the Duke library), you will see that he was deeply interested in constructing a general equilibrium evaluation of the impact of unionism on wages. He summarized his work in a book on unionism and relative wages [Lewis (1963)], which, as I previously noted, I learned about from Ashenfelter and Johnson.

When I was a graduate student at Princeton, Rees received some papers by Lewis on labor supply. Like Becker and Mincer, he was a pioneer in using price theory in the analysis of labor economics. I was very interested in labor supply because of my work on negative income tax issues. I remember reading Lewis's papers on labor supply as I was thinking about the practical problems that came up in analyzing my data. Wages were missing for nonworking women, for example.

At that time, analysts assigned the wage of people who were observed to be working to the nonworkers. We now call it a selection on observables strategy—a term coined in Heckman and Robb (1985). I became interested in how to solve this problem more generally, and got more and more interested in this question of labor force participation and observed wages.

Lewis, then at Chicago, had a series of postdocs there and made a strong imprint on them. There is a paper by Yoram Ben-Porath (1973) on the estimation of labor supply elasticities from labor force participation data. Mincer (at Columbia) had a model of labor force participation that I found very appealing. Mincer's papers, along with Becker's papers, had a huge influence on my decision to go into labor economics. Mincer's work on the labor force participation of married women [Mincer (1962)] was good economics and it explained data. To me it was a model of social science as science.

Lewis was unhappy with the way Mincer tried to estimate labor supply elasticities from labor force participation data. In order to get substitution effects, Mincer assumed that time was a perfect substitute over time, and that the interval sampled in measuring labor force participation was a random sample of the lifetime. I formalized the argument about when the Mincer approach would produce a compensated substitution effect [see Heckman (1978)]. I was a little disappointed that Mincer was never interested in my argument. He just said, "Ah, that's true." He was a very intuitive economist.

Meanwhile Lewis was also interested in this question. Lewis took a different view than Mincer. He wrote out the standard labor supply problem, the standard labor–leisure diagram with the marginal value of time at the zero hours of work position, and determined if the marginal wage was bigger than the reservation wage. Ben-Porath wrote his paper based on Lewis's papers on the interpretation of the substitution effect. Ben-Porath offered coauthorship to Lewis, who declined.

Lewis was a self-effacing person. When you look at his vita, he had about 20 published papers in his lifetime. He spent a lot of time doing administrative work at the University of Chicago. If you read his papers, you will see that he was clearly thinking very deeply, although he was not well versed in econometrics. He viewed himself primarily as a teacher. He was a very thoughtful economic scholar who influenced many young economists. Lewis's work influenced my research on the labor force supply of women.

One of my thesis essays is on the household model of labor supply, because I wanted to apply the standard theory of consumer demand to the household. Ashenfelter and I were reading Theil (1967), and reading all of the consumer demand literature. I thought, “My God, of course it applies to labor economics, and we should apply it.” We did so, in a series of papers [Heckman and Ashenfelter (1973); Ashenfelter and Heckman (1974)].

When I was a graduate student, I had a long conversation with Tukey in an effort to solve the problem of the missing wage data. I asked Quandt about this problem, and he said, “Go see Tukey.” I did. It was quite a traumatic event because when you saw Tukey, he turned his answers to questions into seminars. Tukey’s office had a gallery. Princeton had a very small statistics program. All of the statistics graduate students were summoned to his office, and I went to his board and stated my problem and Tukey and his group listened. I didn’t like his answer and told him so on the spot. His answer was very much like a standard imputation procedure. It ignored the selection problem, and I thought it missed the point.

I got very interested in the question of how to integrate models of labor force participation, hours of work, and missing wages into one unified framework. The first time I think I succeeded was 1972 [see Heckman (1974)]. I remember I showed this synthesis to Reuben Gronau when he was visiting the NBER when it was in New York. (Gronau was fresh from a post-doc at Chicago where he had interacted with Lewis on the labor supply problem.) I was then at Columbia and NBER. At the time, models like the Tobit model were not taught. Goldberger has a little section on the topic in his 1964 book [Goldberger (1964)]—Goldberger’s econometric theory book is a wonderful book on linear regression—but probit and Tobit were then fringe topics.

I had learned some of the material I needed from Quandt, because of his work on travel demand, which we later called discrete choice. Quandt was ahead of McFadden at the time, but he never systematized his work on discrete choice. I learned a lot from him and thought I could apply his work to the problem of labor supply. It’s amazing now how difficult it all seemed to be. I remember painstakingly constructing the likelihood for my paper “Shadow Prices, Market Wages, and Labor Supply” [Heckman (1974)]. It looks very primitive now, but at the time I was very, very pleased with it because of what I was able to do and what hadn’t been done before: to synthesize the hours of work decision, the labor force participation decision, and the missing wage problem all in one model, and with an economic interpretation. Even Mincer liked it.

What were the ingredients? One of the ingredients was the reservation wage and market wage comparison from the Lewis/Ben-Porath paper. But they didn’t have a good statistical framework. Gronau published a paper [Gronau (1973)], where he estimated the value of time but he only looked at participation decisions. Gronau didn’t analyze both participation and hours of work data, and he made some strong independence assumptions that were unnecessary.

I didn’t know Gronau that well. I’ll never forget our interaction at NBER in Summer 1972. It was a very interesting time for me. I felt I knew how to solve this problem. Gronau was very interested in what I was doing, and he was very helpful as a sounding board. Quandt had told me about Reuben Gronau, because Quandt was working on travel demand and Gronau's thesis became a major book on travel demand.

I owe Quandt a lot. Quandt's switching regression model framework was very closely related to the questions I was investigating. My interest in labor supply and his framework all came together in my 1974 paper. People had not formalized the intuitive diagrams on the shadow price of leisure used to analyze labor supply into a useful statistical framework. The previous analyses were all very intuitive, two-dimensional diagrams. I really wanted to know whether you could derive this theory in terms of rigorous economic and econometric theory. I wrote down a set of equations where I could derive the shadow price, got the market wage, solved for the inequality that characterized labor supply decisions, and then wrote out the right likelihood function. That was my entry into that whole question on selection. I view my own ideas as coming from the set of papers I studied from Lewis and Quandt and interactions that I had with a group of people at the NBER. My work in statistics came out of an economic problem. Everything I have ever done has always been motivated by an empirical or economic problem.

The Roy (1951) model was unknown to all of us. Although it was published in 1951, the Roy model got into the mainstream later. Willis and Rosen (1979) noticed that my work on labor supply was essentially isomorphic to the Roy model. The Willis–Rosen model used the econometrics of labor supply to analyze choices in education. I devised a simple way to estimate these models in a 1976 paper [Heckman (1976)]. I noted that the econometric framework applied to a variety of general problems and not just the labor supply problem. I put the selection problem, the Tobit model, and the dummy endogenous variable in a common framework.

My work on labor supply attracted a lot of attention. I left Columbia for Chicago in 1973. I got a tenured offer the first year after I was at Chicago, in 1974. This was in response to tenured offers from Columbia and UCLA. I was four years out of graduate school and I had several papers that were widely read. It was a different world at that time. It was a lot easier to get recognized in it. I thought a lot of Finis Welch, who was at UCLA, and, of course, Mincer, who was at Columbia. I decided I was going to leave Chicago because there was no chance of my getting tenure there. But I was told by several people at Chicago, Becker and others, that Lewis spoke out very strongly on my behalf, and his support was based on the '74 paper. He saw it as an advance, and I was very proud of that. I got tenure and then immediately Lewis said, "You should teach my class." It was based on his book on unions [Lewis (1963)]. I remember learning from that book what we now call the correlated random coefficient model. It was in a footnote in his book. I remember reading it and saying, "My God, I have never seen anything like this before." I wrote up a bunch of notes on his model in the early to mid '70's, which I only later published in a JHR paper in 1998 with Vytlacil [Heckman and Vytlacil (1998)]. It took 20 to 25 years before I published it—surely a career mistake.

My work on correlated random coefficients was done after reading Lewis and formalizing his ideas. A lot of credit belongs to Lewis, who was looking at this problem in the study of unions in a very basic way. Lewis also has a version of what is now called the "Reflection Problem." If you read his book, the notation is horrible. It's very difficult for people to read his work and understand it.

SELECTION MODELS, INSTRUMENTAL VARIABLES, AND THE MARGINAL TREATMENT EFFECT

Ginther: What do you think about the selection problem now? You have your first formulation in your '74 paper, and then you have gone on to develop things like the propensity score, and looking at treatment effects. Then there is this other strand of literature on instrument of variables—sort of a Cambridge approach. So how has your view of the problem changed?

Heckman: I am glad you asked that. I've been doing a lot of work with Ed Vytlacil [Heckman and Vytlacil (1999, 2005, 2007a, 2007b); Heckman et al. (2006b)]. In the general case, the instrumental variables (IV) model and the selection model are the same [see Heckman (2010)]. I think it took me longer to see this than it should have. But if you go back to the original selection model and then look at some outgrowth from that selection work, the link is clear. There is a paper by Björklund and Moffitt (1987) that didn't get much attention when it was published. There is a very interesting economic idea in the paper.

Björklund and Moffitt (1987) consider this question: when you change the costs of entry to a choice state, what's the marginal gain to somebody induced into the state? It is a very interesting paper. And then came along Angrist and Imbens's work on IV [Imbens and Angrist (1994); Angrist et al. (1996)] Those papers are antagonistic toward the selection model and toward economics. They mischaracterize the selection model and the selection model vis-à-vis the IV model. I wrote a paper in the JHR in 1997 [Heckman (1997)] on understanding IV and on the LATE (local average treatment effect) literature.

I pointed out that there are a lot of cases where the parameter that was being estimated by IV didn't mean much. I don't think that my effort was very successful. However, what came out of it was the notion of taking a derivative to interpret the IV estimate. If you take a derivative of the outcome function with respect to an instrument, you can estimate the marginal return to the policy. It turned out that that is more or less what Björklund and Moffitt were doing in their 1987 paper. This point was made forcefully in a comment by Moffitt (1999) on my paper with Vytlacil (1999), which greatly improved on my 1997 effort.

Ed Vytlacil and I, in an effort to understand LATE, developed our own version of the Björklund–Moffitt approach, called the marginal treatment effect (MTE). The marginal treatment effect we develop is very close to the same derivative that Björklund and Moffitt were taking. In fact, the recent IV literature is estimating or approximating the derivative of a selection model. IV is about derivatives, and the selection model is about levels. They are related by integration, so they are the same model, with the difference being that with the IV model you lose some constants crucial to the selection model when you differentiate. If you want things like the average treatment effect, things that require you to know end point conditions and initial conditions, you have to fix them up. The selection model picks them up by taking limit arguments, or by invoking other assumptions. These are testable, however [Heckman and Urzua (2010)]. LATE answers a more limited set of questions. LATE uses finite differences instead of derivatives, but in the limit, in regular cases, LATE and MTE coincide.

If one is interested only in the marginal treatment effect, one can go back and use Björklund and Moffitt, but that requires estimating all of the components of the full selection model. Björklund and Moffitt set up and estimate a standard selection model, differentiate it, and generate the marginal effect. Vytlacil and I estimate it directly by local instrumental variable methods. This is an application of Marschak's Maxim [Heckman (2010)].

Accounting for selection on the basis of heterogeneous returns is a major topic. This was what Lewis was thinking about in his 1963 book. His message got neglected. Recent work revives and extends his insights. Selection models came out of economic theory. The IV approach does not use economic theory. The difference between the approaches is in the use of economic theory to interpret the evidence. My own view is that if you don't trust economics or know it, then you tend to be agnostic about it. I don't believe it's possible to make progress in empirical research without using some aspects of economic theory. Yet many empiricists don't like this approach. I don't believe that a purely empirical approach to economics has a lot of interpretive value. I recently wrote a paper [Heckman (2010)] showing how the LATE model of Imbens and Angrist (1994) can be extended using the economics implicit in it. LATE as formulated in the literature is an instrument-dependent parameter. Proponents of LATE are not clear about exactly what they are estimating or what policy relevance it has. In my 2010 paper, I show that the common interpretation of LATE—as the gross effect of a change in an instrument for those induced to change their treatment status by a change in the instrument—is not, in general, correct [see Heckman (2010)].

Research on the selection problem is ongoing. We all are constructing counterfactuals every day. There is no general rule for constructing counterfactuals. The way that I was constructing counterfactuals for missing wages was to say, "I'm going to write down an economic model on participation and hours of work and I am going to use the cross-equation equation restrictions, and I will use the fact that I know the wages for some people to fill in the wages for the others for whom I do not have wages, and at the same time account for possible selection biases." It took econometricians 25 years to realize how inessential normality really was to the entire selection enterprise. I wrote a paper with Bo Honoré that came out directly of a classroom lecture [Heckman and Honoré (1990)]. Bo and I started interacting and we wrote that paper on the Roy model in '84, '85 when he was still a student at Chicago. It was very edifying to me that we could get rid of normality and still analyze the selection problem using economics. [See also Heckman (1990).] That is the story of science. Science proceeds by understanding assumptions and relaxing them. The recent semiparametric and nonparametric work in econometrics has been very fruitful in asking and answering the question, "Where do you get your source of identification?" Whether or not it's useful in practice is another issue, but it is certainly useful in understanding sources of identification.

Ginther: Yes, that is a real issue. So what do you think of all of this emphasis in labor economics on natural experiments?

Heckman: I think I have influenced this reaction [see Heckman (2010)]. In the late '70s and early '80s there was a strong emphasis in labor economics on the application of selection models. The later work by Hausman on taxes and labor supply popularized it in public finance about five years after it was developed in labor economics [see Hausman and Ruud (1984)]. The problem was that early investigators (including me) made strong distributional assumptions that sometimes made the estimated empirical models at odds with the raw data. It turned out that normality is about the worst assumption you can make in the study of labor supply. There is a big heap of hours worked at 2,000 hours a year and bunching at forty hours a week. There were many examples of crazy estimates that were being produced assuming normality. John Pencavel's survey on male labor supply (1986) shows the wild implausibility of the estimates of certain structural estimates. In a '76 paper I had some labor supply elasticities for women that were 16. That was nuts. I never endorsed them, but Killingsworth made hay out of these numbers in his book [Killingsworth (1983)].

There was a retreat from structural economics and in the retreat and attack on the selection model, everything got bundled together in the minds of IV advocates: normality assumptions and economics. It wasn't understood that you could relax the normality assumption and still keep the economics but that required more analytical work. I have written on this overreaction in Heckman (2010).

The natural experiment literature offers what is apparently a very easy answer to hard questions. Clean answers can be obtained by comparing outcomes of interventions, before and after the interventions occur, using difference-in-differences estimators or else using IV. The new paradigm is the experiment. The language of treatment and control became widely used. My guess is that the vast majority of economists under the age of forty don't know much about income and substitution effects. It's not taught much these days. Price theory is not taught. However, despite its retreat from economics to statistics, the natural experiment literature has made some useful contributions.

Starting in the late '60s and continuing to this day, economists have had access to rich sources of new data. Microdata became widely available at the time I was in graduate school. The PSID, NLSY, and other data sources were being produced and their analysis raised all kinds of econometric problems. Some of the early models were very easy to interpret economically. There was always a requirement that that the numbers should make sense.

I think the natural experiment literature provided a very healthy focus on data analysis for the profession. For certain questions you can find simpler answers. It undermined the structural literature, but it also complemented it. However, in the program evaluation or natural experiment approach, the link to economics has gotten very obscure. Many economists aren't interpreting evidence using economic models. I have recently written on these topics [Heckman (2010)]. If you are interested in very simple, focused questions, you can often get by with much simpler answers, and you don't need to estimate a full structural model. But if you are interested in very elaborate questions, then you generally need to estimate a structural model. Analysts need to state the question being addressed clearly. If one states the question of interest clearly, one can develop a clean answer. If you need assumptions to answer a question, you should be very explicit about those assumptions. I think the natural experiment movement is great in terms of providing new data. It's not so great in providing economic interpretations for what is done with the data.

CONTRIBUTIONS TO MACROECONOMICS

Ginther: That brings me to a bigger question in terms of macro models and the issue of econometrics in calibration. Since this interview is for *Macroeconomics Dynamics* I thought we should have a question about macro.

Heckman: I wrote a paper with Lars Hansen about 14 years ago on calibration [Hansen and Heckman (1996)]. Then with Martin Browning, Lars and I wrote a chapter for the *Handbook for Macroeconomics* [Browning et al. (1999)]. I am optimistic about macroeconomics. I'm more optimistic about macroeconomics than I am about most parts of labor economics these days. Macroeconomists are still very interested in economics. Some people say they are interested in economics to the exclusion of data. That is an overstatement, as applied to most macroeconomists. A group of younger macro people are interested in agent heterogeneity and data. A whole set of empirical issues, including asset pricing anomalies, questions about savings behavior and labor force participation, entrepreneurial activity, and so on, all seem to point to the importance of identifying subgroups in the population. There is increasing interest within macroeconomics in integrating micro data into macro work. I view this enterprise as way of enriching both fields. Many macroeconomists are doing serious micro data analyses, going beyond "treatment effects." They are interested in determining the parameters that can be plugged into macro models to do credible policy analyses. I am very optimistic about this line of work.

On the other hand, some of the calibration analyses in macro have moved away from doing any kind of serious empirical work. In some quarters of macro, calibration is an excuse for not doing serious empirical work. Attending the seminars of the calibrators, I'm reminded of the old tent meetings that I participated in as a child. People are sitting around saying, "this is a set of parameters that are agreed on," so it's a little bit like saying Genesis 26:3 says such and such. They say, "A has said that this is the right parameter," and "B said it's the right parameter," but that doesn't make it the right parameter except by an appeal to authority, which, as Bentham remarked, is the lowest form of argument. Economists need to study the data closely and not casually.

I've been to several conferences in the last few years where macro- and microeconomists are now meeting together and exchanging ideas. I have some work with Lochner and Taber [Heckman et al. (1998)] fitting empirical general equilibrium models. We used a lot of micro data but integrated them up to a macro model. This approach recognizes heterogeneity and produces a much richer macroeconomics. Macroeconomics will be advanced by incorporating evidence-based heterogeneity, because we know there's lots of heterogeneity. The question is which aspects of heterogeneity are most important. I very much enjoy working with empirically serious macroeconomists.

I value greatly my interactions with Lars Hansen and Rob Townsend. We have seminars, and some of the students we produced in the last few years, like Salvador Navarro, Flavio Cunha, and others, have been heavily influenced by our interactions. For example, we analyze the choice of schooling using rigorous macroeconomic models. We examine the role of credit markets. We study what is in the agent's information set. We use micro data to infer how much agents know about their future outcomes. I have a paper with Flavio Cunha, in which we look at how much uncertainty has increased in the U.S. economy, using economic choice models and schooling choice models [Cunha and Heckman (2007)]. A prototype is my paper with Cunha and Navarro [Cunha et al. (2005)]. There is a lot of interest in that topic in macro, and I think problems like these are great stimulants to research in micro. I see macro as a way to take labor economics back to doing economics. Macro theory is a very rich body of ideas that can be fruitfully applied to micro data.

The analysis of the household, the kind of work you're doing, still has strong economic foundations. I wouldn't call it labor economics anymore. Household economics has branched off on its own. It hasn't gotten atheoretical as much labor economics has. In household economics, there is still a real emphasis on interpreting models, interpreting the data, and using economics. We all know that the data will never speak for themselves. At the same time, we don't want to impose too many assumptions on the data. But there is always the question about just how much the data can speak for themselves and how much theory you have to impose to answer interesting questions. This intellectual tension makes an interesting back-and-forth between micro and macro, and I'm very optimistic about where it will lead.

That is why Hansen, Browning, and I wrote our chapter. I have had a high level of interaction with macro students and many are very good. This interaction enriches the study of labor markets by putting more economics into it along with more data. But I'm concerned about labor. A lot of the younger people coming out in labor can't even read Lewis. They've never written down a constrained maximization problem. Even if they did so in their first days of graduate school, they don't use it much in their research. The link between theory and economics and the data has probably weakened in labor over the past twenty years. Do you think that is too pessimistic?

Ginther: Actually I don't. I think there is a sort of a premium on the clever instrumental variable or the unusual natural experiment. I think there is a big return on publishing something clever.

Heckman: I have a view about that, and of course I am surrounded these days by the cute, the clever, and the coy. The "cuteness" line of research doesn't cumulate. I learn that students and teachers cheat on high stakes tests. That is an interesting fact. I would like to read it in the *New Yorker* or the *New York Times*. That doesn't help me too much in understanding the larger world and what would be surprising is if they did not cheat. I think what's missing from cute economics is a vision of a larger body of science. That's why I wonder if my time has passed in labor economics. I don't think it's passed completely because the macroeconomists still believe that there is a body of theory and they're right. But in much of labor economics, there doesn't seem to be that much interest in economic theory, although there are many serious empirical economists not doing cute economics and working on questions of major interest. However, many practitioners these days spout the same old arguments and iterate the same old questions. The intellectual quality of the group as a whole seems lower. I don't want to sound bitter or unhappy, but I think that it's very important to actually link economics with data, and many labor "economists"—really labor demographers—do not feel that way.

GRADUATE STUDENTS AND TEACHING

Ginther: You have an army of former and current students. What is your perspective on training them and how do you move from teaching them to co-authoring with them?

Heckman: I think the most satisfying part of my academic experience, my life after graduate school, has been my interactions with students and colleagues, but especially with my students. I have a great joy in seeing them develop and mature. I also find them to be extraordinarily helpful in testing and questioning my own ideas, and in generating solutions to problems of mutual interest. I had, and still have, an impressive array of students. I've learned something from almost all of them. I value these interactions tremendously. I remember sitting with a graduate student in a seminar room in the economics department around one o'clock in the morning, and we were going to the board and working on some solutions to some identification proofs in a dynamic programming model. He had just taken a job at a good school, and he said, "Gee I'm going to miss this," and I said, "I'm going to miss it too." It's really a great source of pleasure for me to see these students develop into powerful research economists. These interactions are the best way of teaching. I will work with any student who is curious on any topic—it is much more interesting than classroom teaching.

Classroom teaching is very formal: theorem A, theorem B. Working with students is really learning by doing and by iterating. Many students have a wonderful curiosity, which enlivens me. A colleague and a co-author will think about a problem nonstop and you'll get e-mails at three in the morning and phone calls with excitement and that is worth a lot.

Ginther: So do you spend much time in the classroom?

Heckman: I've been teaching a couple of classes a year for the last few years at Chicago, since I have some time released for research. It partly depends on what you call "teaching" and "the classroom." I spend a lot of time working with students on their dissertations. Students come in to talk even when I'm not on their committee. Anybody who wants to talk to me, I make a point of talking to them. I don't know if you count that as teaching or not?

Ginther: Yes, I would.

Heckman: So then I spend a tremendous amount of time teaching. I like teaching one on one. You get somebody who is working on a very interesting problem and they raise all kinds of interesting questions. These are smart people; they're curious people, they're wonderfully intelligent, vigorous, and intellectually honest. They have not become jaded. The one thing I encourage in my office, which is very hard for some students, is to be honest in their criticisms of me and my work. Some of the students who come from countries where a tremendous amount of respect is paid to professors have very great difficulty in living up to the code that I try to enforce. That code is to speak very bluntly, to point out if I have made mistakes and be honest both in class and on the blackboard.

Ginther: So given the number of students that you work with, your schedule, I'm sure you're traveling a lot, how do you produce so much research?

Heckman: First of all, my interactions with students lead to published research. I travel too much and I regret it. In fact I'm doing everything in my power to cut back. There is a superficial aspect to much of this travel. I give many talks. Sometimes these talks are publicly useful. But there is also a sense in which many venues are not intellectually serious. I'm increasingly cutting back on these. I spent a couple years in the early part of this century after I won the Nobel Prize traveling around giving talks. I remember during this time I was at NYU where I gave an academic seminar and my wife was with me. It was one of the few academic seminars I had given that year. It was really great. People questioned me, saying, "That is wrong," I felt good. My wife said, "Look, you should have fewer speeches and more seminars," and I have now vowed to do that. I have cut back the speeches; one can make money giving public talks but that sort of activity is often very unsatisfying. If my goal in life was to make money, I would not have become an academic.

THE NOBEL PRIZE, PUBLIC INTELLECTUALS, AND ECONOMICS AS IT IS CURRENTLY PRACTICED

Ginther: So it seems like you're a bit uncomfortable in the role of a public intellectual.

Heckman: First of all, I think most public intellectuals are frauds. I know there are people around who believe they have answers to almost every question. I wish

FIGURE 2. James Heckman receives the Nobel Prize in Economics from His Majesty Carl XVI Gustaf, King of Sweden, December 2000 Nobel Ceremony. Photo by Eugene Pettler.

that were true, that they did have valid answers. They don't and I don't. When I have something to say, I try to speak. I am lucky that I have a chance to speak when I do have something to say. Partly because of my interest in empirical work, I'm not so quick to go public and speak out on the basis of a purely theoretical analysis untouched by data. I like to have evidence. I'm not a public intellectual and I would never pretend to be.

Ginther: There seem to be a lot of our colleagues in the profession who have embraced this.

Heckman: I think it's a dangerous trend, and I believe that they will have a long-term, negative effect on the future of our field. These people are frequently talking off the tops of their heads. What's interesting has been how much the press has embraced economics. I'll give you an example. I won the John Bates Clark Medal about 27 years ago, and there was no press coverage of the event whatsoever. In fact, I didn't even know I had won until Bob Gordon called me up on a Friday night saying I had won the medal. I thought it was a joke. So I spent the whole weekend, with Jose Scheinkman, trying to track down if I actually won. Gordon went out of contact so I spent the weekend thinking, "Is this a joke?" The medal wasn't covered the way it is today. Nowadays there are *Wall Street Journal* articles, *New York Times* articles: who's going to win the John Bates Clark Medal? When I won, it was a very low-key affair.

The scale of publicity and the interaction with journalists has increased in our profession. Economists have gone public. Economists are making bold and often unsupported statements. The amazing thing is that the public listens to some of these people. In truth, they really don't have much to say. But how would a journalist know that? As a group, journalists are very ignorant people. They don't have a clue about economics. They want somebody to give them something smooth and simple. The danger is that there is an ample supply of people willing to give simple answers to hard problems. This depreciates the reputation of economics as a serious empirical science. I am not a party to that. I could be, you could be. There are lots of forums that I have turned down routinely. Within a year or so of winning the Nobel Prize, I realized that what I wanted to do was what I had been doing before I won, which was basically being an academic and discussing ideas and talking with people whom I respect, not masses of people who are impressed by some award.

Ginther: So that leads to my follow-up question. My husband once asked Doug North what the downside was of winning the Nobel Prize, and it seems like it was, "You have this unanticipated role that was thrust upon you."

Heckman: Well, I think it gives you a great opportunity to make a fool out of yourself. I think there is a sense of responsibility that you have to have. I don't want to be in a position of telling anybody that I should or should not have won the Nobel Prize. I'll take the Prize as something that was given with respect. I think it was recognition for a body of work that I participated in. I said that in my acceptance speech. There was a large body of work on microeconometrics that was recognized in giving the award to me. The citation of the committee for both me and Dan McFadden read that way. It wasn't the two of us alone who created microeconometrics. There was a large body of excellent scholars and I'm sure that there will be other work in this area that gets recognized by the Nobel Committee.

There is a responsibility that goes with winning the Nobel Prize, and the responsibility is that if you have a forum you should use it wisely. But I don't like the publicity of it. I used to get letters, "You're a great man and you've done wonderful things," [from] people who couldn't possibly have read my papers, who had no knowledge of me or my work. It was so insincere, that it became sickening.

I was in Italy a few years ago, going to a conference, traveling by train from one city to another. My host introduced me to some people sitting on our train and told them that I was a Nobel Laureate. This was very distracting. Do you know what Faulkner did when he won the Nobel Prize in literature? He went on a hunting trip in Mississippi for about two weeks with his hunting buddies. None of them even knew what the Nobel Prize was. They left him alone. I understand fully his actions. I value my privacy and the opportunity to learn, to exchange ideas and to participate in interesting worthwhile activities.

The plus side of the Nobel Prize is it has given me an opportunity to work with some really good people. It has opened doors to new work. I think that some people see it as being to their advantage to associate with me. They also take my work seriously, and I appreciate that and all of the feedback and criticism. I view that as a plus—a very big plus. I think there is also a very big risk in all of this of overcommitment. The Prize definitely extends your productive working life and opens up doors.

One of my colleagues at the University of Chicago, Ted Schultz, was a very wise man. He won the Nobel Prize in 1979 at the age of 77. He was near the end of his string at 77. I remember just a few months before he won his Prize that one of his grants was cut. I remember going to his office and patting him on the shoulder telling him not to worry about it, we'll take care of you and so forth and so on. He got the prize shortly afterward. Then he was rejuvenated for about 15 years. The same happened with Hayek. So the Prize is rejuvenating and it does give you a bigger forum.

I have invested the harvest of the Nobel into the work I am doing now in the economics of early childhood and noncognitive skills. I am using my Prize to try to promote scholarly work in this area.

Ginther: So a couple questions about economics in general. Is economics a science?

Heckman: By and large no. I wish it were and I think there is a group of us who have a vision of economics as a science. I hope that economics will become a science. When I see some of the popular books that are out there, the cute papers, the *New Yorker*–style articles that have become common (they're perfect for coffee tables but not for science labs), I get depressed about the future of economics. And I get depressed about some of the younger people who get drawn into being

FIGURE 3. The University of Chicago's four current Nobel Laureates in Economics: James J. Heckman, Roger Myerson, Gary Becker, and Robert Lucas, at the Reynolds Club at the University of Chicago, October 15, 2007.

clever or cute. Cuteness sells in some quarters. Journalists like it. The cute and the coy seem more prevalent than in the past.

I'm lucky. When I entered economics, there was a belief that economics was a science. Like I was saying earlier, with the success of the Kennedy tax cuts in the early 1960's, it seemed that economists could solve the problem of the business cycle, and knew a lot about the economy. The word used then was "fine tuning." It was thought that economists could solve many social problems. The Nobel Prize in economics was established in that era. That pretense to knowledge later blew up in our faces. What has happened in a lot of areas in economics is either degeneration to cuteness or formality; formalism has replaced science in many quarters. Both responses evade economic reality.

Formal economics solves problems, gives logically consistent solutions, and is happy to stop at that. I respect this approach because it is intellectually well defined. It sharpens thinking, can guide empirical work, and can suggest hypotheses. Economic theory is very important. However, some if it is like philosophy, it's like mathematics. It's very intellectually interesting, and I learn from it, but it's not science because it's not making contact with data. However, when it stimulates empirical work, it is science. On the other hand, some people who make contact with data view empirical analysis as an exercise in the cute: producing something scintillating, but not something with lasting value. It's not science. It's journalism.

Ginther: So your interest in the profession if you were to move it forward would be bringing theory to the data or data to theory?

Heckman: The danger of theory without data is that it becomes—it is trite to say this but it's true—like the old scholastics talking about the number of angels on the head of a pin. Fields become sterile, inbred, and self-referential. This is happening in many fields, not just economics. I don't deny the intellectual value of pure theory. I would never tell anybody not to think deeply about something. However, I would make a distinction between science and theory. I would say that any good theory eventually is going to have to be anchored in data, looking at real phenomena and trying to explain them. I am very tolerant of even the crudest theory if it's trying to make contact with data.

Every time we make advances, we know we have to make approximations. I would hope that more people would admit that they were making those approximations, and would be more modest about what they have achieved. I wish the reporting style in economics was different.

My vision for economics when I left graduate school was patterned after papers in physics. You start off with a series of analytical results. You say, "I showed this," and then at the end of the analysis, you give a self-critique saying but what you really need to do is A, B, C, D, E. I remember one of my famous colleagues at Chicago saw me adopt this style and said, "You know you're destroying yourself with these conclusions because everybody is going to read the paper and say yeah, that's all pretty good but for A, B, C, D, and E." I said, yeah, but that's the way that it is. I wish there were more A, B, C, D's, and E's. If economics were a science there would be less breast beating and we would say, "Let's not kid ourselves, we haven't really settled all these issues but we've made a little bit of progress." That is the most one can ever hope for. To get some little, tiny progress is amazing if you can do it. It's wonderful if you can do it. Instead of pretending that we've explained everything with each paper, we should say, "We've explained something, and it feeds into this larger enterprise, but this is rock solid and the rest is speculation, and let's be clear about the difference." If this were the style, evidence across papers would accumulate as in science.

Journalists would never like this style. They don't want the qualifications. They want sound bites. If you go on television, journalists want at most thirty seconds, one minute; tiny sound bites. That's deadly to the process of thinking, and to science. In the end it's deadly to scholarship, and to economic policy. People aren't going to take economists seriously if the trend continues. Some economists run regressions and make very bold statements based on a cursory analysis. One may get thousands of citations for such claims, lots of press coverage, and then it's forgotten because it's fragile. More careful scholars say, "Well wait, can we really conclude this?" and they test and retest the results, and usually discredit or substantially qualify them. In contrast to the scientific approach, many empiricists in economics are in a hurry to publish and to get press.

Epidemiologists, for example, say that if you eat grapefruit and cereal, you lower cancer risks. The next week they say no, this practice raises the risk. That kind of epidemiological analysis makes a short-lived splash that hurts the reputation of that field in the long term. The same is true in economics. We read that abortion reduced the crime rate. That made a huge splash. But then more careful analyses reveal that it is an unfounded claim. Journalists reward the fast, the dirty, the cute, and I think that is harmful. They find it hard to understand serious analyses and seize on tidbits that are easy to understand and titillating even if they are wrong. I hope that this trend will pass. It hurts the credibility of economics.

RECENT RESEARCH ON NONCOGNITIVE SKILLS

Ginther: So two more questions. What is specifically your research on noncognitive skills? This is very interdisciplinary. You've gone on to work in developmental psychology and sociology, and other disciplines. What is your definition of noncognitive skills and why are you focusing on it so much?

Heckman: That is a good question. First of all, I find my interactions with psychologists to be very stimulating. It's not that I'm trying to escape economics. Right now in economics there is a group of people doing economic psychology. A lot of this is behavioral economics and it's all about nonstandard preferences and choices that people make. That is interesting work.

My perception has been, though, that there has been a real neglect of developmental psychology, which is viewed as a softer subject by the cognitive psychologists who command the heights in psychology. Developmental psychologists are people worrying about babies making "goo-ing" and "gaa-ing" sounds and to many it looks like a really soft subject. Actually, developmental psychologists have a lot of hard knowledge. It's mostly empirical—there are few analytical models. Developmental psychologists and personality psychologists have a lot of information that is useful for economics, and organizing that information in formal conceptual frameworks is a valuable task.

Economists have much to learn from both fields. These fields shed light on the race question we talked about earlier. Human skills matter. The work I did on the GED a few years ago with Yona Rubenstein [Heckman and Rubinstein (2001)] was very informative for me because it showed that some aspects of human character are not captured by the standardized tests we use to measure success in our schools. People who take the GED are essentially as smart as other individuals. They are as smart as high school graduates, but they are earning the wages of dropouts. So that analysis gave me a tip-off that something besides cognitive skills was important in the workplace.

I started a series of studies that I am continuing to this day. The work is culminating in a book [Heckman et al. (2010a)]. I believe that this research is important. I think it's helpful in explaining differences across people. "Noncognitive skills"—the term itself is too inclusive to be useful – embrace personality traits, aggressiveness, time preference. They also include health. The full range of relevant noncognitive skills remains to be mapped out. But this bundle of traits, aside from cognition, has predictive power. Another aspect of my interest in psychology is understanding the importance of early childhood years. The family is the major source of inequality in American society, in most societies. Right?

Ginther: Right.

Heckman: Early intervention programs enrich adverse family environments. The largest effects of the early intervention programs are on noncognitive traits. Now what do I mean by that? I mean perseverance, motivation, self-esteem, and hard work.

A key exhibit is the Perry Preschool Program. The Perry project was an intervention with kids who were disadvantaged. All were three to four years of age. They were all subnormal IQ, they were all African American. The program taught cognitive skills, but it also taught children to plan and execute projects, and to get along with others [Heckman et al. (2010b)]. The study followed kids to age forty, using randomized trials to evaluate the effect of the program. IQ was not raised. But the behavior pattern of the treatment group was changed.

These interventions operate primarily through changing aspects of social and emotional skills, motivating people, making them easier to work with in class, integrating them into the larger society. This is a major finding. Interpreting and recognizing that capabilities can be changed by interventions and that the programs operate mainly through raising noncognitive traits should change a lot of thinking in conventional labor economics.

Test scores are the focus of many public policy evaluations. Look at Herrnstein and Murray (1994). Herrnstein and Murray were using AFQT [Armed Forces Qualification Tests] as a measure of IQ. If I were to criticize Herrnstein and Murray now [I did write a critique of them fifteen years ago, Heckman (1995)], the major addition to my previous critique would be that they used AFQT, a measure of achievement test scores used by the Army, as a measure of IQ.

These scores are affected by the level of schooling attained by an examinee and other parental environmental variables [see Hansen et al. (2004)]. They are also affected by motivation—both the motivation students had to take the test and do well on it, and the motivation to acquire the skills that produce higher scores on that test. The old human capital literature talked about ability versus genes. We've learned since then that test scores have a very high environmentally determined component. There is a whole body of literature in economics that shows this [see Borghans et al. (2008)].

Test scores are an amalgam of cognitive and noncognitive components. It's very hard to raise IQ after ages 8–10, but not so hard to raise the noncognitive components at later ages [Cunha and Heckman (2009); Cunha et al. (2010)]. When we recognize the malleability of noncognitive skills, we recognize the potential for interventions in those dimensions. We see another avenue for social policy.

Most of the focus of studies of the black–white gap is on gaps in test scores, which are substantially determined by noncognitive factors. Adverse family environments do not promote beneficial noncognitive traits. There is a real opportunity in American society to close those gaps. We need a better understanding of how gaps emerge and what their causes are.

The noncognitive aspects of human capabilities are the underresearched part of the story. Personality economics is underresearched. I see opportunities for going beyond the standard practice of dumping a bunch of test scores in regressions and predicting outcomes. We need to understand where noncognitive abilities come from, how they change, how predictable they are of a range of behaviors, and how stable they are.

Ginther: As you know, I share your interest in research on children. I'm looking forward to seeing the results. Thank you so much.

Heckman: Thank you.

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